

**THE CONSTRUCTION OF MEANING IN THE
CORRESPONDENCE OF CHARLES DARWIN**

By

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Abstract

This thesis analyses the process of construction of linguistic meaning from a diachronic perspective. The analysis is based on the theories of intertextuality and social construction and applies a corpus analysis of collocations and paraphrases of key notions discussed in Darwin's correspondence. In particular, the focus of the analysis is on the terms *species* and *varieties* with the aim to observe how the meanings of these terms are formed in a process of social negotiation. The thesis analyses the difference in the meanings of these terms, but also focuses on the diachronic dimension of their use in the correspondence, which allows for the observation of how different interpretations of meanings emerge in discourse. Thus far, diachronic studies in the field of corpus linguistics focused on comparing different historical corpora rather than observing the diachronic change of the immediate contextual environment of particular terms. The results presented in this thesis show that the meanings of terms are not only formed of different interpretations in discourse, but that these interpretations can be specific to particular temporal spans in discourse.

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1 Introduction

*What's in a name? that which we call a rose
By any other name would smell as sweet
(Shakespeare, Romeo & Juliet)*

1.1 Introduction

The aim of this thesis is to analyse the diachronic aspect of intertextual construction of meaning in the correspondence of Charles Darwin. This is achieved by a corpus analysis of collocation and paraphrases of key notions discussed in Darwin's correspondence, in particular, the notions of *species* and *varieties*. Namely, as the correspondence corpus offers the possibility to observe discourse as a diachronic phenomenon, collocation and paraphrase analysis will demonstrate that certain meanings, i.e. interpretations of meaning realised as paraphrases, are specific to particular diachronic points in discourse. In addition to collocation and paraphrase analysis, the thesis focuses on the analysis of markers of intertextuality, i.e. expressions which explicitly link subsequent paraphrases to previous discourse. This will demonstrate, not only that meaning changes diachronically, but that the change is the product of negotiation in discourse. From the diachronic perspective, this analysis will not only show that certain meanings are added in new discourses, but also that certain meanings are abandoned in later discourses. In this sense, meaning and language are seen as social rather than psychological phenomena.

The issue of meaning has been the focus of study of many philosophers who discussed it at lengths to discover its very nature. Meaning itself is not a uniform concept; sounds, images, words, objects, actions, intentions, colours etc. can all have meaning, mean multiple things at the same time and even be meaningful in different ways. Ruth Millikan (2004) devoted a whole book on different varieties of meaning and how they relate to one another. However, this thesis does not attempt to come up with a theory of meaning, or even with an answer to the question of what meaning is. The focus of this thesis is on linguistic meaning, or rather how meaning is constructed in language. The aim of the thesis is to demonstrate that meaning in language is constructed socially by never-ending negotiation. This will be demonstrated by analysing the correspondence of Charles Darwin with the aim of showing that even when describing the natural world, the meaning of the words used is constructed in discourse.

As Darwin's focus was on the origin of species, this thesis examines the origin of meaning, that is, what makes the noises and images of language meaningful. Although Darwin is celebrated for his theory of evolution, the questions he set out to answer, i.e. the origin of species, and more importantly what species are, are still a matter of debate. Ever since Plato and Aristotle, theories have been proposed to answer the question of species. From the moment language started to be considered as separate from the surrounding world, this question got a new dimension, namely whether species actually existed in nature as natural kinds rather than as categories of language.

The aim of this thesis is not to answer the question of whether species actually exist in the world, but to demonstrate that linguistic meaning is a product of social interactions in discourse rather than a mirror physical reality, or innate to human beings. For example, most linguists would agree that a word like *chair* is merely a conventional symbol representing a particular object, having no direct relation to the object itself. In other words, any other symbol could be used in its place, like *Stuhl* in German or *stolica* in Croatian language. However, the origin of the meaning of these symbols is a matter of debate among linguists. For some cognitive linguists based around Noam Chomsky's theory of universal grammar, the meaning of such symbols is innate in some primordial mental language of concepts that all humans possess (see Section 2.1.1.2 Cognitivist approach). Furthermore, theorists like John Searle (2010) argue that these concepts are based on 'brute facts' and the reality that surrounds us. On the other hand, the approach taken in this thesis is based on theories which define the meaning of such symbols as a product of interactions in discourse. In this sense, just like the symbol itself, its meaning is defined as convention, thus it is contingent and constantly (re)negotiated in discourse.

The aim of this introductory chapter is to firstly provide a background of the study, namely state the theoretical and empirical assumptions the research is based on. Secondly this chapter aims to offer a rationale for research into the issue and outline the main research goals.

1.2 Background

This research builds, to some extent, on the more recent work of Wolfgang Teubert, published in his book *Meaning, Discourse and Society* (2010) and the work of his former PhD student Lisa Mei Ling Cheung (2009) on collaborative knowledge construction. Teubert's (2010) argument is that linguistic meaning is a product of language itself and that there is little evidence of any discourse-independent or universal meanings. This argument contradicts cognitive theory of language as realisation of mental concepts (See Section 2.1.1 Language as a psychological phenomenon). Similarly, Cheung's (2009) thesis focuses on the processes of collaborative knowledge construction, examining how students negotiate and develop meaning by discussing the definitions of concepts on web bulletin boards.

My thesis is similar to their work in the sense that it presents an argument that linguistic meaning is a product of social interactions. The thesis focuses on a discourse of scientific correspondence, namely that of Charles Darwin, and analyses the language surrounding Darwin's notion of *species*. My approach is different in that it takes a single notion, i.e. the term *species*, and observes its negotiation in discourse diachronically. The aim of such analysis is to observe the instances of the term *species* over the whole period of Charles Darwin's correspondence in order to demonstrate, firstly, that meaning of the term changes over time and, secondly, that the formulation and the change of meaning occurs in discourse. Thus, the thesis aims to support Teubert's (2010) and Cheung's (2009) arguments that linguistic meaning is a product of discourse.

Consequently, by demonstrating the process of meaning construction in discourse, this thesis argues against realist and cognitivist theories of meaning construction, such as those by Searle (2010) and Chomsky (2006) (See section 2.1.1 for more details). For example, according to such approaches to the construction of meaning, as natural objects, species must be something occurring in nature even if they are not discussed in discourse. Thus species are merely discovered; they are not invented, just like Darwin reports in his letter to his cousin William D. Fox: “Of Course you have heard of the new species of wild Swan, discovered in England, by Mr. Yarrell” referring to a discovery made by the famous British ornithologist. Although from such a statement it is implied that species exist independently of human interaction, declaring something as a new species may be seen as a discourse action. For example, because of Mr Yarrell’s expertise, Darwin expresses no doubt about the truthfulness of this news, on the contrary, he adds: “Yarrell himself, has pronounced it to be the new sort, so there can be no doubt.”¹ However, even in Searle’s (2010) theory, pronouncing an object a new species can be seen as an act of *declaration*, which is in itself an act of constructing social reality.

The aim of this thesis is to demonstrate that the meaning of the word *species* is formed in discourse regardless of whether species actually exist as discourse-independent natural kinds. As Roland Barthes (1968, p. 10) states ‘objects, images, patterns of behaviour can signify, and do so on a large scale, but never autonomously; every semiological system has its linguistics admixture.’ Thus

¹ [www.darwinproject.ac.uk/letter/entry-00081]

the meaning of discourse objects *swan* or *species* is not based on the properties of the natural objects, but it is rather constructed in discourse.

Ferdinand de Saussure (1974, p. 116) in his *Course in General Linguistics* proposed that meanings are language specific, which he demonstrated by turning to the variety of languages: '[i]f words stood for pre-existing concepts, they would all have exact equivalents in meaning from one language to the next; but this is not true'. Thus, for example, in English language tells apart *cod* from *haddock* in its everyday use, whereas they are both translated as *bakalar* in Croatian.

Furthermore, as Teubert (2010, p. 11) notes, the discourse-external reality will not tell us whether an elevation is a *hill* or a *mountain*, because 'nature does not come with categories.' Teubert's argument that categories 'hill' and 'mountain' are products of discourse is essentially nominalist. Nominalism, unlike Searle's (2010) proposition of different principles for constructing social reality and reporting on external reality, allows language to operate on the same principles regardless whether the discussed object exists as a natural object or a cultural institution. As it seems unlikely that the process or mechanism of meaning construction is domain specific (i.e. different in literature and law as opposed to biology, physics, etc.), nominalism appears to be more likely proposition for meaning construction. Furthermore, proposing meaning as a convention formed in discourse allows us to analyse its construction empirically.

Joseph Dalton Hooker, a botanist and a friend of Darwin's, demonstrates in a letter to Darwin how the meaning of the word *species* is a matter of discourse convention: "Species (or what we call species) may be mutable."² Here a fellow scientist of Darwin's indicates the possibility that it is the discourse construct we refer to as *species* that may be mutable. Darwin often discussed the classification of species with the fellow naturalist and the curator of the Zoological Society of London, George Robert Waterhouse. In the opening of one of such letter, Darwin expresses his opinion and asks others to freely criticise it, or as he puts it – 'hoot at' it, just as much as he ought to have criticised their opinions earlier:

"According to my opinion, (which I give every one leave to hoot at, like I should have, six years since, hooted at them, for holding like views) classification consists in grouping beings according to their actual relationship" [www.darwinproject.ac.uk/letter/entry-684]

Darwin's argument corresponds with the argument proposed by the Russian Marxist philosopher, Mikhail Bakhtin, who argued that every utterance responds to previous one but also seeks a further response. If the utterance did not relate to previous and future utterances it would be meaningless (Bakhtin, Holquist, & Emerson, 1986, p. 72). The above example demonstrates that Darwin's opinion is indeed a reaction to what others have said six years before him and invokes further dialogue on the issue. For this reason, Bakhtin describes language as dialogic since '[l]anguage acquires life and historically evolves [...] in concrete verbal communication, and not in the abstract linguistic system of a language,

² [www.darwinproject.ac.uk/letter/entry-00784].

nor in the individual psyche of speakers' (Bakhtin, et al., 1986, p. 95). Thus, to Bakhtin, language has to be expressed to be alive and to develop – it cannot live and develop in the mind.

However, we can observe Darwin's realist stance in this example, proposing that classification is based on *actual relationship*. In this sense, the meaning of *species* is formed as a representation of a true natural kind; it is based on what Searle (1995) calls a 'brute fact' rather than constructed as a contingent category of discourse. As brute facts are not directly represented by language, Searle and other cognitivists, particularly Jerry Fodor (1975) and Steven Pinker (1994), argue that they are represented as mental concepts. From Darwin's correspondence, we can infer that at times Darwin describes *species* (or what we call *species*) as clear natural kinds which are just a matter of description:

"I shall bring home a very great number of undescribed species both from Brazil & the Rio Plata. — It may be a foolish fear, but I often wonder, if any person will be found who will describe so many minute insects." [www.darwinproject.ac.uk/letter/entry-227]

Thus Darwin believes, in line with Cartesian reasoning, that we understand what species are and that we are perfectly able to tell them apart. It is only classification and systematization that are negotiable; at least until the peer community of biologists agree on a 'scientific' method of distinguishing between *species* and *varieties*.

Since empirical research can only be done on observable data, an abstract system and mental concepts are not helpful constructions for the analysis of how meaning is constructed in language. Meaning can even be described as a precondition for discourse, since without meaning discourse would be just

noise. Thus, one is the precondition for the other and vice versa, or just as Teubert notices: 'it is meaning that creates society, it is society that gives rise to meaning' (Teubert, 2010, p. vii). Although Teubert here focuses on society rather than discourse, it can be argued that society would not exist without discourse just as discourse cannot exist without society. It is the social interaction, i.e. discourse, that creates and demonstrates meaning, at least the linguistic meaning. Hence, if we accept Barthes' premise that there is no meaning without some linguistic admixture, we can conclude that to look for meaning one has to look into discourse.

Although we can utter infinity of possible meanings, as most of the daily social interaction is relatively routine we tend to repeat ourselves. Sinclair (1991, p. 108) noticed discourse is mostly "made of the occurrence of common words in common patterns, or in slight variants of those common patterns.' Despite the infinite number of possible linguistic combinations, certain words tend not only to occur together, but have particular meaning depending on that combination: '[m]ost everyday words do not have an independent meaning, or meanings, but are components of a rich repertoire of multi-word patterns that make up text. This is totally obscured by the procedures of conventional grammar' (1991, p. 113). Furthermore, according to Sinclair (ibid), frequent words, or frequent senses of words, 'have more ambiguous meaning than less frequent words or senses.' However, in a sentence, they are very rarely ambiguous, which indicates that the usage, that is discourse, determines the meaning. Corpus linguists have long argued that the meaning of words can be identified from the way they are used in discourse and have thus revolutionised modern

lexicography. The theories of intertextuality, social constructivism and constructionism, hermeneutics and various discourse studies have similar assumptions on meaning, discourse and society.

In the never-ending discourse, linguistic meaning cannot be fixed and is perpetually renegotiated. Julia Kristeva when forming her theory of intertextuality adopts this stance and describes text as a state of production, rather than a finished product. Influenced greatly by the work of Bakhtin, Kristeva sees text as production irreducible to representation since the subject is involved into the production of meaning (Kristeva & Moi, 1986, p. 86). Thus discourse is not a representation of meaning coming from another source like the mind for example, but the 'space' where discourse members produce meaning.

Kristeva (1986), similarly to Sinclair, proposes that, for the study of linguistic meaning, the focus should be on the articulations of a word in a sentence in order to observe 'the same functions and relationships at the articulatory level of larger sequences'. Kristeva (ibid) argues that the meaning of the word is essentially the totality of its articulation in discourse. However, the proposal to look for recurring patterns, as well as the practice established by corpus linguists in lexicography, tells us that it is not the totality of the articulation that creates the meaning but rather the repetitions that remain in the constant renegotiation of meaning. To some extent all articulations add to the meaning, but it is only those articulations that are accepted and repeated by the discourse community that constitute the meaning of a word. This inclusion of others'

words, the mosaic of quotations, intertextuality, in a new utterance creates the meaning.

British geneticist, Richard Dawkins (2006) in his book *The Selfish Gene* compared this process to genetic transmission and related the natural and linguistic evolution. Since natural evolution occurs in the process of genetic replication, Dawkins proposed a unit of cultural replication, or imitation, which he called a *meme*. Like with the gene, Dawkins (2006) stressed that the longevity of a particular unit is not as important as its fecundity. Thus, for a particular unit of meaning it is most important for it to be replicated, i.e. repeated in discourse, since this replication 'affirms continuity and complexity as recipe for long-term success and evolution, but with similarity *and* some difference as the all-important leaven' (Dawkins, 2006, p. 103). In other words, through repetition, but with some difference, new patterns start to occur, thus expanding the meaning of the unit.

Consider the following example from Darwin's letter to Joseph Dalton Hooker, in which Darwin expresses his uncertainty about the recent work of his colleague, Swainson, particularly expressing uncertainty about the meaning of the phrases 'typical forms' and 'wide ranges'. However, when a similar mark is made by Waterhouse, whom Darwin regards highly, the statement, in line with Dawkins' theory, becomes more believable. Thus the longevity of Swainson's statement is clearly less important than its fecundity:

"Swainson has remarked (& Westwood contradicted) that typical genera have wide ranges: Waterhouse, (without knowing these previous remarkers) made to me this same observation: I feel a laudable doubt & disinclination to believe any statement of

Swainson's, but now Waterhouse remarks it, I am curious on the point. There is, however, so much vague in the meaning of "typical forms" & no little ambiguity in the mere assertion of "wide ranges", (for zoologist seldom go into strict & disagreeable arithmetic, like you Botanists so wisely do) that I feel very doubtful, though some considerations tempt me to believe in this remark."[www.darwinproject.ac.uk/letter/entry-744]

In the following example, Joseph Dalton Hooker is just as confused about the meaning of 'typical form', but in his reply he tries to contribute to the discussion by expanding on the remarks of Swainson and Waterhouse that Darwin had inquired about. Particularly interesting is how he reformulates Swainson's statement that '*typical genera have wide ranges*' into that the most typical genera are the ones '*most widely diffused*', illustrating the combination of repetition with a degree of difference to formulate a new meaning. Finally, in the last sentences of the example below, Hooker provides Darwin with an alternative formulation which he also expands with his criticism:

"I hardly know what is always meant by a typical form. The character of a group should be founded on the most important objects it contains in the œconomy of nature. The most important genus of a class is surely generally either the largest or the most widely diffused; if the largest genus is the type, we have already seen that large genera are generally most widely diffused. The type of a group often turns out (on extended knowledge of that group) to be the most aberrant form in it.—

Perhaps Swainson has put the cart before the horse & should have said "a typical group or genus is that which is the most widely diffused"—

Some however I think define typical forms as those which are most fully developed or what they call most perfect, now though it may be very easy in any group to point out many which are not the most perfect or fully developed, a great many remain amongst which it is difficult to say which has the advantage of the other in organization."
[www.darwinproject.ac.uk/letter/entry-745]

Darwin acknowledges to Hooker that his reformulation made it clearer for him to understand, not only what Swainson and Waterhouse meant when using the phrase ‘typical forms’, but actually how typical forms of genera and species are determined in botany and zoology:

“You seem, however, to have put the case of “typical forms”, in a clearer point of view, than I ever saw it & stripped the word of half, if not all its mystery: I have long suspected that typical & abnormal forms consist only of those, in which a greater or less variety have been created or modified— with this excellently!! expressed sentence, I will conclude” [www.darwinproject.ac.uk/letter/entry-746]

From these examples we can observe how the meaning of ‘typical forms’ is shared in the in the discourse between Hooker and Darwin, but also Swainson and Waterhouse who are by reference a part of that shared discourse since it is their work that is being interpreted. Thus, the above examples represent a series of interactions in which the meaning of the notion of ‘typical forms’ is not merely interpreted, but rather constructed or defined in a process of collaborative negotiation.

The analysis presented in this thesis is based on the principles shown in the examples above. Namely, the thesis takes the approach that linguistic meaning is a product of social interaction, much in the way the notion of ‘typical forms’ was defined through the interactions of Darwin and his correspondents. In a sense meanings are formed in relation to one another in discourse. Thus by analysing the discourse, or rather the types of interpretations of meaning such as those demonstrated by Darwin and his correspondents in the examples above, we can observe the processes of the construction of meaning.

1.3 Research goals

The aim of this thesis is to show that the processes of meaning construction are collaborative acts of meaning interpretation and reformulation. This thesis focusses on the notions of the construction of meaning, knowledge and reality in discourse. The key argument is that apart from the discourse, there is nothing else available to linguists to study how the meaning is constructed and negotiated. The minds of Charles Darwin and his correspondents are not available, even if they were still alive, for us to analyse. Thus, the thesis will analyse the discourse of Charles Darwin's correspondence in order to find evidence to support the hypothesis that meaning is a result of perpetual negotiation in discourse.

The first goal of the analysis is to analyse the intertextual and dialogic aspect of meaning construction and negotiation. This analysis is based on the theories of dialogism (Bakhtin, et al., 1986; Bakhtin, Morris, Voloshinov, & Medvedev, 1994) and intertextuality (Barthes, 1968; Barthes & Heath, 1977; Kristeva & Moi, 1986; Orr, 2003). These theories imply that the meaning of discourse depends on social interaction and negotiations. Thus the aim of the analysis is to demonstrate that the meaning of the term *species* is formed in the correspondence of Charles Darwin through negotiation in discourse.

The second goal of the analysis is to demonstrate that meaning is a diachronic property of discourse. Namely, by assuming dialogism, intertextuality and social construction of meaning, the meaning is locked in a constant state of renegotiation. Thus the aim of the analysis is to demonstrate the diachronic

aspect of meaning of the term *species*. Namely, the aim is to demonstrate that certain interpretations which form the meaning of the term *species* are limited to a particular temporal span in discourse and gradually disappear from the general discourse.

1.3.1 Research Questions

The research goals outlined in this section can be summarised in the following research questions.

1. Is there any evidence to suggest that the meaning of the term *species* is a product of a generative language system and thus independent from discourse?
2. Is there any evidence to suggest that the meaning of the term *species* is a product of negotiation in discourse?
3. Is there any evidence to suggest that the meaning of the term *species* is either synchronic or diachronic? In other words, does the meaning of the term *species* change over time?

2 Literature review

2.1 On the origin meaning: approaches in language theory

This chapter provides a historical overview of the development of linguistics regarding its relation to the construction of meaning. Starting from Saussure's definition of a linguistic sign as the smallest unit of meaning, constituted as a dichotomy of concept and signifier, the development of linguistics was followed by so-called *cognitive* approach formed around Chomsky's theory of universal grammar. In reaction to structuralist and later cognitivist theories, Marxist theorists developed a social approach to language that views language not as a psychological system, but rather as a system of social relations and power.

Corpus linguistics focuses on the analysis of large amounts of language in use, i.e. discourse, and where meaning is concerned, it focuses on its realisation in discourse. The most prominent application of these methods has been achieved in the field of lexicography. Thus corpus linguistics is a suitable approach to the study of meaning. However, hitherto, few attempts have been made to observe the processes of meaning construction, with the focus mostly being the meaning of particular lexical items.

In this thesis, corpus analysis is underlined by the theoretical formulation of language as a social phenomenon in order to support the approach to the diachronic analysis of the construction and change of linguistic meaning. Thus it is necessary to review different theoretical approaches to language and

meaning in order to support the argument that linguistic meaning is a social and contingent construction.

Firstly, the theoretical approaches that define language as a psychological phenomenon which acts as the underlying mechanism for all language output will be reviewed. The aim of this section (2.1.1 Language as a psychological phenomenon) is to argue why such theoretical approaches are incompatible with the corpus approach to the diachronic analysis of meaning construction presented in this thesis. The section is characterised by theories that argue for an abstract language system, often defined as part of the mental capacity of the speaker, which governs both the production and interpretation of meaning.

In contrast, a review of theories that define language as a social phenomenon is used to support the argument that meaning is a property of discourse, and as such it is not only socially constructed, but also perpetually renegotiated. This section (2.1.2 Language as a social phenomenon) is characterised by theories which argue that meaning is formed in the discourse, thus arguing that it is the social aspect of language that maintains the structure of language and ensures both the production and interpretation of meaning.

Finally, the key aspects of the theories covered in this chapter are summarised and contrasted in relation to the study of linguistic meaning and its construction.

2.1.1 Language as a psychological phenomenon

The theories reviewed in this section are characterised by their approach to linguistic meaning as a product of an abstract language system. The main two

approaches reviewed are the *structuralist* approach developed from the work of Ferdinand de Saussure and the *cognitive* approach developed from the works of Noam Chomsky. The common aspect of these approaches is that they view language as a dichotomy of language in use, i.e. discourse, and the underlying language system which governs the production of discourse. Thus, meaning is independent of the individual and usually operates on a subconscious or unconscious level.

The assumption that language operates on a subconscious or unconscious level leads to conclusions that language system is a mental structure; and for some even an innate structure. This implies that humans are biologically wired to possess a language, with the differences between languages being only superficially different. The linguistic meaning is thus a product of unconscious deeper structures which the language user is unaware of. For this reason, system oriented approaches often ignore discourse as only a product of deeper structures focusing rather on the structures that govern its production, such as phonology, morphology, syntax, etc. The focus on structures of phonemes, words or sentences leads to study of language as a *monologic* phenomenon, where no discourse participants or exchanges are relevant to the analysis. The construction and interpretation of meaning is a mechanical or a computational process where structure and lexis are combined to realise meaning. In this sense, language production is independent of its main function of social communication.

Approaches to language as a psychological phenomenon are reviewed in order to be contrasted by the approaches to language as a social phenomenon. The

aim is to demonstrate the inability of these approaches to define and account for all aspects of meaning construction and contrast these with the social and discourse approaches to language. Specifically, as the aim of the thesis is to argue for a diachronic approach to the study of meaning construction, negotiation and change, it is necessary to demonstrate that these are major aspects of meaning construction which the theory of language as a psychological phenomenon cannot account for.

2.1.1.1 Structuralist approach

In *Course in General Linguistics*, Ferdinand de Saussure (Saussure, et al., 1974) redefined the science of language by defining the linguistic sign as the object of its study, which was to be studied as a part of a language system, or *langue*, rather than as it occurs in discourse, or *parole*. Saussure argued that it is not the spoken language that is natural to the man, but the faculty responsible for its production (Saussure, et al., 1974, p. 10). Thus, language in general was divided into two distinctive planes: *langue*, or the language system, and *parole*, or discourse. The language system, *langue*, is defined as the unconscious part of language, only passively registered by the individual, whereas *parole*, or discourse, is defined a deliberate act of will and intelligence realised by an utterance (Saussure, et al., 1974, p. 14). This division of language into discourse and linguistic system is still a foundation of many modern approaches in linguistic science. For example, a similar view on the aspects of language is taken by Noam Chomsky (1957, 1965) who divides language into the aspects of *competence* and *performance*, which correspond to Saussure's *langue* and

parole respectively, in the sense that the former is the unconscious systematic plane of language and the latter its realisation in the utterance.

Saussure (1974) describes *langue* as the governing mechanism which rules the production of *parole* and makes utterances structured and comprehensible. Similarly, Chomsky describes his language system as 'constituted by rules that interact to determine the form and intrinsic meaning of a potentially infinite number of sentences' (2006, p. 62). Thus, although the speaker may choose what to say, the structures of the language system, of which the user is unconscious, determine how that something is uttered.

Another important development presented in *Course* was Saussure's formulation of the linguistic sign as a dichotomy of the *signifier* and the *signified* which disassociated the linguistic sign from the natural object it represents. Specifically, the objects of the world are recreated in language as symbolic representations to which specific sounds and images are assigned, called *signified* and *signifier* respectively. The Stoics have long before Saussure disassociated the sound-image of a word from its meaning; defining the relation between the two as the product of convention, consensus, and reason (Harris, 1993, p. 12). However, for Saussure (1974), the signified is not a mental replication of a natural object, but a construct of an abstract language system in which the meaning of signs is defined through their systematic relations. Specifically, Saussure defined the *signified* negatively as what distinguishes it from all the other *signifieds*. Thus, Saussure never defined what *signified* actually is, in the sense that, for example, cognitive linguists define mental

concepts, but focused his analysis on the study of signs, of which *signifier* and *signified* are just its two aspects.

For Saussure, the meaning of linguistic signs is not based on the relation between the *signifier* and the *signified*, since then language would be reduced to mere nomenclature (Saussure, et al., 1974, p. 112). The relations between signs are defined as structures that form the language system, *langue*. Discovering these structures was the goal of Saussure's linguistic research, which became commonly known as *Structural Linguistics*. To Saussure (1974, p. 120) both the concepts-meanings and the sound-images of language, i.e. the *signifieds* and the *signifiers*, constitute the structures of the language system, but also at the same time they are the products of that system:

'Whether we take the signified or the signifier, language has neither ideas nor sounds that existed before the linguistic system, but only conceptual and phonic differences that have issued from the system.'

To demonstrate his point that meanings arise from the language system, Saussure (1974, p. 116) turned to the variety of languages and the lack of concord between concepts in different languages most evident in translation: 'If words stood for pre-existing concepts, they would all have exact equivalents in meaning from one language to the next; but this is not true.' As an example Saussure used the French word *mouton* and its English counterparts *mutton* and *sheep*. Where the English language has two signs, one for the animal and the other for the type meat, the French language only has one. Similarly, Teubert (2010, pp. 55-56) offers the example of the German words *Kummer*, *Trauer*, and *Gram*, which are translated into English as either *grief* or *sorrow*. Another example would be the English verbs *lend* and *borrow* which are both

translated in Croatian with the verb *posuditi*, which does not make any difference between the acts of giving or receiving. To Saussure that is sufficient evidence to conclude that words do not refer to a set of universal concepts or signifieds, but that these are determined by the language they are expressed in. Similarly, sound-images of the language, i.e. the *signifier*, also come from the system; for example, the differences between voiced and voiceless sounds can only be realised in relation to one another. The systematic relations of sounds were particularly well investigated by Roman Jakobson (1978) who argued that although acoustics can provide an image of each sound in great detail, it cannot interpret that image; it cannot tell whether a particular sound is a variation of another or if they are two different sounds. Furthermore, Jakobson (1978) demonstrated that the selection of each sound restricts the selection of the sounds that follow it, thus the sounds of a language form systematic relations – they form signs. As signs, they are distinct from one another not based on their acoustics, but on their relation to other signs of the system.

The relation between signs within the system Saussure (1974, p. 113) defined as *value*: '[a]s an element in a system, the word has not only a meaning but also – above all – a value.' Value involves the similarities and dissimilarities between particular signs, which help define their particular meanings (ibid). However, Saussure extended the notion of value beyond the plane of the language system to the plane of discourse. The reason for this lies in the diachronic dimension of discourse – sounds and ideas are neither uttered nor perceived instantly, but in succession and '[a]most all linguistic units depend either on what precedes or follows in the spoken sequence, or else on the

successive parts of which they are themselves composed' (Saussure, et al., 1974, p. 126). In the sequence of the utterance, signs are also realised in syntagmatic relations, which complement the paradigmatic relations stemming from the system. For example, Jakobson (1978) demonstrated that a selection of one phoneme limits the selection of the following. Thus, Saussure does not see a clear boundary between *langue* and *parole* when syntagmas are considered, thus accepting many syntagmatic relations are product of both *langue* and *parole* in proportions that cannot be accurately measured (Saussure, et al., 1974, p. 123).

Although Saussure assigns the construction of the meaning of linguistic signs to the language system, he also expands the process of meaning construction with the notion of *value*. The distinction between meaning and *value* is not clearly defined by Saussure, but by including the syntagmatic relations he adds another dimension to the formulation of meaning in language, i.e. discourse or *parole*. However, Saussure's formulation of the linguistic sign without positive terms allows him to focus on the structures of *langue* and ignore the syntagmatic aspects of *value* realised in *parole*.

Analysing discourse is, for Saussure, in the domain of historical linguistics as discourse interactions effectively represent historical events. The comparison of different historical discourses leads to the study of diachronic events, rather than synchronic states of language (Saussure, et al., 1974, p. 87). Saussure supports his argument with an analogy to a chess game, where the state on the chess board corresponds to the state of language. Thus, in a game of chess, the rules of the game, analogue to the structures of the system, govern the

possible position of the pieces. The knowledge of the rules is perfectly sufficient for any observer to understand the current state at any point in the game. Saussure (1974, p. 88) argues that the observer who has observed the game from the beginning does not gain any advantage from the diachronic perspective in understanding the current position. In other words, the observer who understands the rules of chess will, according to Saussure, assess the current situation on the chess board just as well as someone who had the chance to observe the diachronic evolution of the game.

In this sense, Saussure acknowledges that languages evolve and change, but rejects the study of these diachronic changes defining them as mere realisations of the underlying system. Thus the systems primordial status as discourse production mechanism is confirmed, as all linguistic changes are essentially products of that system. However, as discourse is always the product of the system, one can observe discourse as a series of diachronic language events and look for certain consistencies in order to observe its structures. To Saussure and structural linguists, this is the process by which children acquire language. Saussure's the notion of 'grammatical' implied 'synchronic' and 'meaningful' (Saussure, et al., 1974, p. 133); thus discourse is just the means to study linguistic continuity which arises from the system and forms the system and meaning.

In order to study the system through discourse, which essentially encompasses all human interaction in the infinite number of possible utterances, Saussure (1974, p. 89) argues that only those linguistic changes that exhibit a form of continuity, i.e. the ones that become a part of the system, and hence

synchronic, should be studied. Otherwise, diachronic point of view is not examining language but only 'a series of events' (ibid). These historical events are objects of the discourse, which is infinite in the number of possible events but also endless – the discourse always continues. As the new discourse always replaces the old, Saussure concludes that no synchronic phenomenon has anything in common with any diachronic phenomenon, since one is a relationship between simultaneous elements, and the other a substitution of one element for another in time (Saussure, et al., 1974, p. 90). In other words, the relevant aspects of language are the stable values of language that apply as much in the present as they did in the past, whereas the changes of written form or sound merely replace the old form for the new. For this reason, Saussure argues that diachronic changes are insignificant in comparison with linguistic continuity as only 'a synchronic law simply expresses an existing order' (Saussure, et al., 1974, p. 91).

Saussure's system is one of linguistic signs, where both signified and signifier, i.e. content and form, are products of the system. This system is symbolic and both the sounds and ideas of the system come from it. However, its origins, for Saussure, come from the practice of speech, i.e. discourse. Namely, Saussure noticed that in language there are many series of phrases and sentences that the individual does not combine himself but have become a part of language (Barthes, 1968, p. 19). These patterns come about through repetition and imitation by members of a discourse community until they become a part of language. Thus the values of a system are formed by usage and general agreement in discourse: '[n]othing enters the language before having been tried

out in speech. [...] a speaker had first to improvise it, and others to imitate it and repeat it, until it became accepted usage' (Saussure, Bally, Sechehaye, Riedlinger, & Harris, 1983, p. 167). From this we can see that discourse plays an important role in the formation of the language system. Roland Barthes (Barthes, 1968, p. 15) affirms this premise saying that 'there is no language without speech, and no speech outside language'; one cannot exist without the other.

Defining the system as accumulated through discourse implies that it is a collective phenomenon. Thus Saussure (1983, p. 13) argues the system exists in 'in the brains of a group of individuals; for the language is never complete in any single individual, but exists perfectly only in the collectivity.' Similarly, Barthes argues that as a collective phenomenon the system can only exist perfectly 'in the speaking mass' (Barthes, 1968, p. 16). Thus, initially, Saussure argued that the language system is the social part of language, external to the individual, who by himself is unable to create or modify it. Furthermore, the structures of *langue* are not something an individual is aware of. Thus, for Saussure, the unconscious of language was collective and not individual.

However Saussure (1974, p. 19) was not consistent with his formulation of the system stating later that *langue* 'takes the form of a totality of imprints in everyone's brain, rather like a dictionary of which each individual has an identical copy.' This latter formulation by Saussure implies that *langue* is a psychological phenomenon and that the speakers of language are unaware of the structures of language that govern the formulation of meaningful discourse. Thus despite initially forming the system 'in the collectivity', Saussure placed it

inside the brains of individuals. In this other sense, language system is identical for each member of the language community. Thus Teubert (2010, p. 46) argues that because of this placing of the system in the brains of individuals Saussure can be considered as 'a precursor to cognitive linguistics, but not as a cognitive linguist.'

As we have observed so far, for Saussure, meaning is the product of the relations within the language system. This is best exemplified by Saussure's formulation of binary oppositions (Saussure, et al., 1983, p. 120) – Saussure's system is one without positive terms, only oppositions and differences exist between the elements of the system; where there is synonymy, there always is a degree of difference and opposition. Thus, for example, the linguistic sign species acquires meaning from its syntagmatic and paradigmatic relations to other signs. If we consider associative relations, that is, the paradigmatic relations of the sign species, we may come up with signs such as *varieties, genera, kind, race, breed* etc. Each of these signs is conceptually related to the sign *species*. These paradigmatic relations between *species* and other related signs determine its meaning in the system; whereas syntagmatic relations contribute to the meaning of *species* in the context of the utterance.

The division of language into the planes of discourse and language system allowed linguists to study language as a system of laws similar to the laws of universe. The proposition of two different aspects of language had significant impacts on the construction of linguistic meaning. As the system was seen as the generative mechanism of language, it was natural to assume that linguistic meaning is its product. This shift of the analysis of language towards the

unconscious structures allowed for the first time in social sciences for the observer to separate himself from the object observed. This level of “scientific objectivity” was only made possible by Saussure’s formulation of language as a system without positive terms.

French anthropologist, Claude Lévi-Strauss (Johnson, 2003) was particularly impressed by the development in structural linguistics and wanted to extend the application of structural analysis not only to anthropology but all humanist sciences. Lévi-Strauss saw society as a system of communication, and as individuals are unaware of the structures of the language, they are also unaware of the underlying structures of their social interactions. Thus, as these structures are unconscious they ‘precede individual agency and resist historical contingency’ (Johnson, 2003, p. 107). Similarly, Emile Durkheim argued that collective ideas are independent in relation to the individual, with the cultural forms such as marriage and kinship demonstrating a ‘deeper reality’ operating unconsciously and underlying all social phenomena (Durkheim & Allcock, 1983; McCarthy, 1996).

Lévi-Strauss (1987, pp. 33-36) constructed his theory around the idea of a ‘mental unconscious common to all subjects, regardless of cultural variations.’ This unconscious Lévi-Strauss described as the deep structures of language revealed by structural linguistics, which guaranteed ultimate intelligibility of different systems of representation radically different from our own; for example, there is no human language so remote that we would be unable to learn it (Johnson, 2003, p. 70). Furthermore, Lévi-Strauss (Johnson, 2003) argued that by uncovering linguistic structures one can look into the deepest structures of

the human mind – which are also universal. Apart from the universality of social structures, there was nothing to explain why, in various cultures throughout the world, social institutions such as kinship and marriage rituals were so similar. Particularly the example of the universal incest prohibition among different cultures was central for the argument for universal mental concepts (Johnson, 2003). Similarly, Jakobson (1978) posited the question whether language, although a social phenomenon, is ‘in some way imposed on us by nature?’

For structuralists, the appeal of studying an unconscious abstract system of language was that it represented itself as a system akin to the laws of the nature. The innateness and universality of such system meant that language was a natural object rather than a product of human activity. In other words, unlike works of literature, a systematic unconscious system of language exists regardless of human action, thus allowing the researchers to remove themselves from the object of their study. However, in that case, the language system must be considered as synchronic and monologic. A common unconscious language system can only function as a synchronic system so that it can account for all the historical and cultural differences shown in different languages. As a generative mechanism of language, the system is also inherently monologic since meaning is formulated in the unconscious and precedes the act of communication.

With respect to the analysis presented in this thesis, structural linguistic theory supports the study of discourse for features such as collocations as these features can be related to the linguistic structure to some extent. However, the diachronic and dialogic aspects of meaning construction, such as the process of

negotiation of meaning, are not supported by this theory. As mentioned above, diachronic change is seen as substitution of one element with another performing the same functions within the synchronic system. On the other hand, the dialogic dimension is completely ignored by this approach. Even with Saussure's alternative formulation of the system as a 'common unconscious', the construction of meaning is considered as realised in the system independently of the communicative event. Thus structural approach to language analysis completely ignores the main aspect and purpose of language – communication.

From the aspect of the construction of meaning, structural approach accepts that certain aspects of meaning are formed in communication, i.e. discourse. By ignoring the communicative aspect of language, this approach cannot support the analysis of meaning construction. But more importantly, by accepting and ignoring the fact that meaning is at least partly a product of communication, structuralist theory knowingly produces an incomplete answer to the problem of meaning construction. As such, structural theory cannot be used for the study of the construction meaning in language.

2.1.1.2 Cognitivist approach

Cognitive theories reviewed in this chapter are based on the work of Noam Chomsky and his theory of generative-transformational grammar (Chomsky, 1957, 1965). Although the aim of generative-transformational grammar was to introduce a new approach to the study of language, Chomsky's theory had

many implications on the origin of linguistic meaning. Furthermore, Chomsky's theory inspired theorists such as John Searle (1969, 1995, 2010; 1980), Steven Pinker (1994) and Jerry Fodor (1975, 1998), who further developed the cognitive approach to language and meaning.

Cognitive theories are important to review since they reject the two main assumptions this research is based on: a) that meaning is a product of social negotiations in discourse; and b) that discourse and meaning inherently have a diachronic dimension. Additionally, much like the propositions of Lévi-Strauss discussed in the previous section (2.1.1.1 Structuralist approach), the characteristic feature of these theories is that they also propose an innate language generative system. For these reasons it is necessary to examine these theoretical assumptions in detail in order to demonstrate the shortcoming of these theories in regard to meaning construction.

Noam Chomsky criticised the approach of analysing discourse in order to discover structures of language as the study of the empirical evidence itself, stating that was comparable to designating natural science as 'the science of meter readings' (Chomsky, 2006, p. 57). Chomsky's criticism was aimed at the approaches of American structuralism in the tradition of Leonard Bloomfield, which to Chomsky were too preoccupied with phonology and morphology and ignored the issue of meaning. Chomsky argued that a sign like 'apple' is not involved in meaning until it is realised in a structure like 'John ate an apple' or 'Did John eat an apple' (Chomsky, 1957, p. 71).

However, for Chomsky, meaning is not realised in utterances like these, but rather the underlying structures which make them meaningful, namely the

syntactic structures. Chomsky (2006, p. 27) argued that the problem with the 'surface structure' of a sentence is that it 'generally gives very little indication in itself of the meaning of the sentence'. Thus, rather than observing linguistic performance, Chomsky suggested that the goal of linguistics should be a direct analysis of linguistic competence as 'a system constituted by rules that interact to determine the form and intrinsic meaning of a potentially infinite number of sentences' (Chomsky, 2006, p. 62), which he labelled simply as *Generative Grammar*. In this aspect, Chomsky often refers to Wilhelm von Humboldt, defining this generative system as a system which allows the speaker to, in Humboldt's terminology, 'make infinite use of finite means'(1965, p. 7; 2006, p.15).

Chomsky argues that generative grammar is a system that every speaker of language has mastered (Chomsky, 1965). Similarly, Searle (1969, p. 13) argues that the knowledge of how to speak a language 'involves a mastery of a system of rules' which makes the use of the elements of that language regular and systematic. However, Chomsky does not imply that the speaker's intuitive knowledge of language renders all utterances accurate and grammatical (Chomsky, 1965, pp. 7-8). Namely, the mental processes of such a system are 'far beyond the level of actual or even potential consciousness (Chomsky, 1965, p. 8).' As opposed to Saussure and other structuralist approaches, the goal of generative grammar is not the study of what the language user reports about his knowledge of language use, but to specify the actual knowledge of language itself (ibid). Thus, to Chomsky linguistic *competence*, i.e. this knowledge of

language, is not something the language user demonstrates, but the generative structure behind the linguistic performance.

In this sense, an analysis of discourse cannot provide insights into the aspects of meaning construction in language, as individual utterances cannot show, according to Chomsky's theory, the true mechanisms of language production, and hence observe the processes of meaning construction. Thus, it is necessary to review the theories based on Chomsky approach in order to refute the arguments against the discursive analysis of meaning.

In *Aspects of a Theory of Syntax*, Chomsky (1965, p. 10) argues that the analysis of performance can only unveil as much as the understanding of competence permits. He supports this with the argument that the most significant advances in the analysis of linguistic performance came when they were based on investigating some sort of formal structure. He singles out 'bracketing into constituents of various types, that is, the "tree structure"' as the most obvious formal property of utterances (Chomsky, 1965, p. 12). To account for the difference between structures of performance and competence Chomsky introduced the notions of "surface" and "deep structures". As the name implies, surface structure is the observable sentence structure manifested in discourse through linguistic performance, whereas the notion deep structure refers to the underlying language system, i.e. competence. According to Chomsky, each sentence of a language like English will have a deep structure to which the surface structure relates (Chomsky, 2006, p. 93). Deep structures are, according to Chomsky, universal for all languages, which only differ in their surface structures. For Chomsky, analysing performance leads only to the

analysis of surface structures, whereas only by applying formal models, namely syntactic analysis, deep structures can be discovered. For such analysis there is no need to analyse actual linguistic performance, i.e. discourse.

Emile Benveniste(1971, p. 10) criticises Chomsky's approach arguing that such method cannot lead to an analysis of language: '[t]he segmentation of the statement into discrete elements does not any more lead to an analysis of language than the segmentation of the physical universe leads to a theory of the physical world.' On the other hand, Teubert (2010) criticises the idea of transformation of surface structures into deep structures by discussing the assertion from *Aspects of a Theory of Syntax* (Chomsky, 1965) that in terms of deep structure all sentences consist of subject noun phrase and a verb phrase, offering an example of sentence without subject noun phrase "Mir [dative] ist kalt" (German for 'I am cold', but literally closer to 'To me is cold'). For Chomsky, this sentence is a transformation of a deeper structure, e.g. "Es ist mir kalt" (It is cold to me). However, Teubert (2010, p. 35) argues that Chomsky's deep structures are effectively arbitrary, since one can transform any type of surface structure into any kind of deep structure.

Although Chomsky argues against the analysis of performance, i.e. discourse, like Saussure, he runs into the same problem of separating discourse and language system, where the construction of meaning is concerned. Namely, in his formulation of *value*, Saussure had to extend the notion to cover both the relations of the system and the syntagmatic relations of the utterance. Similarly, Chomsky had to acknowledge that both surface and deep structures are involved into construction of meaning:

It seems that both deep and surface structure enter into the determination of meaning. Deep structure provides the grammatical relations of predication, modification, and so on, that enter into the determination of meaning. On the other hand, it appears that matters of focus and presupposition, topic and comment, the scope of logical elements, and pronominal reference are determined, in part at least, by surface structure. (Chomsky, 2006, p. 97)

Thus, context and surface structure, which fall in the sphere of discourse, contribute to the determination of meaning. Furthermore, just as Saussure's value had to be defined on two axes, since clearly separating discourse and system creates a problem of meaning, Chomsky also cannot separate the contribution of discourse and system to the construction of meaning:

It is not clear at all that it is possible to distinguish sharply between the contribution of grammar to the determination of meaning, and the contribution of so-called "pragmatic considerations," questions of fact and belief and context of utterance. (Chomsky, 2006, p. 97)

Although the approaches based on the works of Saussure and Chomsky both argue strongly for the analysis of an abstract unconscious system of rules and laws, a complete separation from discourse seems impossible, especially where the construction of meaning is concerned. Chomsky's proposal of deep structure as a generative mechanism for production of meaning fails almost immediately as the key aspects of the meaning of the utterance are completed by what he describes as surface structure. Furthermore, Chomsky's structures are mainly focused on the syntax of utterances and do not say much about meaning of particular lexical item.

By comparing the generative grammars of different languages, Chomsky argues that one will notice that the similarities are marked and the differences are 'few and marginal' (Chomsky, 2006). Thus, he argues that under the

surface structures of each language lie 'deeper structures' that are universal to all languages, and more importantly to all human beings within whose minds they exist.

Hilary Putnam (1979) argued that even if languages showed 'significant uniformities' there could be other explanations for that other than universal grammar, such as, for example, common origin of languages. However, Chomsky rebuffs Putnam's critique as misunderstanding of the problem. Chomsky (2006) is interested in first language acquisition arguing that an underlying structure of language is necessary for a child to adopt a language:

'it seems that knowledge of a language's grammar – can be acquired only by an organism that is "preset" with a severe restriction on the form of grammar. This innate restriction is a precondition, in the Kantian sense, for linguistic experience, and it appears to be the critical factor in determining the course and result of language learning. The child cannot know at birth which language he is to learn, but he must know that its grammar must be of a predetermined form that excludes many imaginable languages.'

For Chomsky the problem is to find a hypothesis about initial structure rich enough to account for the fact that a specific grammar is constructed by the child, but not so rich as to be falsified by the known diversity of language. This concept of innate structure applies also to acquisition of meaning. Specifically, as the acquisition of words is based on minimal exposure, both the sounds and the meanings children acquire must be in some way pre-determined:

'It is hard to imagine otherwise, given the rate of lexical acquisition, which is about a word an hour from ages two to eight, with lexical items typically acquired on a single exposure, in highly ambiguous circumstances, but understood in delicate and extraordinary complexity that goes vastly beyond what is recorded in the most comprehensive dictionary, which, like the most comprehensive

grammar, merely give hints that suffice for people who basically know the answers, largely innately.’ (Chomsky, 1995, p. 15)

Thus, according to Chomsky, we are born with predetermined meanings which are merely activated upon encountering the word. Similarly to Lévi-Strauss’ and Jakobson’s propositions this inborn structure accounts for the universality of underlying structures among various languages and cultures. Without the concept of deep structure, Chomsky (2006) argues that language is ‘reinvented’ each time it is learned.

The system of innate meanings is further developed as *language of thought* (Fodor, 1998) or *mentalese* (Pinker, 1994). These theories take up Chomsky’s idea of deep structures and transformational grammar and develop dictionaries of concepts which account for the syntagmatic selection of units, and hence indirectly the syntagmatic meaning realised in the utterance. Jerry Fodor forms his argument on the notion that in order to learn a second language, one must possess the ability to speak one language. To Fodor, learning a language like English is not to learn what the sentences of the language mean, but to relate the sentences to the corresponding thoughts (Fodor, 1998, p. 9). Language of thought is thus segmented; it consists of discrete concepts which act as categories into which we assign objects through perception. Jackendoff (2011, p. 689) conceptualises such language as an ‘asset of stored structures and stored relations among structures’. Segmentation of language into concepts is vital as it allows formation of new expressions through (re)combinations in syntactic structures. For Jackendoff (2011, p. 695) meaning is regarded as a mental structure which is linked to other deep mental structures that determine its sound-image and its syntactic properties. Thus, the concept ‘cat’ consists of

not only conceptual features, such as 'feline' and 'animal', but also syntactic features, such as 'noun', 'singular', and 'countable' (ibid). Selectional rules, proposed by Chomsky (1965), which deem his famous example 'colorless green ideas sleep furiously' as ungrammatical, require mental concepts to possess such features to make meaningful sentences.

Teubert (2010) argues that the proposition of a language of thought brings about the issue of the structure of such language, namely the question if it is like natural languages – a symbolic system of concept signs or if it is content without form? If it is symbolic, it is not unlike Saussure's *langue*. In that aspect, the language of thought is not at all different from the natural language in terms of its structure, but only in the aspect that it is innate and not necessary to be learned. But essentially, as a symbolic language it is still subject to interpretation. However, the case of content without form corresponds to 'an image that does not represent anything but itself, and thus would have to be identical with the object for which it stands' (Teubert, 2010, p. 49). This image would correspond to what Peirce calls 'icon' or 'likeness':

'The likeness has no dynamical connection with the object it represents; it simply happens that its qualities resemble those of that object, and excite analogous sensations in the mind for which it is a likeness. But it really stands unconnected with them.' (Peirce, 1998, p. 9)

A photograph is an example of likeness, which is a form, an image, representing certain meaning – it is also a sign. Therefore, even a mental image is essentially a sign needing interpretation (Teubert, 2010, p. 49). Deferring the meaning from a natural language to the language of thought would not result in dealing with pure ideas but only with another structure of signs. As Jacques

Derrida (cited in Wood, 1992; Wood & Bernasconi, 1988) argued in his formulation of *différance*, there is no such thing as a transcendental signified, i.e. content without form, a pure idea, only an endless deferral of signification from one sign to another - every attempt to reach the signified is deferred to other signifiers.

Furthermore, from the aspect of the acquisition of symbolic meaning, there is criticism that the underlying language of thought is not a necessary assumption. For example, Ruth Millikan (2004, p. 133) argues that '[t]he infant learns what kitties look like in various postures, what they feel like, the sounds they make, and what they sound like through language.' All of these are in a way signs of kitties, whether natural or symbolic. For Millikan, there seems no reason why this last would require that the infant employ a theory of mind or concepts of mental states.

Chomsky (1995) argues that an underlying structure, as some sort of language of thought, is a necessary assumption because of the speed a child acquires language. For Chomsky (1995, p. 15), first language is acquired almost under minimum exposure and at the speed of roughly one word per hour between the second and eighth year of life. However, Martin (1987, p. 32) argues that when you consider the huge amount of exposure children have to language and all the reinforcement and correction they experience, Chomsky's argument about speed of acquisition is not as convincing. For example, even low-IQ children learn language fairly well by the time they are ten years old, but ten years of intensive training is long enough to teach anybody anything (1987, p. 32). Thus, if the child possesses innate structures of language we would expect it to

master the language much quicker than that. Given the huge amount of training we give our children before they can master our language, one does not need to suppose innate knowledge to start off with (ibid). Furthermore, Teubert (2010, p. 57) finds Chomsky's claim also highly speculative, calculating that if his claim was accurate the average 8-year-old would possess a vocabulary of 26,280 words (12 words × 365 days × 6 years). In that case, Chomsky's 8-year-olds are linguistically competent to determine the meaning of more than 26,000 words based only on single exposure. Even in a case of an individual with such a remarkable vocabulary, Cooper (2003, p. 55) argues that Chomsky's concept of competence is ill formed:

'After all, we judge a person's competence in a language not on the basis of a contextless blurting out of statements, impeccably true as they may be, but on that of the appropriateness of his statements, not only to extra-linguistic situations, but to discursive context.'

With respect to universal structures, Teubert (2010, p. 36) reports that out of 35 language universals proposed by Chomsky and his colleagues, defined as unconscious and untaught structures of language, only recursion has yet to be refuted. Additionally, the notions of innate structures and mental concepts have been shown as unnecessary propositions to account both for the speed of language acquisition and for similarities between certain languages. And finally, as all languages are subject to interpretation, the proposition of a language of thought merely defers the process of meaning interpretation to another structure as mental concepts constitute merely another set of linguistic signs that need interpretation, rather than 'pure' meaning without form. Similarly to the structuralist approach, cognitive theories also result with a problem of meaning, having to accept that certain aspect of meanings are constructed in

communicative event and are not controlled by the structures of the language system. This supports the hypothesis that linguistic meaning can be subject to construction and negotiation in discourse.

It has been shown that regarding the construction of meaning, it is not only necessary to include the communicative aspect of language, but as we have hitherto argued, there is little or no reason to define language as a psychological phenomenon. This will be demonstrated in the following section (2.1.2 Language as a social phenomenon), where it will be argued that language and meaning are primarily products of social interactions.

2.1.2 Language as a social phenomenon

The main aspect of the formulation of language as a social phenomenon is that the main aspects of language depend on the community of language speakers. In other words, the use of language is not considered an innate ability but something that is taught interactively. Thus the communicative aspect of language, realised as discourse, plays the integral part in the acquisition of language. As we have argued in the previous section (2.1.1.2 Cognitivist approach) there is no need to assume any kind of innate unconscious language mechanisms to explain language acquisition. Language is taught in social environment, which preserves the structures and meanings of language and passes it onto new generations.

Defining language as a social phenomenon incorporates its main function, i.e. communication, into its very formulation. Without communication there is no language acquisition or meaning construction. Thus, although language

acquisition implies a psychological aspect of language, in the sense that language is learned and memorised, without the social aspect, e.g. other speakers of language, the communication and formulation of meaning is not possible. On the other hand, the structure of language is realised by the conventions of discourse, thus the systematic aspect of language is transformed from psychological to the social and collective plane of language. Furthermore, as a social phenomenon, meaning is a contingent property which can be socially constructed and negotiated.

This section reviews the reformulation of language as a social phenomenon, which can be traced back to Bakhtin's theories of *dialogism* and *heteroglossia* which stressed the importance of the communicative aspect of language, but also of the ideological influence on meaning. Bakhtin's work was further reformulated by Julia Kristeva, who initially developed the theory of intertextuality based on Bakhtin's *heteroglossia*, but stated within the framework of structuralism. Finally, the complete reformulation of language as social phenomenon and the turn towards the study of discourse is credited to Michel Foucault. The review of the development of this approach presents the arguments for changing the focus of language study towards discourse. The arguments are based not only on the inherent social and communicative functions of language, but also on the problems of construction formulated in the structural and cognitive approaches. Finally, the formulation of language as a social phenomenon is a prerequisite for the theory of meaning construction in terms of social construction and negotiation.

2.1.2.1 Turning to discourse

Saussure's, and Lévi-Strauss', but particularly Chomsky's propositions imply that language is, in a way, self-sufficient; once established, language system exists independently of discourse. However, as we have discussed in the previous chapter, where meaning was concerned, all the system focused approaches had to take into account the context of utterance. Since the meaning is, at least partly determined by the context, to analyse meaning one has to analyse discourse; analysis of a mental system or deep structures, no matter how defined and carried through, could not completely account for the meaning of an utterance.

One of the strongest arguments for departing from the focus on the language system was proposed by a Russian linguist, Bakhtin who was very critical of Saussure's idea of a language system, questioning whether language can exist for the speaker's subjective consciousness as 'an objective system of incontestable, normatively identical forms' (Bakhtin, et al., 1994, p. 32). Bakhtin sees in Saussure the product of a long tradition of linguistic and philological thought, 'in which the essence of language has been thought of as a system of rules' (Dentith, 1995, p. 26). This tradition has led linguists like Saussure and Chomsky to consider the monologic utterance as the prototype of language use. However, Bakhtin argues that this monologic utterance is an abstraction just like Saussure's *langue*. Saussure's *langue* is not a mechanism responsible for the understanding of a particular utterance since the process of understanding is not merely recognising the elements of the utterance, but rather understanding them in the concrete context they occur in (Bakhtin, et al., 1994, p. 33). To

understand the meaning of the utterance amounts to 'understanding its novelty and not recognizing its identity' (ibid). Thus understanding is not the same as recognition; only a sign can be understood, what is recognised is the signal (signifier). However, one does not hear 'words' as signals but as content. To Bakhtin, words are always filled with content and meaning drawn from our social behaviour or ideology (Bakhtin, et al., 1994, p. 33). We do not hear words arranged in structures but statements of what is true or false, good or bad, important or unimportant etc. This is probably why we can easily repeat the 'message' but are rarely able to repeat the exact words of even the shortest utterances (Olsson, 2004, p. 130).

Furthermore, any utterance responds to a previous utterance or an event, and is calculated to be responded to in turn (Bakhtin, et al., 1994, p. 35). Utterances are always dialogic rather than monologic. There is always an intended recipient of the message, whom we expect to respond; apart from the purpose of communication, there is no need for language. Luhmann (1995, p. 143) in this sense writes: '[c]ommunication is made possible, so to speak, from behind, contrary to the temporal course of the process.' In other words, communication depends on the addressee, his interpretation and response to the utterance. According to Bakhtin every utterance responds to previous one, but also to pre-existing patterns of meaning and evaluation. Thus, the speaker is always an addressee first; the speaker is not 'the biblical Adam' and his utterance always responds to utterances that precede it (Bakhtin, et al., 1986, pp. 93-94). In other words, everything that is being said is influenced by what has been said before. As such each utterance promotes and seeks to promote further response; thus

one cannot understand an utterance or even a written work as if it were singular in meaning, unconnected to previous and future utterances or works (Bakhtin, et al., 1986, p. 72). Understanding is a process that happens in the interaction between speaker and listener; the meaning is placed in that particular context. Therefore, all utterances are inherently dialogic, as their meaning and logic depend upon what has previously been said, and on how they will be received by others (ibid). Bakhtin calls this phenomenon *Dialogism*.

As every utterance is a response to previous one and seeks further response itself, there is a multitude of voices in it; it reflects on previous value judgment, offers its own, and seeks further value judgment itself. Bakhtin (Bakhtin, et al., 1994) call this *heteroglossia* as the meaning of an utterance is inhabited by a multitude of voices. Language always represents a 'co-existence of socio-ideological contradictions between the present and the past, between differing epochs of the past, between different socio-ideological groups in the present, between tendencies, schools, circles and so forth' (Bakhtin & Holquist, 1981, p. 291). By relating language and society, the struggle of the classes becomes effectively a struggle of discourses. In the struggle of discourses, Bakhtin (Bakhtin & Holquist, 1981) identified two opposing forces: *centripetal*, or the top-down, and *centrifugal*, or the bottom-up, forces. Centripetal forces represent the language controlled by the society's institutions, which tend to resist language change (ibid). Centrifugal forces, on the other hand, represent the language of general population and are responsible for the language change (ibid). Just as with the struggles of the classes, there is a struggle between discourses, between ideologies, schools, viewpoints, etc. Centrifugal forces represent the

struggle for one's voice, which is inevitable: 'one's own discourse and one's own voice, although born of another or dynamically stimulated by another, will sooner or later begin to liberate themselves from the authority of the other's discourse' (Bakhtin & Holquist, 1981, p. 348) Similarly, Sinclair argues that actual utterance 'shows a curious tension between personal and social pressures' (Sinclair & Carter, 2004, p. 67). The system one has to 'liberate from' is discourse in the broader sense. As any utterance is produced by a member of a culture or civilization, it is always part of the social world, i.e. discourse. Thus, as Derrida summarises: 'everything [is] discourse' (Derrida & Bass, 1978, p. 280). Language is never fixed, but it continuously evolves; there will always be new utterances, and hence, new value judgments and meanings, language is in a ceaseless state of becoming.

2.1.2.2 Structuralism and discourse – Kristeva and Barthes

Bakhtin's arguments against the monologic approach to the study of language were relatively unknown until they were reported by Julia Kristeva in 1960s. At the time, the mainstream approach among the French intelligentsia was heavily influenced by Lévi-Strauss' Structuralism. Thus, Bakhtinian ideas were initially reported by Kristeva from a structuralist point of view reconciling the new ideas with the mainstream structural approaches.

Assuming discourse as the realisation of language, it is natural to ask how it can be a system responsible for its own production at the same time. As we have argued above, Bakhtin did the first necessary step by redefining language as dialogic. Kristeva (1986, p. 36) noticed that Bakhtin's formulation of language as dialogue replaced Saussure's static hewing out of texts, his system of *langue*,

with a model in which structure does 'not simply exist but is generated in relation to *another* structure'. Thus, Bakhtin replaced Saussure's synchrony with diachrony. Kristeva here refers to *text* as the totality of spoken and written utterances, in other words, as discourse in the general sense.

Similarly as Bakhtin differentiates between understanding and recognition, Kristeva argues that writing (*écriture*), in the sense of utterance, is an act of production rather than representation. Kristevan texts, i.e. both written and spoken discourse, do not represent pre-existing meaning embedded in an abstract system of language, but create their own meanings in relation to previous ones. Otherwise, she argues, one would be 'aware' of all the possible meanings and the intellectual exercise would be that of recognising them rather than understanding new meanings. Kristeva thus concludes:

'The text is therefore productivity, meaning that (1) its relation to the language in which it is sited is redistributive (destructive-constructive) [...]; (2) it is a permutation of texts, an intertextuality: in the space of a text, many utterances taken from other texts intersect with one another and neutralize one another.' (Kristeva (1966-7), *Le Texte clos*, adapted from Orr, 2003, p. 27)

Kristeva's arguments are significantly influenced by Bakhtin's work; her text as productivity has elements both of dialogism (1) and heteroglossia (2). Similarly, for John Sinclair (2004, p. 52) language in use has two aspects: the interactive plane, which involves a continuous negotiation and corresponds to Bakhtin's dialogue; and the autonomous plane which is a record of experience, in the sense of intertextuality and heteroglossia, and involves recalling and reworking previous words 'in the new contexts provided by the movement on the interactive plane'. The autonomous plane of discourse is concerned with 'the

organization and maintenance of text structure' and does not refer to the world outside discourse (Sinclair & Carter, 2004, p. 53); and we can say that the same applies for Kristeva's intertextuality. Thus, the corpus linguistic approach defined by Sinclair forms the language system within discourse; discourse is both the system and its product at the same time, since new utterances and structures are continuously constructed in relation to the previous discourse.

However, the notion that discourse generates itself in relation to previous discourse does not fit in Bakhtin's formulation of dialogue, which involves at least two participants. Kristeva argues that in an utterance the addressee is a construct of the discourse; thus the addressee is a 'text in relation to which the writer has written his text' (Kristeva & Moi, 1986, p. 37). Furthermore, the speaker itself becomes a subject of an utterance, which is product of discourse, thus the relation speaker-addressee corresponds to the relation text-context (Kristeva & Moi, 1986, p. 37). The speaker is projected as the subject of the utterance, but the reader recreates the intentionality of the speaker much in the same way as the speaker creates the addressee. As the speaker becomes a subject and hence a text, where other texts intersect, it is the reader that uses his judgement on the text, bringing about a multitude of texts into the process of understanding. Thus the meaning is not fixed into the utterance by the speaker, rather it is brought into it by the addressee in the process of understanding; the meaning of a text comes from language viewed intertextually.

The text has a structure of definable elements, and yet its intertextual relations can never be stabilized, exhaustively located and listed. The text combines structure and infinity of meaning. Since a multitude of other texts and voices

make up a text, the meaning is deferred from the author to the reader: '[t]he reader is the space on which all the quotations that make up a writing are inscribed without any of them being lost; a text's unity lies not in its origin but in its destination' (Barthes & Heath, 1977, p. 148). This intertextuality should not be confused with some origin of the text as an attempt to uncover the underlying citations; 'the citations which go to make up a text are anonymous, untraceable, and yet *already read*: they are quotations without inverted commas' (Barthes & Heath, 1977, p. 160). For Sinclair (Sinclair & Carter, 2004, p. 14) this intertextuality necessary for understanding, and hence, communication is defined as shared knowledge:

'The whole text is present in each sentence. The meaning of each previous sentence is represented simply as part of the shared knowledge that one is bringing to bear in the interpretation of a text at any point.'

Similarly, Ene-Reet Soovik argues that Kristevan intertextuality as 'a state of already-writtenness is the necessary condition for all discourse' (Soovik, 2004, p. 59); it is a history of meanings one brings into the act of interpretation and against which one creates new meanings.

The theories of *Dialogism* and *Intertextuality* are of major importance for the study of meaning construction as they abandon the study of the monologic utterance. Thus the synchronic aspects of language are replaced by a diachronic structure in relation to which new meaning are constructed. Utterances are redefined as reactions to previous discourse and the construction of meaning becomes dialogic and diachronic, and the interpretation of new meaning is based on the history of meanings. The inclusion of dialogic

aspect into the process of meaning construction accounts for the discursive aspects of meaning construction, but also necessitates a redefinition of the language system.

2.1.2.3 Discourse as a system

The shift of focus of linguistic research to discourse does not abolish the notion of linguistic system, but rather merges the two. Sinclair (2004) defines language by dividing it into interactive and autonomous planes. Interactive plane is characterised by the negotiation of meaning (Sinclair & Carter, 2004, p. 52). On the other hand, autonomous plane is defined as the record of language experience (ibid). In this way, the interactive plane corresponds to the dialogic aspect of discourse and the autonomous plane corresponds to the systematic aspect of discourse. The autonomous plane represents the shared experience of language, without which it would be hard to imagine communication (Sinclair & Carter, 2004, p. 53).

Theories of *Dialogism* and *Intertextuality* have led to the replacement of the synchronic language system with a form of historical record of previous meanings. By replacing the abstract and unconscious language system, discourse inherited the role of the system. As discourse is itself is often described as the product of a system, the system is described by Maturana and Varela (1980, p. 13) as an autopoietic system - a system in which all the processes and structures are produced by the system itself. Namely, as long as the discourse continues, new structures are perpetually generated in relation to the previous ones, which accounts for its diachronic dimension. Replacing the synchronic system with the diachronic discourse does not result in discourse

becoming the new system of underlying structures that govern the production of meaning. On the contrary, building on the theories of dialogism and intertextuality, Michel Foucault in *Archaeology of knowledge* (1972, p. 138) concludes that the analysis of discourse does not seek to discover hidden structures:

Archaeology tries to define not the thoughts, representations, images, themes, pre-occupations that are concealed or revealed in discourses; but those discourses themselves, those discourses as practices obeying certain rules...It is not an interpretative discipline: it does not seek another, better-hidden discourse.

The analysis of discourse does not aim to define 'underlying' structures of discourse, but aims to define those discourses themselves. In other words, the analysis of a notion like 'verb' is not seen as an analysis of an underlying language system feature, but as an analysis of a discourse construct assigned with a particular meaning and function. Thus, Foucault (1972, p. 38) views any attempt into the analysis of underlying structures unnecessary as the only conclusion will be that these structures are ultimately the constructions of discourse:

'whenever, between objects, types of statement, concepts, or thematic choices, one can define a regularity (an order, correlations, positions and functionings, transformations), we will say...that we are dealing with a discursive formation.'

Everything we observe about language is realised in discourse and all the observed features of discourse are formations of discourse itself. Thus, Teubert (Teubert, 2010, p. 10) argues that there is no vantage point outside discourse from which we can observe it. Namely, for Teubert (ibid) we can be aware of only the notions that already exist as discourse constructions. In this sense, the

act of interpretation is realised as a reformulation or definition using one's own words, which are inevitably a part of discourse. Furthermore, any attempt of interpretation is an act of extending the meaning; any attempt to define and narrow down meaning of any term results in new interpretations adding to its meaning. Foucault (1972, p. 80) exemplifies this by trying to define the meaning of *discourse*:

Instead of gradually reducing the rather fluctuating meaning of the word 'discourse' I believe I have in fact added to its meanings: treating it as sometimes the general domain of all statements, sometimes as an individualisable group of statements, and sometimes as a regulated practice that accounts for a number of statements.

Because of the intertextual and dialogical relations within discourse, the three definitions in the example above, each serving as a separate interpretation of the term *discourse*, all add to the meaning of the term. In this sense, interpretations are statements that express what is currently being said, whereas meaning is formed of the totality of all interpretations, like in the above example where the three interpretations form the meaning of the term *discourse*. This corresponds to Wittgenstein's argument that 'the meaning of a word is its use in language' (Wittgenstein, Anscombe, Hacker, & Schulte, 2009, p. 62). Furthermore, discourse and meaning have inherently a diachronic dimension since any act of interpretation and utterance depends on previous discourse.

Having defined language, that is discourse, as a social phenomenon, we can define discourse as an autopoietic system. Namely, as discourse is constantly expanded by interpretations formed within discourse, we can argue that discourse reproduces itself. However, as soon as this reproduction is finished

language becomes dead (Maturana & Varela, 1980). The best examples are forgotten languages, e.g. Etruscan, which have become meaningless once their reproduction through discourse stopped. For Niklas Luhmann (2002) languages are reproduced through communications, which only exist in relation to other communications – thus language for him is a system of communications. Without communications, we are left only with words, perhaps only even with sounds, but those cannot produce further communications, like in the case of the forgotten Etruscan language. The agent, i.e. the human being, is not itself a part of the discourse system, but is represented in the discourse as the subject; and following Kristeva's (1986, p. 37) argument, the subject is a discourse construction. Thus, Luhmann concludes: 'Only communications can communicate' (Luhmann & Rasch, 2002, p. 169).

Structures of a discourse system, or in Luhmann's sense, a system of communication, are not predetermined as in structuralist theories, but are products of the system itself. As Foucault (1972) states, all structures of discourse are discursive formations themselves. For Luhmann (1995) structures are formed as 'expectations', as every communication predetermines the possible further communications. Furthermore, certain expectations are perpetually communicated, thus they function as structures. Similarly, Teubert (2010, p. 230) argues that communication in each 'discourse community has its own conventions.' One can, of course, flout these conventions and invent their own structures, but if they are not reproduced in further communications they will disappear. Discourse conventions 'provide the necessary continuity and are therefore relatively stable' (ibid). Thus we can see grammatical structures as a

form of expectation. The whole structure of language is based on expectations and cooperation rather than natural laws; thus the stability of structures is based on the desire to be understood rather than innate mental structure. Millikan (2004, p. 105), similarly argues that speakers are used to having hearers respond to certain forms, which in turn reinforces the production of these forms. Millikan compares these forms to Dawkins' (2006) replicating *memes*. For Dawkins (ibid), it is not only the structures and patterns which we use to express ourselves that are matter of convention and acceptance, but also meanings. For a particular structure or unit of meaning, it is most important to be replicated, i.e. repeated in discourse, as many times as possible since this replication 'affirms continuity and complexity as recipe for long-term success and evolution, but with similarity *and* some difference as the all-important leaven' (Dawkins, 2006, p. 103). In this sense, to express new meanings, the utterance will always have to refer to and be composed of similar, previously known meanings, showing signs of consistency and repetition, but also a degree of variation.

The discourse as a system is not a system of elements, but a conceptual unity that is only divided into parts by discourse itself. All the elements of discourse, as Foucault stated, are products of discourse itself. The analytical work of Sinclair (1991; 2004) showed that meanings in the text arise from particular choices of the component elements. For Sinclair the approach to language has to be reversed from the cognitive and conventional grammar models, as '[t]he flow of meaning is not from the item to the text but from the text to the item' (Sinclair & Carter, 2004, p. 135). In other words, one cannot expect to

understand a text by adding up successive meanings of each individual linguistic item, rather meaning of individual items is only realised in relation to one another. Thus, for example, if the meaning of a word is closely related to the words that surround it, 'then consequences of studying its meaning in isolation are unpredictable' (Sinclair & Carter, 2004, p. 137). Similarly, Millikan (2004) argued that if sign is just a sound-image then context must be relevant.

Another problem with system focused theories comes with words that have multiple meanings. For example, Sinclair (1991, p. 103) argues that the problem with polysemous words, which if they really stood for several meanings, would make communication based on selection of predefined concepts, such as described by Chomsky, virtually impossible. Namely, it would be in certain situations impossible to know which of the word meanings is meant, particularly with monologic utterances taken out of the context. Furthermore, what Chomsky's approach also does not show is that the majority of discourse is made of the occurrence of common words in common patterns, or in slight variants of those common patterns (Sinclair, 1991, p. 108). In these patterns, words, including polysemous words, do not have 'an independent meaning, or meanings, but are components of a rich repertoire of multi-word patterns that make up text', which is 'totally obscured by the procedures of conventional grammar' (ibid). Thus the ambiguity and polysemy are not usually present in discourse, but are rather products of identifying meaning with individual words outside the context. The polysemy of meanings is not resolved even when the word is observed in the structure of the sentence, when words are viewed as individual elements of the structure. On the other hand, multi-word patterns and

words observed in context usually have clear unambiguous meanings, indicating that delimitation of language into words is an arbitrary and conventional rather than fundamental process.

Without clearly defined elements, a generative language system is an unlikely proposition. As the meaning of a word is defined by the words that surround it in an utterance and its use in the previous discourses, it is only in the act of interpretation that meaning is realised. Thus Luhmann (1995) argues that the meaning of a communication is realised only retrospectively through later communications. Language and meaning depend on discourse as the place where structural forms and meanings are constantly reproduced in relation to previous discourses. For this reason, Teubert states that discourse has a diachronic dimension and is 'neither stable nor finite' (Teubert, 2010, p. 220). As discourse always continues, language and meaning are always in the state of becoming, rather than a finished product, as in the case of Saussure's or cognitive systems.

Language as a social phenomenon assigns dialogic and diachronic aspects to language, particularly with respect to meaning construction. The focus on the communicative aspects of language also redefines the language system, necessary to ensure stability and meaningfulness of language, as a fluid self-generating system. As opposed to the static generative language systems defined in the cognitive and structural approaches, discourse as an autopoietic system does not encounter the problem of meaning caused by the separation of the language system and discourse. This is due to social, rather than cognitive, formulation of the system, as well as its fluid formulation as a diachronic

system. Furthermore, in the Foucauldian sense, the structures of the system are discursive, rather than fundamental, formations; thus, discourse is not a system of elements, but a system of communications. The elements of the system emerge through repetition in discourse, and the smallest unit of meaning will encompass both the morphological elements such as plural suffix '-s' as well as phraseological expressions e.g. 'kick the bucket'. These meaningful expressions are not fundamental elements of a generative system, but rather elements that have emerged out of discourse through repetitions. The structures and meanings of such system are generated in relation to previous discourse, thus such system exists only in a discourse community. Although the language also 'exists' in the memory or minds of the speakers, its meaning is realised in communication, or rather in the act of interpretation. Thus, Roland Barthes (1977) famously announced the 'death of the author', claiming the meaning depended on the interpretation of the reader. Discourse system is thus a system and a history, or memory, of communications. Meanings are interpretations realised by reformulations of the known. Like with translations, the utterance is always rephrased in the known language.

The analysis of social construction and negotiation of meaning is based on the formulation of language as a social phenomenon with discourse defined as a system of communications. Cognitive theories focus on the unconscious generative mechanism of language, where meaning construction precedes the communicative event, thus excluding the possibility of meaning negotiation. On the other hand, structural approaches lack the dialogic and diachronic elements necessary to observe the process of negotiation, focussing instead on the

contextless discourse and synchronic aspect of language. Thus, only by assuming that meaning is constructed and realised in discourse it is possible to analyse the social construction and negotiation of meaning. This is only possible by observing acts of interpretation, realised in discourse in the form of paraphrases, which are defined in detail in the following chapter (2.2 Constructing meaning and reality).

2.1.3 Summary

In this chapter, approaches to language as a psychological and a social phenomenon have been reviewed and contrasted with respect to the implications on the construction of meaning in language. Theories that define language as a psychological phenomenon include structural and cognitive theories, which are characterised by the formulation of a generative language system that governs the production of all discourse. In both approaches this system is defined as synchronic, meaning that the structural system is unaffected by historical changes of language. Structural approaches account for historical changes by defining the changes merely as new forms replacing the old, but keeping their systemic function. On the other hand, historical change, or any other aspect of discourse, is not in the scope of analysis of cognitive approaches which focus rather on deeper unconscious structures of language. In a sense, the two approaches both focus on synchronic structures of language, which users are unaware of and which resist historical change shown in discourse. With such formulation, the analysis of language change realised in

discourse cannot lead to analysis of meaning construction, since discourse shows forms that merely fulfil a function defined by the system.

Both the structural and cognitive approaches argue that the diachronic and dialogic aspect of language is irrelevant compared to the underlying structures that govern the production of language, and hence construction of meaning. However, in the definition of the process of meaning construction, both approaches admit that certain aspects of meaning are constructed within the scope of discourse. Thus the theory of language system as an underlying generative mechanism cannot fully account for the meaningfulness of discourse.

Although the system focused theories ignore discourse, and hence the communicative aspect of language, the reasoning for such approach lies in the ability to detach the observer from the analysed object. In other words, the analysis of abstract systems is justified by the prospect of analysing a system of laws and rule, not unlike the laws of physics and universe which are the focus of natural science. For this reason, both approaches are focused on the analysis of subconscious or unconscious structures of language which govern the production of meaning. However, not only that these structures are not directly observable but they are themselves constructions of discourse. Thus, as Teubert (2010) argues, one cannot have a vantage point from which to observe discourse independently as all observations and interpretations are formed in discourse.

Since Saussure, the work on discourse has 'rejected the belief that a single and general system lies behind all discourses' (Macdonell, 1986, p. 9). The most

important conclusion about the nature of language is that meanings are to be found only in the concrete social communication and institutional practices (see Barthes, 1988; Barthes & Heath, 1977; Cooper, 2003; Kristeva & Moi, 1986; Millikan, 2004; Sinclair & Carter, 2004; Teubert, 2005).

Meaning as a product of discourse has quite different formulation than in the case when it is attributed to abstract mental systems. Namely, the linguistic meaning is realised in communicative situations; thus, in Bakhtin's words (Bakhtin, et al., 1986; Bakhtin, et al., 1994), language is dialogic. This does not limit the language to the actual dialogic situations, but rather defines meaning as dependant on both the speaker and the addressee. Dialogic formulation of language led to the conclusion that meaning is realised in the act of interpretation. Specifically, the meaning of an utterance is interpreted by relating it to the previous meanings known to the addressee. Meaning is, thus, based on a multitude of utterances one has encountered before; in that sense, Teubert (2010, p. 216) argues: '[m]eaning is the result of adding, over time, one interpretation on top of the other.' The interpretations have to be realised in discourse to constitute meaning, they have to be uttered, but most importantly they have to be (re)interpreted.

Defining discourse as a system based on the accumulation of interpretations, meanings and structures used in concrete communication supports the empirical approach to the analysis of social meaning construction and negotiation. Such system accounts for both the dialogic and diachronic, and structural aspects of language. Furthermore, without the dichotomy between the system and discourse, there is no problem of meaning construction being

attributed to different planes of language. Also, the formulation of meaning as a totality of interpretations allow for the analysis of meaning construction, change and negotiation through the analysis of paraphrases. The definition of paraphrase as an act of interpretation is reviewed in the following section.

2.2 Constructing meaning and reality

In this chapter main aspect of discursive meaning construction will be reviewed, namely the notion of social construction of meaning, definition of meaning as a totality of interpretations and the definition of paraphrase as an act of interpretation. As all of these aspects of meaning construction can be observed in discourse they are key theoretical formulations for the empirical analysis of meaning construction.

In the first section it is argued that meaning is constructed socially through the process of negotiation, which is also closely related to the social construction of reality, since shared meanings can form shared realities. The second section explores the relation between interpretation and meaning in more detail. Here meaning is defined in the Foucauldian sense as everything that has been said about a particular notion, thus meaning is made up out of a totality of interpretations. Finally, the third section deals with the mechanism of the social construction of meaning, namely the notion of paraphrase, which is defined as a realisation of interpretation, but also as a realisation of intertextuality. These three premises will form an important theoretical framework for the analysis of meaning construction in discourse.

2.2.1 Social construction by negotiation

Language theories focussing on discourse, as discussed in the previous chapter (in Section 2.1.2 Language as a social phenomenon), argue that meaning is realised in the acts of actual communication. As such, meaning is not based on mental concepts but on negotiation and interpretation in discourse and the shared experience of such interaction. The relation of meaning to the language external reality is realised in the acts of interpretation, when the perception of the world is reinterpreted in discourse. As with Saussure's formulation, the relation between the world and language is one of reference. This chapter aims to present theories which argue that within the scope of language, all meanings are products of the language itself, by means of social construction and negotiation. Linguistic meaning is thus a product of the autopoietic system. Only such formulation of language allows for the analysis of meaning construction by negotiation in discourse.

On the other hand, in cognitive theories (See Section 2.1.1.2 Cognitivist approach), linguistic meaning is a representation of mental concepts, which are by some authors even described as innate. The argument for innate concepts is based on the universality hypothesis, which argues that the structures and concepts are universal among cultures. For cognitive linguists, concepts can be rearranged in any order to create new meanings in language, but are essentially mental constructs detached both from the language external reality and the discourse. Cognitive concepts presuppose a natural pre-linguistic categorisation of the world, on which the meaning is based. Such pre-linguistic categories imply either that all humans have the same mechanism for pre-linguistic

categorisation of the world, or that this categorisation is based on true natural categories found in the world itself. In either case, such categorisation, and hence perception of the world, and ultimately the meanings of language, are not constructed or negotiated in discourse.

However, the discourse oriented approach, taken in this thesis, argues the reverse, i.e. that meanings divide and categorise reality where language is concerned. Thus, as meaning is imposed on reality, language effectively creates its own reality by interpreting the reality using symbols it has created. In this sense, Benveniste (1971, p. 25) argues that words are not reflections of the world, but tools that categorise and organise the linguistic reality.

Meanings, categories and symbols are all contingent constructs which we use to refer to the world that surrounds us. However, the social reality does not replace the objective reality of the world we live in, but only provides the means of categorising and organising our perception and understanding of the world. Discourse theories do not presuppose any kinds of pre-linguistic categorisations, but see all categories as contingent constructs of language.

There is no natural, immediate, and direct relationship between man and the world or between man and man. An intermediary is necessary: this symbolizing apparatus which has made thought and language possible. (Benveniste, 1971, p. 26)

The symbolic meaning is realised by adding layers of interpretation on top of one another. Thus meanings of words grow as they are used in different contexts expressing new interpretations of world that surrounds us. Since there is no supposition of an innate structure, our interpretations are based on the history of interpretations of our linguistic community. In this sense, Durkheim

(1983) argues that our reason, and hence what we think to be true, is based on our collective understanding of the world: '[a]ll that constitutes reason, its principles and categories, has been made in the course of history.' This corresponds with the theories of dialogism and intertextuality, discussed in the previous chapter, in the sense that any interpretation of the surrounding world will be formed in relation to the shared history of all such interpretations. Interpretation and understanding as such cannot be 'strictly separated from the construction of new realities' (Teubert, 2010, p. 239). Namely, as interpretations are based on previous interpretations, language imposes itself on reality by defining the ways we interpret the world. Thus Benveniste (1971, p. 46) concludes that for 'the speaker there is a complete equivalence between language and reality. The sign overlies and commands reality; even better, it *is* that reality'.

As society presupposes language, and language presupposes society, meaning is a phenomenon of culture. Assigning meaning to objects is of pragmatic nature because of the function they serve. Thus, an object is converted into a sign, when it starts performing a certain function, e.g. a rock becomes a paperweight, which gives the appearance that rock carries meaning. Barthes describes this as a 'conversion of culture into pseudo-nature which can define the ideology of our society' (Barthes, 1988, p. 190). However, the meaning still resides in the culture, i.e. linguistic community, where it is realised in communication.

As meaning arises in communication, meaning and language are collective phenomena, and in turn ideas, thoughts, and concepts are collective

representations of the world (Durkheim & Allcock, 1983). Thus, meanings, categories and truths are effectively collective phenomena on which society is formed. As a collective phenomenon, meaning tends to be fixed and tries to resist historical change, much in the sense Bakhtin described as centripetal and centrifugal forces (see Section 2.1.2.1 Turning to discourse). However, collective meanings also impose themselves upon us (Durkheim & Allcock, 1983); we think and communicate only through words and meanings imposed on us. As all interpretation is based on the meanings imposed on us, the change of meaning is a rare and slow process. Meaning consists of a multitude of interpretations, thus as Bakhtin argued, it is inhabited by many voices, interests and ideologies.

Interpretation of nature creates discourse objects which can be operated in language. It is not an act of representation, but an act of construction because the interpretation is based on the known meanings. As Paul Ricœur (1981, p. 68) states: '[h]istory precedes me and my reflection; I belong to history before I belong to myself.' Thus, for Ricœur one's understanding of the world, the reality one lives in is the product of discourse. Reiner Keller (2011) describes this as sociology of knowledge, where collective stocks of knowledge appear as institutions, like language itself, which form a historical apriori; members of the collective interpret the world based on the knowledge at hand, not as transcendental subjects. Thus, collectivity maintains ideas and representations and imposes them on individuals; for Durkheim (1983) that is the base for truth conditions. In this sense, Nietzsche concludes that truth is also a product of

collectivity, an illusion similar to the illusion of the sign as a pseudo-natural object described by Barthes above:

'What, then, is truth? A mobile army of metaphors, metonyms, and anthropomorphisms – in short, a sum of human relations, which have been enhanced, transposed, and embellished poetically and rhetorically, and which after a long use seem firm, canonical, and obligatory to a people: truths are illusions about which one has forgotten that this is what they are; metaphors, which are worn out and without sensuous power; coins which have lost their pictures and now matter only as metal, no longer as coins' (Nietzsche "On truth and Lies in an Extra-Moral Sense" in Ormiston & Schrift, 1990, p. 43)

Nietzsche's argument above indicates that all the discourse about the truth and reality is merely a set of rhetorical functions that are so deeply immersed in our culture, or discourse, that they appear as facts. Thus, with respect to linguistic meaning, language creates its own reality based on its own traditions and rules. In this sense, Durkheim (1983, p. 74) states that ideas are nothing more than words 'which we can twist as we like when there is no objective reality (provided by sensations) which prevents us from doing so'. However, as we have argued thus far, the reality preventing us from 'doing so' is rather the shared reality of discourse; the restriction is based on language and history of interpretation not language external objects.

Hey (2001, p. 38) offers an interesting example of a discourse construct showing independent meaning from the external world, namely the term *gene*. When the term was coined it 'had an unambiguous precise meaning, although that meaning was entirely theoretical' (ibid). What happened next, Hey describes as an ironic, although common, 'switch' that comes with the development of understanding:

'As we learn more about something, the more our word for that something becomes ambiguous. We do not suffer the ambiguity, generally, for we have a broader understanding. We can explain phenomena, but if pushed for a precise definition that is also widely applicable, we may be stuck.' (Hey, 2001, p. 38)

Thus, discovering the natural object that is supposed to stand for the term *gene* resulted in blurring of the meaning of the term. Namely, such a discovery has prompted new discourses and interpretations of the terms, which made its discourse meaning expand and become more complex. Extending the knowledge of the notion inherently implies extending the discourse about the notion. However, ironically, Hey (2001, p. 39) argues that the formulation of the term is not as important as its reference:

'words are important, but not that important. I would conjecture that there is not a single context, wherein one of these words is ambiguous, that biologists cannot provide an explanation and an understanding of the processes therein.'

As we have argued so far, it is precisely the different contexts the word *gene* is used in that formulates its meaning through interpretation by various discourse communities; ambiguity only results when the word is taken out of its context. Furthermore, Hey even explicitly states that it is the biologists who will provide the explanation of meaning, which will resolve any ambiguity in a particular context. Thus, we can observe that discourses of particular subdisciplines have developed their own precise interpretations of terms standing for the same natural object, indicating that their meanings are defined more by the particular discourse than by the objects' natural properties.

As we have discussed so far, any object that signifies anything in the world is mixed up with language; language always intervenes between objects and

meaning (Barthes, 1988, p. 180). In this sense, Barthes argues that meaning is always imposed on objects of the world; 'there is no object which escapes meaning, [...] when they feign to have none, then precisely they end up having the meaning of having no meaning' (Barthes, 1988). Words refer to the objects in the world, but do so on their own terms. There is no truth that imposes itself on meaning that is not itself a product of discourse.

2.2.2 Interpretation and meaning

Thus far we have argued that linguistic meaning is the product of negotiation in discourse. As such, meaning is formed of a multitude of interpretations. This aim of this section is to clearly define the difference between the notions of interpretation and meaning, and also to review the literature supporting such formulation.

The proposition that meaning is defined as a multitude of interpretations is deeply immersed with the theories of dialogism, intertextuality and the formulation of discourse as an autopoietic system of communication. The first formulation of meaning and discourse as a totality of interpretation can be attributed to Michel Foucault. In relation to the development of the notion of intertextuality within the scope of structuralism, Foucault concluded that:

'There is nothing primary to interpret, because at bottom everything is already interpretation. Each sign is in itself not the thing that presents itself to interpretation, but the interpretation of other signs.'
(Michel Foucault, "Nietzsche, Freud, Marx" 59-67 in Ormiston & Schrift, 1990, p. 64)

Theories of dialogism and intertextuality define all signs, language and meanings as formed in relation to previous discourse. By defining language as a self-generating system in which meaning are formed in relation to one another, there is no primary meaning to interpret, only elements that point one another. In this sense, Foucault concludes that interpretation only leads to other interpretations.

By defining discourse as a dialogic system in which all utterances are formed in relation to one another, linguistic meaning is formed of a totality of interpretation realised in discourse. This view is taken by Teubert (2010, p. 216) who argues that: '[m]eaning is the result of adding, over time, one interpretation on top of the other.' As all interpretations are formed in reaction to previous discourse, meaning has a diachronic dimension (ibid).

Interpretation, on the other hand, is the act of making sense (Teubert, 2013, p. 275) and declaring what is meant at the particular moment in time. Similar view can be observed in Berger and Luckmann's (1990, p. 33) formulation of social reality where '[e]veryday life presents itself as a reality interpreted by men and subjectively meaningful to them as a coherent world.' In this sense, interpretation is an act of relating the perception of reality to the meaningful experience.

An interesting example of the relation between interpretation and meaning can be observed in the correspondence of Charles Darwin. Namely, in a letter to the translator of his manuscript, Darwin reports the complexity of meaning of the term 'natural selection' as a good thing since it prevents other men from formulating their own interpretations of the notion:

Several scientific men have thought the term "Natural Selection" good, because its meaning is not obvious & each man could not put on it his own interpretation, ...[www.darwinproject.ac.uk/letter/entry-2698]

As the meaning of *Natural Selection* was hard to relate to other meanings, the notion has effectively become hard to interpret in one's own words.

Interpretation becomes part of the meaning once it has been uttered. As an intentional utterance interpreting the meaning of a particular aspect of discourse, interpretation can be defined in concrete discourse as an act of paraphrase. Thus, interpretation is formed as a sentence that represents reality for the speaker and recreates that reality for the hearer (Benveniste, 1971, p. 22). As a reaction to previous meanings, the interpretation does not involve any kind of repetition but always a re-creation of meaning; that is the essence of interpretation and language. Similarly, Barthes (1988, p. 134) argues that language as such arises from 'the necessity to vary and transcend the first *form* available man, i.e., repetition: a sequence is essentially a whole at the heart of which nothing is repeated.' By reformulating the 'original' meaning in one's own words, the act of interpretation corresponds with the discourse act of paraphrase.

2.2.3 Paraphrase as Interpretation

Thus far it has been argued that meaning is a product of social negotiation, formed as a totality of interpretations, which are realised in discourse as paraphrases. This section reviews the literature that supports such formulation of the notion of paraphrase as well as other definitions of the notion. The aim of

the section is both to support the definition of paraphrase as an act of interpretation, but also to review the literature on the formulation and function of paraphrases in discourse.

In discourse, in the Bakhtinian sense, every utterance is regarded as a response to previous utterances, acting to refute, affirm, supplement or somehow take it into account (Bakhtin, et al., 1986, p. 91). As such, these utterances have an interpretative function. Furthermore, as we have argued thus far, interpretations form meanings, thus utterances that act as an explanation meaning of a particular notion at a given point in time can be defined as paraphrases of a particular meaning.

The formulation of the notion of paraphrase as an act of explanation of meaning, which essentially encompasses all utterances in discourse due to their relation to previous utterances, can be attributed to Wolfgang Teubert (2005, 2010, 2013). The most prototypical example of an explanation in the form of paraphrase can be found in dictionary definitions. Teubert (2010, p. 219) argues that lexicographic definitions are usually generalised so they can be applied to virtually all discourse occurrences. He gives the example of an explanation of meaning one would not usually find in a dictionary – ‘a heron is a huge target. Hard to miss’ – arguing that such explanations are abundant in discourse (Teubert, 2010, p. 219). By analysing discourse we can observe such explanations in the form of paraphrases, which allow us to observe various interpretations of the meaning of particular discourse objects. In this sense, Wittgenstein argued that:

For a large class of cases — though not for all — in which we employ the word “meaning” it can be defined as thus: the meaning of a word is its use in language. (Wittgenstein, et al., 2009, p. §43)

Wittgenstein’s argument is that for most words in language, their meaning can be interpreted from their use in discourse. As such every utterance presents an interpretation of meaning, and being related to previous discourse, it acts as a paraphrase, i.e. reinterpretation of previous meanings. For Teubert this makes the definition of utterance as paraphrase of meaning essential for the analysis of meaning:

This is what makes paraphrases so essential: they tell us what has been said and can be said about a discourse object. For a corpus-driven theory of meaning, they are crucial. They may contradict each other, they may describe something in such irreconcilable features that it is hard to see it as the same thing, but taken together in all their chaotic diversity they are the very material meaning consists of. (Teubert, 2005, p. 12)

As paraphrases essentially rephrase the words and meanings of others, such formulation corresponds both with the dialogic and diachronic formulation of language. Stahl (2006) argues that discourse is continuous interpretation and as soon as a paraphrase as an explanation of meaning is introduced, it is being interpreted and likely re-interpreted in new paraphrases. In this sense, interpretation are shared and developed collaboratively over a period of time. Thus, meaning in discourse, like discourse itself, is thus ‘neither stable not finite (Teubert, 2010, p. 220).

The importance of paraphrase is not only in their formulations of meaning, but also the manner this is achieved. As the formulation of the notion of paraphrase is based on the theories of dialogism and intertextuality, the realisation of paraphrase in discourse can be used to analyse the intertextual processes in

language. The notion of intertextuality and the formulation of paraphrase as reinterpretation of others' words allows us to observe how meanings are constructed in relation to previous meanings. Thus, Koteyko (2006) concludes that the study of paraphrases in a corpus 'allows a detailed and documented diachronic analysis of intertextual links that uniquely characterise any text segment in the focus of analysis.' In other words, by analysing corpora it is possible to observe the intertextual links between paraphrases and thus observe the process of meaning negotiation. Paraphrase thus also performs an intertextual function, by incorporating previous texts into the act of interpretation.

2.3 Terminology

Observing language from the perspective of social construction and negotiation of meaning requires special attention to the notion terminology. Terms are regarded as having clear and well-defined singular meaning thus showing virtually no ambiguity regardless of the context they occur in. As such, they are considered distinct from general words of language, which usually require context to resolve their polysemous meanings and ambiguities. Furthermore, terms are characterised by being linguistic objects of specialised languages. Thus, only words having a special reference within a specific domain are considered to be the terms of that discipline (Sager, 1990, p. 19).

Hitherto it was argued that meanings of most of the words are ambiguous and realised only in contexts, rather than abstract systems. However, terminology appears as an exception to the rule with the formation of conceptual ontologies. The aim of the section is to demonstrate that even terms are subject to the same laws that words are subjected to, in discourse oriented approaches, and are not in any way equated with the concepts defined in cognitive theories. The chapter also demonstrates the meanings of terms are just as dialogic and diachronic as of 'normal' words.

2.3.1 Special features of terms

Having a singular meaning, i.e. representing only one concept or meaning, terms are most often found in specialised texts, such as scientific and technical writings. This so-called 'univocity principle' is one of the cornerstones of

traditional terminology (Bertaccini, Massari, & Castagnoli, 2010). Terms thus usually refer to single concepts, which in turn are represented solely by that term; as such, in their use, they correspond to proper names (Pearson, 1998, p. 11). In other words, they act as names, or rather labels, for particular well-defined concepts. In this sense, terms are defined in the same manner as Saussure's linguistic sign, with the label as signifier and concept as the signified.

However in terminology, terms are only considered as labels for the abstract concepts, and the primary goal of terminology is defining the concepts, i.e. standardising the meaning before the label (Pearson, 1998, p. 2). The description of the concept is formed by subject specialists, which thus 'becomes the definition of the term' (ibid). Thus, the task of terminology is opposite to the task of linguistics; where linguistics observes the empirical evidence of language use to determine the meaning of a lexical unit, terminology defines the meaning by setting the norms for the formulation of the term in discourse in order to standardise the meaning.

Similarly to Saussure's approach, terminology is not concerned with the formulation, i.e. the definition, of terms in discourse, but rather in establishing structures of knowledge for specific subject domains. Rather than focussing on definitions, terminologists focus on 'comparing and contrasting related concepts' and examining the vertical and horizontal links between them (Pearson, 1998, p. 2). This fully corresponds to Saussure's language system, and is realised in terminology by developing conceptual ontologies: '[a]ll concepts, together with their relationships to each other, are called a conceptual ontology' (Teubert,

2010, p. 68). However, Teubert (ibid) argues that these concepts are not the same as mental entities as described in the works of structuralists and cognitivists; they are the tools of terminology as a science. Conceptual ontologies are models that define terms, or rather the concepts, in specialised language. They have an important role in the translation of terms in technical and scientific texts, but these ontologies do not apply language in general. Teubert (ibid) exemplifies this by arguing that there would not be so many different translations of Shakespeare if conceptual ontologies worked for everyday language, as they would all be identical with all the meanings clearly defined. However, we know well that is not the case as the meanings of Shakespeare's works are still debated among modern scholars and continuously reinterpreted in new translations.

2.3.2 Paraphrases of terms

As it was argued above, the meaning of terms is standardised by setting the language norms such as syntactical structures and lexical context they occur in in order to fix and standardise meanings (Pearson, 1998, p. 11). This results in limiting the scope of formulation of paraphrases which thus become standardised. As argued earlier in this chapter, limiting the possible paraphrases effectively limits the possible interpretations, thus fixing the meaning of terms. Terms are thus words that have become ritualised in use, giving the impression that their meaning has become fixed.

Terms, in this sense, are discourse created lexical objects with a special restriction on their use and interpretation in specialised discourses. They are negotiated and contingent lexical objects, formulated to appear as fixed and language independent objects. Terms are invented by scientists and specialists and their meaning is fixed by an agreed definition, which is explicitly formulated and strictly adhered to (Bloomfield, 1969, p. 38). According to Pearson (1998, p. 22), terms are products of collaboration between terminologists and subject specialists and as such have prescribed meaning. In terminology, this meaning is defined through conceptual ontologies, which may create an illusion that their meaning is not dependant on context and negotiation. However, both the context and social negotiation are necessary for formulation and understanding of terms – the negotiation is implied in the process of creation of terms, whereas the context plays an important role in differentiating terms from ordinary words.

As Pearson (1998, p. 8) argues, in the field of terminology ‘there is no usable definition of term and no adequate communication model which allows us to identify when words are being used as terms.’ Thus, without context it is hard to distinguish terms from words in general. Arguing that terms have meaning regardless of the context would be to disregard the fact that they occur almost exclusively in highly specialised domains and contexts, usually in scientific and technical texts. The definition of terms as lexical items used in specialised language requires that specialised, scientific context in order to construct these lexical items as terms; outside that context they are just signs with a potential for meaning, which is ultimately realised in context.

2.3.3 Summary

Taking the concepts and conceptual ontologies, as tools of terminologists, aside, the conclusion is that meanings, both of words and terms, are defined and negotiated in discourse. To set a definition of a term is to paraphrase the term, offering an interpretation that defines its meaning. Whether that definition is offered in the mode of ontology or paraphrase is not relevant, as both act as a form of discourse interpretation of meaning. This is the social process of construction and negotiation of meaning.

The purpose of terms is to stabilise and fix meaning; thus Sinclair concludes that terms 'do not adapt to their verbal environments; the words are as isolated as the sentences, and the phraseology is in constant danger of becoming ritualized' (Sinclair & Carter, 2004, p. 159). Thus, the purpose of terms is to have constant unchanged meaning regardless of the context they occur in. However, this definition can be reversed by arguing that terms have fixed meaning exactly because of the fixed contexts they occur in. The context of specialised language, such as those of particular subject disciplines, is restricted and characterised by its own language norms, thus restricting the formulation of the paraphrase to a specialised discourse and giving terms the illusion of fixed facts. Outside the discourse of subject disciplines, terms can be, and often are, used as any other words. Furthermore, apart from context, there is no methodology that can differentiate between words and terms. Thus, to conclude, the meaning of both terms and words is constructed in the process of social negotiation and realised by the contexts they occur in.

2.4 The Species Problem

This section reviews the historical context of Charles Darwin's research with the aim to demonstrate why an analysis of his correspondence focussing on the word *species* is a suitable approach for the study of meaning construction. The section will also present an overview of the interpretation of the meaning of the word *species* at the time and review the development of the notion of *species* since the publication of Darwin's theory. The aim of this review is to support the argument that regardless of scientific discoveries, the meaning of the word *species* will always be open to reinterpretation. Thus this section aims to argue that the analysis of Darwin's correspondence can be viewed as an analysis of negotiation of meaning regardless of whether or not species exist as natural kinds.

The analysis of the word *species* is important not only because species were the focal point of Darwin's research, but also because of the historical context his research was conducted in. Namely, Darwin did not publish his theory on transmutability of species until decades after first considering the notion (Ruse & Richards, 2009). Rather, he discussed it with a small group of young researchers, whom he deemed more likely to agree with him. Thus, Darwin's correspondence is the best source for the analysis of how his interpretation of the meaning of the word *species* was formed.

The study of the word *species* is important because demonstrating whether the meaning of the word *species* is based the physical properties of 'species as natural kinds' or is the product of social interactions reflects the theoretical

discussions presented earlier in this chapter. In other words, if there is a 'truth' about species, then the meaning of the word is not so much a product of negotiation but rather the product of uncovering the 'facts' about species. However, with more than a century and a half of scientific progress since the publication of Darwin's theory, the meaning of the word *species* is still a matter of scientific debates. Discovering new 'facts' about species only increased the number of interpretations of the meaning of the term *species* (Hey, 2001, p. 6). Thus I will argue that the problem of species is a problem of meaning.

This problem existed already in Darwin's time, which is evident in his correspondence. Many naturalists had different interpretations of what species are, which often reflected in their work. This was particularly evident in the different approaches to natural classification. For example, in one of his letters, Darwin reports on opposing classifications made by fellow naturalists:

Taking J. D. Hooker & Jordan as representative men for the opposite factions in botany,—'lumpers & splitters', the former would reduce the species of Vascular plants to three score thousand, or perhaps much fewer;—while Jordan would raise them to three hundred thousand. [www.darwinproject.ac.uk/letter/entry-1740]

Until Darwin, the problem was considered merely of finding the right classification; there was a general consensus that species are unique and distinct, but also fixed and immutable (Hey, 2001, p. 7). However, Darwin's assumptions not only contradicted the interpretation of what species were, but, more importantly, they clashed with the creationist views held by many of his clerical mentors. Thus it is understandable why Darwin was very cautious in expressing his assumptions.

Finally, Darwin developed his theory in relation to the context, or discourse, which he was a part of. His research was based on the knowledge and theories some of which he later disproved. Thus Darwin's research can be interpreted as the product of Victorian society, as much as a reaction to it. For these reasons, it is important to review the context of his research in order to gain a better understanding of the connotations of his correspondence.

2.4.1 Historical context of Darwin's theory

Darwin's career as a naturalist effectively started with him setting out on the *Beagle* voyage. During his studies at Cambridge, Darwin became an avid beetle collector and a close friend of his botany professor, John Henslow (Desmond, Moore, & Browne, 2007, p. 13). Henslow taught Darwin natural science and the principles of Linnaean classification, and referred him to Adam Sedgwick, a professor of geology at Cambridge, whom Darwin accompanied to north Wales where he gained field experience in geology. Upon graduation, Darwin was, on Henslow's recommendation, invited to join the *Beagle* expedition by the captain Robert FitzRoy. He was to join the expedition, at his own expense, more as a gentleman scholar and companion to the captain, since the ship had already had a naturalist assigned (for further details about his life see Desmond, 2007).

Although, at the beginning of the voyage, Darwin was practically unknown in naturalist circles, he quickly started making a name for himself. He collected and described innumerable amount of specimens and made important geological observations, which he regularly reported to Henslow or Sedgwick.

Some of his letters were shared among other naturalists by Henslow and Sedgwick; thus after many years on the voyage, Darwin returned to Britain as a young, aspiring naturalist.

Soon after his return from the *Beagle* expedition, Darwin was invited to present his findings to the Zoological Society museum on 4 January 1837 (Desmond, et al., 2007, p. 32). For Darwin, at the time, this was a common scientific practice where scholars would meet at their respective societies and present their papers to a group of peers. Apart from society meetings, Darwin often discussed science at gentlemanly dinners and soirees on a more informal level. With his father's financial support, Darwin fulfilled the conditions and his reputation as a gentleman scholar was quickly advancing. He regularly presented papers at the meetings of Zoological, Geological, Linnaean and other relevant societies, even taking up different roles within them (Desmond, et al., 2007, p. 31). With the publication of his *Beagle* journals, which proved very popular, Darwin fully established himself as a gentleman scholar.

After John Gould identified the supposedly different species of birds from Galapagos Islands as closely allied species of finches, Darwin started to question some the ideas on immutability of species his mentors at Cambridge had instilled in him (Desmond, et al., 2007, p. 33). Darwin set out on a quest of proving transmutation, initially viewing it as a form of natural law akin to Newton's Laws (Ruse & Richards, 2009, p. 3). Thomas Robert Malthus' *Essay on the Principle of Population* directed Darwin on the path to his theory of natural selection. But at this stage his theories were neither fully formed nor fully

supported with evidence; thus he continued to present papers on other topics keeping his theories to himself.

Darwin was reluctant to publicly express his views not only because they contradicted the mainstream interpretations, but also because they had religious connotations. Thus he feared offending 'precisely the leaders of his scientific set – those very men who had nurtured him and made his early career possible' (Ruse & Richards, 2009, p. 8). Since people like Adam Sedgwick and William Whewell controlled his science set (Ruse & Richards, 2009, p. 9), Darwin felt the need to move from the more ecclesiastical scientific circle in Cambridge to London, where he found himself in a more secular milieu, which was more 'conducive to his private musings on extinction and repopulation' (Desmond, et al., 2007, p. 33).

Although Darwin started developing his theory not long after the *Beagle* expedition, there was a delay of more than 20 years before the *Origin* was published. Darwin was very cautious about expressing his ideas fearing they would undo all his hard work to be recognised as a man of science. The publication of his *Journal* gave him a worldwide reputation in scientific society; 'animals and plants were now named after him; among London's savants he was also rated highly – he served as a Geological Society vice-president in 1844' (Desmond, et al., 2007, p. 49) – all that would be at risk had he published his ideas without sufficient evidence. More than just evidence, Darwin felt that he would need supporters before publishing his controversial ideas. Thus he started sharing his ideas on transmutation with 'potential supporters and who, although they may have been cowed by people like Whewell, certainly did not

necessarily agree with them' (Ruse & Richards, 2009, p. 9). He approached the subject of transmutation cautiously 'like confessing a murder'³, fearing the rejection of his ideas. In London, Darwin managed to establish himself in a growing secularist network, 'which was to make the world safe for Darwin's theories' (Desmond, et al., 2007, p. 56). There he met Thomas Huxley and Herbert Spencer, who relaunched the *Westminster Review*, 'seeking to claw power from a church establishment' (ibid). Darwin saw men like Huxley and Hooker as potential key supporters for publishing his theory.

By 1854, it was widely suspected that Darwin was an evolutionist (Ruse & Richards, 2009, p. 4) having discussed the issue with several young researcher privately and encouraged them to follow their research in this direction. When *Vestiges of the Natural History of Creation* was anonymously published in 1844, some people attributed it to Darwin, which made him read its reviews uneasily (Desmond, et al., 2007, p. 49). The book was a popular account of a theory of evolution and received severe criticism from the leading scientific and theological scholars. This made Darwin even more cautious about sharing his views, but also more adamant to gather more evidence to support his theory, further delaying the publication of his essay.

On 18 June 1858 Darwin received a letter from Alfred Russel Wallace outlining a seemingly identical theory of natural selection to his own (Desmond, et al., 2007, pp. 63-64). Darwin and Wallace corresponded previously on the species problem, but not in such detail. Wallace requested his paper presented at the

³ [<http://www.darwinproject.ac.uk/letter/entry-729>]

Linnaean society via Charles Lyell, who, not willing to take away the originality of Darwin's theory, proposed that they present a joint paper (Desmond, et al., 2007, p. 64). On 1 July 1858, both papers were presented at the Linnaean society, with Darwin presenting excerpts from his essay followed by Wallace's paper. Both papers were soon jointly published, and, with his ideas finally publicly expressed, Darwin felt the urge to complete and publish the full account of his theory as quickly as possible (ibid).

Although having some supporters at the time of publishing the *Origin*, Darwin's theory remained a very controversial issue for years to come (Wilkins, 2009, pp. 135-165). After 1866 Darwinism 'began to dominate the relevant sections of the British Association where Darwin's chief supporters, Hooker and Huxley, were presidents respectively in 1868 and 1870' (Desmond, et al., 2007, p. 72). Nevertheless, it took much time and effort for Darwin to convince people to his ideas. Darwin's financial wealth helped him with promoting his theory by subsidising translations around the world and financing the publication of favourable reviews from abroad, notably those of Fritz Mueller and Asa Gray (Desmond, et al., 2007, pp. 72-73).

For the development and success of his theory, Darwin relied much on the correspondence with the more liberal researchers of the time. Not only did Darwin use correspondence to find supporters for his assumptions, he also discussed and further developed his ideas in the process of convincing them to his views. With his ideas kept secret from much of the scientific community, his correspondence represents the account of the development of his theory and the context in which the meaning of the word *species* was negotiated.

2.4.2 Historical overview of the Species Problem

The species problem is a problem of meaning, or rather different interpretations of the meaning of the word *species*. As Hey (2001, p. 5) argues, the problem is not caused by the shortage of answers to the species problem, but rather the 'awkward shortage of consensus.' Similarly, Richards (2010, p. 4) argues new information on species 'seems to have resulted in the multiplication of species concepts.' The multiplication of species concepts becomes the species problem when the term *species* is applied; this is exemplified by Hey (2001, p. 20) who shows an example of classifications of birds which according to one definition produced 9,000 different species, and based on another it can easily amount to 20,000. In essence, this is the so-called *species problem*.

2.4.2.1 Species before Darwin

The argument that species are immutable was based on a tradition of discourse on species dating back to Plato and Aristotle who argued that species are defined by their essences (Wilkins, 2009, p. 102). Thus, species were essentially considered natural, eternal and unchanging entities, a notion that had been widely accepted until Darwin.

For medieval philosophers Peter Abelard and William of Ockham species were defined as mental concepts, which nevertheless reflect 'real features of the world' (Richards, 2010, p. 114). In that sense, Linnaeus' developed his classificatory framework on the assumption that species are natural and immutable kinds. Linnaeus accounted for variation between different kinds by designating the term *species* as the second lowest category in natural

classification and designating the lowest category to the term *varieties* (Sloan, 2009, p. 69). The classification was based on the observation of the natural features of the specimens and grouping of the specimens exhibiting the same features as the same species. This was in philosophical aspect a turn from Aristotelian and Platonic species defined by their intension, i.e. their essence of being species, to an extensional set to which members exhibiting the same qualities or features are included.

Although not challenging the nature of species as natural kinds, Georges-Louis Leclerc, comte de Buffon redefined the Linnaean approach to the classification of species arguing that it was artificial and arbitrary (Sloan, 2009, p. 70). Buffon's species had two dimensions: synchronic, or horizontal, where the membership was defined by relationships of interfertility'; and diachronic where the members of the same kind are connected by descent from common stock (Sloan, 2009, p. 71).

Darwin was much inspired by Buffon's ideas and based his work on classification mostly combining the Linnaean and Buffon's approaches, thus analysing species for similarities and differences, but also considering their descent. However, Darwin came to the conclusion that 'reproduction and sterility, ecological functioning, geographic distribution and similarity' although relevant, were not the core of the definitional structure (Richards, 2010, p. 199).'

Darwin focused more on the distinction between varieties and species, and by applying Linnaean classification, defining the former as 'a group of organisms that were largely similar, but not distinct from each other' and the latter 'as groups of organisms that were distinct from other groups' (Richards, 2010, p.

199). However, when taking Buffon's approach to distinguish different lineages, Darwin realised that there were different ways of drawing boundaries between *species* and *varieties* (Richards, 2010, pp. 199-200). Darwin frequently expressed his inability to distinguish between the two terms, for example, in a letter to Hewett Cottrell Watson he states: '*The difficulty to know what to call vars & what species,—hopeless—*.'⁴ More importantly, developing his ideas of mutation and evolution of species, Darwin created the notion of 'incipient species' as a form of varieties on the verge of becoming new species. With species being formed out of varieties and new varieties formed from species, Darwin's conclusions were that there could not be a clear boundary between the two. This conclusion of Darwin's changed the foundations of the understanding of species. As species were not deemed permanent, the questions were asked if they were real or just mere categories of classification.

2.4.2.2 Species after Darwin

The debate on the nature of species has continued since Darwin to this day. As the science of species evolved, further sub-disciplines emerged and with them further definitions of species. Just the number of sub-disciplines and their aspects of studying species is too long to be listed here, not to mention the near infinite discussions and definitions of the term. However, from the more philosophical aspect, several major formulations of the nature of species have been developed since Darwin. In their abstraction, these formulations touch on the nature of species without forming a specific definition, unlike practical

⁴ [<http://www.darwinproject.ac.uk/letter/entry-1616>]

approaches to the notion which will adapt the definition to the specific aspects of the research.

Hey (2001, p. 104) describes that species in such formulations are 'quintessential natural kinds and they exist as categories in our minds, complete with prototypes'. Further adding that 'many of ... our mentally constructed natural kinds of organisms, actually do correspond to real entities in nature' (2001, p. 105). This proposition is in agreement with the cognitivist description of language meaning, discussed earlier (see Sections 2.1.1 Language as a psychological phenomenon and 2.1.1.2 Cognitivist approach). The implication of this approach is of existence of species as natural kinds, even as mutable, and proposing that human mental capabilities are perfectly able of distinguishing different and similar objects to create a corresponding concept in the minds of individuals. However, from the aspect of meaning construction and its realisation in discourse, it has been thus far argued (see Sections 2.1.2 Language as a social phenomenon and

2.2 Constructing meaning and reality) that the conceptual formulation of meaning is an unlikely hypothesis.

Another formulation of species as kinds is described by Richards (2010) as *pragmatic pluralism*. According to this approach 'we group some plants into *houseplants*, and others into *weeds*. We group some animals into *pets*, others into *wild animals*, and yet others into *farm animals*' (original emphasis) (Richards, 2010, p. 115). Thus, in each case categorisation is based on real features of the observed objects, but the groupings are made in an arbitrary fashion. Essentially, this is how Darwin perceived Linnaean classification,

deeming the difference between *species* and *varieties* arbitrary. This approach corresponds with the theory of social construction of meaning, since social functions of these objects are included into the definition of the notion.

Ontological pluralism is another formulation of species mentioned by Richards (2010). According to this proposal species category is heterogeneous 'and includes different kinds of things' (Richards, 2010, p. 117). Thus, according to this view, a plurality of definitions of species is not only acceptable, but also necessary. For example, Hey (2001, p. 10) reports how in field-biology a set of terms was developed to categorise various groups of closely related plants (e.g., gamodeme, topodeme, ecodeme). He further reports that these words 'were created with clear meanings, and they are sometimes used to considerable effect, vastly clarifying discussion that would be more muddled if only species were used.'

A similar formulation describes species on two separate levels: as theoretical and operational concepts. Richards (2010, p. 119) describes this approach as *hierarchical pluralism*. This view does not see species as necessarily a category of different kinds, as in ontological pluralism, but rather differentiates theoretical and practical definitions. This exemplified by applying working definitions of the term species for particular analysis while assuming that all different definitions of the practical concept define together the theoretical concept. This approach has similarities with discourse construction of meaning (see Section 2.2) where the meaning of a word is seen as a totality of its uses in discourse.

From these different formulations of species, we can observe that the goal of most of these approaches was to establish a unifying definition. Hey (2001, p.

12) argues that there ‘is a partially hidden consensus that we are all trying to explain the same thing and that we can only partly see that thing.’ Regardless of their ‘true reality’, species certainly exist as categories in language, which we can focus on in our analyses. In this sense Hey (2001, p. 107) states that the definitions of species:

‘whether or not any or all of them match up well with real evolving entities out there in nature, they are all categories, each and every one has been devised by a person or persons as a way to help organize our understanding about biological diversity.’

From the history of the debate on the species problem, we can argue that the meaning of the term *species* has hitherto been a product of negotiation in discourse. Some of the problems and interpretations that Darwin faced may have been lost from the debate, but the principle of reaching a consensus to form a working definition of *species* has remained to this day. All the advances in science did not help to resolve the issue and unify the definitions, but rather added new interpretations to the debate.

2.4.3 Summary

The aim of this section was to review the historical contexts of Charles Darwin’s research and the species problem in order to support the argument for the analysis of the process of meaning negotiation using the word *species*.

The review of the historical contexts of Darwin’s research and the species problem demonstrated that the meaning of the term *species* has been negotiated for centuries. The purpose of reviewing the history of the species

problem was to show that scientific progress on the matter did not end the debate on the meaning of the term nor brought it closer to resolution. Hilary Putnam (1979) argued that the authority for the meaning of certain terms in discourse is assigned to the relevant specialist, whom we come to for explanation. However, questioning a multitude of experts will often, if not always, result in a multitude of interpretations. Scientific discoveries and attempts at reformulating the definition of the word *species* has always resulted in new interpretations added to the discourse. As Hey (2001, p. 9) summarised: 'Darwin has left us stranded on a word, and modern biologists are semantic castaways, trapped with a word of little common meaning, struggling to fix the situation by puzzling their way out of it.' Thus, the history of the species problem has shown that the meaning of species is not contained in any single interpretation, but rather in the sum of these interpretations. In other words, to study the meaning of a word like *species* it is necessary to study its interpretations in discourse.

On the other hand, the review of the historical context of Darwin's research demonstrated that Darwin's theory of species was formed gradually over two decades before its publication. As his ideas contradicted the main interpretations of *species*, Darwin negotiated his theory with fellow-minded researchers in order to find supporters before publicising his ideas. However, due to his health problems, much of the discussion was confined to written correspondence, which makes it a significant resource for the study of meaning negotiation.

To conclude, the focus on the word *species* is justified by the fact that the notion was central to Darwin's theory. Focusing on Darwin's correspondence to observe the negotiations of meaning is justified by the extensive discussions spanning more than two decades before Darwin published his theory. For these reasons, the study of Darwin's correspondence is a valid approach to observe how Darwin's own interpretation of the meaning of *species* was negotiated.

3 Research Framework

3.1 Introduction

The theoretical position and the research framework presented in this thesis are based on the conclusions drawn in the literature review. Specifically, the research framework is based on the theoretical assumption that linguistic meaning is both constructed and realised in discourse.

The first part of this chapter focuses on combining the conclusions of the literature review with the approaches in Corpus Linguistics and its practice of investigating discourse with the aim of analysing various aspects of language. In this section, the theoretical assumptions of discursive meaning construction are related to the theoretical positions and research practices of investigating large amounts of discourse data with the purpose of analysing various aspects of language.

The second part focuses on developing a research framework based on specific approaches and methods used in Corpus Linguistics which support the theoretical assumptions developed in the literature review. In particular, this section addresses the practical aspects how the meaning of words can be analysed in discourse, how the processes of meaning negotiation and construction can be observed, and how to observe the diachronic dimension of meaning.

3.2 Theoretical position

The general theoretical and methodological approach in this thesis is based on the British approach to Corpus Linguistics, particularly the Birmingham approach to the study of language. The approach is characterised by the corpus-driven analysis (Hunston, 2002), its focus on the relation between lexis and grammar (Hunston & Francis, 2000), questioning the use of corpus annotation and the focus on collocation in the study of contextual meaning (Sinclair, 1991; Sinclair & Carter, 2004; Teubert, 2005; Teubert & Cermáková, 2007) (for a general overview of different approaches in corpus linguistics see McEnery & Hardie, 2012).

The theoretical position positing the construction of meaning through negotiation in discourse is largely influenced by the theories of *dialogism* and *intertextuality* (See Section 2.1.2 Language as a social phenomenon), and is mostly compatible with corpus linguistics theory and methodology. Namely, the analysis of large amounts of linguistic data makes no assumption on discourse external meanings and facts, with the only aspect of the analysis being what is and can be observed in discourse. This is the theoretical approach outlined by Teubert (2005) with the purpose of applying corpus methodology in the study of meaning.

As discussed in Section 2.1.2 Language as a social phenomenon, it has been demonstrated by corpus analysis that the meaning of words is determined by the linguistic contexts they occur in (Sinclair, 1991; 2004). Furthermore, corpus linguistics has managed to observe meaningful patterns and relations between

lexis and grammar (Hoey, 2005; Hunston & Francis, 2000). Such developments in corpus linguistics allowed linguists to analytically approach the issue of meaning and make significant developments in the field of lexicography. Thus, as Teubert states (2001, p. 136) the assumption that meaning of texts 'can be found solely in discourse' became the basic tenet of corpus linguistics.

With respect to meaning construction, the theoretical framework is based dialogic theories of language, such as Bakhtin's (1986; 1994) notions of *dialogism* and *heteroglossia*, as well as Kristevan and Barthesian notions of *intertextuality* (Barthes & Heath, 1977; Kristeva & Moi, 1986) (see section 2.1.2 for a detailed overview). Specifically, as corpus linguistics is an analytical discipline, for a theory of meaning construction it was necessary to review the philosophical formulations of meaning construction. The main aspect of these theories is that they abandon the monologic approach of traditional grammarian where the utterance is always considered in isolation. For Bakhtin, Kristeva and Barthes meaning is realised in dialogic encounters in which the interpretations of meaning are constantly exchanged and through which new meanings arise. Thus language has not only a dialogic, but also a diachronic dimension, leading Teubert (2010, p. 216) to conclude that '[m]eaning is the result of adding, over time, one interpretation on top of the other.'

To conclude, the propositions of dialogic and intertextual theories are complemented by current corpus methodologies and discoveries. For example, Teubert (2010) argues that the process of reinterpretation of previous meanings can be observed in discourse as an act of *paraphrase*. Furthermore, as paraphrases imply a degree of lexical repetition, the more common and

accepted meaning will be easily identified through the analysis of collocation. Also, apart from the lexical repetition, intertextual relations of interpretations can be identified in discourse through reference markers. And finally, the diachronic dimension can be analysed by observing all of these elements of meaning construction in historically or diachronically marked data sets.

3.3. Research framework

3.3.1 Collocation and Paraphrase

As stated in the theoretical position, collocation and paraphrase are for corpus linguists key elements for the study of meaning in discourse. Collocations show which words typically co-occur together and thus in a way identify contexts, in the sense of immediate textual environment, in which these words occur in. Collocations in Sinclair's (1991) terms are simply words that frequently co-occur with one another. They are usually identified using statistical methods, since the most frequent words in a language are usually the so-called 'grammar words' (Hunston, 2002), which make up the most frequent collocates of almost any word. For this purpose a variety of statistical methods are used; for example, t-score statistics is used to measure the 'certainty' of a collocation (Hunston, 2002, p. 73), meaning that the selected words co-occur together more frequently than it is statistically expected; on the other hand, Mutual Information (MI) score measures collocations that tend to co-occur exclusively, giving a measure of the strength of collocation (ibid). Although there are other statistical measures which are employed in calculating collocations, t-score and MI score are the only ones included in this thesis.

Apart from identifying context in which words frequently occur, the study of collocations can also unveil 'hidden' meanings of particular lexical combinations which are realised as semantic preference (Stubbs, 2001:65) or semantic prosody (Louw, 1993). These are essentially interpretational cues that ascribe certain implied and hidden meanings to particular word combinations. For

example, Sinclair (1991) discusses the collocation 'break out', which usually denotes negative meanings, e.g. 'fire broke out', 'disaster breaks out', etc. Thus a study of collocations in context can unveil meanings which may be beyond the intuition of the speaker and it is only through the analysis of large amounts of discourse data that these meanings can be observed.

Teubert (2005, p. 5) argues that statistical approaches play an important role in identifying the recurrent features in language allowing linguists to make general statements about language, however, on their own, statistics are not sufficient to study the linguistic meaning. Instead, Teubert (2005) suggests that the study of meaning can be achieved in terms of analysing paraphrases as interpretations of meaning. The most prototypical examples of explanation in the form of paraphrase can be found in dictionary definitions (Teubert, 2010, p. 219). Thus, paraphrase acts as a form of explanation, explication or (re)interpretation of particular lexical items (Teubert, 2005). Discourse is abundant with such paraphrases where speakers will explicitly state their interpretations of certain meanings. Furthermore, in the dialogic encounter these interpretations will be negotiated in the act of paraphrase where the agreed notions will be repeated and new information added into the discourse. Thus, Hunston (2002, p. 38) argues that paraphrases, as statements which express interpretations of meaning, can sometimes be more useful in corpus linguistics than the traditional statistical approach.

Paraphrases, being utterances made in relation to previous discourse, necessarily have a diachronic dimension, which makes them ideal objects for the study of the diachronic change of meaning. Furthermore, the dialogic aspect

of paraphrase implies that the paraphrased content is often intertextual, i.e. modifying other interpretations. This is supported by Koteyko (2006, p. 150) who argues that the study of paraphrase allows a 'detailed and documented diachronic analysis of intertextuality.' In this sense, Teubert (2005, p. 12) concludes that paraphrases are the crucial element for the corpus-driven theory of meaning.

3.3.2 Intertextuality

Formulation and negotiation of meaning in discourse takes the form of paraphrase which involves reformulation of previous discourse in a new utterance. In other words, negotiation and interpretation of meaning can only occur as reactions to previous formulations. Thus, as Bakhtin (1986, p. 72) argued, one cannot understand an utterance as unconnected to previous utterances or works. In this sense, paraphrase implies intertextuality, since only the previous and known discourse can be paraphrased.

For this reason, the research on negotiation of meaning is inherently based on intertextual and dialogical theories (see Section 2.1.2 Language as a social phenomenon for a detailed review) which define discourse as the place or process where meaning is realised. Namely, the theory of dialogism (Bakhtin, et al., 1986; Bakhtin, et al., 1994; Dentith, 1995) argues that meaning is only realised in concrete utterance between discourse participants. Specifically, meaning is realised by the interpreter of the message rather than the speaker. As any utterance makes response to a previous utterance and is calculated to

be responded to in turn (Bakhtin, et al., 1994, p. 35), meanings are perpetually reinterpreted and renegotiated in discourse.

The notion of dialogism is adopted and further developed by Julia Kristeva (1986) and later by Roland Barthes (1968; 1977) into the theory of intertextuality, which defines all utterances in discourse as interconnected to one another. Simplified, the theory implies that meaning is formed of a totality of interpretations in discourse.

This view is taken by Teubert (2005), who argues that meaning of individual words can be observed as a totality of their uses in discourse. Thus, any utterance can be viewed as a definition of meaning since it presupposes that it is an interpretation of the previous meanings. In other words, any utterance is effectively a paraphrase of meaning (Teubert, 2010).

By observing paraphrases of previous meanings, it is implied intertextual relations can be observed too. Namely, as intertextuality is realised in paraphrases, it is usually marked by lexical repetitions of some form. In this sense, Stubbs (2005, p. 21) argues that intertextual relations can be discovered by recurrent phrasal patterns. Since, lexical collocations are products of repetition in discourse, they are inherently intertextual constructions. A similar conclusion is made by Cheung (2009, p. 111) who argues that implicit intertextuality is realised in lexical collocations.

The theoretical position in this thesis is that intertextuality is the cause of lexical repetitions and that all paraphrases are inherently intertextual. Intertextuality is realised in, and hence marked by, both the paraphrase and lexical repetitions

realised as collocations. Alternatively, intertextuality can be marked by so-called *intertextuality markers*, which explicitly identify intertextual links to other discourse. For example, Cheung (2009, p. 103) lists the following intertextual markers in her analysis: *(I) agree with (X)*, *same as X*, *according to X*, *as/what X (said)*, *as/what X (mentioned)*, *all of us*, *(I) share as (X)*, and *other than*. In my research, the markers of intertextuality were identified manually by analysing all the paragraphs and lines in which the term *species* appeared.

3.3.3 Diachronic analysis

The study of the construction of meaning based on the theory of intertextuality inherently has to include a diachronic analysis. Teubert (2010, p. 216), in this sense, argues '[m]eaning is the result of adding, over time, one interpretation on top of the other. The discourse has of necessity a diachronic dimension.'

Different interpretations gain on importance by being accepted and repeated by the members of the discourse community. Items of meaning that are repeated act like 'memes' as described by Dawkins (2006), and like successful genes, they remain successful as long as they are repeated. This repetition is realised in a paraphrase, thus it is characterised by an admixture of new interpretation. Intertextuality assures that there is no true repetition only re-interpretation, which brings out the diachronic dimension of meaning. As Dawkins (2006) argued, in terms of evolution, fecundity is the most important aspect. Thus meaning evolves through reinterpretation. Once the reinterpretation stops, no matter how successful the gene or meme was up to that point it quickly disappears and gets replaced by another (Dawkins, 2006). Dawkins' meme

theory forms an important aspect of the diachronic dimension of meaning. Namely, the notion of meme corresponds with the notion of interpretations, thus interpretations which have formed the meaning of a term at one point in time, may no longer be a part of the discourse. In that aspect, linguistic meaning is never stable or finite.

There has been little work on the study of diachronic change of meaning, particularly on the processes that drive that change, i.e. the negotiation of meaning. Partington (2013) reports on several analyses which essentially compared historical corpora, but in terms of a more fluid approach to diachronic analysis there has been little research done, particularly in the field of corpus linguistics.

Cheung's (2009) research is closely related to this one as her research focuses on analysing the paraphrases of meaning in a diachronic dimension and observing the types of paraphrases that cause the diachronic meaning change. However, Cheung's (2009) diachronic approach is realised as a sequential analysis of meaning negotiation and does not observe if any of the interpretations get abandoned by the community.

An alternative approach to a diachronic analysis is to observe how and when certain paraphrases enter discourse, how they are interpreted and how long they remain in the focus of discourse. This approach takes a cue from Maturana and Varela (1980) who argue that once an autopoietic system like discourse stops reproducing it naturally ceases to exist. When this is applied to the interpretation of meaning, the diachronic dimension has to include in its scope the fecundity of interpretations.

The cue for the diachronic analysis comes from Ute Römer's (2010) approach to the study of phraseological patterns in terms of their distribution in textual segments. Namely, by applying the same empirical approach in a diachronic corpus and the analysis focusing on the negotiation of meaning, it is possible to observe the 'life' of interpretations in discourse. The study of paraphrases in a corpus "allows a detailed and documented diachronic analysis of intertextual links that uniquely characterise any text segment in the focus of analysis" (Koteyko, 2006, p. 150).

4 Methodology

4.1 Introduction

As mentioned in the previous chapter, the issue of meaning construction is addressed by using corpus linguistic analyses of collocation, paraphrases and intertextual markers. As the theoretical framework clearly places the meaning construction in discourse and context, and the analytical work in corpus linguistics has so far demonstrated the dependency of meaning on context, corpus linguistics is the ideal method to approach the analysis of the construction of meaning in discourse.

This chapter describes in detail the method of collecting and organising data for the analysis, the methodology used to analyse the data as well as the computational tools used to perform the analysis. Thus, the first section of this chapter describes the methods used to collect the data and build the corpus of correspondence, the organisation of the corpus as well as the statistical information about data included in the corpus. The second section describes in detail the methods used in the research and how they were adapted for the diachronic analysis. Finally, the third section discusses the tools and procedures used in the research.

4.2 On the Corpus

Thus far, corpus linguistics methods have been generally applied to observe language from a synchronous perspective. On the other hand, the diachronic

approaches in corpus linguistics were usually limited to comparing different historical corpora in order to analyse how language changed (See Section 3.3.3 Diachronic analysis). However, for the purpose of this research, it is not necessary to develop completely new methods for a diachronic approach to discourse, but rather to compile the corpus in a way that will represent a diachronic account of discourse. Thus, a corpus of correspondence was ideal for such purpose.

The Darwin Correspondence Corpus was compiled using resources on the Darwin Correspondence Project (Cambridge, 2013) which has made most of the correspondence of Charles Darwin available online. The corpus used for this analysis contains a significant proportion of these letters with a focus on Darwin's scientific work rather than personal letters. This selection was achieved by searching only for the letters containing the key terms of Darwin's scientific work, namely: *species*, *varieties*, *natural selection*, *evolution* and *descent*, as well as their lemma and verb forms, e.g. *vary*, *variety*, *descend*, etc.

The whole corpus consists of 40,002 word types amounting to 1,167,555 word tokens, thus making it as large as some general language corpora, e.g. Brown and Lancaster–Oslo–Bergen (LOB), each of which consists of one million word tokens. The corpus comprises 2,096 letters both written by and addressed to Charles Darwin, which date from June 1828 to late December 1852. The letters are mostly ordered in chronological order and are coded in the same manner as on the Darwin Correspondence Project website. In particular, each letter is numbered in a way that corresponds with its chronological order in the correspondence. Occasionally, the numbering of the letters does not match the

date; however, the size of the corpus is sufficiently large to account for that issue. For example, the key term, *species*, occurs more than 5,000 times; thus, a few inconsistently placed letters will not influence the analysis significantly.

In addition to the main corpus, a smaller sub-corpus consisting of only the paragraphs containing the word *species* has been compiled for the purpose of investigating the markers of intertextuality. The whole corpus, which is in size comparable to general language corpora, contains a very high frequency of intertextual markers, most of which are not relevant to this research as they refer to numerous aspects of Darwin's life. On the other hand, eliminating all the intertextual markers which do not have the word *species* as part of their sentence would eliminate many relevant intertextual markers, which in their immediate context are related to the negotiation of the meaning of the term. For these reasons, paragraph was chosen as the relevant unit for this analysis. Furthermore, by choosing paragraphs which contain the word *species*, the main aspects of the research have remained comparable as the number of occurrences of the term *species* is identical in the main and the sub-corpus.

4.3 Research tools

As the analytical tool for analysing corpus in terms of concordances, collocation analysis and paraphrase analysis, AntConc 3.2.3w (Anthony, 2011) corpus access software is used. AntConc has all the features necessary for the analysis in this thesis built into the default software package. Furthermore, it allows for the search of the corpus using an extensive list of wildcards as well

as regular expressions which makes the search for particular paraphrases more efficient.

Apart from the methods of analysis used in this research, presented above, AntConc supports several other methods for corpus analysis, such as n-gram viewer, labelled as 'clusters' function in the program, file view, keyword analysis and concordance plot. File view is a function allowing, as the name suggests, the search of within the data file, thus allowing the observation of the whole context rather than just observing concordances. Keyword analysis allows for the calculation of keywords by including a reference corpus in the search. Finally, concordance plot shows the distribution of the search terms in the data files, presented as a white long bar representing the file, on which vertical black lines represent occurrences respective to the location in the file.

4.4 Methods

This section describes the methods and procedures used in the analysis, most of which are firmly grounded in corpus linguistic methodology and include the most ubiquitous methods such as key-word-in-context (KWIC) concordances and collocation analysis.

The key-word-in-context concordance is one of the oldest and most common tools in corpus linguistics and is realised as a line of text with the search term, also known as the *node*, centred so that context can be observed on either side of the node (Sinclair, 1991, p. 115). This is particularly useful for observing the immediate context of the search term, which allows for quick observations of

general patterns the search term occurs in. For example, just by searching for the word *species* in the corpus and sorting the concordances by the alphabetical order of the words occurring immediately to the left of the node, we can have a quick glance at the most frequent adjective modifying the node. Thus, this type of concordance is efficient for probing the text or refining search terms which produce too many irrelevant concordances.

Collocation analysis can be defined as statistical analysis of key-words-in-context concordances with the goal to determine which words co-occur with the node word most significantly within a set search span. Thus, Hoey (1991, pp. 6-7) defines collocation as a statistically significant co-occurrence of two or more words. The most ubiquitous statistics used to calculate the strength of collocations in corpus linguistics, also used in this thesis, are the t-score and Mutual Information, or MI-score.

T-score analysis is described by Hunston (2002, p. 73) as 'a measure of certainty of collocation', in comparison to MI-score which was described as a 'measure of strength of collocation' (ibid). Hunston's definition is based on the fact that t-score is more affected by the size of the corpus, or more accurately the frequency of the words calculated, since the most frequent words in language tend to occur more often in larger than in smaller corpora. As t-score is calculated based on the frequency of the item within a corpus, in larger corpora this value will be disproportionately higher thus affecting the t-score value. Thus, Hunston (ibid) describes it as a measure of certainty, since the higher value indicates that particular node frequently co-occurs with the collocate. However, if the node and the collocate were swapped, the t-score

would become inversely proportionate, as the very frequent word would be compared with a word it does not co-occur with as often. On the other hand, MI-score is calculated proportionate to the relative frequency, which indicates which words 'strongly' collocate with one another, but it does not show the most frequent collocates of the node.

In addition to these methods, as this research focusses on wider contexts needed for the study of paraphrase, which cannot be always easily observed in KWIC concordances, the research also uses sentences and paragraphs as concordances. Simply put, these are extracted sentences, and sometimes whole paragraphs, which contain one or more search terms though they are not centred around the node word, as the focus is on wider rather than immediate context. For simplicity, I will use the term 'paraphrase' to refer to these types of concordances since they coincide with the theoretical definition of paraphrase defined by Teubert (2007, 2010) (see Section 2.3.2 Paraphrases of terms for more details).

These corpus methods are applied in four main methodological procedures necessary to answer the research question outlined in the introduction to this thesis, which include: collocation analysis, paraphrase analysis, analysis of intertextual markers and diachronic analysis of paraphrase.

4.4.1 Collocation Analysis

The first analysis essentially consists of comparing the collocations of the terms *species* and *varieties*. All collocation analyses in this thesis have been conducted using the 5x5 word span with a minimum required frequency of at least five instances of collocation, thus ensuring the comparability of the results. Furthermore, in all analyses all the characters were treated as lowercase to avoid distinguishing the capitalised forms as separate.

One important aspect of this comparison is in the nature of these two terms. For the term *species* the singular and the plural form cannot be distinguished. On the other hand, that is not the case with the term *varieties* which also exhibits the singular form *variety*. Although both variants were included in the analysis, only the plural variants will be presented in the collocation. The reason for this omission is that the analysis using the singular form *variety* identified examples usually referring to a particular variety of some plant or animal, e.g. a variety of the garden pea.

The collocations of the terms are calculated, first for t-score values then for MI-score. From the results of the collocation analysis based on t-score calculation, the twenty top ranked 'content' words are extracted for the purpose of comparing the meaning of the terms. Function words, e.g. *the*, *of*, *that*, are not included in the analysis because, outside the context of the utterance, i.e. as items with a statistical value, they cannot give any insight into the kind of particular meaning they may form in context. Furthermore, as both search *species* and *varieties* are likely to collocate frequently with function words, there is no reason to compare their values.

For the calculation of Mutual Information (MI) score there were no function words to exclude from the results; only in the results for the term *species*, a contraction 'Compos.' was removed from the list of twenty highest ranking collocations, as it is a proper noun indicating the name of the species (Composite – a type of flower).

Furthermore, the results of collocation analyses are examined in detail and compared in the contrastive analysis, where collocates of one node were tested against the contrasting node. For example, *distinct* as a strong collocate of *species* was used as the node to test its co-occurrence with the lemma *variety/varieties*.

The contrastive analysis has been conducted using regular expressions as queries accounting for the same span as collocation because the AntConc collocation query tool does not list the concordances of identified collocations automatically, rather when choosing a specific collocate it makes a new concordance line of all its instances in the corpus. Fortunately, the program supports regular expressions and counts the instances of all concordances found, thus performing the same task.

For the purpose of contrastive analysis the exhaustive list of collocations, included in Appendices 1 and 2, have been examined, not just the twenty highest ranking results from the first two analyses. This is because the aim of the analysis is to contrast the meanings of *species* and *varieties*, hence a more exhaustive list is needed. Finally, the results of the analysis are tabulated to stress the differences between the meanings of the two terms.

4.4.2 Paraphrase analysis

Observing meaning in general language is too broad to be comprehensive, thus, Römer (2010) argues, taking cue from Firth (1968), that meaning should be only analysed in specialised or restricted discourse. The correspondence of Charles Darwin can be described as relatively restricted and specialised discourse. However, as mentioned previously (see section 4.2 On the Corpus), the corpus of Darwin's correspondence is comparable in size to some general language corpora. Using the definition of paraphrase as all instances of the use of a particular linguistic item implies a manual analysis of 5,375 instances of the term *species*.

To avoid manually reading through all 2,096 letters, a sub-corpus of all the paragraphs containing the word *species* has been examined for the explicit definitions of the word. This includes utterances expressing definitions of meaning, disagreements on the definition or different interpretations, joint or individual attempts at defining the term etc., which have been extracted for the analysis. This interpretation of the notion of paraphrase focuses only on the explicit attempts of defining and negotiating the meaning. The aim of this analysis is to illustrate that language is abundant in, and depends on definitions of meaning. On the other hand, paraphrases in the broader sense are observed in the analyses of intertextual relations between the interpretations of *species*.

4.4.3 Intertextuality Markers

As mentioned in the previous section, all the paraphrases of the term *species* had to be manually analysed in order to identify the paraphrases expressing *explicit* definitions of meaning. The same process was applied to identify the recurrent markers of intertextuality. However, unlike the paraphrases expressing the definitions of meaning, for the purpose of extracting frequent markers of intertextuality, the paraphrases were merely sampled for recurrent intertextuality markers. Once an exhaustive list of markers had been identified they were extracted automatically from the corpus using regular expressions.

After examining the paraphrases of the term *species*, several markers of intertextuality have been observed:

- (aware* + with| about| on| which| whether| that| of),
- (agree*| disagree* + with | about| on| which | whether |that),
- (what + say* | say* + about| on| that),
- (object + to | objection to),
- (in regard to | regarding),
- (to hear about| that|of|),
- (heard of + the| a| an| some| that| those| these),
- (according to),
- (ask* + that| if| about| whether |which| how),
- (state*| mention* + that),
- (discussion| proposal| hypothesis| assertion + on | about).

As described in the previous section, for this purpose a sub-corpus of paragraphs containing the word *species* was used for this purpose. After all the markers had been identified, they were extracted into separate files which are included in the Appendices. However, it is important to stress that in the appendices included in this thesis, all of the longer paragraphs have been shortened, for the sake of brevity. In certain cases that resulted in the word

species being omitted from the paragraphs included appendices. Finally, the intertextuality markers were then analysed separately for their function in the process of meaning negotiation.

4.4.4 Diachronic analysis

Diachronic analysis has been implemented using collocations based on the MI-score calculations. Thus it represents a corpus driven approach to the study of diachronic meaning negotiation. It is based on the theoretical framework that interprets all collocations and paraphrases as products of intertextuality.

The analysis is conducted using AntConc concordance plot tool and observes the distribution of lexical collocation throughout the correspondence.

The first step of the analysis is based on the results of the MI-score analysis, in which collocates with a high MI-score were chosen for the diachronic analysis. T-score analysis has not been used for collocate selection because these collocates include very frequent words that are usually evenly distributed in discourse.

Ten collocations were chosen for the analysis and analysed for their distribution in the corpus. Points of high concentration were defined as 'topic hotspots' and these were investigated for possible marks of intertextual relationship.

Apart from identifying intertextual relations and evidence of meaning negotiation, this analysis also allowed for a full overview of the 'life' of a particular collocation. That is, using concordance plot it was possible to observe

exactly at which point in discourse particular collocation entered discourse, the number of times it was discussed, the already mentioned 'topic hotspots' which indicate a higher rate of a particular interpretation possibly signalling interpersonal discussion, and finally when the collocation stopped being discussed in the discourse.

A similar analysis has been conducted by Kehoe and Gee (Kehoe & Gee, 2009) who have observed the diachronic distribution of the phrase 'credit crunch' in Google News from 1985 to 2008. They have identified collocates of the phrase using z-score and analysed the values diachronically to identify collocational 'heat maps' using heat 'as a metaphor for collocational strength'(Kehoe & Gee, 2009, p. 267). Similarly to their research, this analysis observes particular interpretations as they occur and disappear from discourse. However, the difference between the two approaches is that the focus of the analysis presented in this thesis is not to provide a diachronic overview of which collocates occurred at a particular time in language history, but rather to observe the process they were negotiated.

5 Analysis

5.1 Introduction

This chapter presents the results of the linguistic analysis of meaning construction. The chapter is divided into sections focussing on the collocation analysis, paraphrase analysis and the diachronic analysis of meaning negotiation.

The goal of the first analysis is to identify the most common interpretations of meaning of the terms *species* and *varieties*. These interpretations will be used in subsequent analyses to observe the processes of diachronic change and negotiation of meaning. The methodology used in this section corresponds to the traditional methodology used in corpus linguistics and lexicography. Namely, the section presents the analysis of collocation of the search terms for the purpose of analysing the contextual environments that define their meaning. This analysis is supported by a contrastive analysis of the key collocations with the aim of demonstrating how even the closely related terms can be clearly distinguished in discourse.

The goal of the second analysis is to demonstrate that paraphrase as an act of interpretation is a collective phenomenon. In other words, the goal of the analysis is to demonstrate that meaning is a product of negotiation in discourse. Building on the theories of dialogism and intertextuality, the analysis aims demonstrate that paraphrases which define the meaning of the term are phrased in relation to one another and in a way that they incite further response, i.e. further paraphrases.

The third analysis focuses on the so-called intertextual markers, which are words that explicitly invoke other texts in the process of formulation of new meaning through paraphrase. The aim of this analysis is to observe the manner in which other texts are invoked in the act of paraphrase and the function these intertextual links perform.

The final analysis observes the diachronic aspect of collaborative negotiation of meaning. This analysis focuses on the diachronic distribution of the key interpretations of the term *species* identified by the collocation analysis. The aim of this analysis is to observe how and when particular interpretations enter discourse, how they are negotiated in the discourse, and how long they remain in the discourse.

5.2 Collocation analysis

As outlined in the introduction to this chapter, this section covers the analysis of collocations which are used to demonstrate that meaning of words can be determined from the context in which they occur. In particular, the analysis is focussed on the meaning of the term *species* as the central concept in Charles Darwin's correspondence and scientific work.

In addition to the term *species*, the analysis of collocation, presented at the beginning of this section, includes the term *varieties*, and its singular form, with the aim to distinguish the meanings of these terms by comparing and contrasting their collocation patterns. For Charles Darwin, the meaning of these terms was not always clearly distinguished, which can be observed both in his

publication and his correspondence. Thus, by comparing the collocation patterns the meaning of both terms will be contrasted.

The analysis is conducted in two stages; firstly the collocations of both terms are tested for their frequency, t-score value, and Mutual Information value. These statistical values offer us insight into most frequent and most characteristic collocations respectively. Thus we can immediately make observations of both the similarities and differences between these two terms. This is followed by a contrasting analysis of collocations which consists of analysing how frequent collocates of one term collocate with the other. In other words, the aim is to identify and group collocates that tend to collocate more with only one of the terms.

5.2.1 Frequency collocations

Regarding frequency and t-score values, the most frequent collocates usually consist of so-called grammatical terms, such as determiners 'the' and 'a', various prepositions and pronouns. These usually occupy the first 10-20 most frequent collocates of virtually all words in English language. As *species* and *varieties* are very close in both the meaning and the structures they appear in, these grammatical and generally ubiquitous words do not contribute much to this particular collocation analysis. Thus, the twenty most frequent lexical items are included in the frequency list, whereas grammatical words are omitted. The results shown below in Table 1 present a list of the most frequent lexical items that collocate with the term *species* with their rank in the full list of collocations

indicated in the second column (for the full list of collocations see Table 1 in Appendix 1).

Table 1: Twenty most frequent lexical collocations of the word *species* calculated in a 5x5 word span

| No. | Rank | Collocate | Frequency | t-score | MI-score |
|-----|------|-----------|-----------|----------|----------|
| 1 | 25 | same | 313 | 17.22498 | 5.24406 |
| 2 | 27 | genera | 284 | 16.5936 | 6.02554 |
| 3 | 28 | some | 281 | 15.74253 | 4.0379 |
| 4 | 33 | origin | 254 | 15.74586 | 6.37883 |
| 5 | 37 | genus | 221 | 14.6366 | 6.01758 |
| 6 | 40 | varieties | 199 | 13.83848 | 5.71664 |
| 7 | 44 | distinct | 188 | 13.53571 | 6.28693 |
| 8 | 45 | new | 174 | 12.74488 | 4.88628 |
| 9 | 50 | different | 166 | 12.55966 | 5.3115 |
| 10 | 61 | common | 123 | 10.73065 | 4.94564 |
| 11 | 62 | each | 121 | 10.67189 | 5.06716 |
| 12 | 64 | number | 117 | 10.52554 | 5.21552 |
| 13 | 67 | found | 113 | 10.12128 | 4.38474 |
| 14 | 69 | several | 110 | 10.16722 | 5.03064 |
| 15 | 72 | allied | 107 | 10.23905 | 6.62183 |
| 16 | 80 | few | 94 | 9.09328 | 4.00926 |
| 17 | 85 | plants | 88 | 8.4911 | 3.39828 |
| 18 | 87 | large | 86 | 8.90031 | 4.63469 |
| 19 | 90 | work | 84 | 7.33126 | 2.32125 |
| 20 | 92 | good | 81 | 8.14219 | 3.39119 |

From Table 1 we can observe the words most frequently co-occurring in the span of five words to the left and right of the word *species*. These collocates define some of the most frequent uses, and hence meanings, of the term *species*.

The results presented in Table 1 can be compared to the list of most frequent collocates of the word *varieties* in order to observe the difference in their general use. As with the term *species*, grammatical terms are omitted from the list, thus Table 2 below presents the list of the most frequent lexical collocates (the full list of collocates is included in Appendix 2).

Table 2: Twenty most frequent lexical collocations of the word *varieties* calculated in a 5x5 word span

| No. | Rank | Collocate | Frequency | t-score | MI-score |
|-----|------|--------------|-----------|----------|----------|
| 1 | 5 | species | 199 | 13.83848 | 5.71664 |
| 2 | 27 | some | 44 | 6.23884 | 4.07196 |
| 3 | 30 | same | 37 | 5.87512 | 4.87255 |
| 4 | 31 | many | 35 | 5.67676 | 4.62765 |
| 5 | 33 | domestic | 34 | 5.79473 | 7.33071 |
| 6 | 35 | more | 32 | 5.24167 | 3.76816 |
| 7 | 42 | different | 29 | 5.26646 | 5.5035 |
| 8 | 45 | marked | 28 | 5.25545 | 7.19727 |
| 9 | 49 | plants | 27 | 4.95051 | 4.40279 |
| 10 | 51 | genera | 27 | 5.06784 | 5.33973 |
| 11 | 57 | distinct | 24 | 4.82382 | 6.02636 |
| 12 | 63 | forms | 21 | 4.4569 | 5.18842 |
| 13 | 65 | intermediate | 20 | 4.44112 | 7.17194 |
| 14 | 70 | several | 18 | 4.12134 | 5.12826 |

| | | | | | |
|----|----|------------|----|---------|---------|
| 15 | 72 | new | 18 | 4.03057 | 4.32232 |
| 16 | 73 | local | 18 | 4.21891 | 7.48212 |
| 17 | 74 | crossing | 18 | 4.20746 | 6.91407 |
| 18 | 85 | large | 16 | 3.86764 | 4.91748 |
| 19 | 86 | cultivated | 16 | 3.98187 | 7.78556 |
| 20 | 87 | crossed | 16 | 3.96832 | 6.98021 |

We can observe from Tables 1 and 2 that the words *species* and *varieties* collocate quite frequently with each other. Furthermore, the terms share 9 out of 20 most frequent lexical collocates. Although the two terms appear to frequently collocate with one another, it seems that the contexts they generally occur in are sufficiently distinct to determine some specific differences in their meaning. For example, from Tables 1 and 2, we can observe that *varieties* are often described as ‘*domestic*’ and ‘*local*’; whereas *species* are described as ‘*common*’ or ‘*allied*’. We will examine these differences in more detail later in this chapter.

Comparing the collocations solely on the basis of frequencies leads to the risk of unbalanced comparison since the word *species* is significantly more frequent in the corpus. Thus, the results can also be compared statistically in order to account to their general frequency, which is done by testing the collocations for their t-score (included in Tables 1 and 2). T-score analysis largely corresponds to the raw frequency rankings, where lexical words are concerned, as it can be observed in the Tables 1 and 2. This is largely due to the fact that lexical words follow a more normal distribution than grammatical words which tend to make

up most of the discourse. Thus, specific focus on the t-score values will not be discussed in this analysis.

5.2.2 Mutual Information

Mutual Information (MI) score describes the most characteristic collocations, i.e. ones which form the most ‘peculiar’ combinations and meanings. Specifically, MI-score ranks the words that most exclusively collocate with the search term. Thus, frequent words in discourse rarely occur on this list, as they tend to collocate with many other words. For the comparison of meaning of closely related terms such as *species* and *varieties*, MI-score is a very effective tool.

The analysis of the collocations of *species* according to MI-score results in a much more specific list of words. The ranking of these words does not necessarily indicate that they are frequent collocates, but that these words collocate more frequently with the word *species* than other words in the corpus. The top twenty ranked collocates of the word *species*, according to MI-score, are shown in Table 3, below (for the full list consult Table 3 in Appendix 1).

Table 3: Twenty highest MI-score ranked collocations of the word *species*, calculated in a 5x5 word span

| Rank | Collocate | MI-Score |
|------|----------------|----------|
| 1 | mutability | 7.42197 |
| 2 | disjoined | 7.3845 |
| 3 | immutability | 7.26825 |
| 4 | representative | 7.19911 |
| 5 | undescribed | 6.83701 |

| | | |
|----|------------|---------|
| 6 | derivation | 6.83701 |
| 7 | immutable | 6.76301 |
| 8 | quasi | 6.65609 |
| 9 | allied | 6.62183 |
| 10 | inhabiting | 6.54062 |
| 11 | identical | 6.49998 |
| 12 | trimorphic | 6.48999 |
| 13 | origin | 6.37883 |
| 14 | endemic | 6.32244 |
| 15 | distinct | 6.28693 |
| 16 | presenting | 6.17805 |
| 17 | ranges | 6.1441 |
| 18 | describing | 6.09316 |
| 19 | genera | 6.02554 |
| 20 | genus | 6.01758 |

From Table 3 we can identify the most exclusive collocates of the term *species*, such as '(im)mutability', 'disjoined', 'representative', 'undescribed', 'derivation', etc.

In the same fashion, the meaning of the term *varieties* can be further examined by observing the collocations of the term ranked on MI-score. Table 4 shows twenty highest ranked words on MI-score (for the full list consult Appendix 2).

Table 4: Twenty highest MI-score ranked collocations of the word *varieties*, calculated in a 5x5 word span

| Rank | Collocate | MI-Score |
|------|---------------|----------|
| 1 | quasi | 8.47206 |
| 2 | maize | 8.47206 |
| 3 | verbascum | 8.27942 |
| 4 | abnormal | 8.15014 |
| 5 | cultivated | 7.78556 |
| 6 | presenting | 7.62407 |
| 7 | fertility | 7.48855 |
| 8 | local | 7.48212 |
| 9 | station | 7.43644 |
| 10 | wheat | 7.41317 |
| 11 | modifications | 7.39678 |
| 12 | domestic | 7.33071 |
| 13 | marked | 7.19727 |
| 14 | intermediate | 7.17194 |
| 15 | peas | 7.17089 |
| 16 | differently | 7.01263 |
| 17 | crossed | 6.98021 |
| 18 | crossing | 6.91407 |
| 19 | normal | 6.7496 |
| 20 | sterile | 6.73871 |

With the MI-score indicating the most characteristic collocates of each term, a comparison of the collocations of each of the terms contrasts their most specific usage. From Tables 3 and 4, we can observe that even with MI-score rankings,

there are certain similarities between these terms. However, the similarity is restricted to the prefix '*quasi-*' and the verb '*presenting*'. Thus, MI-score results provide us with collocates which define the meaning of these terms more narrowly. For example, we can conclude that particular *species* may be described as '*distinct*', whereas *varieties* are more likely to be described as '*marked*'. However, the analysis of collocations does not provide us directly with negative values, i.e. the words which do not occur frequently or score highly in the collocation span. Knowing that a particular collocation is not only strong for one term, but also non-existent with the other, provides a stark contrast of the meaning of these terms. In other words, such analysis will contrast how a particular word collocates both with the word *species* and *varieties*, thus allowing for a more critical analysis of their meanings. Such an analysis is comparable to keywords (Scott, 1997) analysis which compares the values of collocates between two corpora, whereas in this thesis the focus is on contrasting specific terms not corpora. .

5.2.3 Contrasting Collocations

In the previous section we have observed that the terms *species* and *varieties* are defined by a range of collocates. For example, *species* are realised as *allied*, *close* and *true* and are often discussed how they are *descended*, *differed* and *described*. *Varieties*, on the other hand, are described as *crossed*, *mere*, *intermediate*, etc. Furthermore, the results of the collocation analysis show a difference in the meaning which, in some aspects, exhibits a binary distinction

between the two terms. For example, '*representative*' and '*typical*' referring to *species* contrasts the '*distinct*' and '*marked*' of *varieties*. Here, *species* are described as being akin to the prototype, whereas *varieties* are marked for their difference from it. However, without contrasting these two patterns we cannot observe whether *varieties* can also be *representative* or *typical* and vice-versa.

By examining the collocations of each of the terms in more detail (see Appendix 1 for the list of all the collocations observed), we can observe several other binary distinctions which clearly separate the meaning of the two terms. The first of these binary distinctions to be analysed is the group of collocates that demonstrate *varieties* as a local phenomenon, in contrast to the more general and wider ranges of *species*. The second binary distinction distinguishes *species* as '*natural*' and '*wild*' phenomena in contrast to '*domestic*' and '*cultivated*' *varieties*. The third distinction, which we have briefly discussed above, relates to *species* having *typical* and *representative* forms in contrast to *distinct* and *marked varieties*.

The aim of this analysis is to demonstrate that closely related terms can be distinguished by contrasting their strongest collocates. This allows one to establish which collocates are mostly contrasted thus identifying the difference in the meaning of such closely related terms. Furthermore, this analysis will demonstrate that certain collocations are also semantically restricted to their respective terms. In other words, the analysis will show that words of similar meaning tend to collocate with the same nodes.

5.2.3.1 Contrasting collocations: *wide* and *narrow*

Collocation analysis showed that *species* and *varieties* collocate contrastively with words denoting geographical ranges and locations. The term *species*, for example, often collocates with adjectives denoting large geographical areas, such as continents and countries. For example, in Table 5 we can see that adjectives such as ‘*African*’, ‘*American*’, ‘*Australian*’, ‘*European*’, but also ‘*British*’, ‘*English*’, ‘*Galapageian*’, and ‘*Mediterranean*’ are often found modifying the term *species* but not *varieties*. On the other hand, in the whole corpus, there is only one example each of adjectives ‘*American*’, ‘*European*’ and ‘*Indian*’ modifying the term *varieties*. Table 5 below shows the characteristic use of geographical adjectives modifying the terms *species* and *varieties* respectively.

Table 5: Geographical expressions modifying the terms *species* and *varieties*

| Geographical modifier | Collocation frequency | |
|-------------------------|-----------------------|-------------------|
| | Species | Varieties (lemma) |
| African | 7 | 0 |
| American | 14 | 1 |
| Australian | 7 | 0 |
| British | 23 | 0 |
| English | 8 | 1 |
| European | 38 | 1 |
| Galapageian / Galapagan | 5 | 0 |
| Indian | 16 | 1 |
| Northern | 17 | 0 |
| Southern | 16 | 0 |

Apart from the examples presented in the table, adjectives ‘*Californian*’, ‘*Norwegian*’ ‘*Saharan*’, ‘*Sicilian*’ and a few others can be found occasionally modifying the term *species* but not *varieties*. This implies that the meaning of *species* is related to larger geographical ranges.

In contrast to the example above, the collocation analysis showed that *varieties* collocate frequently with geographical adjectives denoting narrower geographical ranges. For example, adjectives ‘*domestic*’, ‘*garden*’, and ‘*local*’ frequently collocate with the term *varieties*, but not as frequently with the term *species*. This is summarised in Table 6 below.

Table 6: Localised geographical adjectives modifying the terms *species* and *varieties*

| Modifier | Frequency | |
|-----------------------|-----------|-------------------|
| | Species | Varieties (lemma) |
| Domestic ⁵ | 5 | 26 |
| Garden | 1 | 9 |
| Local | 4 | 24 |

From the examples in Table 6 above, we can observe that the more localised geographical adjectives co-occur with the term *varieties*, five to nine times more frequently than with the term *species*. This has an even greater significance since the term *species* occurs in the corpus almost five times as often as the term *varieties*. Although there are a few examples where the term *species* collocates with the adjectives ‘*local*’ and ‘*domestic*’, generally the tendency is to

⁵ Used in the sense of *local* or *national*

collocate more frequently with adjectives denoting wider ranges, as noted in Tables 5 and 6 above.

Furthermore, the term *species* collocates much more frequently with the phrase 'wide range(s)' with a total of 19 co-occurrences, whereas the same phrase collocates only once with the term *varieties*. Even that one occurrence is a false positive as it relates to a 'scope of variety' rather than 'ranges in which varieties are found', as it can be observed in the example below:

Example:

- 1) *Is it meant that Man found several canine species just on the verge of passing from permanent varieties into species & before they had diverged from some anterior ancestral type so far as to make frequent interbreeding impossible they were blended so as to produce **a wider range** of variety than would otherwise have come about. [www.darwinproject.ac.uk/letter/entry-2508f]*

Even when the adjective *wide* and its lemmas are removed from the query, the only other example we encounter where *varieties* collocate with 'range' is a question about their geographical distribution, as it can be seen in the example below.

Example:

- 2) *Do you know whether anyone has ever published any remarks on the **geographical range of varieties** of Plants in comparison with the species, to which they are supposed to belong. [www.darwinproject.ac.uk/letter/entry-2218]*

Considering examples from Tables 5 and 6, as well as the examples relating to the expression of 'wide range(s)', we can conclude that generally in the discourse of Charles Darwin's correspondence, the term *species* is used to refer to more widely distributed natural kinds than the term *varieties*. Thus,

although Darwin frequently described the distinction between the meaning of *species* and *varieties* as arbitrary and difficult to determine, the discourse of his correspondence indicates some significant patterns of their use in discourse.

5.2.3.2 Contrasting collocations: natural and domestic

In the previous section, we have observed that the term *varieties* is used to describe natural kinds that occur in narrow geographical areas. However, the adjective ‘*domestic*’, which was associated with the term *varieties*, also carries the connotation of ‘*cultivated*’ or ‘*domesticated kinds*’, which is contrasted to ‘*natural*’ and ‘*wild kinds*’. This distinction is also confirmed in the corpus, where the term *species* tends to collocate with adjectives denoting ‘*wild*’ and ‘*natural*’ states or origins, and the term *varieties* denoting ‘*domesticated*’ and ‘*cultivated*’ kinds. Table 7 summarises these differences.

Table 7: Adjectives denoting natural or domesticated states modifying the terms *species* and *varieties*

| Modifier | Frequency | |
|-----------------------|-----------|------------------|
| | Species | Varieties(lemma) |
| natural | 45 | 14 |
| wild | 58 | 6 |
| domestic ⁶ | 13 | 38 |
| cultivated | 5 | 16 |

From the examples shown in Table 7, we can see that the tendency for species to be described as ‘*natural*’ and ‘*wild*’ and varieties as ‘*domestic*’ and ‘*cultivated*’

⁶ Meaning *cultivated* or *domesticated*

is not as definitive as the previous contrast between local and more global geographic description. However, if we take into consideration the paraphrases in discourse, we can notice from the context that some of these numbers are false positives. Take the following examples of 'natural + varieties' collocations into consideration:

Examples:

- 1) *I have been comparing all the evidence which I can collect on the **natural** crossing of the **varieties** of cultivated Leguminosæ; [www.darwinproject.ac.uk/letter/entry-2003]*
 - 2) *I have been lately collecting all the evidence which I can get from the observation of others and my own, on the **natural** crossing of **varieties** of plants. [www.darwinproject.ac.uk/letter/entry-2012]*
 - 3) *we still cannot but admit that, as a very general rule, species do not intermix in **wild nature**, as **varieties** of the same species (descendants from a common stock) we have every reason to think would do. [www.darwinproject.ac.uk/letter/entry-1760]*
-

In these examples we can observe that relying simply on calculating collocations can produce a few false positive results. Thus, in the Examples 1 and 2 above 'natural' does not directly modify the term *varieties* and similarly, in the third example, 'wild' does not modify *varieties*. Similarly, although there are examples of 'domestic species' in the corpus, some of the collocates, when looked in more detail, do not modify the term *species* but coincidentally occur within the collocation span of the term.

Examples:

- 4) *Besides this the close resemblance of at least three kinds of American **domestic** dogs, to wild **species** still inhabiting the countries where they are now domesticated, seems to almost compel admission that more than one wild *Canis* has been*

domesticated by man.— [www.darwinproject.ac.uk/letter/entry-2543]

- 5) *In these mountains the domestic fowl is much more like the original jungle cock (possibly from intermixing) than the fowls of the plains are. The domestic will always breed with the Chittagong (itself a **domestic** race of **species** unknown)*
[www.darwinproject.ac.uk/letter/entry-1203]
-

Examples 4 and 5 represent another case of misleading collocation value where the words co-occur in the collocation span, but the modifier *domestic* is attributed to another word, in these cases to *dogs* and *race* respectively.

Finally, the collocation '*cultivated*' indicates further that *varieties* stand for domesticated kinds in contrast to *species*, which usually stand for the natural and wild prototype kind to which *varieties* are associated. By observing these collocations in detail, we can observe that *species* are meant to be '*wild*' and '*natural*', whereas *varieties* arise mostly through domestication. Specifically, we can observe the interpretations that *species* are not a product of domestication but of evolution, whereas *varieties* arise in relation to the *wild* and *natural* species. Consider the following examples:

Example:

- 6) *This **species** is curious as being only known in the **domestic** state! no **wild** prototype of it having as yet been sent to our Collection. The Animal is the Japan Pig It is very different in its osteology from any of the Wild or Domestic Swine of Europe or Asia*
[www.darwinproject.ac.uk/letter/entry-3416]
- 7) *nearly all our **domestic races** descended from a multitude of **wild species** now commingled.* [www.darwinproject.ac.uk/letter/entry-2565]
-

- 8) *This work will be my biggest; it treats on the origin of **varieties** of our **domestic** animals and plants, and on the origin of species in a state of nature. [www.darwinproject.ac.uk/letter/entry-2276]*
- 9) *I am even inclined to suspect that there is a tendency to a return to primordial wildness in hybrids between two **domestic species**. — [www.darwinproject.ac.uk/letter/entry-4033]*
-

The discourse meaning of *species* indicates here that species as objects originate in the wild nature, which is in contrast to the meaning of *varieties*, which are deemed as descended from natural species, usually in a cultivated and domesticated fashion. This also implies that *species* are discussed for their features as prototypes for the natural kinds, whereas *varieties* are discussed on their distinctions from the prototype. This contrastive feature is analysed in more detail in the following section.

5.2.3.3 Contrasting collocations: *prototype* and *variation*

The terms *species* and *varieties* differ in the way the distinctiveness of the kind is expressed in discourse. Namely, although both *species* and *varieties* are discussed as ‘*distinct*’ and ‘*marked*’, prototypical nature is only discussed in relation to *species*. Table 8, below summarises the difference.

Table 8: Collocates of *species* and *varieties* expressing distinctiveness of kinds

| Modifier | Frequency | |
|----------------|-----------|-------------------|
| | Species | Varieties (lemma) |
| individual | 39 | 3 |
| representative | 46 | 2 |
| typical | 15 | 0 |
| characteristic | 3 | 0 |

| | | |
|----------|-----|----|
| distinct | 188 | 36 |
| marked | 25 | 35 |

From the table above, we can observe that only *species* are discussed for the ‘*typical*’ and ‘*representative*’ examples. There are no examples of ‘*typical*’ or ‘*representative varieties*’ in the whole corpus, either in the singular or the plural form. The results shown in Table 8 include a few false positive examples which increase the collocation of ‘*marked*’ and *species*. When searching for the instances of ‘*marked species*’ only 7 matches are found, in comparison to 22 ‘*marked varieties*’. When we take into consideration that the occurrences of *species* in the corpus amount to more than four times⁷ that of *varieties*, both in singular and plural form, we can conclude that in relation to *species*, *varieties* collocate significantly more often with ‘*marked*’.

From this contrastive analysis, we can observe yet another distinction in the use, and hence the meanings of terms *species* and *varieties*. Specifically, *species* tend to be individualised for having all the typical features of the kind, indicated by the collocations *representative* and *typical*. On the other hand, *varieties* tend to be described as ‘*marked*’ at best. A possible reason for *varieties* not being singled out in discourse is that their typical example is the form described as the *species*, and the *variety*, as the name suggests, is considered a variation from the prototype. This conclusion leads us to our final analysis; that of *modification*, *mutability* and *change of species*.

⁷ In the corpus there are 5375 instances of *species* & 1230 of *variety* and *varieties*

5.2.3.4 Contrasting collocations: modification and *mutability*

The aim of the final contrastive analysis is to demonstrate another aspect in which semantically related words collocate with the same term almost exclusively. This analysis examines the collocations semantically related to the *changes of species*. From the results presented in Table 9 we can observe that meaning related to any kind of *change* or *modification* of natural kinds is strongly collocated with the term *species*.

Table 9: Collocates of *species* and *varieties* expressing changeability of kinds

| | Frequency | |
|--------------|-----------|-------------------|
| Modifier | Species | Varieties (lemma) |
| modification | 40 | 7 |
| mutability | 38 | 0 |
| change | 64 | 6 |
| variation | 67 | 7 |
| descend | 45 | 7 |

The collocations shown in Table 9 show that on all accounts the term *species* collocates strongly with words expressing change. Conversely, the term *variation* shows little or no tendency to collocate with these words. A possible explanation is that being variations of species, as the name suggests, *varieties* are not discussed with respect to their *modification*, *mutability* or *change*.

5.2.4 Summary

This analysis has demonstrated that by observing collocation patterns it is possible to distinguish meanings of closely related terms, such as *species* and *varieties*. The analysis focussing on the Mutual Information score was particularly successful in uncovering the specific differences in the collocation patterns, and hence in the meanings of the two terms. In this analysis only two of the twenty top-ranking collocates were shared between the terms, thus providing insight into many differences in the meaning of these terms. Furthermore, the analysis also showed that words of similar meaning tend to collocate with the same terms.

Although these analyses are commonplace in corpus linguistics, they are an important part of this thesis. The results of collocation analyses indicate consistent differences in the use of these terms, indicating a difference in meaning which Darwin himself often struggled to identify. This indicates that meaning of these terms was not determined by ‘facts’ about species and varieties, since then Darwin and his fellow naturalists would not have debated so extensively on the particular specimens being species or varieties. Despite labelling the differences between the two terms ‘*arbitrary*’ and ‘*unclear*’, in Darwin’s correspondence, we can observe a conventionalised use of these terms which shows a clear distinction of their meaning and uncovers Darwin’s interpretation of these terms.

The collocation analyses contributed significantly to uncover the meaning of the terms *species* and *varieties*, but they also showed that the interpretation of discourse meaning cannot be reduced to frequency and statistics. In other

words, observing the frequency of words occurring together cannot account for how these words are actually used. This was exemplified by several examples we labelled as false positives in the collocation analysis, since the interpretation of statistical data did not correspond to the actual meanings formed. Thus, when analysing the meaning of any word, it is necessary to analyse its use in a paraphrase.

5.3 Construction of meaning by paraphrase

5.3.1 Introduction

This section analyses the meaning of the term *species* in the context of an utterance. Specifically, the analysis focuses on the explicit definitions of meaning of the term *species*, occurring as part of the process of constructing and negotiating meaning. In other words, the analysis focuses on the paraphrases, which explicitly or implicitly define the meaning of the term *species*. The aim of the analysis is to support the argument that the meaning of *species* is determined through discourse rather than extra-linguistically.

To demonstrate this, the analysis will focus on presenting the examples which best illustrate the problem of species. Thus the aim is not to create an exhaustive list of explicit definitions or analysing discourse markers of such definitions, but rather to present examples in groups which demonstrate that: a) the meaning of *species* is difficult to define because different people will employ different definitions; b) the meaning of *species* is not determined by the extra-linguistic facts; and c) the meaning of species is negotiated in discourse.

5.3.2. Analysis

5.3.2.1 Multiple definitions and interpretations

Defining the meaning of the notion of *species* was one of the main goals of Charles Darwin and his fellow naturalists. Although the notion is frequently used, Darwin and his correspondents struggled to define it clearly. We can

observe this in Example 1 below where *species* is defined as the object of natural classification.

Example:

- 1) *'I beg you, however, to **think clearly & define**, or at least say you cannot define, (as others have done) what they are searching for by a natural classification'*
[\[www.darwinproject.ac.uk/letter/entry-685\]](http://www.darwinproject.ac.uk/letter/entry-685)

From Example 1, we can observe that in a letter to a fellow naturalist, George Robert Waterhouse, Darwin states that certain naturalists have been unable or unwilling to define the notion. This indicates that among scientists of Darwin's time there was a lack of a generally accepted formulation of the term. The inability or unwillingness of some of Darwin's peers to produce a definition certainly implies that the meaning of the term was interpreted differently among scientists. This is shown in Example 2 below, where botanist Hewett C. Watson reports to Darwin the contentious issue of what to call *species* and what *varieties*.

Example:

- 2) *But what is a close species? **Those which Botanist A would call the closest species, Botanist B would deny to be species at all; deeming them varieties.** Those which Botanist A would deem species of only secondary closeness, would by Botanist B be pronounced the closest species he could admit. On which of these Two would you rely'* [\[www.darwinproject.ac.uk/letter/entry-1758\]](http://www.darwinproject.ac.uk/letter/entry-1758)

In Example 2, Watson stresses to Darwin that due to different interpretations of the term, and hence different classifications of species and varieties, there is no point in requesting definitions of the term *close species* as they will vary from person to person.

In the following example from a letter to the American botanist Asa Gray, we can observe Darwin's acceptance that the definition of the term cannot be stated in a way to be universally accepted.

Example:

3) *By the way I met the other day Phillips, the Palæontologist, & he asked me "how do you define a species?"— I answered "I cannot" Whereupon he said "at last I have found out the only true definition,—'any form which has ever had a specific name'!"*
[www.darwinproject.ac.uk/letter/entry-2176]

In Example 3 above, it can be observed that the definition expressed by Phillips corresponds to the notion of meaning defined in our theoretical framework, i.e. that the meaning of *species* corresponds to all the instances of its use. In the words of Phillips from the above example, all that which has been at some point called *species* corresponds to the definition of the term.

The difficulty in formulating definitions of *species*, shown in Examples 1-3, indicates that multiple definitions and interpretations of the meaning of *species* were used in Darwin's time. Thus, as different naturalists applied different definitions and interpretations of the term *species*, they described and categorised *species* and *varieties* differently. This implies that the definition of the term affects the empirical approaches of scientists as well as the interpretation of the results, which is the focus of the analysis presented in the following section.

5.3.2.2 Definitions and extra-linguistic features

The problem of species is not only the product of a lack of a unified definition of the term, but also a product of different interpretations of empirical data. This is

clearly stated in a letter to Darwin from H. C. Watson in which he stresses that the question of distinguishing species from varieties is perhaps not one that can be addressed empirically, especially without prior agreement on the definition:

Example :

- 1) *All investigations have an interest in my eyes, which bear upon the grand question, **whether species are or are not absolutely & always distinct** [...]even supposing Botanists better agreed than they are likely to be in our day, as to what (or which) are species & what are varieties. Really, **they can neither define what species are, nor can they empirically agree in selecting species from varieties, or varieties from species.**— Jordan would make a million species. J. D. Hooker would perhaps allow 50,000.*
[www.darwinproject.ac.uk/letter/entry-1775]

In Example 1, it is implied that there is a multitude of interpretations of the meaning of *species*, as well as a multitude of different approaches to their empirical description. In a sense, one implies the other, since the empirical approach will depend on the interpretation of the term. Example 2 below shows more clearly a case where the interpretation of the meaning of *species* influences the definition and description of specimens.

Example:

- 2) *In all other respects but this genital hooklet, the two species are absolutely identical. They are, with this single exception, as like as two peas. After personally comparing the two, I cannot see how any sane man can infer that each was separately created. **Yet they must be distinct species now, (according to my interpretation or definition of that term)** for otherwise they would intercross & produce races having intermediate genital hooklets and intermediate periodic times, 16 years, 15 years &c*
[www.darwinproject.ac.uk/letter/entry-6332a]

In Example 2, it is explicitly stated by the writer that his interpretation of the meaning of *species* influences the classification of the specimens, which he

might otherwise classify differently: *'I cannot see how any sane man can infer that each was separately created. Yet they must be distinct species now, (according to my interpretation or definition of that term).'* From Example 2 we can notice that interpretation is based on a certain definition of the term which is implied in the hypothetical statement at the end of the example *'for otherwise they would intercross & produce races having intermediate genital hooklets and intermediate periodic times.'* This implies that the definitions of *species* stated in the discourse influence the interpretations of whether specimens are distinct species at least as much as the empirical observations.

In this sense, as noted before, a multitude of definitions implies a multitude of approaches to empirical studies which can be observed in Example 3 below.

Example:

- 3) *It is really laughable to see what different ideas are prominent in various naturalists minds, when they speak of "species" in some resemblance is everything & descent of little weight—in some resemblance seems to go for nothing & Creation the reigning idea—in some descent the key—in some sterility an unfailing test, with others not worth a farthing. It all comes, I believe, from trying to define the undefinable.*
[www.darwinproject.ac.uk/letter/entry-2022]

In Example 3 we can observe that empirical approaches are largely defined by the interpretations of the meaning of *species*. Resemblance, descent and infertility of species hold different weight among Darwin's peers where describing species is concerned. This multitude of interpretations and empirical approaches prompts Darwin's scepticism on the possibility of defining the notion as stated at the end of Example 3: *'trying to define the undefinable.'*

Nevertheless, history has shown that this realisation did not stop Darwin from making his own contributions to defining the meaning of *species*.

Finally, the multitude of interpretations of which physical properties are important for determining what species are indicate that *species*, as well as other classificatory terms such as *orders* and *genera*, are constructs of discourse. Hewett Cottrell Watson expressed this view to Darwin long before the publication of the *Origin*.

Example:

- 4) *I look upon the words 'Orders, genera, species (of books), & varieties,' only as terms to indicate passably well the grades of resemblance between objects.*
[www.darwinproject.ac.uk/letter/entry-1764]

In the above example, Watson carefully chooses his words to express that these words are merely terms used by naturalists to indicate the degrees of resemblance between objects. This is particularly expressed with the phrase '*species (of books)*' which indicates the discursive construction of the notion, i.e. *species* as defined in books, rather than as an abstract idea of natural kinds. Thus what is considered species is a matter of the formulation of the term in discourse rather than identifying its physical properties.

5.3.2.3 Negotiation of meaning

Although the analysis presented here focuses on the narrowed-down notion of paraphrase, i.e. the explicit definitions of meaning, the general implication that each definition is a paraphrase of previous formulations still applies. In other words, new definitions are constructed by adding new interpretations to previous ones.

This is evident in Darwin's earlier letters when he first decided to share his theory with select correspondents. In two letters letter to Joseph Dalton Hooker, Darwin excitedly expressed his definition of *species*, and soon after he reported similar conclusions to Leonard Jenyns. Examples 1 and 2 show the two definitions expressed in the letters to Hooker, which were followed up by a definition sent to Jenyns, shown in Example 3.

Examples:

- 1) *At last gleams of light have come, & I am almost convinced (quite contrary to opinion I started with) **that species are not** (it is like confessing a murder) **immutable**.*
[www.darwinproject.ac.uk/letter/entry-729]
- 2) *The general conclusion at which I have slowly been driven from a directly opposite conviction is **that species are mutable & that allied species are co-descendants of common stocks**.*
[www.darwinproject.ac.uk/letter/entry-782]
- 3) *I am a bold man to lay myself open to being thought a complete fool, & a most deliberate one.— From the nature of the grounds, **which make me believe that species are mutable in form, these grounds cannot be restricted to the closest-allied species; but how far they extend, I cannot tell, as my reasons fall away by degrees, when applied to species more & more remote from each other.** [www.darwinproject.ac.uk/letter/entry-793]*

From these examples we can clearly observe how Darwin's definition of *species* was gradually developed. This development is realised almost as a direct paraphrase of preceding definitions, gradually expanding and refining the meaning of the term. Thus, firstly in Example 1 we observe the definition that 'species are not immutable' which contrasts Darwin's previous beliefs. This is then in Example 2 paraphrased and refined into 'species are mutable & that allied species are co-descendants of common stock.' Here Darwin adds a statement to the original definition regarding allied species being co-

descendants. Finally, the definition shown in Example 3 is extended to ‘*are mutable in form, these grounds cannot be restricted to the closest-allied species*’ where the restriction of co-descent to allied species is removed from the definition. Thus, these examples illustrate a diachronic dimension to Darwin’s definition of the meaning of *species*.

Definition, once expressed in the discourse, can be either accepted and further reinterpreted by the discourse community, or ignored or rejected, in which case it is not repeated or paraphrased and its meaning does not take up in discourse. In Example 4, we can observe how Darwin’s definition (shown in Example 1) was temporarily ignored and did not excite his correspondent, Joseph Dalton Hooker, to engage in discussion.

Example:

- 4) *At present I endeavor to hold aloof from all speculations on the origin of species, & wish to till at any rate this part of my flora is finished. When that is the case I should like to have much talk about it with you, **at present I go on the old assumption that each species has one origin is immutable & migrates.***
[www.darwinproject.ac.uk/letter/entry-947]

From the example above, we can see that Hooker refused to acknowledge Darwin’s assertion maintaining the interpretation of immutability of species. Thus, by refusing to engage in the debate, Hooker effectively suspended Darwin’s definition from their shared discourse.

The evidence that meanings depend on the discourse communities can be observed in the letter from Darwin to Albert Jean Gaudry (Example 5) in which they discuss the general acceptance of Darwin’s theory in France.

Example:

- 5) *Your belief will, I suppose at present, lower you in the estimation of your countrymen; but, judging from the rapid spread in all parts of Europe, excepting France, of the belief in the common descent of allied species, I must think that this belief will before long become universal. How strange it is that the country which gave birth to Buffon, the elder Geoffroy & especially to Lamarck shd **now cling so pertinaciously to the belief that species are immutable creations.***
[www.darwinproject.ac.uk/letter/entry-5794]
-

From the example above we can notice a whole discourse community of French scientists, apart from Darwin's correspondent, adhering by the definitions Darwin so strongly criticised and ignoring his own propositions. Darwin even suggests that he fears his correspondent's reputation is at stake for agreeing with him, probably in the same manner he feared for his reputation prior to the publication of his theory. Examples 4 and 5 illustrate the importance of paraphrase for the proliferation of meaning in the discourse community.

Similarly, in Example 6, we can observe a proposal by Alfred Russel Wallace who, having himself reached similar conclusions, wished to build on Darwin's theory, suggesting the interpretation *survival of the fittest* as an accurate definition of Darwin's 'natural selection'.

Example:

- 6) *Natural selection, is, when understood, so necessary & self evident a principle, that it is a pity it should be in any way obscured; & it therefore occurs to me, **that the free use of "survival of the fittest",—which is a compact & accurate definition of it,—**would tend much to its being more widely accepted and prevent its being so much misrepresented & misunderstood. [*www.darwinproject.ac.uk/letter/entry-5140*]*
-

Example 6 shows Wallace adding a new interpretation of the notion of 'Natural Selection' to clarify its meaning more accurately, i.e. the notion of the "survival

of the fittest”, in order to prevent ‘misunderstandings’. Here Wallace expresses the belief that the theory would be more widely accepted if it was more accurately and compactly expressed. On the other hand, in Example 7, Darwin reports that the obscurity of meaning of the notion of *natural selection* can be seen as positive by preventing too “liberal” interpretations of the term.

Example:

7) *Several scientific men have thought the term “Natural Selection” good, because its meaning is not obvious & each man could not put on it his own interpretation, ...*
[www.darwinproject.ac.uk/letter/entry-2698]

Examples 6 and 7 illustrate meanings are open for interpretation and reformulation once they become part of the discourse. In Example 6, Wallace adds the interpretation of ‘survival of the fittest’ to the meaning of *natural selection*, which was possible due to Wallace’s understanding of Darwin’s proposition. However, in Example 7, some degree of intellectual prestige is attributed to Darwin’s proposal – the example implies that by making the definition not very accessible to wider audiences, Darwin protects his own interpretation from being reformulated.

To summarise, Examples 1-3 show how meaning is expanded by added interpretation. Examples 4 and 5 illustrate the importance of paraphrase for the proliferation of meaning in the discourse community. Finally, Examples 6 and 7 show that the meaning of *species* is always subjected to reinterpretation. Thus, this analysis shows that the meaning of the term *species* depends on the discourse, in which it is constantly renegotiated.

5.3.3 Summary

The analysis of explicit definitions of the meaning of *species* has shown that the meaning of the term *species* was not universal in the scientific community of Darwin's time. This is exemplified by the multiple definitions of the meaning of the term *species* employed by different researchers. By applying different interpretations of what constitutes a species, they described and categorised species and varieties differently (see Section 5.3.2.1). At times the interpretation of the meaning of the term *species* was important than the physical attributes of specimens in the classification of species (see Section 5.3.2.2). Additionally, different definitions put emphasis on different physical features and attributes. This indicates that the meaning of *species* depends more on the definitions of meaning than extra-linguistic phenomenon.

Furthermore, the analysis of has shown that the definitions of the meaning of *species* and *natural selection* are not fixed or property of their authors, but definitions open for interpretation and reformulation (See Section 5.3.2.3). Thus, definitions of meaning are not laws or facts, but rather statements that one may accept, ignore or argue against. Finally, having demonstrated the negotiability of the meaning of *species*, it was evident that meaning is the exclusive property of the discourse.

5.4 Intertextuality

5.4.1 Introduction

The analysis presented in this chapter focuses on the use of intertextuality markers as markers of paraphrase. Paraphrase as an act of definition and interpretation of meaning has been analysed in its most explicit sense of direct definitions in the previous section (See Section 5.3.). However, the analysis presented this section focuses on the analysis of discursive markers of intertextuality. These markers are phrases that explicitly invoke and discuss meanings from other texts, thus they are effectively markers of paraphrase. The aim of this section is to identify these explicit markers of intertextuality and analyse them for the manner and frequency they invoke other texts and meanings in the observed texts.

5.4.2 Markers of Intertextuality

Charles Darwin Correspondence Corpus is abundant with explicit markers of intertextuality because of its epistolary nature which naturally tends to refer to previous texts, i.e. received letters. In fact, it is so rich in intertextual markers that a sub-corpus consisting of only the paragraphs containing the word *species* had to be created especially for this analysis (see Section 4.2 On the Corpus). By focusing on the paragraphs containing the word *species*, most of the intertextual markers unrelated to the negotiation of the meaning of *species* have been automatically excluded from the analysis.

The markers of intertextuality have been identified manually during the collocation and paraphrase analyses and the complete list of the markers of intertextuality used in the analysis is listed in Table 10 below. Furthermore, all the instances of these markers are included in the Appendices 3-12.

Table 10: Markers of intertextuality

| Marker | Frequency |
|---|-----------|
| aware* + with about on which whether that of | 67 |
| agree* disagree* + with about on which whether that | 61 |
| what + say* say * + about on that | 50 |
| object* + to objection to | 47 |
| in regard to regarding | 44 |
| to hear* about that | 43 |
| heard of + the a an some that those these | |
| according to | 38 |
| ask* + that if about whether which how | 36 |
| state* mention* + that | 34 |
| discussion proposal hypothesis assertion + on about | 23 |
| Total: | 402 |

5.4.3 Markers: *aware*, *unaware*

The intertextual markers *aware* and *unaware* are usually used in the collocation with *that* and *of* and make the most frequent intertextuality marker linked to the word *species* in the abridged correspondence sub-corpus. These markers are used to alert the reader to a particular theory, publication or a specimen the

author's statement refers to. Thus, they act as a form of citation or reference expressions.

Examples:

1) *4th. **I am not aware that** the genera which it is difficult to hybridize are slow to sport. Up to this day, tho' I am still trying, I have failed in all attempts to cross Crocuses, yet there is a different either species or vary of Crocus in almost every part of S. Europe, & the garden varieties of Crocus vernus & versicolor are very numerous. [www.darwinproject.ac.uk/letter/entry-503]*

2) ***I am not aware of** any fossil bats or rodents in Australian caves, so the small antiquity of rodentia may be a reason for their not having given origin as yet to Gyrencephala & does there not seem some connection between the low grade of Lyssencephala as the only occupants of that vast Australian continent? [www.darwinproject.ac.uk/letter/entry-2920c]*

3) ***I was not aware that** varieties occurred more in large genera than in small ones,—except from the a priori certainty, that where there are more species to vary, there must naturally be more varieties. [www.darwinproject.ac.uk/letter/entry-2133]*

In Examples 1-3 we can observe how *aware* + *that/of* construction invokes previous interpretations, expressed or referred to by the correspondent. Thus the marker acts almost as a form of citation invoking previous content in relation to which a statement is made. The marker is usually followed by a reference to the original statement which is then commented on. However, the marker can also be used to draw attention to other texts, as shown in the examples below:

Examples:

4) *Have you ever thought of G. St. Hilaire “loi du balancement”, as applied to plants: **I am well aware that** some zoologists quite reject it, but it certainly appears to me, that it often holds good with animals.— **You are no doubt aware of the kind facts I refer to**, such as great development of canines in the carnivora apparently causing a diminution—a compensation or balancement—in the small size of premolars &c &c.—*
[www.darwinproject.ac.uk/letter/entry-996]

5) *As a general rule **you are aware that** throughout the Aculeate Hymenoptera the male is much smaller than the female, whenever the reverse occurs it is for a special purpose; as far as my observation has extended, in the following species it is a beautiful adaptation to a singular point in the economy of the insects; [*www.darwinproject.ac.uk/letter/entry-6534*]*

6) ***You are I daresay aware of the fact that** there is no reason to believe that plants can be artificially acclimated to any extent—Gardeners have hardly made any plant hardy, either by growing it from seeds of an introduced live specimen which did but just ripen, or by grafting on allied hardier species.*
[www.darwinproject.ac.uk/letter/entry-734]

Examples 4-6 show how this construction can be used to introduce new texts into the discourse. However, whether or not the reader is actually aware of these texts is not relevant. The function is to introduce either a statement from another text, or, as in Example 6, make the author's statement appear as such, thus giving it an illusion of objectivity by disguising the statement as a fact.

5.4.4 Markers: *agree, disagree*

Intertextuality markers under this category include the following constructions: *agree** + *with* | *about*| *on*| *which* | *whether*| *that*. The list includes all the lemmas of the word *agree*, as well as the construction with the negative prefix *dis*-. These markers play an important role in collaborative construction of meaning as they regulate the discussion on various theories presented by the correspondents offering both the positive and negative responses. They are also one of the most frequently used intertextuality markers in the sub-corpus. We can observe their use in the following examples.

Examples:

- 1) *To us who theorise I am sure the case is very important. Do the S. American Carabi differ more from the other species, than do, for instance, the Siberian & European & N. American & Himalayan (if the genus exists there); if they do, **I entirely agree with** you that the difference would be too great to account for by the recent Glacial period. **I agree, also, with you** in utterly rejecting an independent origin for these Carabi.—
[www.darwinproject.ac.uk/letter/entry-3532]*

- 2) *You ask for criticisms, I have none to give only impressions.— **I fully agree with** “your skimming-of pot-theory” & very well you have put it.— With respect contemporaneity, **I nearly agree with you**, & if you will look to the d—d— Book 3d Edit p. 349, you will find nearly similar remarks.
[www.darwinproject.ac.uk/letter/entry-3542]*

- 3) *Another Paper which concerns you is one of Lecoq’s on the migration of plants wh special reference to the mountain flora of Auvergne nearly identical with those of the Alps & Pyrenees— **He disagrees with you as** to the glacial epoch and its effect in*

producing the present distribution of plants [...]—but while disagreeing with you on most points he shares your views as to Origin of Species and has anticipated you—
[www.darwinproject.ac.uk/letter/entry-5062]

- 4) *All analogy makes me quite disagree with the Duke that the differences in the beak, wing & tail are not of importance to the several species. In the only two species which I have watched, the difference in flight & in the use of the tail was conspicuously great.* [www.darwinproject.ac.uk/letter/entry-4752]
-

From these examples we can observe somewhat similar use of this intertextual marker to *aware of/that* marker presented in the previous section. Namely, it is used to invoke other texts in the current one. The difference is that a value judgement is added to the invoked proposition. It is used to strengthen or weaken the invoked proposition, hence propagating or negating the proposed interpretation in discourse, while functioning as a form of evaluation.

5.4.5 Markers: *say that, say about*

This intertextuality marker is most simply defined as a form of citation and reference. It is used to invoke other texts in relation to which, usually, a question or a new statement is made.

Examples:

- 1) *It has been said (apparently with little foundation) that amongst birds, species originally coming from distant parts of the world, are more likely to breed together, than those from nearer countries.— Has Mr. Herbert observed anything of this kind in plants?* [www.darwinproject.ac.uk/letter/entry-502]
-

2) You **say** there are **about** 37 new species of Acanth. & according to this proportion there would be about 21 in the other orders. Now do you think it very desirable that all these should be engraved?
[www.darwinproject.ac.uk/letter/entry-538]

3) (In p. 4. of your letter **you say** you give up many Book-species as separate creations; I give up all, & you infer that our difference is only in degree & not in kind. I dissent from this; for I give a distinct reason how far I go in giving up species; I look at all forms, which resemble each other homologically or embryologically as certainly descended from the same parents.)
[www.darwinproject.ac.uk/letter/entry-2922]

4) You **say that** all Botanists would agree that many tropical plants could not withstand a somewhat cooler climate. But I have come not to care at all for general beliefs without the special facts.
[www.darwinproject.ac.uk/letter/entry-5020]

From the above examples we can observe that the invoked statements can be agreed or disagreed on (Examples 3-4), but also that further questions can be posed in relation to these (Examples 1-2). Thus these markers are usually followed by either a request or a form of evaluation.

5.4.6 Markers: *object to*, *objection*

Intertextuality markers included in this category consist of the forms: *object to*, *objecting to* and *objection*. The markers are used in the negative sense, indicating a contentious proposal which the author disagrees with.

Examples:

- 1) *What I should, according to my small proportion of knowledge, **object to**, would be the putting the monotremata into the same group with the Marsupiatæ, not from their resemblance but from the Monotremata consisting of only two species.*
[www.darwinproject.ac.uk/letter/entry-685]
-

- 2) *I had also written to Forbes, before your letter, **objecting to the Sargassum**, but apparently on wrong grounds; for I could see no reason, on the common view of absolute creations, why one Fucus shd not have been created for the ocean, as well as several confervæ for the same end.*
[www.darwinproject.ac.uk/letter/entry-961]
-

The markers can be negated thus stating that there is no direct disagreement with the invoked proposal, as shown in the Example 3 below.

Example:

- 3) *Certainly **there is no objection to** the hypothesis of a Sargassum being an absolute creation, though I see no reason to call for such an aid in this case, the species being in my opinion decidedly the littoral Atlantic one.* [www.darwinproject.ac.uk/letter/entry-964]
-

Additionally, when the marker refers to a statement made by the addressee, it indicates a form of ‘double-intertextuality’ in the sense that invokes a value judgement of another text or even the author’s original proposition. This can be observed in Examples 4 and 5 below.

Examples:

- 4) *II. I have next **to notice your second objection**—that retaining the name of the first describer in perpetuum along with that of the species, is a premium on hasty & careless work. This is quite a different question from that of the law of priority itself, and it never occurred to me before, though it seems highly probable, that the general recognition of that law may produce such a result.* [www.darwinproject.ac.uk/letter/entry-1216]
-

- 5) **A friend objected to** my title that word “Varieties” ought to stand before “Species”. — Another friend **objected** (but illogically) that “genera” & “orders” ought to be inserted. — This has led me to think that word “Varieties” had better be altogether omitted. The case of Species is the real important point; & the title, as now, is rather too long. — So if you do not object, I will omit word “Varieties”; but if on any account you do object, I do not care. — [www.darwinproject.ac.uk/letter/entry-2488]
-

From the examples above, we can observe that this intertextuality marker is used to invoke a statement which is then evaluated, usually in the form of disagreement. However, as Example 3 above shows, agreement with the proposal can be also expressed by negating the marker itself. Furthermore, the marker can stand for an already expressed objection, in which case both the objection and the original statement are invoked in the text.

5.4.7 Markers: *regarding, in/with regard to*

Regarding and *with / in regard to* are common intertextual markers used to introduce citations of other texts, which are usually then followed by evaluation. Their common function is to organise the content of a text by organising the change of topics and relating the text to previous discourse, which can be observed in examples 1-3.

Examples:

- 1) **With regard to** the dissimilarity between the Flora of the several Islds of the group, that is too extraordinary a circumstance for me

to offer any remarks upon, until the florula is drawn up, the further I proceed the more I wonder.

[www.darwinproject.ac.uk/letter/entry-737]

- 2) **With regard to** the value of the pliocene species—identical with a living one—to be of any value the instances must be sound, and unquestionable: and if a sufficient number of good cases is adduced, there is a very wholesome basis for a generalization. **With regard to** the Badger, I cannot say. As soon as I get back to London I will send you Suess paper. I was not a little surprized to find myself so precisely formulated for so decided a Conclusion. Had I not thought that Suess must have sent it to you, I should have forwarded it to you. *[www.darwinproject.ac.uk/letter/entry-4284]*
-

- 3) *I shd. have liked to have seen several examples proving truth (or showing its probability) of some of your remarks; as of best marked vars. being on confines of the range. Or again in regard to your remark of a species remaining for many generations constant under culture & then suddenly commencing to vary.—*
[www.darwinproject.ac.uk/letter/entry-2450]
-

Another function of these markers is to specify the meaning of another expression invoking intertextuality. Thus, in Example 4 the expression ‘What you say about extinction’ already creates an intertextual link, which is then specified using ‘in regard to.’

Example:

- 4) *What you say about extinction, in regard to small genera & local disjunction, being hypothetical seems very just. Something direct, however, could be advanced on this head from fossil shells; but hypothetical such notions must remain*
[www.darwinproject.ac.uk/letter/entry-2125]
-

The primary function of this marker is to organise the text, by indicating the change of topics. In addition to that, the marker can be used to specify an already established intertextual link.

5.4.8 Markers: *hear*

Although its main function is the formulation of a link between different texts, the verb *hear* forms a few distinct intertextuality markers.

Examples:

1) *Of Course **you have heard of** the new species of wild Swan, discovered in England, by Mr. Yarrell. I have had stuffed specimen of it, for 10s.— bad as it is, you may think yourself, lucky in getting. Yarrell himself, has pronounced it to be the new sort, so there can be no doubt.— [www.darwinproject.ac.uk/letter/entry-81]*

2) ***Every one has heard of** the discoloured streaks of water in the Equatorial regions.— One I examined was owing to the presence of such minute *Oscillaria* that in each square inch of surface there must have been at least one hundred thousand present. [www.darwinproject.ac.uk/letter/entry-178]*

3) ***I have heard of** some analogous facts, though on the smallest scale, in certain insects being more variable in one district than in another; & I think the same holds with some land-shells. [www.darwinproject.ac.uk/letter/entry-789]*

In the above examples, *have heard of* is used in a similar way marker *aware* is used, that is, to introduce a text with the illusion of the already known. In this manner, other texts are presented as facts rather than as statements by others.

Another function this intertextuality marker performs is promoting further response in discourse, as demonstrated in the following examples:

Examples:

4) *There may in my opinion have been a series of productions on different spots, & also a gradual change of species. **I shall be delighted to hear** how you think that this change may have taken place, as no presently conceived opinions satisfy me on the subject. — [www.darwinproject.ac.uk/letter/entry-734]*

5) *I am very much obliged for your interesting letter written in such wonderfully good English about climbing plants. The case of *Haplophium* is new to me & I am glad to have seen the tendril of *Strychnos*. I do not suppose I shall attend any more to climbing plants, **but I shd like to hear** if you ever meet with 2 species of the same genus, twining in opposite directions **I should further like much to hear** whether any twiners can ascend thick trunks. [www.darwinproject.ac.uk/letter/entry-4895]*

From Examples 4 and 5 we can observe that the marker can be used to request an opinion or a report on a particular topic, thus promoting further responses on particular issues. Although in this case the marker does not indicate intertextuality *per se*, it is used to prompt a response on a particular issue, making that future statement intertextual.

5.4.9 Markers: *according to*

The use of the intertextual marker *according to* is characterised by reference to other texts, either from the correspondence or external texts. One way it is used

is to attribute the statement to a particular source, effectively acting as a form of reference or citation, which is illustrated in Examples 1 and 2.

Examples:

1) *Parrots **according to** Mr Ehrenberg have moved south in Nubia since Roman times. — [www.darwinproject.ac.uk/letter/entry-534]*

2) *Wallace has, I think, put the matter well; and **according to** his theory, the various domestic races of animals have been fairly developed into species. [www.darwinproject.ac.uk/letter/entry-1792]*

The marker itself has no implications on evaluation, carrying neither negative nor positive prosody. Its main function is not evaluation but rather incorporating others' discourse into the text.

However, the marker is also used to express logical conclusions, or rather interpretations that are made in relations to particular statements. This can be observed in Examples 3 and 4 below.

Examples:

3) *By way of illustration I will suppose I am called upon to point out a type of the order Carnivora. **According to** the first definition I should select a Cat because in the Cat tribe some of the more striking characters of the Carnivora are most strongly developed; but, were I to adopt the second definition I should choose a Viverra because it may be said to possess most evenly developed the greatest number of characters which are found in the species of its order; [www.darwinproject.ac.uk/letter/entry-748]*

- 4) *You say there are about 37 new species of Acanth. & **according to** this proportion there would be about 21 in the other orders.*
[www.darwinproject.ac.uk/letter/entry-538]
-

In this sense, the meaning of ‘*according to*’ is similar to the meaning of ‘*based on*.’ Nevertheless, the marker still indicates an intertextual relation, as interpretation is attributed to a particular definition.

5.4.10 Markers: *ask*

The intertextuality markers included in this category include all the lemmas of the verb *ask* in combination with particles *that, if, about, how, whether* and *which*. These markers are usually used to link the answer to a particular question and are likely to be typical of written correspondence, since in other types of discourse it is unlikely that the questions would be repeated when providing the response. Examples of their use in the corpus are listed below.

Examples:

- 1) *You **ask me whether** I suppose the small proportion of sp to genera in Coral Islets, arises from chance of seeds &c? I cannot answer this, I should say perhaps not:— if genera or small groups are truly natural they are supposed to contain many characters in common, it is but right to assume that the character of transportable seeds should hence be common to some groups above others, the inference I need not state.*
[www.darwinproject.ac.uk/letter/entry-734]
-
- 2) *You **ask whether** the uniformity consists in species or forms. I am inclined to consider that uniformity of species is to a certain extent a sequitur to a uniformity of forms, & that it is a corollary to our Theorem. [*www.darwinproject.ac.uk/letter/entry-739*]*
-

- 3) You **ask what effect** studying species has had on my variation theories; I do not think much; I have felt some difficulties more; on the other hand I have been struck (& probably unfairly from the class) with the variability of every part in some slight degree of every species: [www.darwinproject.ac.uk/letter/entry-1339]
-

5.4.11 Markers: *mention, state*

The intertextual markers in this category attribute the statements to other texts, thus functioning as citation markers. They are not used only for expressing what is stated in other texts, but rather to state what these texts do not mention. This is achieved by questioning the statements (Example 1), reformulating them in new contexts, linking them with or comparing to other interpretations (Example 2), etc. In other words, these markers are used to reinterpret and recontextualise these texts in order to form new meanings.

Examples:

- 1) Mr Herbert incidentally **mentioned** in a letter to me, that the Heaths at the C. of Good Hope were very variable, whilst in Europe they are (?) not so (?); but then the species here are few in comparison, so that the case, even if true, is not a good one. [www.darwinproject.ac.uk/letter/entry-789]
-

- 2) I traced its etymology distinctly in old Fryer's work (1698), where these little birds **are stated to** be brought to Sarat from "Amidavad"; meaning Ahmedabrsquo; in Guzurat, where they still abound! And so the name of that town has become transferred to themselves, as in the more familiar instances of Canary & Bantam! N.B. You will find "avadavats" **mentioned in** Sheridans 'School for Scandal', 1777—act. V, sc. I; so that at that time the

name must have been familiar.
[www.darwinproject.ac.uk/letter/entry-1735]

5.4.12 Markers: *discussion, hypothesis, assertion*

The markers in this category are essentially nominalisations of their respective verbs which also function as intertextuality markers themselves, e.g. say, mention, state, etc. However, they are presented in a separate category as the nominalisation form allows these markers somewhat different function. Namely, as nominalisations these markers present other texts as objects in the new text, allowing them to be discussed and evaluated in single utterances.

Examples:

- 1) *Mr Knight seems to have found this variation in mongrel apples &c &c common, whereas Mr Herbert makes in the Hort. Transacts. an opposite **assertion** with regard to the hybrid Criniums.—* [www.darwinproject.ac.uk/letter/entry-502]

- 2) *I have thought that I ought to state to you the ground for my **assertion** on page 602, that Geology has not afforded facts that sustain the view that the system of life has been evolved through a method of development from species to Species.— There are three difficulties that weigh on my mind, and I will mention them.* [www.darwinproject.ac.uk/letter/entry-3969]

- 3) *When looking over some grass a species of Bromus from the North Western coast of America—with a friend & botanist we noticed some oats that were producing ears of barley this led to the **assertion made by** Elihu Burritt in his “Walk through the Eastern Counties” that a farmer at St. Ives Hunts had for years “transmuted” oats into barley. doubting this I wrote to the writer*

of the enclosed a farmer of 1600 acres a Member of the Council of the Royal Agricultural Society & above all a good & truthful gentleman. [www.darwinproject.ac.uk/letter/entry-5242]

4) *I will trouble you with only one other question. In **discussion** with Mr Gould, I found that in most of the genera of birds, which range over the whole or greater part of the world, the individual species have wider ranges: thus the Owl is mundane, & many of the species have very wide ranges.*
[www.darwinproject.ac.uk/letter/entry-736]

5) *Your **discussion** on connecting & separating forms seems to me so philosophical, that I much hope that someday you will be as good as your word & write an "Essay on Species".*
[www.darwinproject.ac.uk/letter/entry-1725]

5.5 Diachronic analysis of intertextuality

5.5.1 Introduction

The diachronic analysis of intertextuality is based on the analysis of specific collocations of *species*, identified in Section 5.2, with the aim to observe when particular collocations first enter discourse and when they are most frequently discussed. For example, a high concentration of instances in a narrow diachronic span indicates an intensified discussion of the notion. Furthermore, the analysis will observe whether the specific collocations are consistently discussed throughout the corpus or whether they stop being discussed at some point. This will demonstrate the diachronic dimension of a particular set of interpretations by showing how these interpretations enter discourse; at what moments they are intensively discussed; and if they are abandoned from the discourse at any stage.

The collocations of *species* from the MI-score analysis (Section 5.2.2) are used for this purpose and include the following collocations: *mutable + species*, *disjoined + species*, *modification + species*, *describe + species*, *derived + species*, *allied + species*, *origin + species*, *range + species*, *variable + species*, and *close + species*.

These collocates have been chosen as a result of a corpus driven analysis and are based on the list of strongest collocations according to MI-score. The list based on MI-score was used rather than the list based on t-score because the top collocates according to t-score are generally frequent words evenly distributed in discourse. For example, the twenty highest ranked collocates

according to t-score are all function words, whereas the MI-score list includes only the content words (see Appendix 1 – List of collocates of the *term species* for details). Collocates selected for the analysis represent the highest ranked collocations according to MI-score which are related to Darwin's theory. Thus words like *quasi*, *identical*, *trimorphic*, etc. are not included in the analysis, despite ranking highly on MI-score. All collocates have been analysed as described in methodology section, Chapter 4 – using the same 5x5 word span as in the initial collocation analysis presented in Section 5.2 Collocation analysis.

In addition to observing the diachronic distribution of the interpretations of the term *species*, the aim of the analysis is also to identify 'topic hotspots' which indicate a more prominent discussion, or negotiation, of particular paraphrases. Thus while observing the diachronic dimension, we can also observe the intertextual dimension, i.e. when the terms have been most actively negotiated. This is done using the concordance plot tool in AntConc (Anthony, 2011) and searching for collocations using regular expressions to define the 5x5 word span. The results shown in concordance plots show all the occurrences of the search term in the correspondence arranged chronologically. Thus, a concordance plot can be compared to a kind of a timeline representing the discourse of Darwin's correspondence and demonstrating when each instance of the search term occurred.

5.5.2 Mutable + species

This analysis includes four forms denoting the mutability of species, all of which have ranked highly on MI-score list; namely: *mutability*, *immutability*, *immutable* and *mutable*. As we can see from Figure 1 below, this collocation occurs in the correspondence in intervals, when it is used more frequently, thus indicating the topic hotspots.

Figure 1 Distribution of collocation (mutable + species)



The first two hotspots, which we can observe at the left of the plot, are the most interesting to observe in detail as they represent instances at the early stage of Darwin's correspondence, around the time when he started questioning the notion of immutability of species. In the examples below, we can observe the first instances of the collocation *mutable + species* in the discourse.

Letter 782: Darwin to Jenyns on 12 Oct 1844

- 1) *The general conclusion at which I have slowly been driven from a directly opposite conviction is that **species are mutable** & that allied species are co-descendants of common stocks.*
[<http://www.darwinproject.ac.uk/letter/entry-782>]

In the following example Darwin hedges his argument stating that he only proposes an alternative interpretation and does not argue against the canonical interpretation of the immutability of species. Furthermore, Darwin here repeats his interpretation that allied species have descended from common stock.

Letter 789: Darwin to Hooker on 10 Nov 1844

- 2) *I must have been cracked to have written it, for I have no evidence, without a person be willing to admit all my views, & then it does follow; but in my most sanguine moments, all I expect, is that I shall be able to show even to sound Naturalists, that there are two sides to the question of the **immutability of species**;—that facts can be viewed & grouped under the notion of allied species having descended from common stocks.*
[<http://www.darwinproject.ac.uk/letter/entry-789>]
-

The following two examples are from a letter to Leonard Jenyns. In the first instance, Darwin expresses the same argument about his intentions as in the letter to Hooker, shown in the previous example, even using the same exact words ‘*that there are two sides to the question of the immutability of species*’:

Letter 793: Darwin to Jenyns, 23 Nov 1844

- 3) *With respect to my far-distant work on species, I must have expressed myself with singular inaccuracy, if I led you to suppose that I meant to say that my conclusions were inevitable. They have become so, after years of weighing puzzles, to myself alone;; but in my wildest day-dream, I never expect more than to be able to show that there are two sides to the question of the **immutability of species**, ie whether species are directly created, or by intermediate laws, (as with the life & death of individuals).*
[<http://www.darwinproject.ac.uk/letter/entry-793>]
-

However, in the second part of the letter, Darwin slightly alters the definition he expressed to Hooker in the previous letter, and initially also to Jenyns, stating that his arguments ‘*cannot be restricted to the closest-allied species*’:

Letter 793: Darwin to Jenyns, 23 Nov 1844

- 4) *I am a bold man to lay myself open to being thought a complete fool, & a most deliberate one.— From the nature of the grounds, which make me believe that **species are mutable** in form, these grounds cannot be restricted to the closest-allied species; but how far they extend, I cannot tell, as my reasons fall away by degrees, when applied to species more & more remote from each other.*
[<http://www.darwinproject.ac.uk/letter/entry-793>]
-

Although the corpus does not include responses sent back to Darwin, they can be inferred from Darwin's subsequent letters. This is particularly evident in the letter to Jenyns where Darwin apologetically states: '*I must have expressed myself with singular inaccuracy, if I led you to suppose that I meant to say that my conclusions were inevitable.*'

Thus by analysing the collocation *mutable* + *species*, several intertextual features have been observed: 1) paraphrase – Darwin's reformulations gradually remove the restriction of only closely-allied species descending from common stock; 2) lexical repetition – (re)formulations have a similar phraseology; and 3) diachronic dimension of meaning – strength of intertextual links correlates with diachronic proximity, i.e. the closer the two utterances are to one another the more likely they will paraphrase one another and thus use similar phraseology.

5.5.3 Disjoined + species

The case of *disjoined* + *species* exemplifies the main goals of diachronic analysis as it demonstrates clearly how one particular interpretation enters discourse, establishes clear intertextual relations then suddenly disappears from the discussion. Figure 2 shows the topic hotspot in the correspondence.

Figure 2 Distribution of collocation (*disjoined* + *species*)



The discussion on disjointed species was exclusive to Asa Gray and Darwin, thus showing a strong intertextual relation. From the corpus we can observe that Gray initiated the discussion and then upon accepting Darwin's views ended it. Disjointed species are discussed only in a few letters, but we can observe how they evolve as a concept below.

Letter 2089 May 57 Darwin to Gray:

- 1) *I want to know to see more clearly in proportion to your whole Flora how large the proportion of monotypic genera is in the **disjointed species**. This subject interests me very much: I began to try to work out this point in all the cases of much **disjointed species** which I met with; but I failed from want of knowledge: I tried also to make out whether the **disjointed species** would not on average belong to small Families, but here again I failed from want of knowledge; though the cases in which I could find out something, confirmed my very strong expectation **that species having disjointed ranges would belong to small genera**; so you may imagine how much interest I felt in coming on your note on this very subject. — .[www.darwinproject.ac.uk/letter/entry-2089]*
-

Letter 2098 June 1857 Gray to Darwin

- 2) *I did not know at all that you suspected **disjointed species to belong to small genera & small orders, as a general thing**. ... My 76 **disjointed species** belong to 34 families,—and I cannot see that they incline to belong to small families.
[www.darwinproject.ac.uk/letter/entry-2098]*
-

In the following examples, Darwin introduces the notion of extinction as the cause of disjointed species, which is later further developed by Gray.

Letter 2109 June 1857 Darwin to Gray:

- 3) *The notion was grounded on the belief that **disjointed species** had suffered much local extinction & therefore (conversely with the case of genera with many species having species with wide ranges.) I inferred that genera & Families with very few species (ie from Extinction) would be apt (not necessarily always) to have*

*narrow ranges & **disjoined ranges**. You will not perceive, perhaps, what I am driving at & it is not worth enlarging on,—but I look at Extinction as common cause of small genera & **disjoined ranges** & therefore they ought, if they behaved properly & as nature does not lie to go together!— .[www.darwinproject.ac.uk/letter/entry-2109]*

2120 Gray to Darwin July 1857

- 4) *I accept it as best explaining **disjoined species**. I see that the same cause must have reduced many species of great range to small, and that it may have reduced large genera to small, and so of families. But why is it not just as likely that there were as many small genera (nearly) at first as now, and as great a disproportion in the number of their species?*
.[www.darwinproject.ac.uk/letter/entry-2120]
-

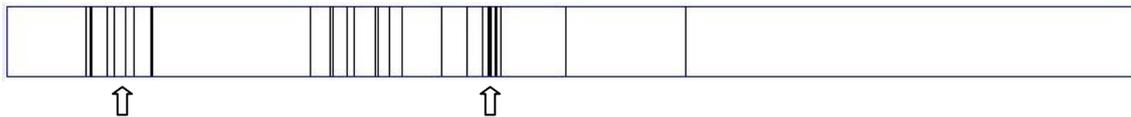
Although in these examples the phrase *disjoined species* is not itself modified, the contexts in which it occurs in do, until the moment agreement is achieved and discussion ended. To summarise, the example of *disjoined species* has demonstrated the same intertextual features as *mutable + species*; namely the use of paraphrase and lexical repetition to reformulate, negotiate and create new meanings. In addition to that, this collocation has illustrated a strong diachronic dimension of meaning by showing how a particular interpretation of *species* can arise and disappear from discourse.

5.5.4 Representative + species

‘Representative + species’ is another collocation which is distributed in the correspondence in such a way as to form topic hotspots. From these hotspots we can observe when in the correspondence was this formulation an important aspect of discourse on *species*. The collocation appears to form topic hotspots

in the early years of the correspondence, then after a period of time it was restarted, only to disappear from the discourse soon after.

Figure 3 Distribution of collocation (representative + species)



From Figure 3 we can identify two major topic hotspots, and in the leftmost, earliest, one we have identified intertextual relations. Figure 4 shows the relevant concordances.

Figure 4 Concordance output of (representative + species) indicating intertextual relations

| | | | | |
|----|--|------------------------|---|--|
| 1 | Archipelago, with the separate islands possessing distinct | representative species | ? I have always intended, (but have not yet done so) to exam | Letter 736: Darwin to Hooker on 23 Feb 1844 |
| 2 | the separate islds of the Sandwich Arch: possessed distinct | representative species | of the same genera of Labiatæ: would not this be worth your | |
| 3 | appears to me, whether any two or three islands have close | representative species | of the same genus; the simple fact of one <u>isld</u> having a spe | Letter 740: Darwin to Hooker on 11 Mar 1844 |
| 4 | may aid you in observing, whether the different islds have | representative species | filling the same places in the œconomy of nature, to know, | Letter 744: Darwin to Hooker on 31 Mar 1844 |
| 5 | then in flower.— Please bear this in mind in comparing the | representative species | .— (You know that Henslow has described a new Opuntia from t | |
| 6 | o many enemies. On the whole I believe that many individual | representative species | of large genera have wide ranges , but I do not consider the | |
| 7 | pecies having a wide range is not large compared with other | representative species | of the same genus whose limits are confined—& further becau | Letter 745: Hooker to Darwin on 5 April 1844 |
| 8 | cause small genera have likewise individual widely extended | representative species | . The converse holds true in a certain degree & in the Cacti | |
| 9 | that it is not proved in Botany that "in mundane genera the | representative species | have a wide range each in its own country", to any remarkab | |
| 10 | ecies have extended ranges but I do not know that it is the | representative species | that have wide ranges each in its own territory, even if tr | |
| 11 | t out, bearing | representative | of widely | |

| | | | | |
|----|---|---------------------------|--|--|
| | directly on the grand question, "Have the 10 | <u>individual</u> species | distributed genera over 10 countries <u>wide ranges</u> | |
| 12 | Isld & Society Isld lists, they are woefully imperfect, the | representative species | are very few, there is a certain similarity between them, f | Letter 804: Hooker to Darwin on 30 Dec 1844 |
| 13 | several Islds of the Sandwich present numerous instances of | representative species | , nothing in this respect is known of the Society. Shall I s | |
| 14 | vely what a third collector might produce. The instances of | representative species | on the several Islets may be divided into 2 groups, 1st. of | Letter 883: Hooker to Darwin on 12 July 1845 |
| 15 | o not know how far some of the genera may not have solitary | representative species | in seperate Islets. Of confined genera there are many examp | Letter 884: Hooker to Darwin mid July 1845 |
| 16 | larly interested with similar facts in the Birds &c: Do the | representative species | actually join on a neutral territory? Are both species, or | Letter 912: Darwin to Hooker 3 Sep 1845 |
| 17 | am much pleased to hear you have worked out the identical & | representative species | of N. temperate & Antarctic regions & shall be exceedingly | Letter 996: Darwin to Hooker 3 Sep 1845 |
| 18 | ish Flora. When such results of any comparisons between the | representative species | of the N. & S. hemispheres that my Flora will shew, shall b | Letter 998: Hooker to Darwin 28 Sep 1845 |

All the instances shown in Figure 4 come from letters written by Darwin and J.D. Hooker, which can be explained by his cautious approach to revealing his theory. We can also observe other intertextual relations in the same letters, which will be discussed later (see section 5.5.10 Range + species), indicating the importance of this correspondence for the development of Darwin's theory.

Finally, what is striking about these examples is how suddenly they disappear from discourse, similarly to the notion of *disjoined species* (Section 5.5.3). However, in this case the topic restarted later, incidentally with a reference to Hooker:

Letter 1863 Darwin to Gray on 2 May 1856:

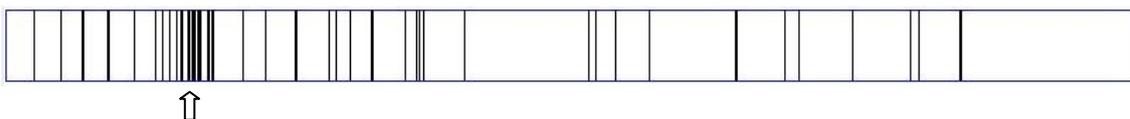
- 1) *The relation of the genera (excluding identical species) seems to me a most important element in geographical distribution often ignored, & I presume of more difficult application in plants than in animals, owing to the wider ranges of plants; but I find in N. Zealand (from Hooker) that the consideration of genera with **representative species** tells the story of relationship even plainer than the identity of the species with different parts of the world.—*

From the example above, we can observe the notion of *representative species* disappeared for more than 10 years from the correspondence and, unsurprisingly, was reintroduced to the discourse roughly around the time Darwin decided to publish his theory – once again illustrating the importance of Darwin’s correspondence in the formulation of his ideas.

5.5.5 Describe + species

The analysis presented in this section looks at the collocation *describe + species*, which includes the adjectives *undescribed* and *described*; as well as the respective verb forms. From Figure 5 we can notice that the collocation is concentrated relatively early on in Darwin’s correspondence. This coincides with the time of his classification work and does not necessarily indicate the negotiation of the meaning of the phrase.

Figure 5 Distribution of collocation (describe* + species)



The logical assumption is confirmed by looking at the concordances showing practically no instances of intertextual relations or meaning negotiations, but rather reports on species description.

Figure 6 Concordance output of (representative + species)

| | | | |
|----|---|----------------------------------|--|
| 1 | very branch. as for insects I trust I shall send an host of | undescribed species | to England.- I believe they have no small ones in the colle |
| 2 | opical countries, I shall bring home a very great number of | undescribed species | both from Brazil & the Rio Plata.- It may be a foolish fear |
| 3 | were on the table at the Zoological Society; Mr Gould also | described the 11 species | from the Gallapagos, & all new.- Sarah Williams cut out thi |
| 4 | might afford, should exclusively be employed on the new and | undescribed species | . Other naturalists and myself are of opinion that 150 plate |
| 5 | pinion, that 150 plates, would be necessary for the new and | undescribed species | , and that the probable expense of drawing and engraving, wo |
| 6 | ength his vivid regret that M. Henslow has not been able to | describe the species | , or even characterize the genera of the very curious collec |
| 7 | e to examine their structure more closely than I did when I | described the species | . Believe me my dear Sir Yours very truly Francis Walker |
| 8 | ding Arctic forms.- These lists are troublesome to make, as | species are so loosely described | . For instance of the arbor. comp. of St Helena in one genus |
| 9 | genus which I know to be found in South America-Babington | described a species | from your collection found at Rio, which is very nearly all |
| 10 | [Identifies CD's fossil shells and marks new species. Many | species have been described | previously by Orbigny. The list is annotated by CD and Geor |

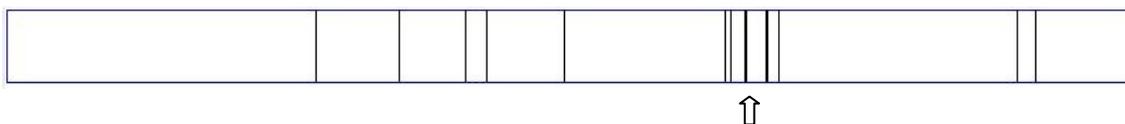
These examples do not indicate an intertextual negotiation of the notion of describe + species. Thus we cannot observe paraphrases indicating the negotiation of meaning. However, we can observe that the collocation mainly occurs at the beginning of the corpus, which corresponds to Darwin's early work, his *Beagle* journey and work on collecting and describing species. Naturally, it is expected for Darwin to discuss undescribed species at this stage.

Conversely, describing species becomes much less relevant later on in the correspondence when Darwin becomes more immersed in defining his own theory. Thus this particular example demonstrates mainly the diachronic shifts in the focus of discourse.

5.5.6 Derive + species

Figure 7 shows the collocation *derive* + *species*, including the nominalised form *derivation*. However, as the number of examples was low, we can only observe one minor topic hotspot.

Figure 7 Distribution of collocation (derive + species)



When looking into more detail, we can observe that the hotspot is caused by a discussion on Owen's paper theorising the derivation of species.

Letter 4024: Darwin to Hooker on 5 Mar 1863:

- 1) *I have half a mind to get Owen's paper on Aye-Aye, in which Lyell tells me, that he claims whole credit of making out the **derivation or origin of species**; & if this is so, write a letter to Athenæum & show, what he has really done.— It is in my Hist. Introduct. to 3d Edit of Origin; but I did not then point out the laugable definition he gives of "Creation"; after doubting whether certain species were "created".— [www.darwinproject.ac.uk/letter/entry-4024]*

Letter 4186: Gray to Darwin on 28 May 1863:

- 2) *Your letter on Heterogeny is keen & good. Owen's rejoinder ingenious. But his dissent from your well-put claims of Nat. Sel. to attention & regard, is good for nothing except on the admission of the view that **species are somehow derived** genealogically—&*

this I judge from various of Owen's statements that he really in his heart believes to be the case,—and was (as I long ago intimated my suspicions) hunting about for some system of derivation, when your book came down upon him like a thunder-clap.
.[www.darwinproject.ac.uk/letter/entry-4186]

Letter 4248: Gray to Darwin on 21 July 1863

3) *have been reading Owen's Aye-Aye paper. Well, this is rich and cool! Did I not tell you in Atlantic long ago that Owen had a transmutation theory of his own. It is your Hamlet, with the part of Hamlet left out. But as you say now you don't so much insist on Nat. Selection if you can only have **derivation of species**,— and Owen goes in for derivation on the largest scale, you may as well lovingly embrace! Oh, it is rare fun. How I could now tease Agassiz, if I could see him,— only he is of late so cross and sore.*
.[www.darwinproject.ac.uk/letter/entry-4248]

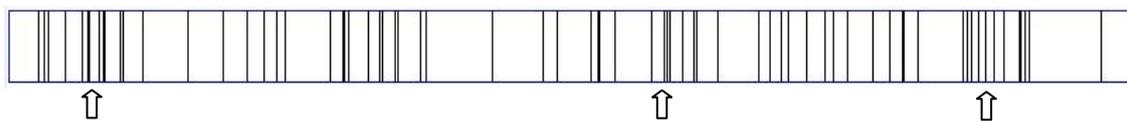
These examples thus show a strong intertextual link to a third text, i.e. Owen's Aye-Aye paper. Thus, the phrasal structure of *derivation* + *species* cannot be attributed to Darwin's interpretation of the meaning of *species*. Apart from the reference to Owen's work, it is very rarely used in the discourse which is the reason why the collocation ranks highly on MI-score, i.e. by being a very unique combination. Nevertheless, by identifying the topic hotspot, the intertextual relationship was quickly uncovered by looking at the wider context than the standard 5x5 span usually used for collocation studies.

5.5.7 Allied + species

Allied is a frequent collocater of *species*, counting 107 instances and the high number of collocations implies that the use of the modifier is relatively evenly distributed. Thus, there are only a few potential topic hotspots where the

collocations are grouped, although it is much harder to delimit where each group starts and begins. This can be observed in Figure 8 below.

Figure 8 Distribution of collocation (allied + species)



It was not only difficult to delimit the hotspots, but the example of *allied species* did not show any particular intertextual relations in the potential topic hotspots. A potential implication of this result is that with highly frequent phrases, which are more likely to be evenly distributed, it may be difficult to identify meaningful topic hotspots.

5.5.8 Close + species

In this analysis both the adjective and adverb form, i.e. *close* and *closely*, are included. The example of *close + species* exemplified in Figure 2 indicates significantly stronger topic hotspots than with *allied + species*, and also that it is a concept frequently and consistently expressed throughout the discourse of Darwin's correspondence.

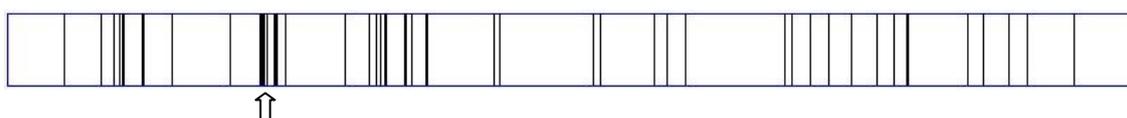


Figure 9 Distribution of collocation (close + species)

The hotspots certainly indicate intertextual relations showing 25 instances of the collocation in the letters spanning from June 23rd to August 24th 1855. For simplicity, the results are presented as concordances in Figure 10, as it makes the comparison easier than listing all 25 examples with full context.

Figure 10 Concordance output of (close+species) indicating intertextual relations

| | | | | |
|----|--|--------------------------------------|--|--|
| 1 | I will just mention what I want, it is, to have marked the “ | close species | ” in a Flora, so as to compare in different Floras whether t | Letter 1695: Darwin to Asa Gray on 8 June 1855 |
| 2 | o compare in different Floras whether the same genera have “ | close species | ”, & for other purposes too vague to enumerate.— I have atte | |
| 3 | le or present varieties. The definition I should give of a “ | close species | ” was one that you thought specifically distinct, but which | |
| 4 | cies, which you believe to be really species, but which are | close species | ;—taking some such definition for a “close species”, as a fo | Letter 1705: Darwin to Henslow on 27 June 1855 |
| 5 | which are close species;—taking some such definition for a “ | close species | ”, as a form, which even to a good Botanist is a little trou | |
| 6 | e unbound sheets of a copy of the Manual, and mark off the “ | close species | ’, by connecting them with a bracket Those thus connected so | Letter 1707: Asa Gray to Darwin on 30 June 1855 |
| 7 | ra of North America —we should find that the proportion of “ | close species | ” to the whole flora increased considerably. But here I spea | |
| 8 | h’s Fl. Germanica —or Godron’s Flora of France —& mark the ‘ | close species | ” on the same principle, you will doubtless find a much grea | |
| 9 | getting you to mark in accompanying list with (X) all the “ | close species | ” ie such as you do not <u>think to be varieties</u> , but which nev | |
| 10 | on subject.— I know & can perceive that the definition of “ | close species | ” is very vague, & therefore I shd. not care for the list be | Letter 1708: Darwin to Henslow on 2 July 1855 |
| 11 | ds) but what you <u>think are really species, but yet are very</u> | closely allied to some other species | : I well know how vague this is, & perhaps you will find it | Letter 1712: Darwin to Henslow on 7 July 1855 |
| 12 | lways the possibility & even sometimes probability of these | close species | turning out varieties.— Ever most truly yours C. Darwin P | |
| 13 | . Darwin Perhaps you might (if you can do the job) mark the | closely allied species | in connection, thus in an imaginary genus diag Quercus pedu | |
| 14 | that you thought Q. pedunculata sessiliflora & humilis very | closely allied species | , but yet real species. I repeat perhaps the job will be imp | |
| 15 | ou I long hesitated whether I would make) about marking the | close species | . I do not quite understand from Hooker’s note, whether the | Letter 1725: Darwin to Asa Gray, 21 July 1855 |
| 16 | o not apply. On the other hand, many which I do not mark as | close species | , simply because entered in the Catalogue as Species & Vary. | Letter 1743: Watson to Darwin on 17 Aug 1855 |
| 17 | s Species & Vary., other botanists would consider to be the | close species | , while those marked would by them be deemed not close enoug | |
| 18 | r object. I think my leading idea was to select examples of | close species | or quasi-species, rather than to make the list numerically | Letter 1747: Watson to Darwin on 23 Aug 1855 |
| 19 | ss large genera in Europe, or elsewhere. In the question of | close species | , I should prefer the testimony of Fries before that of Hook | |
| 20 | h. It would seem to follow as a logical necessity, that the | species are closer | in the large genera: diag 1 2 3 4 5 6 7 8 9 Many sps. 1 – 3 | |
| 21 | nk you enough for the very great trouble which the list of “ | close species | ” must have caused you.— What knowledge & labour & judgment | Letter 1749: Darwin to Asa Gray, 24 Aug 1855 |
| 22 | ies-splitters, to mark (without the object being known) the | close species | in a list; then if I counted the average number of the spec | |
| 23 | it would, to a certain extent, tell whether on average the | close species | occurred in the larger genera. Now in your M.S list (Salix | |
| 24 | ing what is called a genus, some of them are apt to be more | closely allied than are the species | in the smaller genera. Mr. H. C. Watson has marked for me t | |
| 25 | hich was a very long one, & the average of the genera with “ | close species | ” is 6.37 & the same standard of comparison as used with you | Letter 1750: Darwin to Watson, 26 Aug 1855 |

An interesting example of the development of meaning is found in the letters of Charles Darwin to John Stevens Henslow, where Darwin is unwillingly forced to keep expanding on the notion of *close species*. Sadly, Henslow's input is only preserved as references in Darwin's letters.

Letter 1705 Darwin to Henslow on 8 June 1855

- 1) *& mark with cross, all those species, which you believe to be really species, but which are close species;—taking some such definition for a “close species”, as a form, which even to a good Botanist is a little troublesome to distinguish, or which you can just conceive possible, though not probable, that further research will prove to be only varieties.*[www.darwinproject.ac.uk/letter/entry-1705]

Letter 1708 Darwin to Henslow on 2 July 1855

- 2) *I think you have quite misunderstood me in regard to my object in getting you to mark in accompanying list with (X) all the “close species” ie such as you do not think to be varieties, but which nevertheless are very closely allied;—it has nothing whatever to do with their cultivation, but I cannot tell you object, as it might unconsciously influence you in marking them. Will you draw your pencil right through all the names of those (few) species, of which you may know nothing. Afterwards when done I will tell you my object,—not that it is worth telling, though I myself am very curious on subject.— I know & can perceive **that the definition of “close species” is very vague**, & therefore I shd. not care for the list being marked by anyone, except by such as yourself.—*
.[www.darwinproject.ac.uk/letter/entry-1708]

Letter 1712 Darwin to Henslow 7 July 1855

- 3) *I do not think I have yet made it quite clear what I want marked in the Catalogue. (but I am really ashamed to be so troublesome) it is not so much what you “think may turn out varieties” (to quote your own words) but what you think are really species, but yet are very **closely allied to some other species**: I well know how vague this is, & perhaps you will find it impossible to do; but certainly,*

*judging from what I have seen in animals, one pretty often meets a pair or more real (as far as one can judge) species, which yet are far **more closely allied together** than the average.*

- 4) *Of course there is always the possibility & even sometimes probability of these **close species** turning out varieties.*
[www.darwinproject.ac.uk/letter/entry-1712]

From these examples we can observe how Darwin is forced to expand the definition of *close species* in order to get Henslow to interpret it as Darwin intended. Although Henslow's letters are lost, from Darwin's citations we can notice that his interpretation must be different enough to incite Darwin to gradually expand his definition of *close species*.

5.5.9 Origin + species

The example of *origin + species* is particularly interesting as it forms the title of Darwin's most famous work, particularly considering the time the collocation first appeared in discourse. Figure 11 shows the distribution of the collocation in the correspondence.

Figure 11 Distribution of collocation (*origin + species*)



Almost all hotspots indicate a discussion on Darwin's book and show plenty of intertextual relations. However, it is the early hotspots and specifically the lack of them that is interesting. Namely, the first nine instances of the collocation are not attributed to Darwin, and there are only a few instances that can be said to have occurred before Darwin started work on his manuscript of the book. Thus

the distribution of the collocation indicates that Darwin developed his theory after many years of his work with species.

Interestingly, the earliest reference from Darwin to the concept of origin of species is dated December 1855, more than 10 years after starting the development of his theory of transmutation. Thus it is interesting to observe these early instances in order to observe whether they relate to Darwin's formulation of the notion.

The earliest instance of the collocation comes in a letter from J.D. Hooker, where he rejects the discussion on the notion, stating that '*each species has one origin is immutable & migrates*', as seen in the example below:

Letter 947 Hooker to Darwin Feb 1846:

- 1) *At present I endeavor to hold aloof from all speculations on the origin of species, & wish to till at any rate this part of my flora is finished. When that is the case I should like to have much talk about it with you, at present I go on the old assumption that each species has one origin is immutable & migrates.*
[www.darwinproject.ac.uk/letter/entry-947]

Ten years later, in a letter from Edward Blyth to Darwin, we can observe that Hooker himself started publicly 'embracing some speculations on the origin of species.' The implication that Hooker is embracing these ideas implies that they are not necessarily his own; although it is not stated what these ideas might be.

Letter 1776 Nov 1855 Blyth to Darwin

- 2) *I have not yet seen, & look forward with much interest to read, what Hooker and Thomson have written on the general subject of species & **varieties of plants, in their Flora Indica; embracing (if I understand aright) some speculations on the origin of species.** Thomson has been my neighbour for some months; within a few*

miles: but I have not yet been able to find time to spend a day with him, though pressingly asked to do so. Now that the cold weather has fairly set in, I mean to do so soon.

[www.darwinproject.ac.uk/letter/entry-1776]

Although Darwin did not use the term *origin* until December 1855, he was the first to use it with the implications on transmutability of species. In that letter to Edgar Leopold Layard, Darwin states that his theory is a product of many years of collecting data and reasoning about variation and origin of species.

Letter 1794 December 1855 Darwin to Edgar Leopold Layard

3) *I have during many years been collecting all the facts & reasoning which I could, **in regard to the variation & origin of species**, intending to give, as far as lies in my power, the many difficulties surrounding the subject on all sides. One chief line of investigation naturally is concerned with the amount of variation of all our domestic animals.*

4) *[www.darwinproject.ac.uk/letter/entry-1794]*

In the letter to J.D. Dana, Darwin explicitly states that he is already working on his theory, although he does not specify exactly what it is, but expressing it vaguely as drawing up on '*variation & the origin of species, classification &c.*'

Letter 1925 July 1856 Darwin to J.D. Dana

5) *I want to beg one more favour to the many which formerly you have conferred on me. I am extremely much interested in regard to the blind cave animals, described some time since in your Journal by Prof. Silliman Junr., as the subject is connected with a work of somewhat general nature, **which I am endeavouring to draw up on variation & the origin of species, classification &c.***
[www.darwinproject.ac.uk/letter/entry-1925]

Finally, Darwin gives credit to Hooker for his contributions to the formulation of his theory. Another interesting remark is that he understood how little his opinion on the issue is worth without like-minded supporters.

Letter 1938 August 1856, Darwin to Hooker:

6) *I thank you most sincerely for all your assistance; & whether or no my Book may be wretched you have done your best to make it less wretched. Sometimes I am in very good spirits & sometimes very low about it. **My own mind is decided on the question of origin of species** but good Heavens how little that is worth.*
[www.darwinproject.ac.uk/letter/entry-1938]

To summarise, from these examples, we can observe that, prior to the publication, Darwin has extensively discussed his theory without explicitly using the term *origin of species*. This was not necessarily a strategy for keeping his theory secret, but more of a product of the kind of knowledge he was looking for. This interpretation would explain the hotspot distribution starting near the time of the publication, despite the extensive indirect discussion of the notion. From a theoretical perspective, attributing the theory to his correspondent J.D. Hooker reaffirms the proposition of Darwin's theory being a product of a meaning negotiation process.

5.5.10 Range + species

Range + species is another collocation that has well-defined topic hotspots. As it can be observed from Figure 12 the discussion on the topic is generally well clustered in the first half of the correspondence but is almost completely omitted from later discussions.

Figure 12 Distribution of collocation (range + species)



The analysis of correspondence also confirms intertextual relations in topic hotspots. Specifically, the discussion on *ranges of species* is heavily concentrated in a few letters, sent between J.D. Hooker and Darwin.

Particularly interesting are the examples where the expansion of meaning through paraphrase can be observed. Looking in detail, we can observe this process in the following examples.

Firstly, Darwin queries Hooker on the hypothesis that large genera might have wide ranges, about which he has certain doubts, which are expressed later in the letter.

Letter 744: Darwin to Hooker, March 1844

- 1) *suppose a genus with ten or more species, inhabiting the ten main botanical regions, **should you expect that all or most of these ten species would have wide ranges** (ie were found in most parts of) in their respective countries.*
[<http://www.darwinproject.ac.uk/letter/entry-744>]

Hooker responds somewhat in agreement to this proposal, but also tones down its importance, expressing possible exceptions to such a proposal:

Letter 745: Hooker to Darwin, March 1844

- 2) ***On the whole I believe that many individual representative species of large genera have wide ranges, but I do not consider the fact as one of great value, because the proportion of such species having a wide range is not large compared with other representative species of the same genus whose limits are confined—& further because small genera have likewise individual widely extended representative species.***
[<http://www.darwinproject.ac.uk/letter/entry-745>]

In the same letter, Darwin provides an example of the genus of monkey, which, to his belief, demonstrates that the hypothesis is probably false:

- 3) *on the other hand no genus of monkey ranges over so large a part of the world & the individual species in their respective countries seldom range over wide spaces. I suspect, (but am not sure) that in the genus mus (the most mundane genus of all mammals) **the individual species have not wide ranges**, which is opposed to my query.—*
[<http://www.darwinproject.ac.uk/letter/entry-744>]
-

In the response, Hooker addresses several issues raised by Darwin. Firstly he agrees that Darwin's proposal is likely, but devalues the importance, or the generality, of the ranges of individual species stating:

Letter 745 Hooker to Darwin, March 1844:

- 4) *The converse holds true in a certain degree & in the Cacti we have a parallel case to Monkeys, their geographical range is small being confined to warm & chiefly to tropical S. America & Mexico I have somewhere read that **the species are remarkably peculiar to certain narrow limits.***
-

Thus Hooker expands on the proposals of Darwin, firstly by stating that the hypothesis cannot be generalised, and secondly by providing further evidence to support his claim. In the example above, we can also observe how Hooker paraphrases Darwin's '*the individual species have not wide ranges*' into '*the species are remarkably peculiar to certain narrow limits.*'

Finally, Darwin offers two possible scenarios in which genera might be divided:

- 5) *It is evident a genus might be widely diffused in two ways. 1st by **many different species, each with restricted ranges**, & 2d by **many or few species with wide ranges.**— Any light, which you cd throw on this I shd be very much obliged for.*
[www.darwinproject.ac.uk/letter/entry-744]
-

In response, Hooker expands the meaning of Darwin's hypothesis by proposing a third scenario:

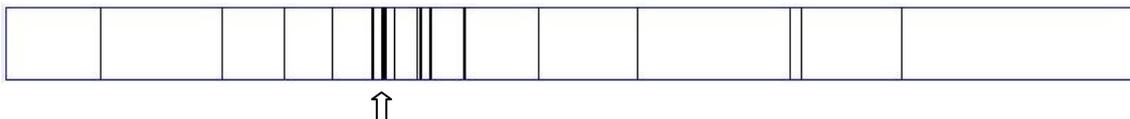
- 6) *..., I believe that most large mundane Genera contain both 1st many different species each with restricted limits, & 2nd also a large proportion of species with very wide ranges besides 3rd many local species with very narrow ranges: but it is not apparent that the proportional number of species distributed under any one of these conditions is larger in general than those distributed under either of the others. — I shall however bear the matter in mind & hope for new lights in time..*
 [www.darwinproject.ac.uk/letter/entry-745]

In the above examples Hooker demonstrates three strategies of meaning negotiation; firstly, he repeats Darwin’s first statement word for word, implying complete agreement in interpretation; secondly, he paraphrases Darwin’s second statement by removing the proposition of a few species with wide ranges determining a genus, thus expressing partial agreement; and finally, Hooker proposes a third alternative introducing new interpretation into the discussion.

5.5.11 Variable + species

Finally, the last collocation presented in this analysis also shows one or two topic hotspots in the first half of the plot. Although mostly concentrated around the hotspots, the idea expressed by this collocation is used sporadically in the correspondence, before it eventually disappears from the discourse.

Figure 13 Distribution of collocation (variable + species)



The following examples from the marked hotspots illustrate the process of meaning negotiation. Firstly, we can see Darwin clarifying his question, since he did not get the answer he expected, indicating that Gray did not understand the

difference between ‘genera with close species’ and ‘genera with variable species.’

Letter 2060 Darwin to Gray, March 1857:

- 1) *There is only one point, to which for myself, I wish to call your attention, viz **whether you rightly understood that my question did not refer to genera having very close species, but to **genera having very variable species.*****

[www.darwinproject.ac.uk/letter/entry-2060]

Similarly, it appears Darwin has posed the same question to Herbert Watson, who expresses uncertainty to what Darwin considers as ‘variable genera.’ Watson proceeds to express three alternative interpretations, each of which he supports with examples.

Letter 2063 Watson to Darwin, March 1857:

- 2) ***I am not quite sure of understanding your question about “variable genera”.** To explain my uncertainty, I will endeavour to define or state the differences for choice.*
 1. *Genera, of which the species are close, & difficult to distinguish by reason of their similarity;—but the species themselves not remarkably variable. Ex: Carex & Ranunculus (excluding Batrachium)*
 2. *Genera, of which the quasi species are so close that it becomes highly difficult to say whether the genus is composed of a comparatively few extremely variable species, or of many very close species. Ex: Rubus & Hieracium.*
 3. *Genera, the species of which are themselves so variable, & approximating, that it becomes difficult to say where one species ends & the next begins. Ex: Viola & Saxifraga, at least in certain sections or subgenera—**[www.darwinproject.ac.uk/letter/entry-2063]*
-

These examples show how the meaning of *variable genera*, can be misinterpreted, reinterpreted and redefined – essentially negotiated and constructed in discourse. Darwin, Gray and Watson, as participants of

discourse, determine the questions and the answers collaboratively. Thus the process of meaning construction is a process of interpretation and reinterpretation of words.

6 Discussion

6.1 Introduction

In this chapter the results of the analysis, presented in the previous chapter, will be compared to the theoretical notions discussed in the literature review and theoretical framework. The aim of this discussion is not solely to support the theoretical approach taken in this research, but also to contrast the results with alternative explanations.

This chapter is divided into sections which correspond to the analyses undertaken in the empirical chapter (Chapter 5 Analysis). Thus, the first section deals with the analysis of discursive, or contextual, meaning of the term *species* which is contrasted to the meaning of the term *varieties*. The aim of this section is to demonstrate that, despite the uncertainty expressed by Darwin and his peers, a clear difference in the meaning of these terms can be observed in their correspondence.

The following section discusses the analysis of the paraphrases of the term *species*. The analysis is discussed in relation to the question of the formulation of linguistic meaning in the act of paraphrase. Namely, based on the notion of paraphrase as evidence of interpretation (Teubert, 2010), this analysis observes how, in the acts of paraphrase, meanings are not just created, but perpetually reformulated in further paraphrases.

The third section discusses the realisation of intertextuality in discourse and the role of intertextual markers in the process of meaning negotiation. The analysis

of intertextual markers is used to support the argument that new definitions of meaning are essentially paraphrases since they are usually reactions to other interpretations.

The fourth section focuses on the diachronic dimension of meaning construction. The aim of this section is to focus on the evidence of diachronic change of meaning to argue against the propositions of structuralist and cognitive theorists that linguistic meaning is a product of universal, unconscious and innate structures of language.

Finally, the last section summarises all the findings in the analysis and relates them to different theories of meaning construction as well as different approaches in linguistics.

6.2 Discourse realisation of meaning

The first analysis presented in this thesis involved the analysis of collocations of the terms *species* and *varieties*. The collocation analyses have demonstrated that the meaning of the terms *species* and *varieties* can be distinguished by analysing the ways they are used in discourse. Furthermore, contrastive analysis of collocations showed clear differences in the meaning between these closely related terms.

Although throughout the thesis the focus was on the analysis of the term *species*, this analysis included a comparison with the term *varieties*, in order to demonstrate that the meanings of such closely related terms can be distinguished relatively easily using these methods. This was motivated by

many examples found in the correspondence where Darwin expresses the difficulty of distinguishing these terms.

- 1) *even supposing Botanists better agreed than they are likely to be in our day, as to **what (or which) are species & what are varieties**. Really, **they can neither define what species are, nor can they empirically agree in selecting species from varieties, or varieties from species**.—*
[www.darwinproject.ac.uk/letter/entry-1775]

In this task, the three-fold analysis of collocations of these terms proved particularly successful in distinguishing their meanings. Thus, the first analysis (5.2.1 Frequency collocations) provided us with some insight on the similarities of the meanings of these terms since 9 out of the 20 highest ranking collocates were shared by the two terms.

The second analysis (5.2.2 Mutual Information) demonstrated a more striking difference in the meanings of these terms by identifying a variety of term-specific collocations. This analysis identified collocations that were more exclusive to the search terms (nodes), which formed more specialised expressions thus providing a starker contrast between the meanings of the two terms.

Finally, the contrastive analysis (5.2.3 Contrasting Collocations) demonstrated several sets of semantically related collocations that distinctly differentiate the two terms. This analysis showed the advantages of combining interpretative and statistical approaches. Namely, based on the lists of collocations for each of the nodes, semantic groups of collocates were identified and compared for each of the search terms; for example terms denoting geographical ranges (see Section 5.2.3.1 Contrasting collocations: *wide* and). A similar approach has

been conducted by Baker *et al.* (2008) where they compared the collocations of *refugees*, *migrants*, and *asylum seekers* by grouping them into semantic categories. Thus the aim of both approaches was to identify semantic preferences (Stubbs, 2001) of these terms. For example, the collocates of *species*, denoting wide geographical ranges, such as *British*, *European*, *Northern*, etc., were tested how frequently they collocated with *varieties*. On the other hand, all the terms denoting narrow geographical ranges, which collocated with the node *varieties* were tested how frequently they collocated with the term *species*. Thus the results were contrasted by cross-referencing the raw frequencies of co-occurrence, which also allowed for the calculation of the 'zero' collocation, i.e. the absence of collocation.

A potential alternative approach to this analysis is to use the *SketchDiff* function in the Sketch Engine corpus access tool (Kilgarriff, Rychly, Smrz, & Tugwell, 2004) which compares the strongest collocations of the two compared nodes. The difference with the approach presented in the thesis is that the *SketchDiff* option does not contrast the terms in semantic groups, but rather focuses on specific grammatical relations (gramrels), which was not the aim of this analysis. Furthermore, by contrasting collocations in semantic groups, we observed tendencies which otherwise would not be noticed because of the individual low frequencies of semantically related terms. Thus, we can make comparisons of collocates which are not frequent or statistically strong enough to be observed in collocation analysis on their own, but as a group show quite distinct patterns.

Although the results of collocation analyses are foreseeable from the perspective of corpus linguistics, in the context of the study of meaning construction, they have provided important evidence to support the argument for discourse construction of meaning. Specifically, collocation analyses successfully distinguished specific meanings of the terms *species* and *varieties* in a discourse abundant with statements indicating the difficulties in differentiating the meanings of these terms. This indicates that the meaning of these terms can be attributed less to the physical properties of objects referred to as *species* and *varieties*, but more to how the terms are used in the discourse.

6.3 Paraphrase

The analysis of explicit definitions of the meaning of *species* aimed to demonstrate that the meaning of the term *species* is not based on innate concepts or physical properties of objects denoted as *species*, but rather that it is constructed in the discourse.

The argument against innate universal concepts is most evident from the multiple definitions of *species* expressed in the correspondence by different naturalists. In the example below, Darwin reports on the general disagreement among naturalists on how to define *species* and *varieties*.

- 1) *even supposing Botanists better agreed than they are likely to be in our day, as to what (or which) are species & what are varieties. Really, **they can neither define what species are, nor can they empirically agree in selecting species from***

varieties, or varieties from species.— Jordan would make a million species. J. D. Hooker would perhaps allow 50,000.
[www.darwinproject.ac.uk/letter/entry-1775]

The lack of agreement among experts on natural classification on how to define *species* and *varieties* certainly questions the argument that their concept of species is universal. Furthermore, from the example we can observe that naturalists referred to in the correspondence do not innately distinguish between *species* and *varieties* even when empirically analysing the physical properties of specimens. On the other hand, the examples from correspondence indicate that the definitions of *species* influence the interpretations of the empirical data; with different definitions emphasising different physical features and attributes:

2) *It is really laughable to see what different ideas are prominent in various naturalists minds, when they speak of “species” in some resemblance is everything & descent of little weight—in some resemblance seems to go for nothing & Creation the reigning idea—in some descent the key—in some sterility an unfailing test, with others not worth a farthing. It all comes, I believe, from trying to define the undefinable.*
[www.darwinproject.ac.uk/letter/entry-2022]

From these examples we can observe that the definitions of the terms *species* and *varieties* affect empirical decisions more than the physical properties of specimens affect the definitions of the terms. This supports the theoretical approach of linguistic meaning being the product of discourse as outlined in Section 2.2 Constructing meaning and reality.

The notion of discourse construction of meaning is based on Teubert's (2010) definition of the paraphrase as an act of interpretation. In the correspondence

corpus we can observe many examples of such definitions representing speakers' interpretations of the meaning of the term *species*. For example:

- 3) *After personally comparing the two, I cannot see how any sane man can infer that each was separately created. Yet they must be distinct species now, (**according to my interpretation or definition of that term**) for otherwise they would intercross & produce races having intermediate genital hooklets and(...)*
[www.darwinproject.ac.uk/letter/entry-6332a.txt]
-

The significance of the notion of paraphrase is not in that it represents different interpretations of meaning, but rather that it illustrates that interpretations are reactions to others' discourse. In other words, the notion of paraphrase is introduced to demonstrate the construction of meaning through negotiation in discourse. Examples 4 to 6 demonstrate how certain interpretations are diachronically developed and expanded. This development is realised almost as direct paraphrases of previous definitions, gradually expanding and refining the meaning of the term. For example, Darwin's initial formulation:

- 4) *I am almost convinced (quite contrary to opinion I started with) **that species are not** (it is like confessing a murder) **immutable**.* [www.darwinproject.ac.uk/letter/entry-729]
-

is refined into:

- 5) *The general conclusion at which I have slowly been driven from a directly opposite conviction is that **species are mutable** & that **allied species are co-descendants of common stocks**.*
[www.darwinproject.ac.uk/letter/entry-782]
-

and finally Darwin expands the meaning even further:

- 6) *From the nature of the grounds, which make me believe that **species are mutable in form, these grounds cannot be restricted to the closest-allied species;***
[www.darwinproject.ac.uk/letter/entry-793]
-

From these examples we can observe that each interpretation is realised as a paraphrase of the previous definition to which new interpretations have been added. Thus, the analysis has shown that the definitions of the meaning of the terms *species* and *natural selection* are not fixed objects of discourse, but rather constructs which are subject to reinterpretation and renegotiation. Therefore, definitions are not property of their authors, but rather propositions which are open for interpretation and reformulation by the discourse community. In other words, definitions of meaning are not systemic laws or natural facts, but rather statements that one may accept, ignore or argue against. In the example below, we can observe how Darwinist theory was rejected by the French scientific community.

7) *How strange it is that the country which gave birth to Buffon, the elder Geoffroy & especially to Lamarck shd **now cling so pertinaciously to the belief that species are immutable creations.***
[www.darwinproject.ac.uk/letter/entry-5794]

In this example, we can observe that without the acceptance of discourse community, particular meanings will not become part of that discourse. Thus, consensus is a prerequisite for meaning to become accepted as part of the discourse. This supports the notion that language is a social phenomenon in which linguistic meanings are constructed in relation to other discourses. Conversely, the lack of agreement on the meaning of the terms *species* and *varieties*, as well as the impact the definitions have on empirical approaches to their study, contradicts the assertion that meanings are synchronic, universal or innate.

6.4 Intertextual markers

Charles Darwin's correspondence as a corpus of epistolary texts is abundant with intertextual markers. These markers of intertextuality function as organisers of discourse but also as links to other texts in relation to which paraphrases are made.

In the process of meaning construction, in this case of the notion of *species*, intertextual markers serve as explicit links to other interpretations that the paraphrase may relate to. In the corpus of Charles Darwin's correspondence the following markers have been identified: (un)aware + preposition, (dis)agree + preposition, what + say, say + preposition, object or objection to, in regard or regarding, hear + preposition or determiner, according to, ask + preposition, state or mention that, and discussion, proposal, assertion.

Intertextuality markers show the stance and strategies people use when negotiating meaning. However, although they have operational functions such as expressing agreement or disagreement, invoking other texts as reference, positioning ones argument etc., the main purpose of these intertextual markers is to invoke other discourses in relation to which a particular a paraphrase is made. For example:

1) You **say that** all Botanists would agree that many tropical plants could not withstand a somewhat cooler climate. But I have come not to care at all for general beliefs without the special facts.
[www.darwinproject.ac.uk/letter/entry-5020]

2) All analogy makes me **quite disagree with the** Duke that the differences in the beak, wing & tail are not of importance to the

several species. In the only two species which I have watched, the difference in flight & in the use of the tail was conspicuously great. [www.darwinproject.ac.uk/letter/entry-4752]

Intertextuality markers illustrate the negotiation of meaning in the sense they make a reference to previous interpretations and introduce a reinterpretation. In other words, they explicitly demonstrate that interpretations are reactions to previous discourse.

Although, intertextual markers illustrate dialogic formulation, their analysis does not directly demonstrate the diachronic aspect of meaning development. As the term implies they connect different texts, with the latter forming a paraphrase of the former, but they do not capture the diachronic dimension beyond that.

Intertextuality markers are closely related to the notion of paraphrase in the sense defined by Teubert (2010); i.e. their function is to relate to other discourses when new interpretations are made.

To summarise, the aim of the analysis of the markers of intertextuality was to illustrate that meanings are not fixed but perpetually negotiated in discourse. Definitions of meaning are motivated as reactions to previous interpretations, which is realised through the use of explicit or implicit intertextual relations. If meaning was a product of a generative system as described by Chomsky (2006) then there would be no reason to form intertextual relations when making definitions of meaning. Thus the extensive use of explicit and implicit intertextuality supports the argument that meanings are products of negotiations in discourse.

6.5 Diachronic analysis

Diachronic analyses of meaning have been conducted before by other researchers (e.g. Kehoe, 2009; Cheung, 2009). However, like in Cheung's (2009) analysis, such analyses usually observed the processes of meaning negotiation as a successive set of reinterpretations. Although, this corresponds to diachronic development of meaning, such research is only possible in controlled environments. Whereas, in general discourse a plethora of topics and meanings are constantly being reinterpreted in an environment where meanings emerge and gradually disappear from discourse.

Charles Darwin's correspondence does not constitute a well-balanced corpus of social interactions, as it is not a complete account of Darwin's correspondence. As such, a detailed diachronic analysis of meaning negotiation is not possible due to the lack of the necessary data. Nevertheless, the diachronic analysis of meaning construction was still possible, albeit in a more general analysis.

The diachronic analysis presented in this thesis analyses how particular collocations are used diachronically in discourse. The focus of the analysis is not so much on how interpretations are being modified, since the corpus does not represent a complete account of meaning negotiation. Rather the analysis focuses on how and when particular meanings enter discourse, when they are discussed the most, and whether they stop being discussed in discourse.

The moments in discourse when particular notions are discussed most frequently are defined as 'topic hotspots'. As these hotspots indicate an

intensified use of a particular expression in discourse, they imply a temporal point where particular interpretations were intensely discussed.

The case of disjoined + species, discussed in Section 5.5.3 Disjoined + species, exemplifies the main goals of such diachronic analysis as it demonstrates clearly how one particular interpretation enters discourse, establishes clear intertextual relations and then abruptly disappears from the discussion. The examples like this one correspond to Dawkins' (2006) meme theory demonstrating that particular interpretations have a life of their own, which depend on their repetition in discourse.

Furthermore, the case of mutable + species, discussed in Section 5.5.2 Mutable + species illustrates the purpose of identifying topic hotspots. Specifically, the two hotspots identified in the early stages of correspondence represent key moments of reformulation of the term *species*. It is in these hotspots where the interpretation of '*immutability of species*' is replaced with the notion that *species* are indeed *mutable*.

These examples illustrate the diachronic nature of meaning. Thus meaning is not a fixed property of language; rather it is perpetually renegotiated in discourse. Different interpretations are constantly being proposed causing a gradual change in meaning. Thus this analysis provides a strong argument against the proposition of synchronic interpretation of meaning and language.

6.6 Summary

The analyses presented in this thesis have been based on the theoretical formulation of discourse as an autopoietic system, where meanings are formulated in relation to one another. The analyses of collocation, paraphrase and intertextuality have all presented strong evidence that meaning is formulated through negotiation in discourse, thus supporting the theories of dialogism (Bakhtin & Holquist, 1981), intertextuality (Kristeva & Moi, 1986), formulation of meaning in discourse (Foucault, et al., 1972; Sinclair & Carter, 2004), and definition of paraphrase as an act of interpretation (Teubert, 2005, 2007, 2010).

Considering the examples of co-existence of multiple interpretations of the notion of *species*, with evidence of intertextual relationships between different definitions and diachronic change of meaning, there is little evidence to support any conceptual or structural theories outlined in the literature review. Chomsky's (2006) argument of generative grammar is contradicted by the high frequency of intertextual relations when making new definitions of the term *species*. The vast number of different interpretations of *species* contradicts the innate and universal structures proposed by structural (Johnson, 2003; Saussure, et al., 1983) and cognitive theorists (Fodor, 1998; Pinker, 1994). Finally, the evidence of the diachronic change of meaning contradicts the proposals of synchronic language systems (Chomsky, 2006; Saussure, et al., 1983).

7 Conclusion

7.1 Summary

This thesis has demonstrated that linguistic meaning has both dialogic and diachronic dimensions. Specifically, the research analysis confirmed the theoretical position that meanings are constructs of discourse and are as such contingent and negotiable. This stance is in contrast to the definition of language as a psychological phenomenon (see Section 2.1.1 Language as a psychological phenomenon), which argues that meanings are generally either innate or unconscious elements of language. Corpus linguists have countered these claims for decades by demonstrating all the meaningful patterns that can be deduced from the observation of linguistic data. However, the aim of this thesis was also to demonstrate that meanings are products of discourse and as such have a diachronic dimension. This was achieved by a corpus analysis investigating the intertextual links and evidence of meaning negotiation. In particular, the methods applied a diachronic approach to the analyses of collocation, paraphrase, and intertextuality, which demonstrated that meanings in language are contingent constructs subject to ongoing negotiation. Furthermore, the thesis has succeeded in answering all the research questions presented in the introductory chapter; namely:

1. **Is there any evidence to suggest that the meaning of the term *species* is a product of a generative language system and thus independent from discourse?**

The analyses of paraphrase and intertextual markers have demonstrated that the definitions of the term *species* are formed in relation to previous discourse. As the definitions of *species* illustrate strong intertextual relationships, it is unlikely that they are a product of a discourse-independent generative language system.

2. Is there any evidence to suggest that the meaning of the term *species* is a product of negotiation in discourse?

Intertextual relations between different definitions of the term *species*, as well as the evidence of paraphrasing previous interpretations, indicate that the meaning of *species* is formulated in relation to previous definitions of the term. Furthermore, there is evidence that some interpretations are not further paraphrased and expanded, but on occasion ignored by discourse communities. This indicates that meanings are products of social negotiation and depend on acceptance by the discourse community.

3. Is there any evidence to suggest that the meaning of the term *species* is either synchronic or diachronic? In other words, does the meaning of the term *species* change over time?

As meanings are subject to negotiation in discourse, they constantly change and are never fixed. This is evident in analyses of paraphrases and intertextual relations which indicate that meanings are constantly reinterpreted. Furthermore, diachronic analysis illustrates how particular

interpretations appear and disappear from discourse. Similarly, topic hotspots help to identify where the meanings are most intensively discussed, thus indicating at what point in discourse the reformulation of meaning takes place.

7.2 Empirical findings

7.2.1 Collocation analysis

The first analysis focused on identifying the most common interpretations of the word *species* through the analysis of collocations. The main purpose of this study was to identify interpretations for the use in diachronic analysis. The secondary goal of the analysis was to contribute to the debate on the meaning of the word *species*, particularly in relation to the closely related notion of *varieties*.

The analysis of collocation using t-score and MI-score statistical measures identified the most frequent and the strongest collocates of the terms *species* and *varieties*. These results identified certain distinctive patterns which differentiate the meanings of these terms, but more importantly uncovered the key information for the subsequent contrastive and diachronic analyses.

The application of these methods is a standard procedure in corpus linguistics and as such does not offer a methodological contribution to the scientific debate. Similarly, from a theoretical perspective, the analysis merely confirmed that the meaning of a word can be observed in the discourse, but without offering any new insights on the process of meaning construction. Nevertheless,

as mentioned above, the main goal of this analysis was to uncover the key interpretations of the terms *species* and *varieties*, which were subsequently used in contrastive and diachronic analyses.

7.2.2 Contrastive collocation analysis

The idea for the contrastive collocation analysis came from the method in which t-score and MI-score collocation analyses were conducted. Namely, for both analyses the minimum number of collocates was set to at least five instances. In this manner, the collocation analysis identified only those linguistic items that co-occurred with the search term at least five times in the corpus.

To contrast the meaning of the terms *species* and *varieties*, their collocates were grouped based on their meanings, forming semantic collocation groups. Thus collocates of each term belonging to the same semantic group were compared. This approach has some similarities with the SketchDiff function in Sketch Engine corpus access tool (Kilgarriff, et al., 2004), but the difference between the two is that SketchDiff is calculated taking into account the grammatical relations between the two terms, whereas this analysis focused on the terms' semantic preferences. This showed a clearer contrast in the way the terms *species* and *varieties* were used. For example, the word *species* was more frequently often used to describe organisms with wide natural habitats, collocating with words such as *British*, *European*, *American* etc. On the other hand, the word *varieties* collocated more frequently with terms denoting narrow habitats, e.g. *local* and *garden*.

By grouping the collocates this analysis has also demonstrated that words of similar meaning tend to collocate with the same nodes, although this finding is expected, especially when proposing that meaning is a product of paraphrasing.

7.2.3 Paraphrase analysis

The aim of this analysis was to demonstrate that the act of the explanation of meaning is essentially an act of negotiation of meaning in which new interpretations are constructed in relation to the previous ones. Furthermore, the aim was also to demonstrate that the meaning of the word *species* depends more on the social consensus than the natural and physical properties of the referent.

The results of this analysis show that in the discourse of Charles Darwin's correspondence many different interpretations of the meaning of the term *species* exist. The multitude of different interpretations of the meaning of *species* indicates that the meaning of the term is not based either on the physical properties of *species*, or some innate mental conception. Namely, if either of these proposals were accurate we would expect more unified interpretations of the meaning of *species*.

The key observation of this analysis is that the interpretations of the meaning gradually expand by adding on new information to previous interpretations. This is clearly shown in Darwin's initially hesitant formulations of the mutability of *species*. This indicates that the meaning is product of a gradual addition of interpretations realised in the acts of paraphrase.

7.2.4 Intertextuality markers

The analysis of intertextuality markers has been implemented to observe the process of meaning negotiation. However, the issue with analysing intertextuality markers for the purpose of analysing meaning construction was that they could not be analysed on their own in the whole correspondence corpus since they could refer to virtually any point in discourse. Thus the analysis had to be performed on a sub-corpus made of only the paragraphs containing the word *species*. In that way, only the markers of intertextuality in the immediate context of the word *species* have been observed. The analysis observed ten intertextual markers which demonstrated that meanings are discussed, negotiated and constructed in discourse.

Having observed the use of intertextual markers in the context the term *species* was discussed, the analysis has identified a set of functions that these markers performed in the discourse. The most common function intertextual markers are used for is simply, as their name suggests, to create an intertextual link between two texts, or in other words to incorporate a statement made by others into the text. However, intertextual markers also perform more complex functions:

- draw attention to a statement from another text or to make the author's statement appear as a reference to another text, with the aim of creating an illusion of objectivity. (See Section 5.4.3 Markers: *aware*, *unaware*)
- evaluate a statement from another text, without necessarily expanding or reducing its meaning. (See section 5.4.4 Markers: *agree*, *disagree*)
- to elicit information from the addressee (See Section 5.4.8 Markers: *hear*)

- invoke a text which can be used to act as a reference to support the speakers statement. (See Section 5.4.9 Markers: *according to*)
- restate the questions to which the author is providing the answer to in order to provide the context for the response (See section 5.4.10 Markers: *ask*)
- to refer to previous discourse (See Sections 5.4.5 Markers: *say that, say about*)
- to relate disagreement to previous discourse (See Section 5.4.6 Markers: *object to, objection*)

7.2.5 Diachronic analysis

Diachronic analysis presented in this thesis focused on identifying the prominence certain interpretations gain at particular moments in time. In other words, the analysis focused on uncovering how and when particular meanings entered discourse, when they were discussed the most, whether and when they stopped being discussed in discourse. The aim of this approach was to demonstrate the diachronic, or rather, contingent nature of linguistic meaning. This was achieved through the identification of time spans in which particular interpretations occurred, which allowed for the application of diachronic analysis to a more general diachronic corpus.

Apart from observing the “life-spans” of selected interpretations, diachronic analysis was also applied to detect the moments when particular interpretations were discussed most frequently. These periods of intensified use of a particular collocation were labelled as ‘*topic hotspots*’ and were identified using dispersion plots. The main application of these topic hotspots was to identify moments in

discourse when a particular notion is discussed most frequently. The major benefit of this approach is that it allows the identification of potential intertextual paraphrases. This is best demonstrated by the examples of *close species* (see Section 5.5.8) and *variable species* (see Section 5.5.11 Variable + species), where a degree of intertextual negotiation of meaning was necessary firstly to convey the desired meanings and secondly to develop them further.

To conclude the diachronic analysis presented in this thesis successfully observed how certain interpretations enter and leave discourse. However, the process of meaning construction by negotiation has not been thoroughly observed. One of the main reasons for the smaller number of examples of meaning construction by paraphrase is attributed to the corpus. Namely, it is rather the structure than the size of the corpus that caused the problem with the analysis of meaning negotiation, since the corpus does not include the complete correspondence of Charles Darwin. Furthermore, unlike the corpora based on forums or other interactive discussion boards, the written correspondence of Charles Darwin will not represent the whole account of the debate on the meaning of *species*, much of which must have been conducted verbally. Thus, the corpus does not represent a full account of the debate.

7.3 Theoretical Contributions

The research presented in this analysis supports the formulation of language as a social phenomenon (See section 2.1.2 Language as a social phenomenon) characterised by the theories of dialogism (Bakhtin & Holquist, 1981; Bakhtin, et

al., 1994) and intertextuality (Barthes, 1968; Barthes & Heath, 1977; Kristeva & Moi, 1986).

The negotiation of meaning in discourse has been demonstrated by several analyses, in particular by the analysis of intertextual markers (Section 5.4 Intertextuality) and the diachronic analysis of collocation (Section 5.5 Diachronic analysis of intertextuality). The evidence presented strongly argues for the notions of social construction and negotiation of meaning and supports the notion of paraphrase as an act of interpretation through which meanings are constructed, as proposed by Teubert (2005, 2007, 2010).

Furthermore, by successfully demonstrating the formulation of meaning by solely referring to discourse element, this thesis also supports the argument that all meanings in discourse are products of the discourse itself (Foucault, et al., 1972; Maturana & Varela, 1980). As mentioned previously, diachronic analysis has also provided empirical evidence to reinforce Dawkins' (2006) meme theory, by demonstrating that fecundity, that is replication, is a key aspect of meaning construction. This is especially evident in the example of *disjoined species* (Section 5.5.3 Disjoined + species).

To conclude, although the thesis has not provided a new theoretical framework to explain the processes of meaning construction, it has provided empirical evidence to support the diachronic study of discourse as a valid approach to the study of linguistic meaning.

7.4 Methodological contributions

The key contributions presented in this thesis relate to the methodological approaches to the study of meaning. These include a method for analysing intertextual markers that is suitable for application in more general corpora; and a method for diachronic analysis of collocation. In addition to these, a variant of collocation analysis, labelled 'contrastive analysis' has also been proposed.

The contrastive analysis of collocations is not a method designed to study the dialogic or diachronic aspects of meaning. Rather its purpose is, as the name suggests, to conduct contrastive analyses of the meanings of closely related terms. The rationale for such analysis is that by semantically grouping collocates of each search term, semantic preference of each term will be easier to distinguish. Then by contrasting the collocates of one node with the other, and vice-versa, the differences can be determined. In that sense, the first search node is tested against the collocates of the second, testing both their similarities and differences. Although I was not aware of their research at the time of conducting this analysis, Baker *et al.* (2008) used a very similar approach to contrast the meanings of *refugee*, *asylum seeker*, *migrant* and *immigrants*. The advantage of such approach is in that it can be carried out on any type of corpus without using any advanced computational methods and tools to contrast the meaning of terms. On the other hand, a more advanced corpus tools like the Sketch Engine (Kilgarriff, et al., 2004) can perform a similar analysis automatically, with the added advantage of observing grammatical relations between the node and the collocate in addition to the difference in semantic preference.

The study of intertextual markers offers a versatile approach to the study of meaning construction. The method stems from the problem of analysing intertextual markers in written correspondence, where the analysis would result with too many instances referring to a whole plethora of topics. Thus it was necessary to limit the scope which these markers could refer to by relating them to a particular search term, namely the word *species*. Since making the relation on the sentence level risked losing a significant number of intertextual references made in the wider context, the markers were analysed on the paragraph level, which linked the intertextual references with the wider context. By observing the wider context it was possible to observe more intertextual relations and hence demonstrate the construction of meaning through the process of paraphrase.

Finally, the contributions to the diachronic analysis are based on the applicability in larger and more general corpora, rather than just in structured discourse. This approach illustrates when particular interpretations enter and are abandoned in discourse. Namely, by observing dispersion plots it can be determined exactly when a particular term or a phrase first appears in the discourse, when it is most frequently used, and finally when it is abandoned in the debate. The first and last occurrence in the discourse marks the so-called 'life-span' of the search term in the corpus. This may be used to indicate whether particular meanings are accepted or rejected from the debate. On the other hand, the high concentration of the use of the search term at a particular point in discourse may indicate the so-called 'topic hotspot' which may imply a point of intensive negotiation of meaning. The concept of 'topic hotspots' is

similar to the notion of 'heat maps' proposed by Kehoe and Gee (2009). Although both concepts relate to the occurrence of collocations in a diachronic discourse with the purpose of analysing the meaning, they are applied somewhat differently. Namely, Kehoe and Gee (ibid) applied the method to a large, general diachronic corpus to observe when particular collocations first appeared in the discourse, whereas the aim of this thesis was to observe the process of paraphrasing specific collocations. The results of both studies have shown that this method is suitable when dealing with large amounts of data which cannot be analysed as a whole in detail. Thus by identifying topic hotspots, the analysis can focus on particular parts of discourse. This was particularly suitable for this research where the corpus consisted of more than one million tokens, divided into more than two thousand letters all of which mentioning the term *species*. However, one possible drawback is that by focussing on topic hotspots certain important interpretations will be left out of the analysis. For this reason, this approach is best suited for large corpora where the amount of relevant information will be significant enough to drop out such interpretations from the analysis.

7.5 Limitations of the study

The main limitations of this study relate to the data used in the analysis. Although a corpus of correspondence would generally be an ideal corpus for a diachronic analysis with chronologically ordered data, the specific problem with the Charles Darwin Correspondence Corpus is that it does not represent the

complete account of the correspondence. In other words, the corpus does not include the full correspondence necessary for the analysis of meaning negotiation. Furthermore, the size of the corpus also presents a challenge with this type of analysis, with the abundance of data making it difficult to find the best examples and to present an exhaustive list to the reader. Due the limitations of the corpus, the main aspect of the analysis, i.e. the diachronic analysis of meaning construction, did not provide as extensive evidence as the methodology would otherwise allow. This has been demonstrated by the fact that neither the analysis of intertextual markers, nor the analysis of 'topic hotspots' always identified clear examples of meaning negotiation.

Another limitation in the study is that intertextual markers, collocations and paraphrases cannot exhaustively detect all the intertextual relations and interpretations which form meaning. The same applies for the corpus, since no corpus can include all the intertextual references in relation to which interpretations of meaning are made.

From a methodological perspective, the problem with the analysis of intertextual markers, at the level of paragraphs, made the process of identifying relevant example quite laborious due the frequency of both the markers and search terms.

One of the problems in this analysis was how to make the identification of topic hotspots more objective. Namely, higher the number of occurrences, harder it is to delimit what is relevant without reading all the instances. And as it was mentioned above, the size of the corpus makes that very labour intensive.

Finally, to conclude, due to the abstract nature of the notion of intertextuality, the incompleteness of the correspondence and the size of the corpus, this analysis provided more an answer on how to analyse intertextuality in text, than an in-depth account of the notion of intertextual construction of meaning.

7.6 Potential for further research

The diachronic analysis of meaning has a potential for widespread application, most notably in critical and sociolinguistic analyses. Observing the process of meaning construction in a field such as Critical Discourse Analysis (CDA) will allow for a diachronic analysis of the formulation of ideologies, or their influences on the construction of social reality. This can be realised by an in-depth analysis of particular collocations focusing on when particular interpretations enter discourse, how they are introduced into the discourse, and how they are further negotiated and established by discourse communities.

Due to the limitations of this study, further research can be conducted using data that represent a more detailed account of meaning negotiations, such as, for example, transcriptions of public debates, discussion panels, online forums or chat rooms etc. Such data would allow a more thorough observation of meaning negotiation and hence a more comprehensive understanding into the processes of meaning construction.

The method used for diachronic analysis can also be used to track the development of particular topics within texts. Theoretically, the diachronic analysis of collocation could also be used to create a topic profile of a text.

Using this approach one can observe selected patterns for within-text distribution. Alternatively, this could be applied to study the use of meta-discourse markers in specific genre texts, in order to observe in which parts of text they are more likely to occur in.

Additionally, to address the problems of analysing very frequent collocations in a large corpus, which would make distinguishing between topics virtually impossible, further research could focus on developing a software tool or a method that would automatically determine the strength of a hotspot. This could be achieved with a method that focuses on observing which instances cluster closer together and would have to take into account both the collocational patterns, or contexts, as well as the 'distances' between each instance of collocation.

Finally, the arguments for further research into the construction of meaning lie in the potential for wide application of such developments, particularly in the fields of lexicography, semantics, natural language processing and other areas focusing on the construction, realisation or interpretation of meaning.

8 Appendices

Appendix 1 – List of collocates of the term *species*

| n>10 | MI score | Collocate | n>10 | T-score | Collocate |
|------|----------|----------------|------|----------|-----------|
| 15 | 7.42197 | mutability | 3701 | 57.67664 | of |
| 10 | 7.3845 | disjoined | 3628 | 55.69504 | the |
| 22 | 7.26825 | immutability | 1414 | 34.73792 | in |
| 46 | 7.19911 | representative | 1123 | 30.71215 | a |
| 10 | 6.91501 | compos | 1196 | 30.34304 | to |
| 10 | 6.83701 | undescribed | 1112 | 28.18013 | i |
| 10 | 6.83701 | derivation | 711 | 25.4436 | are |
| 10 | 6.76301 | immutable | 717 | 24.24696 | that |
| 13 | 6.65609 | quasi | 597 | 22.37535 | as |
| 107 | 6.62183 | allied | 585 | 21.70249 | is |
| 18 | 6.54062 | inhabiting | 515 | 21.45292 | or |
| 60 | 6.49998 | identical | 580 | 21.41236 | have |
| 12 | 6.48999 | trimorphic | 516 | 20.9557 | on |
| 254 | 6.37883 | origin | 451 | 19.62806 | which |
| 14 | 6.32244 | endemic | 384 | 18.5728 | one |
| 188 | 6.28693 | distinct | 409 | 18.35509 | with |
| 12 | 6.17805 | presenting | 387 | 17.67639 | be |
| 28 | 6.1441 | ranges | 313 | 17.22498 | same |
| 11 | 6.09316 | describing | 376 | 17.09967 | not |
| 284 | 6.02554 | genera | 284 | 16.5936 | genera |
| 221 | 6.01758 | genus | 350 | 16.5726 | and |
| 30 | 5.99748 | created | 325 | 16.47004 | from |
| 49 | 5.9854 | variable | 266 | 15.77772 | two |
| 40 | 5.97641 | dimorphic | 317 | 15.75148 | but |
| 36 | 5.95566 | modification | 254 | 15.74586 | origin |
| 10 | 5.91501 | defined | 281 | 15.74253 | some |
| 21 | 5.9074 | limits | 302 | 15.69041 | this |
| 59 | 5.90419 | closely | 278 | 15.40379 | all |

| | | |
|-----|---------|-------------|
| 28 | 5.88386 | average |
| 16 | 5.88037 | mundane |
| 66 | 5.82441 | close |
| 20 | 5.76301 | indigenous |
| 18 | 5.72348 | belonging |
| 199 | 5.71664 | varieties |
| 18 | 5.70412 | extinct |
| 25 | 5.69262 | belong |
| 16 | 5.63373 | oxalis |
| 10 | 5.62551 | fewer |
| 18 | 5.61101 | varying |
| 45 | 5.60381 | individuals |
| 31 | 5.59827 | existing |
| 41 | 5.597 | recent |
| 23 | 5.58613 | sub |
| 48 | 5.56661 | described |
| 12 | 5.56661 | descendants |
| 30 | 5.56138 | widely |
| 10 | 5.56138 | definition |
| 56 | 5.49355 | european |
| 15 | 5.48008 | typical |
| 29 | 5.47124 | wide |
| 10 | 5.47023 | ramme |
| 32 | 5.45923 | vary |
| 57 | 5.44615 | peculiar |
| 13 | 5.44108 | foreign |
| 22 | 5.41509 | vars |
| 28 | 5.41049 | local |
| 17 | 5.40753 | named |
| 14 | 5.40044 | describe |
| 39 | 5.37599 | individual |
| 166 | 5.3115 | different |
| 35 | 5.30733 | fossil |

| | | |
|------|----------|-----------|
| 358 | 15.29703 | you |
| 281 | 15.18585 | by |
| 320 | 14.74577 | it |
| 221 | 14.6366 | genus |
| 230 | 14.43327 | other |
| 254 | 14.3036 | very |
| 5625 | 14.24641 | species |
| 219 | 14.17306 | many |
| 224 | 14.03739 | there |
| 199 | 13.83848 | varieties |
| 188 | 13.53571 | distinct |
| 249 | 13.13391 | for |
| 174 | 12.74488 | new |
| 190 | 12.66988 | more |
| 189 | 12.66276 | any |
| 166 | 12.55966 | different |
| 198 | 12.39288 | so |
| 163 | 12.00992 | only |
| 214 | 11.85877 | my |
| 173 | 11.84497 | has |
| 161 | 11.23477 | if |
| 152 | 11.18136 | they |
| 169 | 11.04027 | at |
| 154 | 11.03522 | been |
| 142 | 11.00657 | than |
| 168 | 10.93945 | will |
| 135 | 10.86693 | these |
| 141 | 10.78336 | about |
| 123 | 10.73065 | common |
| 121 | 10.67189 | each |
| 135 | 10.60899 | most |
| 168 | 10.5413 | your |
| 117 | 10.52554 | number |

| | | |
|-----|---------|---------------|
| 14 | 5.28496 | authors |
| 10 | 5.27758 | pairs |
| 14 | 5.26658 | localities |
| 37 | 5.25822 | range |
| 313 | 5.24406 | same |
| 27 | 5.24177 | orders |
| 10 | 5.22696 | included |
| 25 | 5.21704 | temperate |
| 117 | 5.21552 | number |
| 45 | 5.1834 | group |
| 10 | 5.17805 | upwards |
| 28 | 5.17805 | differ |
| 14 | 5.17805 | descended |
| 24 | 5.17805 | botanists |
| 10 | 5.1542 | represented |
| 25 | 5.14948 | proportion |
| 16 | 5.1483 | distinguished |
| 29 | 5.1132 | rare |
| 37 | 5.10828 | numerous |
| 19 | 5.10405 | families |
| 11 | 5.07269 | held |
| 121 | 5.06716 | each |
| 22 | 5.06257 | countries |
| 31 | 5.03773 | groups |
| 11 | 5.03262 | tend |
| 110 | 5.03064 | several |
| 12 | 5.02604 | increase |
| 44 | 4.98882 | british |
| 16 | 4.98165 | third |
| 10 | 4.95566 | admitted |
| 123 | 4.94564 | common |
| 266 | 4.93871 | two |
| 10 | 4.93519 | domesticated |

| | | |
|-----|----------|---------|
| 107 | 10.23905 | allied |
| 110 | 10.16722 | several |
| 132 | 10.14599 | would |
| 113 | 10.12128 | found |
| 113 | 10.11695 | into |
| 99 | 9.62137 | another |
| 114 | 9.56466 | what |
| 120 | 9.31631 | he |
| 101 | 9.22029 | were |
| 109 | 9.21226 | no |
| 94 | 9.09328 | few |
| 97 | 9.00188 | out |
| 110 | 8.93293 | do |
| 112 | 8.91085 | s |
| 86 | 8.90031 | large |
| 93 | 8.87842 | being |
| 103 | 8.87334 | c |
| 95 | 8.86544 | believe |
| 88 | 8.72028 | whether |
| 101 | 8.68846 | an |
| 86 | 8.64813 | our |
| 135 | 8.64256 | me |
| 91 | 8.60606 | their |
| 88 | 8.4911 | plants |
| 105 | 8.47369 | am |
| 93 | 8.46263 | may |
| 83 | 8.39794 | work |
| 77 | 8.25977 | having |
| 82 | 8.1652 | such |
| 81 | 8.14219 | good |
| 73 | 8.10325 | forms |
| 71 | 8.02677 | between |
| 66 | 7.98067 | close |

| | | |
|-----|---------|----------|
| 14 | 4.92651 | contains |
| 99 | 4.92069 | another |
| 37 | 4.89031 | marked |
| 174 | 4.88628 | new |
| 47 | 4.88297 | change |
| 15 | 4.86255 | isld |

| | | |
|----|---------|-----------|
| 85 | 7.8244 | think |
| 80 | 7.81604 | when |
| 66 | 7.72397 | both |
| 73 | 7.68783 | its |
| 60 | 7.66038 | identical |
| 79 | 7.61611 | can |

Appendix 2 – List of collocates of the term *varieties*

| n>5 | MI score | Collocate |
|-----|----------|---------------|
| 7 | 8.47206 | quasi |
| 5 | 8.47206 | maize |
| 7 | 8.27942 | verbascum |
| 8 | 8.15014 | abnormal |
| 16 | 7.78556 | cultivated |
| 5 | 7.62407 | presenting |
| 11 | 7.48855 | fertility |
| 18 | 7.48212 | local |
| 5 | 7.43644 | station |
| 6 | 7.41317 | wheat |
| 7 | 7.39678 | modifications |
| 5 | 7.36773 | babington |
| 34 | 7.33071 | domestic |
| 28 | 7.19727 | marked |
| 20 | 7.17194 | intermediate |
| 7 | 7.17089 | peas |
| 6 | 7.01263 | differently |
| 16 | 6.98021 | crossed |
| 18 | 6.91407 | crossing |
| 5 | 6.7496 | normal |
| 10 | 6.73871 | sterile |
| 6 | 6.7351 | etc |
| 7 | 6.66471 | distinguished |
| 7 | 6.59292 | fowls |
| 7 | 6.57898 | sub |
| 9 | 6.47206 | botanists |
| 5 | 6.47206 | attempt |
| 14 | 6.41523 | numerous |
| 5 | 6.40168 | descended |
| 5 | 6.19408 | naturally |

| n>5 | T-score | Collocate |
|-----|----------|-----------|
| 553 | 22.26607 | of |
| 495 | 20.36983 | the |
| 199 | 13.83848 | species |
| 206 | 12.79023 | to |
| 174 | 11.94177 | in |
| 125 | 10.79521 | or |
| 163 | 10.70342 | i |
| 114 | 10.21076 | are |
| 103 | 9.17964 | have |
| 82 | 8.47842 | which |
| 81 | 8.32108 | and |
| 82 | 8.20607 | as |
| 86 | 8.15648 | that |
| 85 | 7.66364 | a |
| 67 | 7.43843 | on |
| 66 | 7.41264 | with |
| 65 | 7.31746 | be |
| 65 | 7.21957 | not |
| 56 | 7.05073 | all |
| 50 | 6.83082 | other |
| 53 | 6.6902 | from |
| 44 | 6.23884 | some |
| 41 | 6.19597 | two |
| 45 | 6.10547 | by |
| 47 | 6.04026 | but |
| 37 | 5.87512 | same |
| 51 | 5.8547 | is |
| 34 | 5.79473 | domestic |
| 35 | 5.67676 | many |
| 34 | 5.60179 | these |

| | | |
|-----|---------|------------|
| 14 | 6.17089 | call |
| 5 | 6.15014 | grown |
| 5 | 6.07975 | six |
| 10 | 6.06607 | garden |
| 24 | 6.02636 | distinct |
| 5 | 6.01263 | fowl |
| 7 | 5.9848 | tendency |
| 8 | 5.97222 | strongly |
| 11 | 5.89258 | races |
| 6 | 5.85735 | area |
| 13 | 5.85509 | together |
| 29 | 5.81632 | between |
| 6 | 5.8091 | series |
| 199 | 5.71664 | species |
| 7 | 5.70199 | pigeons |
| 5 | 5.67505 | kinds |
| 5 | 5.64425 | growing |
| 5 | 5.60417 | yellow |
| 5 | 5.59432 | run |
| 29 | 5.5035 | different |
| 8 | 5.48907 | close |
| 8 | 5.47206 | cross |
| 5 | 5.40168 | variable |
| 5 | 5.39311 | v |
| 5 | 5.34278 | probable |
| 27 | 5.33973 | genera |
| 9 | 5.28884 | why |
| 5 | 5.2862 | rare |
| 7 | 5.27942 | mere |
| 5 | 5.26261 | themselves |
| 844 | 5.2485 | varieties |
| 6 | 5.22414 | consider |
| 6 | 5.21154 | produce |

| | | |
|-----|---------|--------------|
| 50 | 5.58814 | you |
| 33 | 5.34748 | any |
| 29 | 5.2896 | between |
| 29 | 5.26646 | different |
| 28 | 5.25545 | marked |
| 32 | 5.24167 | more |
| 30 | 5.14958 | most |
| 27 | 5.06784 | genera |
| 29 | 5.01753 | c |
| 27 | 4.97896 | being |
| 27 | 4.95051 | plants |
| 26 | 4.9354 | into |
| 31 | 4.91908 | so |
| 30 | 4.91744 | one |
| 28 | 4.9171 | about |
| 27 | 4.91162 | only |
| 29 | 4.86131 | if |
| 31 | 4.85257 | very |
| 27 | 4.84658 | what |
| 26 | 4.83199 | their |
| 24 | 4.82382 | distinct |
| 38 | 4.76969 | it |
| 27 | 4.69416 | been |
| 25 | 4.5673 | they |
| 844 | 4.56703 | varieties |
| 32 | 4.52815 | for |
| 21 | 4.4569 | forms |
| 23 | 4.44336 | them |
| 20 | 4.44112 | intermediate |
| 22 | 4.33693 | than |
| 27 | 4.33294 | this |
| 18 | 4.21891 | local |
| 18 | 4.20746 | crossing |

| | | |
|-----|---------|-----------|
| 21 | 5.18842 | forms |
| 5 | 5.16464 | names |
| 14 | 5.14499 | animals |
| 5 | 5.13578 | supposed |
| 18 | 5.12826 | several |
| 11 | 5.12414 | white |
| 6 | 5.07403 | mention |
| 12 | 5.06834 | three |
| 8 | 5.01263 | produced |
| 6 | 5.00174 | making |
| 10 | 4.99309 | known |
| 26 | 4.96183 | into |
| 41 | 4.95003 | two |
| 17 | 4.94482 | each |
| 16 | 4.91748 | large |
| 15 | 4.91543 | both |
| 5 | 4.89313 | set |
| 50 | 4.87931 | other |
| 13 | 4.87559 | thus |
| 37 | 4.87255 | same |
| 6 | 4.86227 | greater |
| 125 | 4.85948 | or |
| 10 | 4.84271 | true |
| 8 | 4.75097 | called |
| 6 | 4.72164 | near |
| 5 | 4.71718 | european |
| 8 | 4.70058 | generally |
| 5 | 4.6907 | red |
| 34 | 4.66926 | these |
| 12 | 4.63917 | number |
| 35 | 4.62765 | many |
| 6 | 4.58942 | among |
| 27 | 4.58039 | being |

| | | |
|----|---------|------------|
| 20 | 4.20073 | how |
| 22 | 4.15861 | do |
| 22 | 4.12949 | has |
| 18 | 4.12134 | several |
| 19 | 4.07608 | such |
| 18 | 4.03057 | new |
| 18 | 4.01912 | also |
| 17 | 3.98923 | each |
| 19 | 3.98289 | we |
| 16 | 3.98187 | cultivated |
| 16 | 3.96832 | crossed |
| 23 | 3.96009 | will |
| 20 | 3.94444 | would |
| 17 | 3.90796 | our |
| 16 | 3.86764 | large |
| 17 | 3.85178 | its |
| 18 | 3.78049 | no |
| 16 | 3.74672 | well |
| 15 | 3.74465 | both |
| 14 | 3.69782 | numerous |
| 14 | 3.68972 | call |
| 16 | 3.68125 | were |
| 14 | 3.63591 | animals |
| 16 | 3.61419 | when |
| 16 | 3.61384 | now |
| 17 | 3.60726 | there |
| 14 | 3.57852 | common |
| 13 | 3.54326 | together |
| 14 | 3.53242 | those |
| 13 | 3.48273 | thus |
| 14 | 3.46976 | should |
| 19 | 3.46507 | at |
| 15 | 3.42326 | may |

| | | |
|-----|---------|--------|
| 7 | 4.54133 | want |
| 8 | 4.52955 | nearly |
| 14 | 4.51953 | common |
| 114 | 4.51705 | are |

| | | |
|----|---------|-----------|
| 12 | 3.36086 | three |
| 13 | 3.34273 | whether |
| 12 | 3.32509 | number |
| 11 | 3.29816 | fertility |

Appendix 3

Appendix 3.1 Intertextuality marker: *according to*

1. www.darwinproject.ac.uk/letter/entry-171. I have just returned from a walk, & as a specimen how little the insects are known.—Noterus, according to Dic Class. contains solely 3 European species, I, in one haul of my net took five distinct species.— is this not quite extraordinary?—
2. www.darwinproject.ac.uk/letter/entry-376. Also, that I am convinced that independently of the relative percentage of recent shells, about which naturalists may differ according to their notions of what constitutes a specific difference, there are other characters in the entire assemblage of forms of shells belonging to each great tertiary epoch, which will enable us to classify the deposits according to the approach which they make to the type of organisation now existing in the neighbouring seas; and that this approach will serve as a chronological test of the eras to which tertiary deposits may respectively belong.
3. www.darwinproject.ac.uk/letter/entry-538. You say there are about 37 new species of Acanth. & according to this proportion there would be about 21 in the other orders. Now do you think it very desirable that all these should be engraved www.darwinproject.ac.uk/letter/entry-723. I have been progressing with the Antarctic plants, using your's King's & my own at once, & each according to the Nat. Ords. beginning with Ranunculaceæ, where the value of every scrap tells better than it is possible to suppose.
4. www.darwinproject.ac.uk/letter/entry-748. By way of illustration I will suppose I am called upon to point out a type of the order Carnivora. According to the first definition I should select a Cat because in the Cat tribe some of the more striking characters of the Carnivora are most strongly developed;
5. www.darwinproject.ac.uk/letter/entry-897. The word Oniscia is correct—and Stylifer, is also correct— I did not find either of these Genera in the Pacific Ocean they are both to be found in the West Indies— Oniscia is found also in China (two or three Species)— Stylifer is found in the Philippines and the Mauritius and according to Turton it is also British.
6. www.darwinproject.ac.uk/letter/entry-986. I was having some talk with Lyell about coal, when in London: his fossils from Alabama being most of them identical in species according to Bunbury with the coal-plants 20o degrees N. in Europe, is an interesting fact
7. www.darwinproject.ac.uk/letter/entry-1496. (5) When an organ is developed in an extraordinarily great degree it is (according to Waterhouse) apt to be variable in same species; so it is, when developed in an extraordinarily little degree (ie when rudimentary). Is this likewise case when organ developed in some very unusual manner?—
8. www.darwinproject.ac.uk/letter/entry-1607. On the back of this you have a list of 80 very anomalous genera selected by Bentham— I appended 1) the number of species enumerated in Steudel Nomenclator the only catalogue of plants & not a bad one for such purposes & 2) the number of species according to Benthams & my knowledge of the genera, which increases some, diminishes others, & also diminishes the total of species. I append Steudels ratio of species to genera of all plants.— I should suppose however that it would stand considerable reduction & that 1:8 would possibly be a fair estimate or even 1:6.—
9. www.darwinproject.ac.uk/letter/entry-1743. I send the Catalogue of plants by this post, marked in some degree according to your want. Perhaps I have marked too many as resembling pairs, by including some species that are always known when once understood.
10. www.darwinproject.ac.uk/letter/entry-1752. Dieffenbach remarks that, in N. Zealand, “the Cat often runs wild”, and that “these wild Cats soon resume the streaky grey colour of the common wild

Cat" II, 185. In Sardinia, on the contrary, according to Azuni, the feral Cats are, nearly all of them, black! Like the Falkland Island Rabbits!

11. www.darwinproject.ac.uk/letter/entry-1761. Are the humped Camels & tropical cattle species or normal or abnormal varieties? Are they merely analogous to the fat-rumped Sheep or otherwise? According to Buffon, the humps & the callosities, &c of the Camel are the results & badges that it bears of its long servitude; an opinion which would scarcely be backed by anyone now!
12. www.darwinproject.ac.uk/letter/entry-1792. Wallace has, I think, put the matter well; and according to his theory, the various domestic races of animals have been fairly developed into species.
13. True, the contrast is enormous between Guzerat, Rajputana, & Cutch,—the lowland sandy desert,—& the intensely cold elevated regions of Tibet; but though he does not pass through the Punjab & up the wooded regions of the Himalaya, he is nevertheless find in Sindh, Afghanistan, Mesopotamia, & through northern Persia right up to the Aral, in gradually increasing numbers, & then all through Gt. Tartary to the frontiers of Tibet, according to Pallas, & in Tibet as the Khyang. Gmelins supposed wild Ass is merely a Dziggetai with a short shoulder stripe, such as has likewise been seen in Indian specimens, but very rarely.
14. www.darwinproject.ac.uk/letter/entry-1825. As you receive specimens from Madeira, kindly procure me some skins of the wild Canary; also ascertain which is the 'Red-legged Partridge' of Madeira & also of the Canary isles,—*P. petrosa* I suppose. According to Widdrington, this is not found in Spain, but is the only species in Sardinia, as in Barbary. I have no specimen of it. I should like also to see the peculiar Chaffinch of Madeira.
15. www.darwinproject.ac.uk/letter/entry-1830. We have a vast deal yet to learn with respect to the limits of species; the excessive differences in the views of different turalists on this point intro cusion & uncertainty into reasonings on the geogra plants & animals. When that what according to Duval's views are nearly 50 different species of *Solanum*, are considered by Bentham as all referable to the one *Solanum nigrum*; & that another botanist has made 12 species out of our common White Water-lily; it is rather bewildering.
16. www.darwinproject.ac.uk/letter/entry-1905. The littoral shells according to Macandrew imply that Madeira and the Canaries were once joined to the main land of Europe or Africa but that those isles were disjoined so long ago that most of the species came in since. In short the marine shells tell the same story as the land shells.
17. www.darwinproject.ac.uk/letter/entry-1982. Above is the upshot as to our species common to Europe ranging N. I have gone into this matter with no small pains.— Of course further observation all tends one way—i.e to carry the range further north—; but so it is according to our present knowledge— I think you will be a little astonished, at the result.
18. www.darwinproject.ac.uk/letter/entry-2034. Seeing that this seemed so in Persoon, I took our little British Flora, & discriminating trees from Bushes according to Loudon, I have found that the result was in species, genera & Families, as I anticipated.
19. www.darwinproject.ac.uk/letter/entry-2065. *Artemisia*:— species numerous & close, rather than variable if taken singly; but *A. camphorata* is a group of species according to some botanists, a varying species according to others. *Senecio*:—same remark, three or four confused or variable species. *Cirsium*:—many intermediates, supposed hybrids.
20. www.darwinproject.ac.uk/letter/entry-2104. For, see you, Mr. Watson cites *Carex* & *Ranunculus* under this head. Now *Carex*, viewed according to Dr. Boott, is a good case in point. The species very difficult to distinguish by reason of their similarity, but not remarkably variable; because he regards almost every definable form as a separate species. But if Dr. Hooker were to elaborate the genus, how would it be? Would it not fall at once into no. 3? *Potamogeton*, would be placed by Mr. Tuckerman under no 1. —by me under no. 3. etc—

21. www.darwinproject.ac.uk/letter/entry-2250. For example, the Greater Redpole, or Linet which is very abundantly met with in the island, retains its bright carmine plumage through the year; the Herring Gull, also very common, is, according to Dr. Renton, quicker by some months in obtaining its mature garb than with us; & the Black cap Warbler assumes in some instances, an intensity of colour, which has led to its being described by Sir. W. Jardine as a new species."—
22. www.darwinproject.ac.uk/letter/entry-2409. Thus *Forficula auricularia* has an ovary consisting of numerous short tubes, while in *Labidoura gigantea* the ovary is composed (according to Leon Dufour) of a few long egg tubes. What external circumstances can possibly have produced such great differences in animals so
23. www.darwinproject.ac.uk/letter/entry-2520. There seemed to me some little confusion about your fossil Elephants: the species in N. & S. States, I believe, are distinct according to Falconer. The northern one, anyhow, can hardly be adduced as evidence of warmer climate. But it is foolish in me to write on; for I am writing only after one hasty read, several months ago.—
24. www.darwinproject.ac.uk/letter/entry-2794. I believe I succeeded in diminishing, if not entirely removing, the chances of Darwin's being prejudged by many who take their cue in such cases according to views of those they suppose may know something of the matter—
25. www.darwinproject.ac.uk/letter/entry-2927a. Falconer has been holding forth today on the diff, Mastodons & Elephants not coming in chronologically as they shd do, according to yr views, but when one sees the new Maltese dwarf intermediate between *E. antiquus* & *E. meridionalis* & Anca's new Sicilian cave elephant, a modification of the living Indian one leaning towards *antiquus*, & when one thinks that Falconer can distinguish all American varieties of Mammals from all European fossil species, I confess I attach little value to the objection.
26. www.darwinproject.ac.uk/letter/entry-2943. Hence, I do not see that it is "more complex" to suppose in this case an evolution, than a suppression;— when, according to your theory, the evolution must have once occurred, whereas the suppression, at best, only may have occurred.
27. www.darwinproject.ac.uk/letter/entry-2998. How capitably in the Atlantic, you show that Geology & Astronomy are according to Bowen Metaphysics; but he leaves out this rubbish in the 4to. Memoir.—
28. www.darwinproject.ac.uk/letter/entry-3332. The *Oxalis* pairs distinguished on account of the proportion of the stamens and styles, and which have nevertheless been supposed to belong to one species are according to Zuccarini
29. www.darwinproject.ac.uk/letter/entry-4229. Prof. Treviranus says (*Botanische Zeitung* 1863 p. 4.) that according to Koch & Tausch all species of *Primula*, present two forms, except *P. longiflora* which is always short-styled.
30. www.darwinproject.ac.uk/letter/entry-4306. This may be due to there being plenty of space for preservation of intermediates: Can this be accounted for by the land rising, & increased space thus being afforded— We (you & I) clash a little here Extreme diversity of form should according to you, follow on much destruction of individuals.—
31. www.darwinproject.ac.uk/letter/entry-4449. Now there is a species I am informed the *Turdus dactylopterus*, Bonaparte from Syria which has a prominent claw on its carpus. This then according to the theory expounded by you enables us to understand the existence of the wart in so many species of the same group.
32. www.darwinproject.ac.uk/letter/entry-4715. There are no *Crypheæ*, no *Leptodonteæ* no *Leskeaceæ*, no *Cylindrothecieæ* while there is two *Nekeræ* much alike, also two *Antitrocheæ* two or three (according to authors) species of *Pterogonium* three *Alsiæ* and in the great division of the *Hypna* all the species are united in the subgenera: *Isothecium* *Camptothecium* *Scleropodium*, *Eurynchium* with the exception only of one *Shuidium*, one *Thamnium* one *Brachythecium* and the two universal *Amblystegium repens* & *A. riparium*.

33. www.darwinproject.ac.uk/letter/entry-5852. And hence according to Mr Wallace's doctrine it might be inferred that they did so in consequence of their gaudy appearance;—or that only such of them as concealed themselves had been able to continue their races.—
34. www.darwinproject.ac.uk/letter/entry-6095. But I suspect that the sterility is not caused so much by any particular conditions, as by long habituation to conditions of any kind. To speak according to Pangenesis the gemmules of Hybrids are not injured, for Hybrids propagate freely by buds; but their reproductive organs are somehow affected so that they cannot accumulate the proper gemmules, in nearly same manner as the reproductive organs of a pure species become affected when exposed to unnatural conditions.
35. www.darwinproject.ac.uk/letter/entry-6140. On littoral rocks near Desterro, under the oysters and Balanidæ by which they are covered, there lives a Sphæroma interesting by the uncommonly great difference of the two sexes; the female is a true Sphæroma, the male would be, according to Milne Edwards' classification, a Cymodocea. In most or all other species of the genus there is no external sexual difference.
36. www.darwinproject.ac.uk/letter/entry-12532. As to the names of the species by the seeds, it is not clear— But, according to \$Watson\$, who has done his best with them, the one with large ovate turgid seeds rather pointed at one end, the germination of which is figured in Amer. Jour. Sci. & in Text-Book is Megarrhiza Californica, I suppose
37. www.darwinproject.ac.uk/letter/entry-13872. There is much difficulty in deciding on age of the isolated Tertiary deposits on coast of Chile; the most probable conclusion seems to be that they are old Miocene or Eocene.— The generic character, I believe, is Miocene, but nearly all the species are distinct, & all according to M. d'Orbigny.—

Appendix 3.2 Intertextuality marker: *agree*

- 1 www.darwinproject.ac.uk/letter/entry-367 Has your late work at shells startled you about the existence of species? I have been attending a very little to species of birds, & the passages of forms, do appear frightful—every thing is arbitrary; no two naturalists agree on any fundamental idea that I can see.
- 2 www.darwinproject.ac.uk/letter/entry-734 The Eastern limit to agree with this definition in the Straits of Magals. should coincide with the change in Geolog. formation, & with the Beeches, cease at Cape Negro; as however the Patagonian flora is the Negative one, & characterized by the absence of Beech district forms; some of its peculiar plants are probably also found in the latter & I should include therefore those found on the immediate boundary, as those of Elizabeth Isld & Cape Gregory Bay.
- 3 www.darwinproject.ac.uk/letter/entry-884 Kerguelens Land has only 3.. New Zealand & V. D. L. are certainly poor—in Trinidad (of Brazils) I saw only 3, I think, a Hemerobius & the House-flie & cockroach, introduced from a wreck: Canaries & Madeira are poor, I think: Cape de Verds are too dependent on the W. coast of Africa to judge from— nothing struck me as so marvellous as the appearance of 4 Insecta & many Arachnida you mention as on St. Pauls rocks.— Still I agree with you on the main point that such few as there are, wd. be enough for impregnation, if they only went to work about it..
- 4 www.darwinproject.ac.uk/letter/entry-961 It is quite curious how our opinions agree about Forbes views; I was very glad to have your last letter, which was even more valuable to me than most of yours are & that is saying, I assure you, a great deal.—
- 5 www.darwinproject.ac.uk/letter/entry-964 These being done I am again ready to do a little to my notion of the distrib. of Gal. plants, though alas with hands fuller than ever. I think we are agreed on Polymorphism in the sense we did argue it, & also in that we are now about to treat it under.
- 6 www.darwinproject.ac.uk/letter/entry-1239 I have written so lately that I have nothing to say about myself; my health prevented me going on with a crusade against "mihi" & "nobis" of which you warn

me of the dangers: I showed my paper to 3 or 4 naturalists & they all agreed with me to a certain extent: with health & vigor, I wd not have shown a white feather, but with aid of 1/2 a dozen really good naturalists, I believe something might have been done against the miserable & degrading passion of mere naming species.

- 7 www.darwinproject.ac.uk/letter/entry-1520 I happen to be too much occupied to give as much time as I should like to studying your specimens; but after a pretty careful inspection, I am quite inclined to agree with almost all your determinations, and I can feel no doubt that the species considered by you as new, are new. Indeed I consider it as in some degree presumptuous in saying this; for you are evidently an adept amongst the Cirripedia.
- 8 www.darwinproject.ac.uk/letter/entry-1601 All this has amused me, but I daresay you will have a good sneer at me, & tell me to stick to my Barnacles. By the way you agree with me that sometimes one gets despondent, for instance when theory & facts will not harmonise; but what appears to me even worse, & makes me despair, is, when I see from the same great class of facts, men, like Barrande deduce conclusions, such as his Colonies & his agreement with E. de Beaumonts lines of Elevation, or such men as Forbes with his Polarity;
- 9 www.darwinproject.ac.uk/letter/entry-1610 With respect to your speculation on effect of splitting Australia; I agree very far with you; but I think that you somewhat underrate the effect on all the species of such a movement or irruption as you suggest.
- 10 www.darwinproject.ac.uk/letter/entry-1612 I do not quite agree with your "grave objection to the whole process" which is "that if you multiply the anomalous species by 100, & divide the normal by the same, you will then reverse the names" – For, to take an example, ornithoryhncus & Echidna would not be less aberrant if each had a dozen (I do not say 100, because we have no such cases in animal kingdom) species instead of one.
- 11 www.darwinproject.ac.uk/letter/entry-1762 When I disparaged the instinct of the Orangs in comparison with dogs, Dr. Mc Dougall did not agree with me in opinion: but it seems they can scarcely be reared in captivity even in their native country, being so impatient of restraint, & most troublesomely affectionate when at liberty to those they know. Dr. Mc Dougall gives them credit for no small amount of intellect; & he speaks from having had the best opportunity of observation.
- 12 www.darwinproject.ac.uk/letter/entry-1889 After the rect. of your last letter I consulted with Mr Waterhouse again about the Island Faunas—& we agreed that the doctrine might be maintained against all comers. Sir Chas. Lyell is coming to the same opinion—tho' he would account for it metaphysically. We can start with a good case in Tasmania—an island which must have been separated from Australia since the creation of Thylacinus & Dasyurus (which occur fossil in the mainland) & before the arrival of those species peculiar to the continent.
- 13 www.darwinproject.ac.uk/letter/entry-1991 In short I think the warm temperate would be exposed very much longer to those causes which I believe are alone efficient in producing change than the sub-arctic; but I must think more over this, & have a good wriggle I cannot quite agree with your proposition that because the sub-arctic have to travel twice as far, they wd be more liable to change. Look at the two Journeys which the Arctics have had from N. to S. & S. to North, with no change, as may be inferred, if my doctrine is correct, from similarity of Arctic species in America & Europe & in the Alps.—
- 14 www.darwinproject.ac.uk/letter/entry-1997 Therefore, according to my principles, whether right or wrong, I cannot agree with your proposition that Time + altered conditions + altered associates are "convertible terms". I look at first & last as far more important;—time being important only so far as giving scope to selection.— God knows, whether you will perceive at what I am driving.— I shall have to discuss & think more about your difficulty of the temperate & sub-arctic forms in S. hemisphere, than I have yet done.—
- 15 www.darwinproject.ac.uk/letter/entry-2020f But first let me thank you for your paper on Geological Development: everything which you have ever written interests me; but I own this last paper is rather too grand for my digestion; not but that I am very much inclined, from different reasons, to agree with you on antiquity of continents & oceans; & I have lately had some written discussion on subject with Lyell, who differs toto coelo from me.
- 16 www.darwinproject.ac.uk/letter/entry-2034 P.S. | You might give me a valuable piece of information, with very little trouble to yourself.— I have been comparing, as far as I can, Protean genera, & have left off in a maze of perplexity. By Protean genera, I mean such as hardly two Botanists agree in about the

species,—what to call species & what varieties. Now what I want to know is, whether such genera as *Salix*, *Rubus*, *Rosa*, *Mentha*, *Saxifraga*, *Hieracium*, *Myosotis*, &c have equally Protean species in U. States; even if they have only one, but more especially if they have many. I think you have no *Rosa*, & I forget how it is with some of the other genera.— The converse case wd. be equally valuable to me if you would think over your half-dozen or dozen worst genera which have any European species, & then I could find out whether such are very troublesome in Europe.— I think Hooker told me that in Himalaya, *Rubus* & *Salix*, though large genera, were not troublesome to make out.— I think Protean genera of shells are troublesome at all geological times & in all places.

- 17 www.darwinproject.ac.uk/letter/entry-2182 But I cannot agree with you for my object, that general monographs are best: (1st) I presume the varieties wd. be best known in small country like ours; 2d. a very large genus might have very few species in many separate countries & then according to my doctrine, on average it wd not be a numerically increasing or varying genus. Again a genus, though small for its order in a monograph, might be large in any one country, & then it ought to be there on average an increasing or varying genus.
- 18 www.darwinproject.ac.uk/letter/entry-2239 Of course I do not suppose with groups of plants so widely extended as they are, that there ever shd. be such difference, as there might be in case of Mammals. Therefore I agree that orders in a Prodrumus not obeying my rule as with Labiatæ & Verbenaceæ is a serious objection; though not nearly so fatal, in my opinion, if in a local Flora.— I was led to all this work by a remark of Fries, that the species in large genera, were more closely related to each other than in small genera; & I thought if this were so, seeing that varieties & species are so hardly distinguishable, I concluded that I shd. find more varieties in the large genera than in the small: but at first, seeing the many causes of doubt, I certainly did not expect to find more than three-fourths of the Floras, yielding the result, which they have.—
- 19 www.darwinproject.ac.uk/letter/entry-2503 Birds which have struggled in their own homes when settled in a body nearly simultaneously in new country, would not be subject to much modification, for their mutual relations would not be much disturbed. But I quite agree with you that in time they ought to undergo some. In Bermuda & Madeira they have, as I believe, been kept constant by the frequent arrival of, & the crossing with, unaltered immigrants of same species from the mainland. In Bermuda this can be proved; in Madeira highly probable as shown me by letters from E. V. Harcourt.—
- 20 www.darwinproject.ac.uk/letter/entry-2516 I hope that you will read my Book, straight through; otherwise from the great condensation it will be unintelligible. Do not, I pray, think me so presumptuous as to hope to convert you; but if you can spare time to read it with care, & will then do what is far more important, keep the subject under my point of view for some little time occasionally before your mind, I have hopes that you will agree that more can be said in favour of the mutability of species, than is at first apparent. It took me many long years before I wholly gave up the common view of the separate creation of each species.
- 21 www.darwinproject.ac.uk/letter/entry-2575 I have very long interview with Owen, which perhaps you would like to hear about, but please repeat nothing. Under garb of great civility, he was inclined to be most bitter & sneering against me. Yet I infer from several expressions, that at bottom he goes immense way with us.— He was quite savage & crimson at my having put his name with defenders of immutability. When I said that was my impression & that of others, for several had remarked to me, that he would be dead against me: he then spoke of his own position in science & that of all the naturalists in London, “with your Huxleys”, with a degree of arrogance I never saw approached. He said to effect that my explanation was best ever published of manner of formation of species. I said I was very glad to hear it. He took me up short, “you must not at all suppose that I agree with in all respects”.— I said I thought it no more likely that I shd. be right on nearly all points, than that I shd toss up a penny & get heads twenty times running.
- 22 www.darwinproject.ac.uk/letter/entry-2595 He assumes (like old geologists assumed the forces of nature were formerly greater) that species were at first more plastic. His simile of tree & classification is like mine (& others), but he cannot, I think, have reflected much on subject, otherwise he would see that genealogy by itself does not give classification.— I declare I cannot see much closer approach to Wallace & me in Naudin than in Lamarck—we all agree in modification & descent.—
- 23 www.darwinproject.ac.uk/letter/entry-2614 I remember your remarks on non migration but I cannot say that I quite appreciate them. I entirely agree with you that difficulty of not finding intermediate fossils in number is very great, even when looking at the Geological Record, as being as imperfect as I

- believe— No one will think anything of my book, unless his mind leads him to put weight on the apparent explanation offered by the theory from several large classes of facts as affinities,—homologies, embryology &c.
- 24 www.darwinproject.ac.uk/letter/entry-2649 I fully agree that the difficulty is great, & might be made much of by a mere advocate. Will you oblige me by reading again slowly from p. 267–272.— I may add to what is there said, that it seems to me quite hopeless to attempt to explain why varieties are not sterile; until we know precise cause of sterility in species.—
- 25 www.darwinproject.ac.uk/letter/entry-2690 I quite agree with what you say on effect of admission of theory on Systematic work; (see p. 485), not but what I was haunted with endeavouring to guess what cirripedes would be ranked as species by other naturalists. I think the importance of theory bears on opening up new fields of enquiry & in giving a rational, instead of theological explanation of many known facts I am much pleased to hear that you intend reading the Book again.
- 26 www.darwinproject.ac.uk/letter/entry-2728 The Lyells went this morning. I had much talk most interesting to me. & it did not kill me to the extent which I expected. We talked over your Essays & agreed about the Book which you ought to make. What fine materials in all combined, including as Lyell remarked, the Galapagos papers! But I see in the Gardeners' Chron: that you have started on a gigantic task with Bentham. By the way I now quite understand Bentham's silence on the modification of species.—
- 27 www.darwinproject.ac.uk/letter/entry-2759 Although I fully agree that no definition can be drawn between monstrosities and slight variations (such as my theory requires) yet I suspect there is some distinction. Some facts lead me to think that monstrosities supervene generally at an early age; and after attending to the subject I have great doubts whether species in a state of nature ever become modified by such sudden jumps, as would result from the natural selection of monstrosities.
- 28 www.darwinproject.ac.uk/letter/entry-2825a I am inclined to agree with his meaning at lines 2–6 of p. 3, but there again he is not clear N.S. is not dependent on variability in any logical sense— As to the question of personality it is simply ridiculous— I agree with him & you too that the real primary agency is as great a mystery as ever, call it mystery or theistic element or God or nature or what he will, unseen Power if he likes.
- 29 www.darwinproject.ac.uk/letter/entry-2915 I entirely agree with what you say about only one species of many becoming modified: I remember this struck me much when tabulating the varieties of plants. & I have a discussion somewhere on point. It is absolutely implied on my ideas of classification & divergence that only one two species of even large genera give birth to new species; & many whole genera become wholly extinct, ie none of the species.
- 30 www.darwinproject.ac.uk/letter/entry-2922 When I have spoken of dominant forms, it has been in relation to the multiplication of new specific forms, & the dominance of any one species has been relative generally to other members of the same group, or at least to beings exposed to similar conditions & coming into competition. But I daresay that I have not in the Origin made myself clear, & space has rendered it impossible. But I thank you most sincerely for your valuable remarks, though I do not agree with them.)
- 31 www.darwinproject.ac.uk/letter/entry-2940 I ought to apologise for troubling you; but I have at last carefully read your excellent criticisms on my Book.— I agree with much of them, & wholly with your final sentence. The objections & difficulties, which may be urged against my view, are indeed heavy enough almost to break my back; but it is not yet broken!
- 32 www.darwinproject.ac.uk/letter/entry-3047 I quite agree that Phillips is unreadably dull.— You need not attempt Bree,— the man must be a conceited fool.— If you come across Dr Freke on "Origin of species by means of Organic Affinity", read a page here & there just to see the maximum of ill-written unintelligible rubbish, which he tells the reader to observe has been arrived at by "induction", whereas all my results are arrived at only by "analogy".
- 33 www.darwinproject.ac.uk/letter/entry-3098 I quite agree with what you say on Lieut. Hutton's Review (who he is, I know not): it struck me as very original: he is one of the very few who see that the change of species cannot be directly proved & that the doctrine must sink or swim according as it groups & explains phenomena. It is really curious how few judge it in this way, which is clearly the right way.
- 34 www.darwinproject.ac.uk/letter/entry-3235 I have just said that I cannot agree with "which variations are the effects of an unknown law, ordained & guided without doubt by an intelligent cause on a preconceived & definite plan". Will you honestly tell me (& I should really be much obliged) whether

you believe that the shape of my nose (eheu) was “ordained & guided by an intelligent cause” By the selection of analogous & less differences, fanciers make almost generic differences in their pigeons, & can you see any good reason why the natural selection of analogous individual differences should not make new species?

- 35 www.darwinproject.ac.uk/letter/entry-3532 To us who theorise I am sure the case is very important. Do the S. American Carabi differ more from the other species, than do, for instance, the Siberian & European & N. American & Himalayan (if the genus exists there); if they do, I entirely agree with you that the difference would be too great to account for by the recent Glacial period. I agree, also, with you in utterly rejecting an independent origin for these Carabi.—
- 36 www.darwinproject.ac.uk/letter/entry-3542 You ask for criticisms, I have none to give only impressions.— I fully agree with “your skimming-of pot-theory” & very well you have put it.— With respect contemporaneity, I nearly agree with you, & if you will look to the d—d— Book 3d Edit p. 349, you will find nearly similar remarks.
- 37 www.darwinproject.ac.uk/letter/entry-3746 You make important remarks versus natural selection, and you will perhaps be surprised that I do to a large extent agree with you. I could show you many passages, written as strongly as I could in the Origin, declaring that Natural Selection can do nothing without previous variability; and I have tried to put equally strongly that variability is governed by many laws, mostly quite unknown.
- 38 www.darwinproject.ac.uk/letter/entry-3856 I am actually reading de Tocquevilles Democracy in America, it appears to me a most able book, though I do not at all agree with it. (bigger fool you, you may so, & double big fool I am to say so) but I cannot help it. He assumes that D. in America was a success— Now I never regarded America as having cohesion enough to be pronounced either a success or a failure: there has been hitherto far too much freedom of motion there, too little “struggle for existence”—to develop any settled Govt. at all, & it is impossible to predicate what shape the existing introduced form of Govt. would take in 100 years, even if this war had not stepped in to confound all calculations.
- 39 www.darwinproject.ac.uk/letter/entry-4148 You give good advice about not writing in newspapers; I have been gnashing my teeth at my own folly; & this not caused by Owen’s sneers, which were so good that I almost enjoyed them. I have written once again to own to certain extent of truth in what he says; & then if I am ever such a fool again have no mercy on me.— I enclose A. Gray’s letter, as you might like to read all. I quite disagree with what he says about Lyell acting as a Judge on Species; I complain that he has not acted as a judge; I sometimes wish he had pronounced dead against us rather than possessed such inability to decide.—
- 40 www.darwinproject.ac.uk/letter/entry-4217 I can hardly tell why it is, but your address has pleased me as much as Lyell’s book disappointed me,—that is the part on species, though so cleverly written. I agree with all your remarks on the Reviewers. By the way, Lecoq is a believer in the change of species.— I, for one, can conscientiously declare that I never feel surprised at anyone sticking to the belief of immutability; though I am often not a little surprised at the arguments advanced on this side.—
- 41 www.darwinproject.ac.uk/letter/entry-4234 I like and agree to your remark that in Bates’s Geographical varieties, etc. etc. we get about as near to seeing a species made as we are ever likely to get:—and so believing I think your gradual way more likely than Heer’s jumps.
- 42 www.darwinproject.ac.uk/letter/entry-4265 With regard to sports in cultivated plants here I cannot agree with Schomburgk. By neglect dahlias & such degenerate, ie double & fine flowers are no more produced, but otherwise I can see nothing particular. That plants vary here perhaps more than in temperate zones appears to be correct & in this respect I wish to draw your attention to the genus Capsicum.
- 43 www.darwinproject.ac.uk/letter/entry-4642 I will fight you to the death, that as Primrose & Cowslip are different in appearance (not to mention odour, habitat & range) & as I can now show that when they cross, the intermediate offspring are sterile like ordinary Hybrids, they must be called as good species as a man & a Gorilla.— I agree that if Scotts Red Cowslip grew wild or spread itself & did not vary into common cowslip (& we have absolutely no proof of primrose or cowslip varying into each other) & as it will not cross with cowslip, it would be a perfectly good species.—

- 44 www.darwinproject.ac.uk/letter/entry-4752 All analogy makes me quite disagree with the Duke that the differences in the beak, wing & tail are not of importance to the several species. In the only two species which I have watched, the difference in flight & in the use of the tail was conspicuously great.
- 45 www.darwinproject.ac.uk/letter/entry-4778 I think there is some great mystery concealed in this matter, which time & patient observation & experiment will be certain to elucidate. I wish, if you have one, you would send me a copy of your Paper on *Primula*. I saw long ago an abstract with very full extracts of your *Linum* paper in *Silliman's Journal*. I quite agree with you in your note (p. 101) on the irrelevancy of von Mohl's arguments. How can men be so muddle-headed? Has *Leersia oryzoides* ever been observed in a state of nature? If not, I do not think that we can argue conclusively from the habits of a tame organism to the habits of a wild organism.
- 46 www.darwinproject.ac.uk/letter/entry-4969 Of course everybody agrees about species that settled—but why do Entomostraca univalve Mollusca, & amphibia begin with such high forms? I would add Fish, but I might run the risk of saying something outré & you have always Huxley at command.
- 47 www.darwinproject.ac.uk/letter/entry-5007 I quite agree that Agassiz could never mistake weathered-blocks & glacial action; though the mistake has, I know, been made in 2 or 3 quarters of the world. I have often fought with Hooker about the Physicists putting their veto on the world having been cooler; it seems to me as irrational, as if, when Geologists first brought forward some evidence of elevation & subsidence, a former Hooker had declared that this could not possibly be admitted until Geologists could explain what made the earth rise & fall.
- 48 www.darwinproject.ac.uk/letter/entry-5020 You say that all Botanists would agree that many tropical plants could not withstand a somewhat cooler climate. But I have come not to care at all for general beliefs without the special facts. I have suffered too often from this; thus I found in every book the general statement that a host of flowers were fertilised in the bud,—that seeds could not withstand salt-water &c &c.—
- 49 www.darwinproject.ac.uk/letter/entry-5062 Another Paper which concerns you is one of Lecoq's on the migration of plants with special reference to the mountain flora of Auvergne nearly identical with those of the Alps & Pyrenees— He disagrees with you as to the glacial epoch and its effect in producing the present distribution of plants. He says the former greater extension of glaciers was rather due to a higher than to a lower temperature and says he has anticipated Frankland & Tyndall in this point— see his book *Des glaciers et des Climats*— He considers that birds and the winds have effected the colonization of Alpine & Arctic plants in the Auvergne mountains—but while disagreeing with you on most points he shares your views as to Origin of Species and has anticipated you— see his *Etudes sur la géographie botanique de l'Europe* tom i. p. 140. tom iv. p 245–277.
- 50 www.darwinproject.ac.uk/letter/entry-5546 I quite agree about filiation of species, in a century to come there will be splendid work— I also agree how much unknown in Embryology & causes of each variation—utterly unknown [after del illeg]— | My book in French | I am aware that few follow your views & Most of your leading men are bitterly opposed | Paris—your papers
- 51 www.darwinproject.ac.uk/letter/entry-5554 I have been very glad to read this latter paper, as all inosculating forms are very interesting to me. I quite agree with what you say on the extreme interest of attempting to affiliate extinct & existing Species.
- 52 www.darwinproject.ac.uk/letter/entry-5565 The Duke of Argyll's book is very fair & manly. He cannot agree with you, but he writhes about under you as one who feels himself likely to be beat. What he says about the humming birds is his weakest part. He utterly overlooks sexual selection by the females, as one great branch of Natural selection.
- 53 www.darwinproject.ac.uk/letter/entry-5591 With respect to mimetic plants I remember Hooker many years ago saying he believed that there were many, but I agree with you that it would be most difficult to distinguish between mimetic resemblance & the effects of peculiar conditions. Who can say to which of these causes to attribute the several plants with heath-*Erica*-like foliage at the C. of Good Hope? Is it not also a difficulty that quadrupeds appear to recognize plants more by their odour than their appearance?
- 54 www.darwinproject.ac.uk/letter/entry-5642 I quite agree with your Chairman that you have put the whole argument better than I have done. But I disagree with you, & it is the only point on which I do disagree, when you say that there is nothing in your article original. As I am writing I will ask you two questions, but if you cannot answer them easily, pray do not take any trouble on the subject; Firstly.

- Where have you seen an account of inherited baldness & deficient nails; & 2ndly of the case of the plane which sent up an evergreen sucker or shoot.
- 55 www.darwinproject.ac.uk/letter/entry-5762 I have sometimes thought that a strong argument might be drawn against your Theory—when extreme about the Origin of Species (I quite agree with you except when, as I think you get into extremes) when you look at the extraordinary differences in the mode & organs of procreation.
- 56 www.darwinproject.ac.uk/letter/entry-5861 I wholly disagree with you that the *Numida ptilorhyncha* of Eastern Africa is the true original type of the domestic guinea-fowl. As regards *Gallus ferrugineus* (*v. bankiva*), you have not seen my remarks on this bird in the Ibis (commentary on Jerdon's book). Are you aware that the game fowls of India & the Malay countries are totally different from our game-fowls, heavy and thick in the leg, & inelegant to my eye, though with inveterate pluck, & too heavy for our form of gamecock, which they would be an overmatch for, as remarked by Crawford.
- 57 www.darwinproject.ac.uk/letter/entry-5997 I agree with what Dr Wallace says about collecting larvæ and as those of many species of Macro-Lepidoptera differ very much in size—the females being much larger than the males—they are more conspicuous and consequently are more often collected than the smaller ones which produce males— I do not suppose Mr Stainton has ever reared whole broods of any of the Micro-Lepidoptera from the eggs—it is the only way to ascertain the numbers of each sex—
- 58 www.darwinproject.ac.uk/letter/entry-6095 I agree with my son's argument & not with rejoinder. The cause of our difference, I think, is, that I look at the number of offspring as an important element (all circumstances remaining the same) in keeping up the average number of individuals within any area.— I do not believe that the amount of food by any means is the sole determining cause of number. Lessened fertility is equivalent to a new source of destruction.
- 59 www.darwinproject.ac.uk/letter/entry-6192 I quite agree with what you say, that entomologists have the best means of proving the derivation of species, & I have often much wished that I was one of the class, but it evidently requires the labour of a life to study carefully even one division of so gigantic a class.
- 60 www.darwinproject.ac.uk/letter/entry-6427 I am delighted that you, with whose name I am familiar, should approve of my work. I entirely agree with what you say about each species varying according to its own peculiar laws; but at the same time it must, I think, be admitted, that the variations of most species have in the lapse of ages been extremely diversified; for I do not see how it can be otherwise explained that so many forms have acquired analogous structures for the same general object, independently, of descent.
- 61 www.darwinproject.ac.uk/letter/entry-6728 I quite agree with you that Wallace's sketch of Natural Selection is admirable. I wrote to tell him so after I had read the article & in regard to the concluding theory I reminded him that as to the origin of man's intellectual & moral nature I had allowed in my first edition that its introduction was a real innovation interrupting the uniform course of the causation previously at work on the earth.

Appendix 3.3 Intertextuality marker: *ask*

- 1 www.darwinproject.ac.uk/letter/entry-359 The marine Saurian, which you were asking me about last night, inhabits the Galapagos Archipel., which is situated under the equator, and about 600 miles from the West coast of America.—
- 2 www.darwinproject.ac.uk/letter/entry-392 — You ask me how many species of fish would be published in each number; there would probably be from 12 to 16.— The numbers come out on alternate months, but as I have said, invertebrate animals might alternate with the fish if more than one number is produced.—
- 3 www.darwinproject.ac.uk/letter/entry-722 I am very much obliged to you for your interesting letter; I have long been very curious even for as short a sketch, as you have kindly sent me, of the botanical geography of the southern hemisphere.— I shall be most curious to see your results in detail. From my entire ignorance of botany, I am sorry to say, that I cannot answer any of the questions, which you ask me.—
- 4 www.darwinproject.ac.uk/letter/entry-734 You ask me whether I suppose the small proportion of sp to genera in Coral Islets, arises from chance of seeds &c? I cannot answer this, I should say perhaps not:— if genera or small groups are truly natural they are supposed to contain many characters in common, it is but right to assume that the character of transportable seeds should hence be common to some groups above others, the inference I need not state.
- 5 www.darwinproject.ac.uk/letter/entry-739 You ask whether the uniformity consists in species or forms. I am inclined to consider that uniformity of species is to a certain extent a sequitur to a uniformity of forms, & that it is a corollary to our Theorem.— Thus, uniformity of Flora must depend upon the genera being widely diffused, genera being forms, I think that is evident, again we have (or suppose we have) proved that it is the largest genera which are most widely diffused, & that a larger proportion of their species have wide ranges than those of small genera, whence I think it follows that in all countries of uniform floras, certain single, species should to a certain extent, be widely distributed.—
- 6 www.darwinproject.ac.uk/letter/entry-880 — You ask about Amber, I believe all the species are extinct, ie without the amber has been doctored) & certainly the greater number are.—
- 7 www.darwinproject.ac.uk/letter/entry-914 So as *S. pinnat.* does not at Valp. vary into big pods, I am more persuaded that yours is a rep. species of W. coast of Am.— That Neutral territory of rep. species you ask about is just what I want to work out but it needs great materials
- 8 www.darwinproject.ac.uk/letter/entry-1093 I, also, return Silliman & am glad to have seen Dr. Morton's article: my opinion, of it, as you ask for it, is that it is in main part, a merely tabulated compilation from Griffith's Cuvier, with a few other facts interpolated.
- 9 www.darwinproject.ac.uk/letter/entry-1339 You ask what effect studying species has had on my variation theories; I do not think much; I have felt some difficulties more; on the other hand I have been struck (& probably unfairly from the class) with the variability of every part in some slight degree of every species: when the same organ is rigorously compared in many individuals I always find some slight variability, & consequently that the diagnosis of species from minute differences is always dangerous.
- 10 www.darwinproject.ac.uk/letter/entry-1351 You ask me whether I am now at work on the Pedunculated or Sessile cirripedes; I have finished in M.S. the former & have just described my 41th species of the genus *Balanus*: the sessile cirripedes will take me at least six months more.
- 11 www.darwinproject.ac.uk/letter/entry-1463 You formerly asked me for specimens for Ipswich, I have consequently packed up 20 to 25 specimens of Cirripedia of the several leading genera, & have named them: I have chiefly selected British species.
- 12 www.darwinproject.ac.uk/letter/entry-1719 You ask how far I go in attributing organisms to a common descent; I answer I know not; the way in which I intend treating the subject, is to show (as far as I can) the facts & arguments for & against the common descent of species of same genus; & then show how far the same arguments tell for or against forms, more & more widely different: & when we come to forms of different orders & classes, there remain only some such arguments as those which can perhaps be deduced from similar rudimentary structure, & very soon not an argument, is left.—

- 13 www.darwinproject.ac.uk/letter/entry-1735 You ask me about the trained Otters of “S India”, where the only species is *Lutra nair*; as docile no doubt as any of its congeners, but I never heard of its being made use of in that part of the country.
- 14 www.darwinproject.ac.uk/letter/entry-1752 You ask if I consider the *Columba intermedia* to be a species, or a mere race of *C. livia*. I did not reply to this at the time; but referred you to a notice by the Prince of Canino of many such races, admitted by him as species.
- 15 www.darwinproject.ac.uk/letter/entry-1792 You ask me to publish my ideas about the domestic Cat. I may do so in a series of papers which I have promised to write for the ‘Calcutta Sporting Review’, where I have undertaken “the Feline animals of India”,—have already got a long article on Asiatic Lions printed off, and have partly written one on the Tiger—
- 16 www.darwinproject.ac.uk/letter/entry-2060 You ask about my doctrine which led me to expect that Trees would tend to have separate sexes. I am inclined to believe that no organic being exists which perpetually self-fertilises itself.
- 17 www.darwinproject.ac.uk/letter/entry-2167 Some time ago you asked me to furnish you with remarkable instances of disparity in form etc—in workers of Insects living in community— As one is apt to forget these things at the moment they are asked for I send you one that is a truly remarkable instance—
- 18 www.darwinproject.ac.uk/letter/entry-2251 You ask what my Book is about, I fear it is almost *de rebus omnibus*: my attempt is to look at all facts in Nat. Hist & Geology under the two points of view,—has each species been created independently or have species, like varieties, descended from other species?
- 19 www.darwinproject.ac.uk/letter/entry-2470 You ask about specific centres, if you change terms into specific areas, my theory quite requires them; i.e. it is, I think, next door to an impossibility that the same species should have been formed identically the same in any two areas. This point is discussed in my volume.—
- 20 www.darwinproject.ac.uk/letter/entry-2621 As I believe fully in your theory of the origin of species I shall be glad to do anything for you in advancing same and shall attend to the questions you send me and send you answers as soon as possible If you have more points I shall attend to them if you drop me a line— The question you ask me about the Chilean breed of sheep—I can in part answer now—
- 21 www.darwinproject.ac.uk/letter/entry-2780 You ask me how the blind Paussi in foreign Countries get from one ants nest to another— Your question is pertinent enough as regards some insects such as *Claviger*, *Batrissus*, &c., which are blind & live in ants nests— But the Paussi are not blind (at least I know of no blind ones).
- 22 www.darwinproject.ac.uk/letter/entry-3084 I am not in the least surprised at your demurring to accept my notions on species. It took me long years before I converted myself; though daily thinking and observing on the subject. You ask why I should not draw a line and allow natural selection to do a little work and no more. I can give no direct answer to this.
- 23 www.darwinproject.ac.uk/letter/entry-3104 With regard to the question you ask me—whether an intermediate local form B is numerous & widely dispersed between the ranges of its extreme forms A & C: the facts that I have on the subject are numerous & rather ill digested at present. They are complicated in themselves & difficult.
- 24 www.darwinproject.ac.uk/letter/entry-3480 Again you ask “if variety A succeed in locality I. why should it not succeed at locality II. if it had ever been existent there”—my answer is that no locality II. is identical with a loc. I. & that Nat. Selection will act on an imperceptible difference—it searches where no faculty of man can follow.
- 25 www.darwinproject.ac.uk/letter/entry-3542 You ask for criticisms, I have none to give only impressions.— I fully agree with “your skimming-of pot-theory” & very well you have put it.— With respect contemporaneity, I nearly agree with you, & if you will look to the d—d— Book 3d Edit p. 349, you will find nearly similar remarks.
- 26 www.darwinproject.ac.uk/letter/entry-3851 Dr. Hooker tells me that you ask him if I had your ‘Origin of Species’, if ever in my life I could be induced to tell an untruth, it would be here the case, because I should consider it the highest compliment to receive a work like yours from the hand of the Author, and on returning to Ch. Church, I shall present my copy to our embryo of a library, so that I can then fairly say, I do not possess it.

- 27 www.darwinproject.ac.uk/letter/entry-3913 You ask what I think about Falconer; of course I am much pleased at the very kind way he refers to me; but, as I look at it, the great gain is for any good man to give up immutability of species: the road is then open for progress; it is comparatively immaterial whether he believes in N. Selection; but how any man can persuade himself that species change unless he sees how they become adapted to their conditions is to me incomprehensible.—
- 28 www.darwinproject.ac.uk/letter/entry-4053 You ask about Sprengels "Dichogamy": he means by this a plant in which each flower first matures & sheds its pollen & then has its stigma mature; & much more rarely matures its stigma first & subsequently its pollen: so that these plants are in function monoœcious. I am sure his observations are to large extent correct, & the case is very common.
- 29 www.darwinproject.ac.uk/letter/entry-4642 In your last note you ask what the Bardsfield oxlip is—it is *P. elatior* of Jacq. which certainly looks when growing to common eyes different from common oxlip. I will fight you to the death, that as Primrose & Cowslip are different in appearance (not to mention odour, habitat & range) & as I can now show that when they cross, the intermediate offspring are sterile like ordinary Hybrids, they must be called as good species as a man & a Gorilla.—
- 30 www.darwinproject.ac.uk/letter/entry-5140 Now it seems to me that you have yourself led to this objection being made, by so often stating the case too strongly against yourself. For Example, at the Commencement of Chap. IV. you ask, if it is "improbable that useful variations should sometimes occur in the course of thousands of generations";—and a little further on you say, "unless profitable variati do occur natural selection can do nothing." Now such expressions h given your opponents the advantage of assuming that favourable variations are rare accidents, or may even for long periods never occur at all, & thus Janet's argument would appear to many to have great force.
- 31 www.darwinproject.ac.uk/letter/entry-5174 P.S. 2d. As you were asking about Books on "Origin"; a very good Zoologist Claus has just published one, with my name on title-page—the subject being an investigation of the amount of individual variability in the Copepodous Crustaceans & he shows it is wonderfully great in many organs & that some co existing vars, are apparently passing into distinct species.—
- 32 www.darwinproject.ac.uk/letter/entry-5389a I am much obliged to you for your kind letter (without date) in which you ask about the number of capsules produced by the Maxillaria with the larger pods. I am told by a french collector M. Gautier that it is the Maxillaria tetragona; however his names are not always to be relied upon.—
- 33 www.darwinproject.ac.uk/letter/entry-5429 I have also to thank you for your kind letter of Jan. 1st, and will now first answer the questions, you ask in this letter. As to *Adenanthera pavonina*, the only tree, I know, stands in a garden; but I had not even suspected, that it had been planted there, and still less, (as we are here very rich in Mimoseæ) that the species had been introduced from India.
- 34 www.darwinproject.ac.uk/letter/entry-5633a Since you wrote me, I have examined the genus in the Herbarium, and did not find a single species without perfect flowers. As you ask me if ever I have seen *Vandellia* with perfect flowers, I append enumeration of the species I have examined they may interest you and may possibly thus also hit upon the species which you have in view.
- 35 www.darwinproject.ac.uk/letter/entry-6016 When you asked whether other birds beside the Gallinaceæ were polygamous, I presume you had not forgotten the Ruff (*Machetes pugnax*); it is most remarkable that in such a very dull colored group as the Tringidæ and the allied family Scolopacidæ, that the only polygamous species (I believe) should have such marvellous nuptial appendages as the ♂ *pugnax*, small wart like wattles, and large hackles capable of being erected like a Gallus.—
- 36 www.darwinproject.ac.uk/letter/entry-6269 Your references & remarks will be of great use should a new edition of my book be demanded; but this is hardly probable, for the whole edition was sold within the first week, & another large edtion immediately reprinted which I shd think wd supply the demand forever. You ask me when I shall publish on the variation of species in a state of nature.

Appendix 3.4 Intertextuality marker: *aware*

- 1 www.darwinproject.ac.uk/letter/entry-503 I am not aware that the genera which it is difficult to hybridize are slow to sport. Up to this day, tho' I am still trying, I have failed in all attempts to cross Crocuses, yet there is a different either species or vary of Crocus in almost every part of S. Europe, & the garden varieties of Crocus vernus & versicolor are very numerous.
- 2 www.darwinproject.ac.uk/letter/entry-734 You are I daresay aware of the fact that there is no reason to believe that plants can be artificially acclimated to any extent— Gardeners have hardly made any plant hardy, either by growing it from seeds of an introduced live specimen which did but just ripen, or by grafting on allied hardier species.—
- 3 www.darwinproject.ac.uk/letter/entry-737 I was not aware of the analogous fact with regard to the Sandwich group; nor have I yet examined the Canary Isld. Campbell Isld, 2 degrees further S. than Auckland, contains several species not found in the former, though the latter is the smallest & furthest South— I should not think however that it would hold with Islds in the more temperate zones generally, as the Azores which have not very many peculiar plants I shall however sift this subject with my friend Mr H. Watson..
- 4 www.darwinproject.ac.uk/letter/entry-788 I am perfectly aware of the extreme caution that should be used in this instance, & the propriety of keeping the two species distinct in V. D L., or finding some character between the V. D L. form of varium which appears there as Selago,—if in either case, any, however slight, a character can be found between them; but I can find none, the V. D L. Selago is, to my eyes, identical with the English & V. D L. appears to me to possess all intermediate states between selago & varium: this is not the passage merely of one species into another, but of two groups of the genus differing originally toto caelo.
- 5 www.darwinproject.ac.uk/letter/entry-996 Have you ever thought of G. St. Hilaire "loi du balancement", as applied to plants: I am well aware that some zoologists quite reject it, but it certainly appears to me, that it often holds good with animals.— You are no doubt aware of the kind facts I refer to, such as great development of canines in the carnivora apparently causing a diminution—a compensation or balancement—in the small size of premolars &c &c.
- 6 www.darwinproject.ac.uk/letter/entry-1544 – I have not a well defined dissection to appeal to. I have so often, as I remark above, seen the mouth far posterior to the organs referred to, that I had hardly doubted the relation I have suggested until you expressed your opinion about it.— I was not aware of the objectionable character of the terms Kingdom & Subkingdom as I have used them in my Chapter on Dist. of Crustacea, until I read your inferences from them.
- 7 www.darwinproject.ac.uk/letter/entry-1735 You are doubtless aware that, for the most part, the Estreldinæ represent in India & the Malay countries, exclusively in Australia, & to a great extent in Africa, the Fringillinæ of northern climes (inclusive of the Himalaya); & we have also 3 species of Ploceinæ, one of which (with a fourth) is likewise found in Java, the rest of this group being African—
- 8 www.darwinproject.ac.uk/letter/entry-1761 The Achatina perdix will doubtless spread, & rapidly, over the whole country; and I am not aware of any species enemy that it has encountered as yet.
- 9 www.darwinproject.ac.uk/letter/entry-1776 Are you aware that the old Peruvians had domestic Dogs, regular parti-coloured curs! Vide Tschudi's plates on Peruvian Antiquities. (Pl. 23, I think); & I especially call your attention to this.
- 10 www.darwinproject.ac.uk/letter/entry-1790 Among species separated by a wide extent of Seas you are probably aware that Pupa Anconostoma Lowe is common to Madeira & the Canary Isles The lost shell which I reported as found by me on the summit of St Helena, and lately refound by E Layard, proves to be the same species
- 11 www.darwinproject.ac.uk/letter/entry-1825 I was not aware of what you mention concerning the muscular foundation of the tuft in the Polish (Polled?) fowls. I have been trying to hunt up a notice I remember reading some time ago, in some French work, respecting the wild range of the Golden Pheasant extending I think to Orenbourg; a very remarkable fact, which may account for the ancients having some knowledge of it, however vague, which Cuvier connects with the old descriptions of the Phœnix!!

- 12 www.darwinproject.ac.uk/letter/entry-2005 I was not in the least aware that those Leguminosæ, which have apetalous flowers, “were almost without anthers”: you once told me before, about the apetalous Leguminosæ, & I think I wrote down other names besides Ononis, Lespediza & Clitoria: I will, if I do not hear to the contrary quote this fact on your authority, viz the apetalous condition of “many” Leguminosæ, which in this condition are almost without anthers & yet produce more seed than the ordinary flowers.
- 13 www.darwinproject.ac.uk/letter/entry-2076 I am not aware that insects will ever “fight for their females”. I know however, from painful experience, that they will fight for themselves (having been bitten by a Scarites very severely some months ago); & I believe that “No. 1” is their motto,—caring little for No. 2 (or their mates), except now & then to eat them.—
- 14 www.darwinproject.ac.uk/letter/entry-2133 I was not aware that varieties occurred more in large genera than in small ones,—except from the ‘ a priori certainty, that where there are more species to vary, there must naturally be more varieties.
- 15 www.darwinproject.ac.uk/letter/entry-2629 Let us, I say return to your Tucotuco— We learn (& I remember examining the specimen dissected by Mr Reid) that its thigh bone is destitute of the Ligamentum teres,— accounting for its queer movements.— Now this ligament is wanting in very few mammals,—(but I am not aware of its absence in any birds) among which are four of very different character—viz the Orang, & the Elephant—The Seal tribe & the 3-toed Sloth.
- 16 www.darwinproject.ac.uk/letter/entry-2920c I am not aware of any fossil bats or rodents in Australian caves, so the small antiquity of rodentia may be a reason for their not having given origin as yet to Gyrencephala & does there not seem some connection between the low grade of Lysencephala as the only occupants of that vast Australian continent?
- 17 www.darwinproject.ac.uk/letter/entry-3061 I am much obliged to Mr. Marshall, of Ely, for his statement that the 15 plants of Fly Orchis (*Ophrys muscifera*) which does not grow in his neighbourhood, but which flourished in his garden, had not one of their pollen masses removed. The Orchis maculata, on the other hand, which likewise does not grow in the neighbourhood, had all its pollen masses removed. Mr. Marshall is not perhaps aware that different insects haunt different Orchids, and are necessary for their fertilisation.
- 18 www.darwinproject.ac.uk/letter/entry-3121 In p. 422 of your new edition you say “Madeira does not possess one peculiar bird”— This is not quite the fact. Out of the 99 birds given by Mr Vernon Harcourt in Ann. N.H. June 1855 as belonging to its Fauna one—*Regulus maderensis* is peculiar— it is of course an altered form of *R. auricapillus*. Three others *Fringilla canariensis*, *Cypselus unicolor* and *Columba trocaz* if not peculiar are distinct from European species and only found elsewhere in the Canaries— I am not aware that *Cyps. unicolor* has been met with even there.
- 19 www.darwinproject.ac.uk/letter/entry-3407 I have never succeeded in growing *Cynoches*.— & I do not recollect ever to have had *Mormodes*, but as this last is one of the aberrant forms of *Catasetum*, I suspect it follows its habits— You are doubtless aware that *Myanthus* & *Catasetum* are identical though totally unlike.— Twenty years & more ago, I flowered a most beautiful specimen of *Myanthus barbatus*.—two full spikes of flower, exactly like the plant represented in an early volume of Paxtons Mage.
- 20 www.darwinproject.ac.uk/letter/entry-3937 I am not aware that any sexual difference has been noticed in its flowers. Acct. wd. have to be taken of the Algae in such great preponderance submerged altogether,—fertilising of course by antherozoids. Some (as some Fuci) are monoecious & I fancy have both sexes in same conceptacle.
- 21 www.darwinproject.ac.uk/letter/entry-4071 P.S. By way of experiment, I gave a large plant of *Phaius Grandiflorus* a good ‘shaking’ to try if irritation would do anything in the way of inducing seed and I find some half dozen pods upon the plant. I was induced so to do as probably you may be aware that *Erica elegans* will not cross with any other species of heath neither will it set a single pod unless some artificial mode be adopted; but by shaking the plant the pollen will disperse and the plant thereupon will produce seed in abundance.
- 22 www.darwinproject.ac.uk/letter/entry-4257 You are doubtless aware of the cosimilarity of tendency to change of the Japanese as compared with the Chinese birds with the British birds as compared with those of the European continent. The causes are probably due to the influence on the respective atmospheres produced by the Gulf streams of the Pacific and the Atlantic.

- 23 www.darwinproject.ac.uk/letter/entry-5218 So much is this the case that some Norfolk farmers say that *Carduus arvensis* will not propagate from seed and I heard when there that one farmer had said that he would give 5s/ to any one who would prove that it could be raised from seed— I may possibly have repeated this story and that may have led some one to suppose that I believe that *C. arvensis* did not produce fertile seed—whereas I am perfectly aware that it is often introduced into fields by seed— when there it generally spreads by its rhizomes
- 24 www.darwinproject.ac.uk/letter/entry-5826 In retiring into a hole the large hand must block up the hole from its size: therefore I do not see the weight of M. Edwards quotation of M. Marion de Procé. You are probably aware that in some Gelasimi the hand can be of no use in feeding itself in as much, that the claw is so large that it cannot be brought to reach the mouth— In this Singapore species before me, the hand passes far beyond the mouth,
- 25 www.darwinproject.ac.uk/letter/entry-6130 I was much struck with your remarks about the Ducks with the white crescentic mark on the breast, you are probably unaware that in Sussex Ducks so marked are very common, I have stood by a ponds side & been amazed to observe every duck in some instances so marked, you will observe in several wild species there is a tendency to two colors on the breast, perhaps the uniformity of coloring adverted to may arise from atavism, my Brother who had some so marked thought it was an extension of the white ring of the Mallard.
- 26 www.darwinproject.ac.uk/letter/entry-6169 I am not aware that in any other species of Dog this difference is so great. It does not seem to be so in the common Greyhound, or in the Setter or Bulldog. It is strange that it should be so, yet we see it in some wild animals, tho' not in others; for instance in the Capercaillie and Black-cock, but not in the wild-duck or goose.

Appendix 3.5 Intertextuality marker: *discussion, hypothesis, assertion*

- 1 www.darwinproject.ac.uk/letter/entry-736 I will trouble you with only one other question. In discussion with Mr Gould, I found that in most of the genera of birds, which range over the whole or greater part of the world, the individual species have wider ranges: thus the Owl is mundane, & many of the species have very wide ranges. So I believe it is with land & fresh-water shells—and I might adduce other cases.
- 2 www.darwinproject.ac.uk/letter/entry-739 Thus, uniformity of Flora must depend upon the genera being widely diffused, genera being forms, I think that is evident, again we have (or suppose we have) proved that it is the largest genera which are most widely diffused, & that a larger proportion of their species have wide ranges than those of small genera, whence I think it follows that in all countries of uniform floras, certain single, species should to a certain extent, be widely distributed.— There is no occasion to suppose they are distributed to such an extent as to invalidate a hypothesis that “in each group of tropical S. S. Islds the several Islets have distinct floras;—”.. I consider the S. S. Islds as a whole, or Oceania to have a most distinct & peculiar flora, not from possessing any one very large group peculiar to itself, (as America has her Cacti &c) but because she has a mixture of the peculiarities of N & S. America, Australia, India & perhaps N. Asia.
- 3 www.darwinproject.ac.uk/letter/entry-964 Certainly there is no objection to the hypothesis of a Sargassum being an absolute creation, though I see no reason to call for such an aid in this case, the species being in my opinion decidedly the littoral Atlantic one
- 4 www.darwinproject.ac.uk/letter/entry-1856 You will probably object, why have so many more Northern species & forms gone to the south, than southern forms come to the north; I can explain this only on a pure hypothesis of cold having come on first from the north; but there has been some migration from south to north, as of Australian forms on Mountains of Borneo. And I am sure I have notes of a few S. African forms, as wanderers across the Tropics, into N. Africa & Europe: is not this so with *Gladiolus*, *Stapelia*(?).
- 5 www.darwinproject.ac.uk/letter/entry-1863 The discussion on Social plants (vague as the term & facts are) in De candolle strikes me as the best, which I have ever seen: two points strike me as eminently remarkable in them; that they should ever be social close to their extreme limits; & secondly that species having an extremely confined range, yet shd. be social where they do occur: I shd. be infinitely obliged for any cases either by letter or publicly on these heads, more especially in regard to a species remaining or ceasing to be social, on the confines of its range.
- 6 www.darwinproject.ac.uk/letter/entry-2020f But first let me thank you for your paper on Geological Development: everything which you have ever written interests me; but I own this last paper is rather too grand for my digestion; not but that I am very much inclined, from different reasons, to agree with you on antiquity of continents & oceans; & I have lately had some written discussion on subject with Lyell, who differs toto coelo from me. In my coral volume, I remember coming to same conclusion of continents now & lately having risen, ocean-beds having sunk, & great archipelagoes having oscillated.
- 7 www.darwinproject.ac.uk/letter/entry-2599 With respect to food of large animals, if you care about the subject, will you turn to my discussion on this subject partly in respect to the *Elephas primigenius* in my *Journal of Researches* (Murray’s Home & Colonial Library) Ch V. p. 85.— In this country we infer from remains of *Elephas primegenius*, that the climate at the period of its embedment was very severe, as seems countenanced by its woolly covering,—by the nature of the deposit with angular fragments,—the nature of the coembedded shells, & coexistence of the Musk Ox.
- 8 www.darwinproject.ac.uk/letter/entry-2639 I remember putting the case to myself almost in your words,—Why does not every individual become converted into distinct species? The answer, which I framed, and the discussion on this curious point, which I have somewhere in M.S. is nearly to the

- following effect: Although the number of species supported over equal areas under apparently nearly similar conditions is now very different in different quarters of the world; and although we are far from knowing that the most prolific area is fully stocked with species, as perhaps may be inferred if not so from some European plants having become naturalized even at Cape of Good Hope, yet Geology shows us, at least within the whole immense Tertiary period, that the number of species of shells & probably of mammals has not increased.
- 9 www.darwinproject.ac.uk/letter/entry-2649 The only point which I might add to my short discussion on this subject, is that I think it probable that the want of adaptation to uniform conditions of life in our domestic varieties has played an important part in preventing their acquiring sterility when crossed. For this want of uniformity & changes in the conditions of life seems only cause of the elimination of sterility (When crossed) under domestication.
 - 10 www.darwinproject.ac.uk/letter/entry-2898 The facts of Embryology & of morphology are favourable to belief in an intimate relationship of organism to organism throughout nature; & this relationship is explicable on the hypothesis of a community of descent: & though these facts are not contrary to a notion of separate creation they are unexplained by it. —
 - 11 www.darwinproject.ac.uk/letter/entry-2908a But the antiquity of islands void of mammals checks the hypothesis of the easy adaptability of one species of a genus to new conditions. On a new volcanic island before all the best places are seized upon, seals or walrus's, manatees, dolphins & other cetacea, when pressed hard for food wd go up the rivers or if amphibious devour the eggs or young of land-birds.
 - 12 www.darwinproject.ac.uk/letter/entry-2927a I return the M.S. on dogs which I think excellent. The case you make out seems very strong not only of crosses from distinct living species having blended into the dog, but in favour of difft savage races having domesticated different canine types, wolves, jackalls &c by domestication in accordance with the hypothesis of Pallas having eliminated the dislike to cross with other species as well as the tendency of such crosses to sterility.
 - 13 www.darwinproject.ac.uk/letter/entry-3345 I have hardly anything to remark. That is a capital discussion on the effects of climate on the dress of species; very original & throws new light on subject.—
 - 14 www.darwinproject.ac.uk/letter/entry-3603 The general hypothesis of indefinite transmission across centuries of forms with more or less marked modifications seems preferable to any other, but I am uncertain that natural selection is the means for it. There are so many factors that for a long time keep forms the same from generation to generation or that cause them to revert! It is so rare for a new form to be preserved without the protection of man! I know of no proven instance of the latter case.
 - 15 www.darwinproject.ac.uk/letter/entry-3913 I have just received long pamphet by Alph. De Candolle on Oaks & allies, in which he has worked out in very complete & curious manner individual variability of species, & has wildish speculations on their migrations & duration &c. It is really curious to see how blind he is to the conditions or struggle for life; he attributes the presence of all species of all genera of trees to dryness or dampness! At end he has discussion on "Origin"; I have not yet come to this, but suppose it will be dead against it.
 - 16 www.darwinproject.ac.uk/letter/entry-3969 I have thought that I ought to state to you the ground for my assertion on page 602, that Geology has not afforded facts that sustain the view that the system of life has been evolved through a method of development from species to Species. —
 - 17 www.darwinproject.ac.uk/letter/entry-4108 So little do we know of the conditions of life all around us, that we cannot say why one native weed or insect swarms in numbers, and another closely allied weed or insect is rare. Is it then possible that we should understand why one group of beings has risen in the scale of life during the long lapse of time, and another group has remained stationary? Sir C. Lyell, who has given so excellent a discussion on species in his great work on the 'Antiquity of Man,' has advanced a somewhat analogous objection, namely, that the mammals, such as seals or bats, which alone have been enabled to reach oceanic islands, have not been developed into various terrestrial forms, fitted to fill the unoccupied places in their new island-homes; but Sir Charles has partly answered his own objection.
 - 18 www.darwinproject.ac.uk/letter/entry-4167 May 22d. I have been very bad & chiefly confined to bed; but will amuse myself by writing a little more to you. With respect to Bates & Wallace having distinct views on species during their Journey; what does astonish me is the extreme poverty of

- observation on this head in Wallace's book; with one discussion on very dissimilar Birds feeding alike showing, as it seemed to me, complete misunderstanding of the economy of nature.
- 19 www.darwinproject.ac.uk/letter/entry-4715 More than twenty years ago, I read in the account of your travels in the Southern hemisphere an assertion which I quoted in a book on the peat bogs (Neuchatel 1844). It is: that in the peat bogs of South America contrary to what happens in Europe, no species of mosses enters into the composition and formation of the peat: that *Astelia pumile* Br. & *Donatia Magellanica* cover nearly entirely the peat bogs of Terra del Fuego and that both these plants are the essential constituents and agents in the formation of the peat &c. I quote from memory.
 - 20 www.darwinproject.ac.uk/letter/entry-5242 When looking over some grass a species of *Bromus* from the North Western coast of America—with a friend & botanist we noticed some oats that were producing ears of barley this led to the assertion made by Elihu Burritt in his "Walk through the Eastern Counties" that a farmer at St. Ives Hunts had for years "transmuted" oats into barley. doubting this I wrote to the writer of the enclosed a farmer of 1600 acres a Member of the Council of the Royal Agricultural Society & above all a good & truthful gentleman.
 - 21 www.darwinproject.ac.uk/letter/entry-5280 We are going to have a discussion on "Mimicry, as producing abnormal sexual characters" at the Entomological tonight. I have a butterfly (a *Diadema*) of which the female is metallic blue, the male dusky brown contrary to the rule in all other species of the genus, & in almost all insects;—but the explanation is easy;—it mimics a metallic blue *Euplœa*, and so gets a protection perhaps more efficient than its allies derive from their sombre colours, & which females require much more than males.
 - 22 www.darwinproject.ac.uk/letter/entry-5416 When our discussion on Mimicry took place a most interesting little fact was mentioned by Mr. Stainton. After mothing he is accustomed to throw all the common species to his poultry & once having a lot of young turkeys he threw them a quantity of moths which they eat greedily, but among them was one common white moth (*Spilosoma menthastri*) One of the young turkeys took this in his beak, shook his head & threw it down again, another ran to seize it and did the same, and so on, the whole brood in succession rejected it!
 - 23 www.darwinproject.ac.uk/letter/entry-6140 As to "Pangenesiis", on which you wish to hear my opinion, my first impression, when for the first time I rapidly read your exposition, was the very same, which I formerly had at the first lecture of your "Origin of species", viz. that it was a fanciful speculation; but you know, that notwithstanding this first impression I am now fully convinced of the truth of the views maintained in the "Origin". The hypothesis of "Pangenesiis" would certainly account for, and connect several great classes of facts hitherto isolated and unexplained; it can also hardly be doubted, that eggs, spermatozoa, ovules of plants and pollengrains, notwithstanding their minute size and apparent simplicity, must be highly complicated structures, containing, as it were, a photograph of the whole organisation, from which they are derived.—

Appendix 3.6 Intertextuality marker: *hear*

- 1 www.darwinproject.ac.uk/letter/entry-81 Of Course you have heard of the new species of wild Swan, discovered in England, by Mr. Yarrell. I have had stuffed specimen of it, for 10s.— bad as it is, you may think yourself, lucky in getting. Yarrell himself, has pronounced it to be the new sort, so there can be no doubt.—
- 2 www.darwinproject.ac.uk/letter/entry-178 Every one has heard of the discoloured streaks of water in the Equatorial regions.— One I examined was owing to the presence of such minute *Oscillaria* that in each square inch of surface there must have been at least one hundred thousand present.—
- 3 www.darwinproject.ac.uk/letter/entry-785 The specimens on a cursory inspection appeared to possess the exact similitude to the Genus *Trinoton*. The occurrence of which on any other than water Fowl I believe has never been noticed, at all events, I never heard of an instance in which species of the same Genus were found, some on Birds others on Mammals.

- 4 www.darwinproject.ac.uk/letter/entry-789 What a curious, wonderful case is that of the Lycopodiums; I suppose you would hardly have expected them to be more varying than a phanerogamic plant. I trust you will work the case out & even if unsupported publish it, for you can surely do this with due caution. I have heard of some analogous facts, though on the smallest scale, in certain insects being more variable in one district than in another; & I think the same holds with some land-shells.
- 5 www.darwinproject.ac.uk/letter/entry-864a I am delighted to hear that you are grappling with the Galapagos insects: the more I go into the Fauna, the more peculiar it is: out of 17 land shells 16 are new species confined to the group!
- 6 www.darwinproject.ac.uk/letter/entry-924 I am glad to hear that you are hard at work again & continue to find interesting geographical results: assuredly, as you say in your Preface, geographical distrib: will be the key which will unlock the mystery of species.
- 7 www.darwinproject.ac.uk/letter/entry-951 I am very glad to hear that you mean to attack this subject some day: I wonder whether we shall ever be public combatants: anyhow, I congratulate myself in a most unfair advantage of you, viz in having extracted more facts & views from you than from any one other person.
- 8 www.darwinproject.ac.uk/letter/entry-1082 I shd. like to see sometime the war-correspondence: have you the Phytologist & cd. you sometime spare it; I wd go through it quickly. — I have not heard from Murray; I am rather sorry to hear about Dr. Holland being probably the reviewer: he does not know enough of Nat. Hist. & between ourselves he is so dreadfully conceited & vain that he never wd. condescend to learn, or to think enough of the labours of others. —
- 9 www.darwinproject.ac.uk/letter/entry-1202 I am glad to hear that you are struck with my case of the Supplemental males: I have lately reworked them most carefully. They have no mouth or stomach, but the natatory larva or rather pupa (for the larva in 2d stage in no cirripede, I find, has a mouth) fixes itself on the hermaphrodite, develops itself into a great testis! & then dies & is succeeded by a fresh crop of these temporary Supplemental males.
- 10 www.darwinproject.ac.uk/letter/entry-1480 I am very much pleased to hear that you have not given up the idea of noticing my Cirripedia volume. All that I have seen since confirms everything of any importance stated in that volume. More especially I have been able rigorously to confirm, in an anomalous species, by the clearest evidence, that the actual cellular contents of the ovarian tubes, by the gland-like action of a modified portion of the continuous tube, passes into the cementing stuff: in fact cirripedes make glue out of their own unformed eggs!.
- 11 www.darwinproject.ac.uk/letter/entry-1750 I am quite delighted to hear that you can perhaps illustrate apparent rule, about difference between species & variation of the individual. —
- 12 www.darwinproject.ac.uk/letter/entry-2060 These notions are too long to give & indeed not worth giving, as far as America is concerned, & I can see from your letter that we shd. take very much the same view. I am very glad to hear that you think of discussing the relative ranges of the identical & allied U. States & European species, when you have time.
- 13 www.darwinproject.ac.uk/letter/entry-2134 I am very glad to hear that you have been tabulating some Floras about varieties. Will you just tell me roughly the result?— Do you not find it takes much time? I am employing a laboriously careful Schoolmaster, who does the tabulating & dividing part into two great cohorts more carefully than I can.
- 14 www.darwinproject.ac.uk/letter/entry-2192 I am extremely glad to hear that you are attending to distribution in accordance with theoretical ideas. I am a firm believer, that without speculation there is no good & original observation. Few travellers have I cannot say that I am; for so very few naturalists care for anything beyond the mere description of species.
- 15 www.darwinproject.ac.uk/letter/entry-2475 I am sorry to hear that you are so much worked with many subjects & building.— Never mind about Goodnea: perhaps I cd. get some species to flower in my greenhouse.— I thought the Acacias had been crossed.—
- 16 www.darwinproject.ac.uk/letter/entry-2690 I think the importance of theory bears on opening up new fields of enquiry & in giving a rational, instead of theological explanation of many known facts I am much pleased to hear that you intend reading the Book again. If not too troublesome I shall be most grateful for any other remarks.
- 17 www.darwinproject.ac.uk/letter/entry-2711 I am extremely much pleased to hear that you like my Book. I look at every geological believer of the mutation of species as a most important gain.—

- There will be a long & stiff battle before such doctrines are generally admitted. The progress of subject will now depend far more on such men as yourself than on anything I can do.—
- 18 www.darwinproject.ac.uk/letter/entry-2928 I hope I have not blundered likewise in its coexistence with extinct species. What horrid blundering. I am grieved to hear that you think I must work in the notes in text; but you are so much better judge, that I will obey. I am sorry that you had trouble of returning Dog. M.S,—which I suppose I shall receive tomorrow.—
- 19 www.darwinproject.ac.uk/letter/entry-2931 I am very glad to hear about the Germans reading my Book. No one will be converted, who has not independently begun to doubt about Species.— Is not Krohn a good fellow? I have long meant to write to him. He has been working at Cirripedes & has detected 2 or 3 gigantic blunders, but in very difficult points. & about which, I thank Heaven, I spoke rather doubtfully, such difficult dissection that even Huxley failed.
- 20 www.darwinproject.ac.uk/letter/entry-2993 I am delighted to hear that you, with all your large practical knowledge of Nat. History, anticipated me in many respects & concur with me.— As you say I have been thoroughly well attacked & reviled, (especially by entomologists, Westwood, Wollaston & A. Murray have all reviewed & sneered at me to their hearts' content) but I care nothing about their attacks; several really good judges go a long way with me, & I observe that all those who go some little way tend to go somewhat further.
- 21 www.darwinproject.ac.uk/letter/entry-3086 I am most uncommonly pleased, I can assure you, to hear that you are become “heretical” on species.— I perceived with no surprise that you were at first dead against me: in fact I cannot say that I respect anyone who has knowledge & can change his opinion suddenly on such a point.— If any fair & good opportunity occurs in any publication, I hope that you will say a word on our side: for I observe that those opposed write vehemently, & those on our side are silent; consequently the public have no means of knowing how many are on our side.
- 22 www.darwinproject.ac.uk/letter/entry-3854 It pleases me much to hear that you are not a believer in the immutability of species,—a doctrine perfectly adapted to stop philosophical research.— I trust that you some day will write on variation in Butterflies, & express your beliefs on the subject of species.—
- 23 www.darwinproject.ac.uk/letter/entry-3945 I am heartily glad to hear that your great labours over your Book are drawing to a close: I know that I for one shall read it with real interest.— Pray thank Wallace when you see him about Melastomas; I have in truth given more trouble on this, than case deserves, & am truly obliged to you. The fact is I cannot endure being beaten by a beggarly flower to the degree, which these confounded Melastomas have beaten me.—
- 24 www.darwinproject.ac.uk/letter/entry-4024 Cordial thanks for your deeply interesting letters about Lyell, Owen & co. I cannot say how glad I am to hear that I have not been unjust about species-question towards Lyell. I feared I had been unreasonable.
- 25 www.darwinproject.ac.uk/letter/entry-4046 I have received your kind letter of the 31st. January, and am very glad to hear that my Orchid sketches & obsns. interested you. You are quite right in supposing that there is no movement of the pollinia in the species I have described; I imagine that such motion is chiefly found in those species which have a cap of membrane over the viscid matter of the pollinia discs in situ,—certainly none of the Cape species that I have seen (all of which have the discs naked) present any phenomenon of the kind.
- 26 www.darwinproject.ac.uk/letter/entry-4167 I am heartily rejoiced to hear that you intend to try to bring L. & F. together again: but had you not better wait till they are a little cooled? you will do science a real good service. Falconer never forgave Lyell for taking the Purbeck bones from him & handing them over to Owen.—
- 27 www.darwinproject.ac.uk/letter/entry-4185 I have been glad to hear about Mercurialis; but I will not accept your offer of seed on account of time, time, time, & weak health. For same reason I must give up Primula matter. What a wonderful indefatigable worker you are! You seem to have made a famous lot of interesting experiments. D. Beaton once wrote that no man could cross any species of Primula, you have apparently proved the contrary with a vengeance.—
- 28 www.darwinproject.ac.uk/letter/entry-4569 I naturally feel much curiosity on the progress of opinion on the descent of species, & I am delighted to hear that the subject is progressing in Germany which so abounds with great naturalists.— But what you tell me about yourself interests me the most, & I thank you sincerely for your confidence. I feel what you say in praise of my book &

- your intention of carrying onwards & perfecting the subject, as by far the greatest honour which could be paid me.
- 29 www.darwinproject.ac.uk/letter/entry-4763 I am much obliged for yr letter & am particularly glad to hear that you are going to experimentise on Passifloras. & perhaps publish the results. If published I shd very much wish for a copy.—
 - 30 www.darwinproject.ac.uk/letter/entry-4786f I am pleased to hear that you have been interested in my work on the Origin of Species. Your law of inheritance I dare say is true & may have some bearing on the modification of Species; but I fear it would be difficult to prove it with sufficient clearness to allow of its use
 - 31 www.darwinproject.ac.uk/letter/entry-5007 It seems that I erred greatly about some of the plants on the Organ Mts., but I am very glad to hear about Fuchsia &c. I cannot make out what Hooker does believe, he seems to admit the former cooler climate, & almost in the same breath, to spurn the idea. To retort Hooker's words "It is inexplicable to me" how he can compare the transport of seeds from the Andes to the Organs Mts. with that from a continent to an island: not to mention the much greater distance, there are no currents of water from one to the other, & what on earth shd make a bird fly that distance without resting many times.
 - 32 www.darwinproject.ac.uk/letter/entry-5097 I have almost finished correcting the new Ed. of the Origin & I am pleased to hear that my labour will be so much the more advantageous as a 3rd German Ed. is immediately to be printed revised by Prof. Leonhard. As you feel interested on the subject, I may mention that I have lately read two pamphlets in our favour, by good men, one by Oscar Schmidt & the other by Carl Nägeli.
 - 33 www.darwinproject.ac.uk/letter/entry-5202 I am delighted to hear that you keep on your zealous love & work for Nat History & British Assoc— New discussion—on modification of species
 - 34 www.darwinproject.ac.uk/letter/entry-5499 In that case nothing wd give me higher satisfaction than that Schweizerbart shd arrange with you, if that be possible, for a translation; for I have often heard of the fame of your excellent translations. My present work I greatly fear is of much greater length than value.
 - 35 www.darwinproject.ac.uk/letter/entry-5618 I am very glad indeed to hear that you take an interest in my few essays written rather by a learner than a teacher. In all of them I have tried as far as possible to break down the barrier between Pleistocene and living species, by attaching weight to individual variation, and to results of living under different conditions. Thus the Pleistocene animals utterly extinct amount only to about 10 or 11 out of 56.
 - 36 www.darwinproject.ac.uk/letter/entry-5794 I am delighted to hear that you intend to consider the relations of fossil animals in connection with their genealogy,; it will afford you a fine field for the exercise of your extensive knowledge & powers of reasoning.
 - 37 www.darwinproject.ac.uk/letter/entry-5920 Lately again I have had the pleasure to hear that two of my correspondents have become ardent followers of your theories: Dr Speyer, known as a perfect connoisseur of Germain Lepidoptera is about to show the descent of the Lepidoptera from the Phryganidae (I have sent him my collection of Westfalian Phryganidae for his exploration); another friend of mine, Röse, is about to explain the genealogical connection of some mosses. Every one who has been instructed by the reading of your works is full of admiration and of thanks for you.
 - 38 www.darwinproject.ac.uk/letter/entry-6075 I am delighted to hear that you uphold the doctrine of the Modification of Species, and defend my views. The support which I receive from Germany is my chief ground for hoping that our views will ultimately prevail. To the present day I am continually abused or treated with contempt by writers of my own country; but the younger naturalists are almost all on my side, and sooner or later the public must follow those who make the subject their special study.
 - 39 www.darwinproject.ac.uk/letter/entry-6091 Many thanks for your last M.S. I am very glad to hear about the colours of Hylobates. Unfortunately, I cannot find in Isidor Geoffroy Hist. des Anom. any statement in regard to which digits in man are most liable to syndactylism; if I cd ascertain this point your remark wd be most interesting. I knew about the variability of the junction of the digits in some of the species.
 - 40 www.darwinproject.ac.uk/letter/entry-6292 I am glad to hear that you are going to touch on the statement that the belief in Nat. selection is passing away; I do not suppose that even the

- Athenæum wd. pretend that the belief in the common descent of species is passing away, & this is the more important point. This now almost universal belief in the evolution (somehow) of species I think may be fairly attributed in large part to the "Origin."
- 41 www.darwinproject.ac.uk/letter/entry-6390 Almost the one exception, as far as I know is Mr. Gaudry—& I think he will be soon one of the Chief Leaders in Zoological Paleontology in Europe & now I am delighted to hear that in the sister department of Botany you take nearly the same view.
 - 42 www.darwinproject.ac.uk/letter/entry-6427 I am very glad to hear that you have been arguing against Nägeli's law of perfectibility which seems to me superfluous. Others hold similar views, but none of them define what this "perfection" is, which cannot be gradually attained through Natural Selection.
 - 43 www.darwinproject.ac.uk/letter/entry-8719 I am glad to hear that *D. filiformis* catches only small insects, as I suspected this. I have observed with care several other species of *Drosera*.

Appendix 3.7 Intertextuality marker: *mention, state*

- 1 www.darwinproject.ac.uk/letter/entry-421 Meanwhile I will just state that with a few exceptions the Chalcidites of S. America, & also those of Australia are remarkably like the European species, many of them are very nearly allied to the British species, & 2 or 3 are absolutely identical with the latter.
- 2 www.darwinproject.ac.uk/letter/entry-591 I write now to inform you that I have probably led you into one small error— I stated that the Fungus grows on the *Fagus antarctica*, but I am almost certain that the *F. betuloides* is the common tree of Tierra del Fuego, & as this Fungus abounds every where it must grow on this latter species— it may (& I believe does) grow on the *F. antarctica*.
- 3 www.darwinproject.ac.uk/letter/entry-718 I have one criticism to make about your circles —that is that I think you are bound to state that they do not necessarily represent (without you think they do) groups of equal value & though all touching, the affinities are not necessarily equally strong.— I believe infinite harm has been done by these circles, which catch the eye as of equal size, & inevitably lead the mind to suppose they are of equal value— it is by this artifice, as I believe, the possibility of making the Quinarian system appear probable has chiefly rested: Moreover it shd be stated by everyone, I think, who indulges in these vicious circles, that confessedly there is no standard to judge of the value of groups.— Who can prove that the woodpecker are not a group of equal value with the Hawks.— I suspect that number of species, ie amount of variation of one common type does silently come into play in estimating the value of groups.—
- 4 www.darwinproject.ac.uk/letter/entry-722 P.S. I have been looking at my poor miserable attempt at Botanical-landscape-remarks, & I see that I state that the species of Beech which is least common in T. del Fuego, is common in the forest of central Chiloe. But I will enclose for you this one page of my rough journal.—
- 5 www.darwinproject.ac.uk/letter/entry-736 Is it so with those plants, which are peculiar to this archipelago; you state that their numerical proportions are continental (is not this a very curious fact?) but are they related in forms to S. America.— Do you know any other cases of an Archipelago, with the separate islands possessing distinct representative species?
- 6 www.darwinproject.ac.uk/letter/entry-788 I have not forgot a question you once propounded to me as to whether the species of large-groups-which-were-local were local also, & am inclined to think that species of local large nat. ords. are local also, Cacti I think I mentioned before as an example & Proteaceæ & Epacridæ I take to be two others.
- 7 www.darwinproject.ac.uk/letter/entry-789 In my Journal, (p. 342) I see I state that in South Chiloe at height of about 1000 ft the forest had a Fuegian aspect: I distinctly recollect, that at sea-level in middle of Chiloe, the forest had almost a tropical aspect. I shd. like much to hear, if you make out, whether the N. or S. boundaries of a plant are the most restricted; I shd have expected that the S. would be, in the temperate regions, from the number of antagonist species being greater.

- 8 www.darwinproject.ac.uk/letter/entry-974 I do not quite understand why you state that the species return in each country to a few typical forms, instead of supposing that the same typical forms have been originally widely spread, & have in each country varied a little. —
- 9 www.darwinproject.ac.uk/letter/entry-1093 I, also, return Silliman & am glad to have seen Dr. Morton's article: my opinion, of it, as you ask for it, is that it is in main part, a merely tabulated compilation from Griffith's Cuvier, with a few other facts interpolated. He is, I think, too credulous; but it is a pretty good compilation: his worse fault is that he has not gone to his original source,— thus he makes a great flourish about perfect fertility of the genus *Crax*: whereas the original authority, namely Temminck says speaking of these hybrids, "a great number are quite sterile, others have bred once, & a smaller number have produced a good many young," & it is not stated that the crosses have been inter-se; on the other hand between two other species of *Crax* it is stated that the crosses were with either parent: no doubt by these crosses a multitude of hybrids have been produced, but there is a want of exactness in the manner Morton gives the facts.
- 10 www.darwinproject.ac.uk/letter/entry-1226 Now I, in my last letter, not knowing the history of the word *Lepas*, followed the last principle, but as you state that old authors used it only in a pedunculate sense, of course *L. anatifera* is the type of *Lepas*, Linn. & accordingly *Lepas* restricted, should, as you propose, supersede *Anatifera*, *Anatifa*, &c.
- 11 www.darwinproject.ac.uk/letter/entry-1317 I shd feel particularly grateful, if you would take the trouble to answer me the following questions pretty soon.— (1) *Skania* age of beds? Von Buch states that only Upper Chalk occurs in north. (2) Is White Chalk of Denmark same age or stage with Faxoe. (3) Do you know what the age of the Kreidemergel Grunsand (?) of Saliberg Quedlingburg is? I suppose it is in Westphalia.— (4) Is *Legina Thy* in Denmark? Do you feel pretty sure that the large valv. lat. sup. (named *P. lævis* & figured 9. Pl. V.) belongs to a loose valve marked *P. lævis* in softer white chalk in same little box with *Anatifera cretæ*.— 5th & lastly, Do you feel pretty sure, & may I state so, that your *Anatifera cretæ* had only five valves?—
- 12 www.darwinproject.ac.uk/letter/entry-1480 I am very much pleased to hear that you have not given up the idea of noticing my *Cirripedia* volume. All that I have seen since confirms everything of any importance stated in that volume. More especially I have been able rigorously to confirm, in an anomalous species, by the clearest evidence, that the actual cellular contents of the ovarian tubes, by the gland-like action of a modified portion of the continuous tube, passes into the cementing stuff: in fact cirripedes make glue out of their own unformed eggs!.
- 13 www.darwinproject.ac.uk/letter/entry-1529 Now for a few mere details,— diag p. 30. Bory St Vincent *Voyage dans les quatre principales îles des mers d'Afrique* (1801. 1802).— p. 31. Lindley quite lately stated that the sexual structure of Ferns as just discovered, showed that hybridising was very possible.— p. 33. "such a view", I think this expression wants expanding p. 37. "from whence" I hate this expletive; is not whence enough?
- 14 www.darwinproject.ac.uk/letter/entry-1664 I wish I had noted these & various other points more thoroughly when I was there; indeed I never can investigate any thing connected with the Floras of the Cape or of South America, without deeply regretting that I did not make better use of my opportunities. You may like, however, to have one or two instances of what I last mentioned.
- 15 www.darwinproject.ac.uk/letter/entry-1670 www.darwinproject.ac.uk/letter/entry-1685 I should mention that Pursh, from the inspection of Peck's herbarium, gave us two alpine species in his *Flora* that no one has verified; indeed, we may now say that they do not grow on the White Mts. viz. *Alchemilla alpina* & *Dryas integrifolia*:—though it was not unlikely that they should occur. They ought therefore to be excluded.
- 16 www.darwinproject.ac.uk/letter/entry-1735 You are doubtless aware that, for the most part, the *Estreldinæ* represent in India & the Malay countries, exclusively in Australia, & to a great extent in Africa, the *Fringillinæ* of northern climes (inclusive of the Himalaya); & we have also 3 species of *Ploceinæ*, one of which (with a fourth) is likewise found in Java, the rest of this group being African— Mentioning the *Estreldinæ* reminds me of the common "al Munia" (as it is here termed), respecting which poor Strickland asked me if I could inform him of the origin of its English name *Amadavat* (whence the specific name *amandava*, auct., & the generic name *Amadina*, Sw.). I traced its etymology distinctly in old Fryer's work (1698), where these little birds are stated to be brought to Sarat from "Amidavad"; meaning Ahmedab; ad in Guzurat, where they still abound! And so the name of that town has become transferred to themselves, as in the more

- familiar instances of Canary & Bantam! N.B. You will find “avadavats” mentioned in Sheridan’s ‘School for Scandal’, 1777—act. V, sc. I; so that at that time the name must have been familiar.
- 17 www.darwinproject.ac.uk/letter/entry-1764 Posted with this sheet, but apart; is a copy of the London Catalogue of British plants. In the 3rd. paragh. within the Wrapper (ii) it is stated that the vars. there retained are (with few exceptions) the species of some botanists. By striking out those exceptions in the copy sent, we get the foundation for a list of debateable species (dubieties: i.e. varieties or species).
 - 18 www.darwinproject.ac.uk/letter/entry-1803 Hence, also, as he asserts, the seedlings of one variety can never be confounded by an experienced eye with those of another variety, being as distinct as were their parents. Moreover, he states that the fruit of seedling Pears and Apples, though differing greatly in size, succulency, and flavour from those of their parents, yet resemble them in the more important characters of form and in the nature of their seeds.
 - 19 www.darwinproject.ac.uk/letter/entry-1835 There is a notice (I think in the journal (geological)) of some deposits in which I believe it is mentioned that a stratum of extinct freshwater shells, cover a deposit of marine mollusca of still existing species. I believe it is alluded to to by Sir R. Murchison in his Alps paper, but I have not the reference.
 - 20 www.darwinproject.ac.uk/letter/entry-2030 The connateness of elytra (as having merely a character, & not an organ) will not perhaps suit you.— otherwise I might mention that the only Harpalus (I believe) on record in which the wing-cases are ever joined is the *H. vividus* of the Madeiran Islands (opus diab. p.p. 56, 57); but that character (anomalous as it is) does not always occur in that species,—the elytra being sometimes connected, sometimes sub-connected, & occasionally almost (if not entirely) free.—
 - 21 www.darwinproject.ac.uk/letter/entry-2235a (1) In your work on Bees, you state that you have seen stray workers inhabiting the nest of a distinct species. Have you ever seen a stray fertile female *Bombus* inhabiting the nest of other species?
 - 22 www.darwinproject.ac.uk/letter/entry-2359 And as he states that there is no indigenous bee (perhaps this statement applies to bees resembling hive or humble bees, for some other genera are known to inhabit New Zealand), the fact that these plants seeded freely at first appears quite fatal to my doctrine. But Mr. Swale adds that he believes that three species of a wasp-like insect performed the part of bees, before the introduction of the latter; unfortunately he does not expressly state that he has seen them sucking the flower.
 - 23 www.darwinproject.ac.uk/letter/entry-2749 But I have been assured by Mr Cattell of Westerham, that the several vars. of Sweet Pea can be raised close together for a number of years without intercrossing. But on other hand he stated that they go over the beds, & pull up any false plant which they very naturally attribute to wrong seeds getting mixed in the lot.—
 - 24 www.darwinproject.ac.uk/letter/entry-3658a Having seen it stated that the cells were larger, I procured (through the kindness of Mr. R. Hill, of Spanish Town), some bees and comb. The bees have been carefully examined by Mr. F. Smith, of the British Museum, and pronounced to be the common species.
 - 25 www.darwinproject.ac.uk/letter/entry-3805 Yet when I call to mind the state of the placenta in *A. luteola*, I am astonished that they should produce ovules. You will see in my book that I state that I did not look at the ovarium of *A. Loddegesii*. Would you have the kindness to send me word, which end of the ovarium is meant by apex (that nearest the flower?) for I must try & get this species from Kew & look at its ovarium.
 - 26 www.darwinproject.ac.uk/letter/entry-3929 You most kindly permit me to mention any point on which I want information. If you are so inclined, I am curious to know from systematic experiments, whether Mr. D. Beaton’s statement that the pollen of two shortest anthers of scarlet *Pelargonium* produce dwarf plants, in comparison with plants produced from same mother-plants by the pollen of longer stamens from same flower.
 - 27 www.darwinproject.ac.uk/letter/entry-4142 I hope that you will grant me space to own that your Reviewer is quite correct when he states that any theory of descent will connect, “by an intelligible thread of reasoning,” the several generalizations before specified.
 - 28 www.darwinproject.ac.uk/letter/entry-4881f In *Cardiospermum* you state that the common peduncle which bears the subpeduncles with the flower-buds and the pair of short tendrils, although it spontaneously revolves, does not bend on contact or contract spirally; hence it may be

- worth mentioning, as showing a difference in the action of the tendrils in related genera, that in *Serjania* the common peduncle contracts spirally when the single tendril which it bears has clasped, as frequently happens, the plant's own stem.
- 29 www.darwinproject.ac.uk/letter/entry-5181 Beatson's bird appears to be one of the Gallatores & such ought not to be called land birds & in my journal I especially exclude them, but state that they are the first immigrants on almost every island. At p. 422 of *Origin* you will find something about land birds on islands, which with respect to Madeira stands in *New Ed.* thus corrected.
 - 30 www.darwinproject.ac.uk/letter/entry-5567 But what extremely concerns me, is R. statement that I require million of years to make new species; but I have not said so, on contrary, I have lately stated that the change is probably rapid both in formation of single species & of whole groups of species, in comparison with the duration of each species when once formed or in comparison with the time required for the development of a group of species— with respect to Classification, it is the idea of a natural classification, which the genealogical explains.
 - 31 www.darwinproject.ac.uk/letter/entry-5649 I have done nothing worth mentioning this summer, as all my time has been consumed in correcting horrid proof sheets. I may mention one little fact which may possibly interest you. A man in Natal sent me a little packet by post of the dung of locusts with the statement that it was believed that locusts brought new plants to the districts which they visited. Six Grasses, belonging to at least two species have germinated out of the dung, & the seeds were fairly enclosed in the little pellets, as I ascertained by dissection. This verifies what I said in the *Origin*, that many new methods of transport wd be discovered; for locusts are often blown many 100 miles out to sea.
 - 32 www.darwinproject.ac.uk/letter/entry-5840 First of all, allow me to express my most cordial thanks to you for kindly sending me your book "on the variation of animals and plants". Your rich and multifaceted knowledge in all branches of biology and especially in the natural history of domestic animals and cultivated plants has amazed me anew, and I was most keenly interested in many of the facts you mentioned that are new and unknown to me. Still, all the special proofs that you produce for the theory of *selectio naturalis* do not hold the same kind of interest for me as they will for most readers.
 - 33 www.darwinproject.ac.uk/letter/entry-5893 Mr Stainton said that observations of numerical proportion of the sexes, in collecting were quite unreliable, for it was a well-known fact that in collecting *Tineina* moths the females were nearly in all species very scarce, whereas in breeding the females proved to be more numerous—as 2 to 1 to the males. Mr F. Smith also stated that observations in the field were of little value. He quoted some most interesting experiments he was making in breeding some British Sawflies—all the individuals turning out to be females. These I think will prove to be cases of parthenogenesis: Mr Smith will write to you himself.
 - 34 www.darwinproject.ac.uk/letter/entry-6281 The following quotation from the *Ibis* for this month will interest you, & probably you have not yet heard of the discovery of *Pavo nigripennis*, Sclater, in Cochin China— Swinhoe writes—"In the aviary of the Prefect of Hainan I saw Sclater's peafowl (*Pavo nigripennis*), which the Prefect assured me came from Annam or Cochin China (proper). There is a pair of the same species at this moment in a bird shop here" (Hong Kong); "and I now believe *P. nigripennis* to be the species known as the 'bird of Confucius', the train feathers of which are worn in mandarin's hats as tokens of merit. Chinese works state that the peacock occurs in the west of China, bordering Cochin China".

Appendix 3.8 Intertextuality marker: *to object*

- 1 www.darwinproject.ac.uk/letter/entry-425 In one point of view, your grand discovery proves, I think, in the most striking manner, the weight of my principal objection to the argument of De Beaumont. You remember that I denied that he had proved that the Pyrenees were elevated after the cretaceous period, although it is true that the chalk has been carried up to their summits, and lies in inclined beds upon their flanks; for who shall say that the movement was not going on during

- the cretaceous period? Now in your lines of elevation, there will doubtless be coralline limestone carried upwards, belonging to the same period as the present, so far as the species of corals are concerned. Similar reefs are now growing to those which are upraised, or are rising.
- 2 www.darwinproject.ac.uk/letter/entry-685 I see our argument about the Marsupiata has become almost a verbal one— I have not the slightest objection to the Monotremata & other common Marsupials being united, as I said, under one main group, & that group being called Marsupiata, if I was wrong in supposing that term already used for the common marsupials & in that case I shd have thought some other term ought to have been invented for the group containing the common marsupiata & monotremata. What I should, according to my small proportion of knowledge, object to, would be the putting the monotremata into the same group with the Marsupiata, not from their resemblance but from the Monotremata consisting of only two species.— Even this may be right, but I revert to my old opinion, that all rules for a natural classification are futile until you can clearly explain, what you are aiming at.
 - 3 www.darwinproject.ac.uk/letter/entry-688 But as the term ‘natural’ certainly has a very vague meaning when thus used, I have no objection to apply the word ‘useful’ instead— In using the word natural for a kind of classification I merely follow others— it is no doubt presumptuous— But after all I do not see why we should not set up a kind of artificial standard of perfection in classification—
 - 4 www.darwinproject.ac.uk/letter/entry-718 These Mammalian groups, partake of some of the important characters (which no one would consider as merely adaptive) of Birds & Reptiles & I cannot see any objection to considering them as links— I do not know, however, whether you would object to calling them so, as long as they are clearly admitted amongst the Mammalia.— You speak rather undecidedly whether all such affinities are “adaptive”.— I quite admit the validity of this distinction, but it appears to me so vague, when applied to organs of such importance as the intestines & teeth of the wombat, that I think you are bound either clearly to define (a tough job I expect) the distinction or to confess that it is vague.—
 - 5 www.darwinproject.ac.uk/letter/entry-939 I see no objection to this addition being inserted, provided that the fact of Mr. Cuming having found a living species of *Struthiolaria* at Arica be given on the authority of Mr. Cuming himself and not on mine.
 - 6 www.darwinproject.ac.uk/letter/entry-961 It is quite curious how our opinions agree about Forbes views; I was very glad to have your last letter, which was even more valuable to me than most of yours are & that is saying, I assure you, a great deal.— I had written to Forbes to object about the Azores on the same grounds, as you had, & he made me some answer, which partially satisfied me, but really I am so stupid I cannot remember it. ... He had one other reason, to my mind still less trust worthy.— I had also written to Forbes, before your letter, objecting to the Sargassum, but apparently on wrong grounds; for I could see no reason, on the common view of absolute creations, why one *Fucus* shd not have been created for the ocean, as well as several *confervæ* for the same end.
 - 7 www.darwinproject.ac.uk/letter/entry-964 Certainly there is no objection to the hypothesis of a Sargassum being an absolute creation, though I see no reason to call for such an aid in this case, the species being in my opinion decidedly the littoral Atlantic one
 - 8 www.darwinproject.ac.uk/letter/entry-989 The Conception Nautilus I cannot satisfactorily identify with any other. I propose to call it *N. D’Orbignyianus* if you have no objection. It comes very near some lower & middle cretaceous species.
 - 9 www.darwinproject.ac.uk/letter/entry-1216 II. I have next to notice your second objection—that retaining the name of the first describer in perpetuum along with that of the species, is a premium on hasty & careless work. This is quite a different question from that of the law of priority itself, and it never occurred to me before, though it seems highly probable, that the general recognition of that law may produce such a result.
 - 10 www.darwinproject.ac.uk/letter/entry-1223 If however there is the least chance of developing this silly vanity into an honourable ambition for the advancement of knowledge, by means of your proposed plan of quoting good Books in preference to first descriptions, I am sure I have no objection to it, and if you like to try the plan in your Cirriped work, I shall look with much interest to see how the plan answers.—
 - 11 www.darwinproject.ac.uk/letter/entry-1251 I believe I told you that I require to disarticulate a specimen of each species; without this being done it is impossible to describe or recognise the

- species: I trust you will not object to this; should you do so, there will be time to inform me. The *Ibla* is a species (not published) which I have named *I. Cumingii* & was found in numbers by Mr Cuming at the Philippines: I have never seen it from elsewhere, except the specimen from Tavoy sent by you: it is in some Anatomical respects, perhaps the most interesting cirripede in the world.
- 12 www.darwinproject.ac.uk/letter/entry-1359 I should esteem it a great favour & most valuable aid if you would entrust me with your collection of recent species; but you will almost certainly object to send me unique specimens, when I tell you that I cannot recognise the species, & will never name any species, without disarticulating one specimen; I can, however, almost always gum together the valves so that they look nearly as well as ever.—
 - 13 www.darwinproject.ac.uk/letter/entry-1473 Have you published any name: in my M.S. I have called it, *Acasta sporillus*: do you object to this, supposing no name yet published? I presume I may state that I owe the specimens to you.?
 - 14 www.darwinproject.ac.uk/letter/entry-1611 My grave objection to the whole process is, that if you multiply the anomalous species by 100 & divide the normal by the same you will then reverse the names, for anomaly is to a great extent synonymous with rarity or representation by few species. EG. as plants go now adays, *Lepidodendron* is an anomaly; but as years ago i.e. in the coal epoch no doubt a *Rose* would have been an anomaly— also substituting space for time & we have *Proteaceæ* anomalies in the S. American Flora & *Fuchsias* in the Australian—
 - 15 www.darwinproject.ac.uk/letter/entry-1612 I do not quite agree with your “grave objection to the whole process” which is “that if you multiply the anomalous species by 100, & divide the normal by the same, you will then reverse the names” For, to take an example, *ornithorynchus* & *Echidna* would not be less aberrant if each had a dozen (I do not say 100, because we have no such cases in animal kingdom) species instead of one.
 - 16 www.darwinproject.ac.uk/letter/entry-1694 It is not necessary to suppose that (granting this true) there has been a progressive developement (by species or by varieties); ie that Grasses were created before Lilies &c; for we may assume that the lilies &c which coexisted with the earliest created existing grasses have been killed, & replaced by others. The objection to this is that it would argue a gradual spread & monopolization of the soil by the lowest orders, for which there is much to be said in favor.
 - 17 www.darwinproject.ac.uk/letter/entry-1774 I am not at all sure that I understand myself!! my objection to *Decaisne* & I believe it is only to his not boldly calling his sous-especies either vars. or species.
 - 18 www.darwinproject.ac.uk/letter/entry-1989 I shall not consider all your notes on my M.S. for some weeks, till I have done with crossing; but I have not been able to stop myself meditating on your powerful objection to mundane cold period, viz that many fold more of the warm-temperate species ought to have crossed the Tropics than of the sub-arctic forms.— I really think that to those who deny modification of species, this would absolutely disprove my theory.
 - 19 www.darwinproject.ac.uk/letter/entry-1991 Look at the two Journeys which the Arctics have had from N. to S. & S. to North, with no change, as may be inferred, if my doctrine is correct, from similarity of Arctic species in America & Europe & in the Alps.— But I will not weary you; but I really & truly think your last objection is not so strong as it looks at first. You never make an objection without doing me much good.—
 - 20 www.darwinproject.ac.uk/letter/entry-2060 Now I have thought that you would not object to my sending the latter half of your note with list of such American genera to Mr. H. C. Watson, of whose great clearness of mind & acuteness I have from long correspondence the highest opinion.—
 - 21 www.darwinproject.ac.uk/letter/entry-2239 Of course I do not suppose with groups of plants so widely extended as they are, that there ever shd. be such difference, as there might be in case of Mammals. Therefore I agree that orders in a *Prodromus* not obeying my rule as with *Labiatae* & *Verbenaceae* is a serious objection; though not nearly so fatal, in my opinion, if in a local Flora.— I was led to all this work by a remark of *Fries*, that the species in large genera, were more closely related to each other than in small genera; & I thought if this were so, seeing that varieties & species are so hardly distinguishable, I concluded that I shd. find more varieties in the large genera than in the small: but at first, seeing the many causes of doubt, I certainly did not expect to find more than three-fourths of the Floras, yielding the result, which they have.—

- 22 www.darwinproject.ac.uk/letter/entry-2240 Ledebour shall go tomorrow you are most welcome to it whenever you want it always. I quite see in what respect local Floras are much the best suited to your purpose; or rather, how they would be so, if they were worked out upon the same principles as the general Floras, but the fact that they are not so, and that they are hotbeds of bad big genera, is a very serious objection to the use of them. I cannot so well see, & in plain truth I cannot at all appreciate your objections to such a monograph as that on *Urticeæ*. Are not the number of varieties registered by Weddell very many? From what I know of *Urticeæ* I think it would not be difficult so to alter Weddells genera & species as to suit your views—thus—by raising to species a few of the varieties in those genera with 6–7 species & several varieties—*Parietaria* for instance, which would thus come under the genera with more than 10 species, & materially alter the result.
- 23 www.darwinproject.ac.uk/letter/entry-2307 I have still one objection to discuss—if as many middling sized genera are decreasing as are increasing how do we know which are which.— Do the species of big genera run to varieties in decreasing as in increasing. perhaps they should do so—yet you say “it is idle to discuss this”!
- 24 www.darwinproject.ac.uk/letter/entry-2488 A friend objected to my title that word “Varieties” ought to stand before “Species”.— Another friend objected (but illogically) that “genera” & “orders” ought to be inserted.— This has led me to think that word “Varieties” had better be altogether omitted. The case of Species is the real important point; & the title, as now, is rather too long.— So if you do not object, I will omit word “Varieties”; but if on any account you do object, I do not care.—
- 25 www.darwinproject.ac.uk/letter/entry-2649 I am surprised considering how ignorant we are on very many points, that more weak parts in my Book have not as yet been pointed out to me. No doubt many will be. H. C. Watson founds objection in M.S. on there being no limit to infinite diversification of species: I have answered this I think satisfactorily, & have sent attack & answer to Lyell & Hooker. If this seems to you good objection, I would send papers to you.—
- 26 www.darwinproject.ac.uk/letter/entry-2703 With respect to your objection of multitude of still living simple forms, I have not discussed it anywhere in the Origin, though I have often thought it over.— What you say about “progress being only occasional & retrogression not uncommon” I agree to; only that in animal kingdom I greatly doubt about retrogression being common.— ... I think I must in any future Edit. discuss a few more such points— & will introduce this & H. C. Watson’s objection about infinite number of species—& the general rise in organisation. But there is a directly opposite objection to yours, which is very difficult to answer, viz how at first start of life, when there were only simplest organisms, how did any complication of organisation profit them? I can only answer that we have not facts enough to guide any speculation on the subject.—
- 27 www.darwinproject.ac.uk/letter/entry-2741 Your fifth objection (p. 21) shows me that you think my idea of the spreading of the dominant species & their subsequent multiplication not satisfactory.—
- 28 www.darwinproject.ac.uk/letter/entry-2927a Falconer has been holding forth today on the diffit, Mastodons & Elephants not coming in chronologically as they shd do, according to yr views, but when one sees the new Maltese dwarf intermediate between *E. antiquus* & *E. meridionalis* & Anca’s new Sicilian cave elephant, a modification of the living Indian one leaning towards *antiquus*, & when one thinks that Falconer can distinguish all American varieties of Mammals from all European fossil species, I confess I attach little value to the objection.
- 29 www.darwinproject.ac.uk/letter/entry-2932a What you say as to my difficulty is I suspect an explanation up to a certain point, but I hope to put the whole more clearly soon. Dogs of multiple origin & leporines would greatly weaken the objection to regarding the negro as of a different species, in the same sense that the Prairie wolf & common wolf may be. I should like a good naturalist to give me a list of reputed species in Mammalia not more remote than negro & white man. Would they not be many?
- 30 www.darwinproject.ac.uk/letter/entry-3036 I think little of his objection on the score of varieties of the lineal descendent of *A.* becoming confounded with those of no doubt such cases occur I have somewhere I think alluded to them as very probably frequent. now put into this category all those anomalous plants which hover between two otherwise very remote species in genera or families. It struck me that such cases were well explained by your divergent series— Such cases if decided ones should be rare— so they are—

- 31 www.darwinproject.ac.uk/letter/entry-3131 This leads me to remark how singularly few have judged the argument on right principles many complain that I have not proved that any one species changes into another and they ignore the fact that the view given, apparently groups together and explains many phenomena. No one urges as a fatal objection to the Theory of Light that the undulations in the ether cannot be proved—or the very existence of ether, yet because the undulatory theory explains much it is now universally admitted—
- 32 www.darwinproject.ac.uk/letter/entry-3223 The view that each variation has been providentially arranged seems to me to make natural selection entirely superfluous, & indeed takes whole case of appearance of new species out of the range of science. But what makes me most object to Asa Gray's view, is the study of the extreme variability of domestic animals.— He who does not suppose that each variation in the Pigeon was providentially caused, by accumulating which variations man made a Fantail, cannot, I think, logically argue that the tail of the Woodpecker was formed by variations providentially ordained.— It seems to me that variations in the domestic & wild conditions are due to unknown causes & are without purpose & in so far accidental; & that they become purposeful only when they are selected by man for his pleasure, or by what we call natural selection in the struggle for life & under changing conditions.
- 33 www.darwinproject.ac.uk/letter/entry-3480 Another objection to my line of argument is the changes wrought in bees by either feeding or heat (as the case may be).— but this again is change of individual, & is not propagated, for the Queen after all lays again males & drones, not Queens. Darwin I believe holds with you as to the influence of external conditions on the variation of the brood. I have however failed to be convinced by him of it, & I do not think he recognises the facts of variation to the extent I do.
- 34 www.darwinproject.ac.uk/letter/entry-3831 You have not, as you ought, begun by attacking old false doctrines, that “like does produce like” the first chapter of your book should have been devoted to this & to nothing else. But there is some truth I now see in the objection to you, that you make N.S. the “Deus ex machina.” for you do somehow seem to do it.—by neglecting to dwell on the facts of infinite incessant variation,— Your 8 children are really all totally unlike one another they agree Exactly in no one property how is this?
- 35 www.darwinproject.ac.uk/letter/entry-4108 Dr. Carpenter seems to think that the fact of Foraminifera not having advanced in organization from an extremely remote epoch to the present day is a strong objection to the views maintained by me. But this objection is grounded on the belief—the prevalence of which seems due to the well-known doctrine of Lamarck— that there is some necessary law of advancement, against which view I have often protested. ... Therefore, it does not seem to me an objection of any force that certain groups of animals, such as the Foraminifera, have not advanced in organization. Why certain whole classes, or certain numbers of a class, have advanced and others have not, we cannot even conjecture. ... Sir C. Lyell, who has given so excellent a discussion on species in his great work on the ‘Antiquity of Man,’ has advanced a somewhat analogous objection, namely, that the mammals, such as seals or bats, which alone have been enabled to reach oceanic islands, have not been developed into various terrestrial forms, fitted to fill the unoccupied places in their new island-homes; but Sir Charles has partly answered his own objection. Certainly I never anticipated that I should have had to encounter objections on the score that organic beings have not undergone a greater amount of change than that stamped in plain letters on almost every line of their structure.
- 36 www.darwinproject.ac.uk/letter/entry-4459f There is scarcely a county in England in which one or more plants will not be in danger of extirpation by the collectors for these prizes. Neither will the prizes promote scientific botany amongst the class for whose benefit they are intended, for there is nothing to ensure the recipient of a prize himself knowing the names or localities of the plants in his collection, or that he has examined a single botanical book, gathered any of the specimens, or even seen any of them. But supposing the case not to be so bad as this, the objection will probably apply, in some degree, to every collection sent to the Society; for no attempt is made (indeed it would be next to impossible) to ensure the collection being really formed, named, mounted and arranged by the candidate himself, without the help of other persons.
- 37 www.darwinproject.ac.uk/letter/entry-4746 We must indeed know far more than we do before we can dogmatise on the irrelevancy of particular colours to the well-being of a species. He ought also

- to define beauty, and tell us whether it is in reference to man or bird. I have no objection to the idea of beauty or variety for its own sake, but to assume it so positively is unphilosophical.
- 38 www.darwinproject.ac.uk/letter/entry-4752 It seems to me extremely clever like every thing that I have read of his; but I am not shaken; perhaps you will say that neither gods nor men could shake me. I demur to the Duke reiterating his objection that the brilliant plumage of the male humming bird could not have been acquired through selection, at the same time entirely ignoring my discussion (p. 93 3rd Edition) on beautiful plumage being acquired thro' sexual selection.
- 39 www.darwinproject.ac.uk/letter/entry-5013 You attach little importance to the Physicists objection;—I attach great;—because in the main, physical phenomena regulate the production & distribution of organisms; & because the your theory in contrast to it is a crude one not in harmony with the fact, that the relations of Life to conditions are intricate, & the results can only be accounted for after exploring a Labyrinth of conflicting facts. This is why I call yours a sledge-hammer hypothesis.
- 40 www.darwinproject.ac.uk/letter/entry-5140 Now it seems to me that you have yourself led to this objection being made, by so often stating the case too strongly against yourself. For Example, at the Commencement of Chap. IV. you ask, if it is “improbable that useful variations should sometimes occur in the course of thousands of generations”;—and a little further on you say, “unless profitable variati do occur natural selection can do nothing.” Now such expressions h given your opponents the advantage of assuming that favourable variations are rare accidents, or may even for long periods never occur at all, & thus Janet’s argument would appear to many to have great force.
- 41 www.darwinproject.ac.uk/letter/entry-5656 The “machine metaphor is not mine, but the N.B. reviewers. I merely accept it and show that it is on our side and not against us, but I do not think it at all a good metaphor to be used as an argument either way. I did not half develop the argument on the limits of variation, being myself limited in space; but I feel satisfied that it is the true answer to the very common and very strong objection, that “variation has strict limits”. The fallacy is the requiring variation in domesticity to go beyond the limits of the same variation under nature.
- 42 www.darwinproject.ac.uk/letter/entry-5742 It at once occurred to me that you would not object to give me a letter to Mr. Murray in which you might refer to me as the person mentioned in the note to your “Origin of Species”. Of my competence to render the ideas from the High Dutch into the English idiom so far as they are capable of being transfused, you can, of course, say nothing.
- 43 www.darwinproject.ac.uk/letter/entry-5922 Also, I do not see your objection to sterility between allied species having been aided by natural selection. It appears to me that,—given a differentiation of a species into two forms each of which was adapted to a special sphere of existence,—every slight degree of sterility would be a positive advantage, not to the individuals who were sterile, but to each form.
- 44 www.darwinproject.ac.uk/letter/entry-6045 If the difficulty of grafting was as great as the difficulty of crossing, and as regular, I admit it would be a most serious objection. But it is not. I believe, many distinct species can be grafted while others less distinct cannot. The regularity with which Nat. species are sterile together, even when very much alike, I think is an argument in favour of the sterility having been generally produced by Nat. Select. for the good of the species.
- 45 www.darwinproject.ac.uk/letter/entry-6153 I hardly see your difficulty or your objection to the case of the ♀ protected butterflies. You argued before (& have proved) that, “characters appearing in one sex are sometimes transmitted to that sex exclusively”. The cases of these ♀ protected butterflies (by mimicry) are so few that we may well suppose the proper variations to have occurred sufficiently in that sex only.
- 46 www.darwinproject.ac.uk/letter/entry-6375 Exactly a similar objection is made to the whole doctrine of “mimicry”. Why, it is asked, are there any white and yellow, quite unprotected (apparently) *Leptalis*? We answer, we do not know, but we firmly believe that they have some protection which the others have not, & wh. exactly balances it.
- 47 www.darwinproject.ac.uk/letter/entry-8725 I have read and studied all your books, including the latest, that on “Expression of the Emotions”, and find no serious objection to your theory of Development or Evolution, except that it seems to me “not proven”. It may be the true theory. You say much to make it seem probable; and I could accept it without giving up my belief, that the first

forms of life were created, and that the “variations”, without which “natural selection can do nothing”, may, also, be due to creative agency.

Appendix 3.9 Intertextuality marker: *regarding, in/with regard to*

- 1 www.darwinproject.ac.uk/letter/entry-502 (1st.) In describing what can be effected by crossing different species of plants, Mr. Herbert speaks, almost in the same terms, used by breeders of animals in regard to the almost unlimited effects of judgment and skill in uniting and opposing the different qualities of the parents. But breeders of animals always insist upon the great importance of choosing an animal, which not only possesses the requisite qualities, but which has possessed them for several generations, whereby they become fixed in the breed.
- 2 www.darwinproject.ac.uk/letter/entry-607 We should be astonished did one genus of monkeys adopt from another a particular manner of opening hard-shelled fruit; how much more so ought we to be in a tribe of insects so pre-eminent for their instinctive faculties, which are generally supposed to be in inverse ratio to the intellectual! Moreover, from what I have above stated regarding the Antirrhinum, I much suspect that the practice of boring holes in its flowers is likewise a piece of acquired knowledge, whether the Humble-bees do it instinctively or not in other cases.
- 3 www.darwinproject.ac.uk/letter/entry-734 I have notes on the comp. number of sp to gen in various places but they are at the Admiralty, for though I have been 4 months at home they have not yet returned me the notes drawings &c Botanical & others which I gave up as per order. When I receive them I shall tell you what little I have done; the results I think were curious regarding Arctic forms.— These lists are troublesome to make, as species are so loosely described.
- 4 www.darwinproject.ac.uk/letter/entry-736 With regard to the dissimilarity between the Flora of the several Islds of the group, that is too extraordinary a circumstance for me to offer any remarks upon, until the florula is drawn up, the further I proceed the more I wonder. .
- 5 www.darwinproject.ac.uk/letter/entry-844 From all that I heard at Leyden, the Indian Islands seem not only to be peculiarly rich in species, but also to present many curious facts regarding the distribution of the individuals & species in the different localities. I talked much with Schlegel, who appears a very nice fellow, he is strongly in favor of a multiple creation & against migration, & as he drew most of his arguments from Zoological grounds, I could not follow him well, he says he has long studied the subject & has come to that conclusion after a full consideration of the number of cases, in which a species is common to two narrow areas separated by large tracts equally capable to all appearance of supporting the said species: from what I know of the Botany of these regions I incline decidedly to the migration principle, the number of dispersed species being very great & belonging to very transportable orders.
- 6 www.darwinproject.ac.uk/letter/entry-983 In regard to your question respecting variations among the Mollusca I can only say that as far as my observations have gone I think their range is greater among fossils than in recent species & the most extraordinary varieties are from the Mam: Crag among estuary shells these are in some instances of extraordinary proportions removing the extremes of variation far beyond what is generally considered the limit of a species these variations (or perhaps more properly distortions) consist principally in the elongation or shortening of the spiral cone beyond its general proportions without affecting in any way the form of the aperture consequently there need be little or no alteration in the animal inhabitant & this I have always considered as depending upon an external cause & more particularly with regard to the Crag shells whose location was in an estuary where a sudden alteration or reduction of temperature may have partially paralyzed or injured the natural powers of the animal. the shells of the present day from similar localities do not appear to be similarly affected which I woud. attribute to a more uniform or rather a less variable temperature
- 7 www.darwinproject.ac.uk/letter/entry-1708 I think you have quite misunderstood me in regard to my object in getting you to mark in accompanying list with (X) all the “close species” ie such as you do not think to be varieties, but which nevertheless are very closely allied;—it has nothing whatever

- to do with their cultivation, but I cannot tell you object, as it might unconsciously influence you in marking them.
- 8 www.darwinproject.ac.uk/letter/entry-1752 This species also was seen in a state of domestication in the interior of the Tippera hills, by the Rev. J. Barbe, a missionary of the Romish faith, & a fair naturalist, who even brought me a pair of horns of one of these domestic individuals to convince me that he was right regarding the species. The Gasur has generally been considered as untameable; & it is the finest of all known living Boves.
 - 9 www.darwinproject.ac.uk/letter/entry-1792 Mr. Wallace could also well support his views by reference to the Helices & Bulimi collected in the Philippines by Cuming, —& also to the varieties of the Indian Melaniæ. What do you think of the paper in question? Has it at all unsettled your ideas regarding the persistence of species,—not perhaps so much from novelty of argument, as by the lucid collation of facts & phenomena.
 - 10 www.darwinproject.ac.uk/letter/entry-1794 I have just remembered one other point, on which I shd. be very much obliged for an answer, viz whether you are sure that the Cyclophorus stemostoma Sow. & the Pterocyclus bilabiatus Sow. are certainly found in Ceylon: I have had a letter from Mr. Benson in answer to a query of mine whether any alpine shells were common to the heights of Ceylon, Neilgherries & Himmalaya & he speaks somewhat doubtfully on your authority in regard to the two above-named species.
 - 11 www.darwinproject.ac.uk/letter/entry-1825 I am pleased to find that Owen does not oppose my views regarding the “great Orang-utan question”. I have a long letter from you undated, but mentioning in a P.S. that you had just recd. mine of Oct. 8th. & 22nd.
 - 12 www.darwinproject.ac.uk/letter/entry-1832 And there can be no mistake regarding my experiments with the 1/2 bred Gallus Sonneratii of both sexes, which were infertile inter se, and either of them with other fowls; although the hen produced many eggs, & the cock was particularly salacious; for the eggs in which either was concerned never would hatch, although other eggs placed with them hatched as usual: & surely this instance of infertility may be fairly ascribed to the hybridity.
 - 13 www.darwinproject.ac.uk/letter/entry-1837 Though I have nothing very particular to say I must thank you cordially for the extremely kind manner with which you have received my letter. I remember at Oxford that you had attended to many of the points on which I was then & am now so much interested. I hope that you will publish some of the facts on variation to which you allude: I shd. be particularly glad to see in print or M.S. some particulars in regard to the species from different elevations, which show different degrees of capacity for cultivation at a new level: Hooker has published a similar case in regard to the Himmalaya Rhododendrums.
 - 14 www.darwinproject.ac.uk/letter/entry-1845 I suppose, ere this reaches you, that you will have read my article on wild Asses. Strange to say, the very day of its publication, the subject was engaging the attention of the Académie des Sciences in Paris, as I see by the Comptes Rendus; on the occasion of the presentation of two animals from the Syrian desert (as you will see by a note), which Is. Geoffroy distinguishes as a new species, E. hemippus, & I think he is quite right. But my suspicions are confirmed regarding the indigenous abode of the Ass; and I now feel satisfied that it is an African rather an Asiatic quadruped! I also now suspect that Chesneys “wild Horses” in N. Arabia refer to the hemippus, & his “wild Asses” in S. Arabia to asinus ferus (vel Onager), also that Wellsteads Sacotran “wild Asses” are asinus aboriginally wild & not ‘feral’! Already I have prepared another paper on the subject, in which I have gone sufficiently into details— Is. St. Hilaire, I perceive, confirms what I say regarding the voice of the Indian Ghor-Khur; & is of the same opinion as myself respecting the identity of the Indian animal with the hemionus of N. Asia, which is undoubtedly the Tibetan Kyang. But now comes the question regarding the relative distribution of these animals in S. Asia. The Mesopotamian is, in all probability, hemippus, or may not different species occur in the same region, to a greater or less extent?
 - 15 www.darwinproject.ac.uk/letter/entry-1925 I want to beg one more favour to the many which formerly you have conferred on me. I am extremely much interested in regard to the blind cave animals, described some time since in your Journal by Prof. Silliman Junr., as the subject is connected with a work of somewhat general nature, which I am endeavouring to draw up on variation & the origin of species, classification &c.—

- 16 www.darwinproject.ac.uk/letter/entry-1938 I suppose in regard to specific centres, we are at cross purposes; I shd. call that kitchen garden, in which the Red Cabbage was produced, or the Farm in which Bakewell made the Short-Horn Cattle, the specific centre of these species! and surely this is centralisation enough!—
- 17 www.darwinproject.ac.uk/letter/entry-2034 I have received the 2d. part of your paper, & though I have nothing particular to say, I must send you my thanks & hearty admiration. The whole paper strikes me as quite exhausting the subject, & I quite fancy & flatter myself I now appreciate the character of your Flora. What a difference in regard to Europe your remarks in relation to the genera makes! I have been eminently glad to see your conclusion in regard to species of large genera widely ranging: it is in strict conformity with the results I have worked out in several ways.
- 18 www.darwinproject.ac.uk/letter/entry-2125 What you say about extinction, in regard to small genera & local disjunction, being hypothetical seems very just. Something direct, however, could be advanced on this head from fossil shells; but hypothetical such notions must remain.
- 19 www.darwinproject.ac.uk/letter/entry-2136 In regard to my abstract you must take immensely on trust; each paragraph occupying one or two chapters in my Book. You will, perhaps, think it paltry in me, when I ask you not to mention my doctrine; the reason is, if anyone, like the Author of the Vestiges, were to hear of them, he might easily work them in, & then I shd have to quote from a work perhaps despised by naturalists & this would greatly injure any chance of my views being received by those alone whose opinion I value.—
- 20 www.darwinproject.ac.uk/letter/entry-2359 From the statement in regard to the Clover in New Zealand, I wrote to Mr. Swale, of Christchurch in New Zealand, and asked him whether Leguminous plants seeded there freely before the hive bee was introduced; and he in the most obliging manner has sent me a list of 24 plants of this order, which seeded abundantly before bees were introduced.
- 21 www.darwinproject.ac.uk/letter/entry-2443 I hasten to thank you for your obliging letter of yesterday & for the interesting details regarding your work on Species contained in it. —
- 22 www.darwinproject.ac.uk/letter/entry-2450 I shd. have liked to have seen several examples proving truth (or showing its probability) of some of your remarks; as of best marked vars. being on confines of the range. Or again in regard to your remark of a species remaining for many generations constant under culture & then suddenly commencing to vary. —
- 23 www.darwinproject.ac.uk/letter/entry-2520 Hooker sent me a few pages in which you propound a doctrine of migration into America like that, which I sent you last summer in a letter. Dana is of course a far better authority than I am; but his arguments have by no means convinced me in regard to the warm period subsequent to the Glacial period. I daresay I may be wrong, but I rather doubt whether his & your view will explain facts of distribution so well as my view of migration during the certainly warmer period anteriorly to the Glacial period.
- 24 www.darwinproject.ac.uk/letter/entry-2582 I have heard rumour that Busk is on our side in regard to Species: is this so? It would be very good.— [Enclosure] The drawings of Pigeons are by Wolstenholme (an excellent Fancier himself) & were made for Mr J. M. Eaton, author of excellent treatise, chairman of a Club & great winner of prizes.
- 25 www.darwinproject.ac.uk/letter/entry-2608 Since receiving your last letter on Hooker, I have read his Introduction as far as p xxiv where Australian Flora begins; & this latter part I liked most in the Proofs.— It is a magnificent Essay.— I doubt slightly about some assertions, or rather shd have liked more facts, as, for instance, in regard to species varying most on confines of their range.— Naturally I doubt a little whether his remarks about “divergence”, & about domestic races being produced under nature, without selection.
- 26 www.darwinproject.ac.uk/letter/entry-2719 I think you expect too much in regard to change of opinion on the subject of species. One large class of men, more especially I suspect of naturalists, never will care about any general question, of which old Gray of Brit. Mus. may be taken as a type; & secondly nearly all men, past a moderate age either in actual years or in mind, are, I am fully convinced physically incapable of looking at facts under a new point of view.
- 27 www.darwinproject.ac.uk/letter/entry-2783 Sedgwick is to illuminate us on Monday at the Philosophical Society in regard to your supposed errors! How can Owen be so savage with your views when his own are to a certain extent of the same character— If I understand him, he thinks the “Becoming” of species (I suppose he means the producing of species), a somewhat rapid & not a

- slow process—but he seems to think them progressive organised out of previously organized beings—
- 28 www.darwinproject.ac.uk/letter/entry-2794 Yesterday at my lectures I alluded to the subject, & showed how frequently naturalists were at fault in regarding as species, forms which had (in some cases) been shown to be varieties, and how legitimately Darwin had deduced his inferences from positive experiment— Indeed I had, on Monday, replied to a sneer (I don't mean from Sedgwick) at his pidgeon results, by declaring that the case necessitated an appeal to such domestic experiments, & that this was the legitimate & best way of proceeding for the detection of those laws which we all endeavouring to discover—
- 29 www.darwinproject.ac.uk/letter/entry-2901 In connection with the above I would also refer to what you say in regard to our old friend the tucu-tucu. (*Ctenomys*.) Would not the blindness be explained in that case in some other way than as the result of accidental injury? I believe it is generally admitted by physiologists that the effects of mechanical injuries are not transmitted by inheritance & in view of the fact that the Jews after so many centuries of mutilation persist in being born with a prepuce, I am inclined to believe it.
- 30 www.darwinproject.ac.uk/letter/entry-2932a What you say as to my difficulty is I suspect an explanation up to a certain point, but I hope to put the whole more clearly soon. Dogs of multiple origin & leporines would greatly weaken the objection to regarding the negro as of a different species, in the same sense that the Prairie wolf & common wolf may be. I should like a good naturalist to give me a list of reputed species in Mammalia not more remote than negro & white man. Would they not be many?
- 31 www.darwinproject.ac.uk/letter/entry-3104 I do not think the result I have come to regarding the persistence of the Guiano-Amazonian fauna can be shaken. I can see no traces of a migration of high-temperate-zone forms across the region. Now there are a number of genera of insects characteristic of the high-temperate zones, which are now common to S. America & N. America with Europe; but they have not a single representative in Amazonia, although some of them have representatives along the Andes of New Granada & Peru. Your hypothesis only requires (as you have stated) a migration along belts of latitude & therefore I do not think that the conclusion I have come to affects your position much after all.—
- 32 www.darwinproject.ac.uk/letter/entry-3170 I am pretty confident there is a fine open field for research in regard to crossing varieties which have been greatly neglected under a scientific point of view, though largely & loosely practised by gardeners. Species on the other have been largely experimented on. As you have lived so much abroad, German is probably quite familiar to you (I wish it were to me) & I would most strongly advise you to get Gartners admirable “Versuche ueber die Bastardzeugung. 1849” & study it.—
- 33 www.darwinproject.ac.uk/letter/entry-3387 Man is the great stumbling block in regard to all recent theories of species. He stands by himself as a Creation I think, & the Records in regard to him are explicit. I cannot overlook them in considering his plan in creation.
- 34 www.darwinproject.ac.uk/letter/entry-3658a I am very much obliged to your several correspondents for their information in regard to the supposed differences in the bees of Britain. Possibly some few of your readers may be interested in the following case:— The hive bee was introduced many years ago into Jamaica. Having seen it stated that the cells were larger, I procured (through the kindness of Mr. R. Hill, of Spanish Town), some bees and comb.
- 35 www.darwinproject.ac.uk/letter/entry-3815 I am in the same predicament in regard to your other question on the greater facilities certain species present for fertilisation by other pollen than their own. I have only one experiment I can give you on this point and it is quite inconclusive.
- 36 www.darwinproject.ac.uk/letter/entry-4284 With regard to the value of the pliocene species— identical with a living one—to be of any value the instances must be sound, and unquestionable: and if a sufficient number of good cases is adduced, there is a very wholesome basis for a generalization. With regard to the Badger, I cannot say. As soon as I get back to London I will send you Suess paper. I was not a little surprized to find myself so precisely formulated for so decided a Conclusion. Had I not thought that Suess must have sent it to you, I should have forwarded it to you. He deals directly with natural selection, under its German designation
- 37 www.darwinproject.ac.uk/letter/entry-4615 I beg to thank you for your so kindly acknowledging my inquiries regarding your works on “Orchids” and “The Cirripedia”. My friend, I have since heard,

- did not know of Professor Bronn's translation of the former work, although he possesses himself the late professor's Translation of your excellent work on "The Origin of Species": of which translation, I understand, the second edition of 1863 seems to be nearly exhausted.
- 38 www.darwinproject.ac.uk/letter/entry-4663 Amongst other things, he observes in regard to my charge that Prof. Agassiz had misstated your theory, that "Darwin in a recent letter to father thanks him for the manner in which he has conducted his opposition to his theory. So you see that opinions may differ on the misstatement of Darwin's Theory". In reply, I suggested that that fact merely showed that having been abused as an atheist, deist, infidel &c by other writers, you probably felt grateful to any writer who was willing to allow you "a spirit quite as reverential as his own". (Meth. Study Pref. p. iv.)
- 39 www.darwinproject.ac.uk/letter/entry-4753 With regard to Mr Marshalls statement respecting the Polynesian Lice, it is a very extraordinary statement, & I cannot see any reason why the Lice of one Human being should not live upon another! It is a fact that the parasites of one Genus of animals do not live upon animals of another Genus.
- 40 www.darwinproject.ac.uk/letter/entry-5031 I am much obliged to you for the privilege of reading it; and in regard to the notes prepared for the new edition, I am amused to find how many of the topics are the same as those treated of in the letters of yourself, Hooker, and Bunbury, in commenting on the observations by Agassiz of marks of glaciation in the Organ Mountains.
- 41 www.darwinproject.ac.uk/letter/entry-6083 Up to this time I can not yet give a precise answer as regarding the seeds of 1866, but the close of the experiment is approaching. Euryale is a very extraordinary plant as regards the condition of its stigma, indeed so much, that I know of no parallel.
- 42 www.darwinproject.ac.uk/letter/entry-6095 My son, I am sorry to say, cannot see full force of your rejoinder, in regard to second head of continually augmented sterility. You speak in this rejoinder & in Par. (5) of all the individuals becoming in some slight degree sterile in certain districts; if you were to admit that by continued exposure to these same conditions the sterility would inevitably increase there would no need of Nat. Selection.
- 43 www.darwinproject.ac.uk/letter/entry-6728 I quite agree with you that Wallace's sketch of Natural Selection is admirable. I wrote to tell him so after I had read the article & in regard to the concluding theory I reminded him that as to the origin of man's intellectual & moral nature I had allowed in my first edition that its introduction was a real innovation interrupting the uniform course of the causation previously at work on the earth.

Appendix 3.10 Intertextuality marker: *say about, say that*

- 1 I quite agree with you in what you say about minute insects not having been commonly collected abroad—I have very seldom seen any but those remarkable for size or beauty in any Cabinets.—
- 2 With respect to my far-distant work on species, I must have expressed myself with singular inaccuracy, if I led you to suppose that I meant to say that my conclusions were inevitable. They have become so, after years of weighing puzzles, to myself alone;; but in my wildest day-dream, I never expect more than to be able to show that there are two sides to the question of the immutability of species, ie whether species are directly created, or by intermediate laws, (as with the life & death of individuals). I did not approach the subject on the side of the difficulty in determining what are species & what are varieties, but (though, why I shd give you such a history of my doings, it wd be hard to say) from such facts, as the relationship between the living & extinct mammals in S. America, & between those living on the continent & on adjoining islands, such as the Galapagos— It occurred to me, that a collection of all such analogous facts would throw light either for or against the view of related species, being co-descendants from a common stock. A long searching amongst agricultural & horticultural books & people, makes me believe (I well know how absurdly presumptuous this must appear) that I see the way in which new varieties become exquisitely adapted to the external conditions of life, & to other surrounding beings.— I am a bold man to lay myself open to being thought a complete fool, & a most deliberate one.— From the nature of the grounds, which make me believe that species are mutable in form, these grounds

- cannot be restricted to the closest-allied species; but how far they extend, I cannot tell, as my reasons fall away by degrees, when applied to species more & more remote from each other.
- 3 www.darwinproject.ac.uk/letter/entry-1058 I hardly know when I shall come to Kew for a morning to hear what you have to say about my species-sketch: when there I shall get you to look over a paper with me in the *Annales S. Nat.* on the Norfolk Isld Flora—a very nice resumé, but it quite omits all notice of the general affinities of the indigenous species, which I daresay by running over the genera you cd. tell. How I do wish you had time to discuss all insular Floras, as far as present knowledge; what a truly splendid paper you cd make—the African islands—, Tristan d’Acunha Juan Fernandez, the Society Isd which you have partly done.— But I suppose I must remain content with wishing for it.—
 - 4 www.darwinproject.ac.uk/letter/entry-1484 I cannot tell you how much gratified I am at what you say about the Cirripedia. I really feel rewarded for more labour than you would readily believe it possible could have been bestowed on the work. If I am ever proved wrong in it, I shall be surprised. But my pen is running away with me,—it is your fault for I have been so much pleased with what you say.
 - 5 www.darwinproject.ac.uk/letter/entry-1542 I was astonished at parts & could not at all, understand his reasons:— But I have an unbounded respect for M. Edwards as a Naturalist From some old theoretical notions, I was interested by what you say about Crustacea not having been most developed in Tropics: should you ever work this out in other branches, either in regard to mere numbers of species, or their rank I shd. be particularly glad to hear the result.
 - 6 www.darwinproject.ac.uk/letter/entry-1562 By the way, I hope, when you go to Hitcham towards the end of May you will be forced to have some rest. I am grieved to hear that all the bad symptoms have not left Henslow; it is so strange & new to feel any uneasiness about his health.— I am particularly obliged to you for sending me Asa Gray’s letter; how very pleasantly he writes. To see his & your cautions on the species-question ought to overwhelm me in confusion & shame; it does make me feel deuced uncomfortable. I cannot quite understand why you & he think so strongly that it “does more harm than good to combat such views.”— It is delightful to hear all that he says on Agassiz: How very singular it is that so eminently clever a man, with such immense knowledge on many branches of Natural History, should write such wonderful stuff & bosh as he does. Lyell told me that he was so delighted with one of his (Agassiz) lectures on progressive development &c &, that he went to him afterwards & told him “it was so delightful, that he could not help all the time wishing it was true”. I seldom see a Zoological paper from N. America, without observing the impress of Agassiz’s doctrine’s,—another proof, by the way, of how great a man he is.— I was pleased & surprised to see A. Gray’s remarks on crossing, obliterating varieties, on which, as you know, I have been collecting facts for these dozen years.—
 - 7 www.darwinproject.ac.uk/letter/entry-1610 I solemnly vow I will not write again for an enormous age,—not even if you were to say that you had nothing to do, & liked answering never-ending, nowhere-going questions.— But I want you “twice awfully” (as Lenny says) to ask Mr. Bentham (whom I fancy you see often) whether he excluded from his list of aberrant genera, genera simply because they contained many species; for I am staggered at the fewness of the species; & cannot but fear that he has done so.
 - 8 www.darwinproject.ac.uk/letter/entry-1696 What you say about no one realising creation strikes me as very true; but I think & hope that there is nearly as much difference between trying to find out whether species of a genus have had a common ancestor & concerning oneself with the first origin of life, as between making out the laws of chemical attraction & the first origin of matter.
 - 9 www.darwinproject.ac.uk/letter/entry-1792 Wallace has, I think, put the matter well; and according to his theory, the various domestic races of animals have been fairly developed into species. I think that I before said that the Peacock had not varied for so many centuries of domestication in Europe, further than as regards albinism, complete or partial; but I overlooked the interesting “sport” of colour, exhibited by the Japanned (not ‘Japan’) variety, which seems to have originated more than once,—vide especially Sir R. Heron, in the *Proc Zool. Soc.* A trump of a fact for friend Wallace to have hit upon!
 - 10 www.darwinproject.ac.uk/letter/entry-1924 I am delighted at what you say about Huxley’s answer & I agree most entirely: it is excellent & most clear; I thought from the first that he was right, but was not able to put it clearly to myself.— What you say about the importance of the infertility of

- species, when crossed, I quite agree with; & I think those who attribute our domestic varieties to the crossing of ma[n]y primordial forms, greatly underrate the evidence on this question—
- 11 www.darwinproject.ac.uk/letter/entry-2060 Your last letter, like all its predecessors has been very valuable to me; & every word in it has interested me. When I said that your remarks on your alpine plants “riled” me; I did not mean to doubt them, except in the Agassian sense that they went against some theoretic notions of mine. These notions are too long to give & indeed not worth giving, as far as America is concerned, & I can see from your letter that we shd. take very much the same view. I am very glad to hear that you think of discussing the relative ranges of the identical & allied U. States & European species, when you have time. Now this leads me to make a very audacious remark in opposition to what I imagine Hooker has been writing & to your own scientific conscience. I presume he has been urging you to finish your great Flora, before you do anything else. Now I would say it is your duty to generalise as far as you safely can from your as yet completed work. Undoubtedly careful discrimination of species is the foundation of all good work; but I must look at such papers as yours in Silliman as the fruit. As careful observation is far harder work than generalisation & still harder than speculation; do you not think it very possible that it may be overvalued? It ought never to be forgotten that the observer can generalise his own observations incomparably better than anyone else.
 - 12 I think (without going afresh into the statistics) that you may say that “23/29 endemic genera have all their species either apterous, or incapable of flight”.—
 - 13 www.darwinproject.ac.uk/letter/entry-2124 www.darwinproject.ac.uk/letter/entry-2125 What you say about extinction, in regard to small genera & local disjunction, being hypothetical seems very just. Something direct, however, could be advanced on this head from fossil shells; but hypothetical such notions must remain. Thank you heartily for what you say about my Book; but you will be greatly disappointed; it will be grievously too hypothetical.
 - 14 www.darwinproject.ac.uk/letter/entry-2252 Your kindness to me is really beyond thanks. Believe me that I feel it. By an odd chance yesterday morning, before I got your letter, I had just written down what I had to say on closely allied species in large genera; & I thought that you had forgotten all about your list, & knowing how hard you were worked, to my credit be it said, I firmly resolved that nothing shd. induce me to remind you. Therefore you may believe how delighted I was to get your list, which is now being tabulated.—
 - 15 www.darwinproject.ac.uk/letter/entry-2254 I am extremely interested at what you say about the Cape animals being the same with European species during Glacial epoch: it is just like what I fancy has happened in case of Brazil in regard to Cordillera mammals.— According to my theoretical notions, I am not satisfied with what you say about local plants in S.W. corner of Australia & the seeds not readily germinating: do be cautious on this;—consider lapse of time.
 - 16 www.darwinproject.ac.uk/letter/entry-2371 But I have been astounded at what you say that SE & SW Australia differ as much as Australia from world.
 - 17 www.darwinproject.ac.uk/letter/entry-2501 In the first place at p. 480 it cannot surely be said “that the most eminent naturalists have rejected the view of the mutability of species— You do not mean to ignore G. St. Hilaire & Lamarck— As to the latter you may say that in regard to animals you substitute natural selection for volition to a certain considerable extent, but in his theory of the changes of plants he cd. not introduce volition—he may no doubt have laid an undue comparative stress on changes in physical conditions & too little on those of contending organisms— He at least was for the universal mutability of species & for a genealogical link between the past & the present.—
 - 18 www.darwinproject.ac.uk/letter/entry-2531 I have this morning received a message from Mrs. Butler, (who was a Miss Edgeworth) that Madame Belloc Rue Ecole de Medicine (5.) Paris wishes to translate my Book on Species into French.— She translated some of Miss Edgeworths novels very well, & some of Sismondi’s works. She writes in Revue des deux mondes. All this sounds very well; my only doubt is whether she is scientific enough. I am, also, excessively puzzled to think how she could have heard of my Book.— I presume that she wd. require to read it before agreeing to translate. I am extremely anxious for the subject sake (& God knows not for mere fame) to have my Book translated; & indirectly its being known abroad will do good to English Sale.— If it depended on me I shd. agree without payment & instantly send copy & only beg that she would get some scientific man to look over the Translation. But I suppose the affair rests in your hands. If you think

- favourably, will you at once communicate with her (for she begs for immediate answer) & let me hear result. It will save much time if you will write direct. But please apologise for my not writing direct & you may truly say I am much out of health. You might say that though I am a very poor French scholar, I could detect any scientific mistake, & would read over French proofs. Please act as you think fit, remembering how earnestly I wish my views to be known & discussed.
- 19 www.darwinproject.ac.uk/letter/entry-2572 www.darwinproject.ac.uk/letter/entry-2575 I have very long interview with Owen, which perhaps you would like to hear about, but please repeat nothing. Under garb of great civility, he was inclined to be most bitter & sneering against me. Yet I infer from several expressions, that at bottom he goes immense way with us.— He was quite savage & crimson at my having put his name with defenders of immutability. When I said that was my impression & that of others, for several had remarked to me, that he would be dead against me: he then spoke of his own position in science & that of all the naturalists in London, “with your Huxleys”, with a degree of arrogance I never saw approached. He said to effect that my explanation was best ever published of manner of formation of species. I said I was very glad to hear it. He took me up short, “you must not at all suppose that I agree with in all respects”.— I said I thought it no more likely that I shd. be right on nearly all points, than that I shd toss up a penny & get heads twenty times running. I remember your remarks on non migration but I cannot say that I quite appreciate them. I entirely agree with you that difficulty of not finding intermediate fossils in number is very great, even when looking at the Geological Record, as being as imperfect as I believe— No one will think anything of my book, unless his mind leads him to put weight on the apparent explanation offered by the theory from several large classes of facts as affinities,—homologies, embryology &c. &c— With respect to not finding intermediate gradations, I can only repeat what I have said that it is very unlikely that a future geologist, a few million years hence will be able to prove that certain forms, which are probable varieties are really varieties by discovering the intermediate links— I wish some sound Conchologist would compare Eocene Middle Tertiary & Recent Shells with the object of seeing whether any or how many of the middle Tertiary species stood in some degree intermediate between Eocene & Recent.—
- 20 I quite agree with what you say on effect of admission of theory on Systematic work; (see p. 485), not but what I was haunted with endeavouring to guess what cirripedes would be ranked as species by other naturalists. With respect to your objection of multitude of still living simple forms, I have not discussed it anywhere in the Origin, though I have often thought it over.— What you say about “progress being only occasional & retrogression not uncommon” I agree to; only that in animal kingdom I greatly doubt about retrogression being common.—
- 21 www.darwinproject.ac.uk/letter/entry-2773 Hybridisation on the supposition (singular enough as it seems to me) of all the males of a rare species dying. Do you not think that you ought to say that this applies only to the unisexual animals & to but few plants—unless indeed you choose to suppose the male organs to fail in acting, during same season in all the individuals of a species. Such astonishing precautions to prevent extinction, seeing that the extinction of every form of life in the course of time is a law of nature, seems to me rather improbable.—
- 22 www.darwinproject.ac.uk/letter/entry-2915 I entirely agree with what you say about only one species of many becoming modified: I remember this struck me much when tabulating the varieties of plants. & I have a discussion somewhere on point.
- 23 www.darwinproject.ac.uk/letter/entry-2943 1st. — I never supposed you to say that Natural Selection could act without previous Variability. On the contrary, throughout your book Natural Selection is represented as dependent on “favourable” variations & conditions, ready to take advantage of, to perpetuate & accumulate any profitable item of differentiation;—but, in strict language, to originate nothing. Hence in my letter I have called her a Wetnurse, rather than a Mistress, & hence I can see how we may accept her as the manufacturer of multitudes of “species” (so called), such as they exist in our arrangements; and yet reject her as explaining unlimited divarications.
- 24 www.darwinproject.ac.uk/letter/entry-3047 I most fully agree to what you say about Huxley’s article & power of writing. What a smasher for Owen! The whole Review seems to me excellent. How capitally Oliver has done the resume of Bot. Books. Good Heavens how he must have read!
- 25 www.darwinproject.ac.uk/letter/entry-3098 I quite agree with what you say on Lieut. Hutton’s Review (who he is, I know not): it struck me as very original: he is one of the very few who see that

- the change of species cannot be directly proved & that the doctrine must sink or swim according as it groups & explains phenomena.
- 26 www.darwinproject.ac.uk/letter/entry-3123 As you are working at Birds of S. America, & for my credit sake do oblige me & look at Birds in Zoolog. of Beagle p. 67 & see what I say on the 3 species of *Opetiorhynchus* & consider whether I am likely to have blundered when I observed difference of Habits of the species; so at p. 74 on *Scytalopus*, when I specify difference of habits. Let me add another sentence.— Why should you or I speak of variation as having been ordained & guided more than does an astronomer in discussing the fall of a meteoric stone. He would simply say that it was drawn to our earth by the attraction of gravity, having been displaced in its course by the action of some quite unknown laws.— Would you have him say that its fall at some particular place & time was “ordained & guided without doubt by an intelligent cause on a preconceived & definite plan”? Would you not call this theological pedantry or display? I believe it is not pedantry in the case of species, simply because their formation has hitherto been viewed as beyond law,— in fact this branch of science is still with most people under its theological phase of development.— The conclusion which I always come to after thinking of such questions is that they are beyond the human intellect; & the less one thinks on them the better. You may say, then why trouble me? But I shd. very much like to know clearly what you think.
- 27 www.darwinproject.ac.uk/letter/entry-3329 What you said about Brown having observed the ducts running wrong in *Habenaria* is very likely (though he ought to have said so) but then he does apparently trust to position of ducts as far as he traced them by transverse sections.
- 28 www.darwinproject.ac.uk/letter/entry-3439 It is very true what you say about the higher races of men, when high enough, replacing & clearing off the lower races. In 500 years how the Anglo-saxon race will have spread & exterminated whole nations; & in consequence how much the Human race, viewed as a unit, will have risen in rank.
- 29 www.darwinproject.ac.uk/letter/entry-3544 I am going to bother you. Looking over some of your old notes, I see that you have kept the wild breed of Turkeys from Ld. Leicester & Powis. You know that they now say that the common Turkeys have descended from a southern so-called species. Have you ever crossed intentionally or accidentally your wild & common; & did you ever cross the hybrids inter se or with either pure parent & were they quite fertile? Have you ever given half-bred birds to other people, & did they with them become mingled with common Turkeys? Can you recognise the half-breds by their appearance? I shd be grateful for any information, which I might quote on your authority.— Do you remember about my Boy, Horace, on the natural selection of coward adders? I must tell you that the other day he overheard me talking about species; & afterwards he came to me, with his eyes open with astonishment & asked “Did people formerly really believe that animals & plants never changed? I answered oh yes. “Well then what did they say about the kinds of cabbages & peas in the Garden?” I answered that these were all due to man’s agency. “But do not wild plants vary”. I answered that they varied within certain fixed but unknown limits. To this he shrugged his shoulders with pity for the poor people who formerly believed in such conclusions.— I believe Horace is a prophetic type, as Agassiz would say, of future naturalists.—
- 30 www.darwinproject.ac.uk/letter/entry-3918 You not only give me information of much value, but you give it in the kindest manner possible. All that you say about peaches is particularly interesting, as the case struck me much in many respects. I have alluded in my M.S. from Gard. Chronicle to the Double-flowering peaches of China, though I have never seen them. The case struck me as good in showing what man can do by continued selection in two different lines on the same species, viz flowers & fruits—
- 31 www.darwinproject.ac.uk/letter/entry-3935 I have been extremely much interested by what you say about the tracks of supposed mammalia.— Might I ask if you succeed in discovering what the creatures are, you would have the great kindness to inform me.—
- 32 www.darwinproject.ac.uk/letter/entry-4028 I hope to Heaven I am wrong (& from what you say about Whewell it seems so) but I cannot see how your Chapters can do more good than an extraordinarily able review.”
- 33 www.darwinproject.ac.uk/letter/entry-4052 I see what Treviranus says about *Primula longiflora*; I shd. like to know (if you are up in *Primula*) whether this species is closely allied to *P. Scotica*; because Mr J. Scott of Bot. Garden of Edinburgh, has been carefully observing *Primulas* (& I feel a

- conviction that he is trust-worthy) & he says *P. Scotica* is never dimorphic, & is much surprised, as he says it is so like *P. farinosa*: he has sent me plants of both, but they look very sickly.
- 34 www.darwinproject.ac.uk/letter/entry-4148 I have written once again to own to certain extent of truth in what he says; & then if I am ever such a fool again have no mercy on me.— I enclose A. Gray's letter, as you might like to read all. I quite disagree with what he says about Lyell acting as a Judge on Species; I complain that he has not acted as a judge; I sometimes wish he had pronounced dead against us rather than possessed such inability to decide.—
- 35 www.darwinproject.ac.uk/letter/entry-4153 The last I was glad to receive on Lyell, & will tell him, when I write, what you say on Species-portion. I am pleased at it; but cannot quite agree.
- 36 www.darwinproject.ac.uk/letter/entry-4554 In the last Nat. Hist Review there is an interesting article which must be by you, in which you say that *Trichonema* & some other plants present 2 forms. I shd be particularly obliged if you cd give me the names of these plants; for then perhaps I cd get seed. For instance, you told me of *Oxalis* & I wrote to the Cape & have got about 20 species some of which are grandly Dimorphic. I know that I exaggerate the interest of whatever I am about, but the *Lythrum* case seems to me so surprising that I wish to pursue the subject. In yr *Oxalis* letter you say that *Ægiphila* is dimorphic; now I have no idea whether the species are rare or whether it wd cause much trouble to send me a dried flower of the 2 forms of any species. If it does not give much trouble I shd much like to see whether they are really dimorphic like *Primula* &c or like *Thyme* which latter is a very different case. I believe that you told me that this genus is a Labiate, but Lindley makes it one of the *Verbenaciæ*. I know how busy you are but I trust to your kindness to forgive me for troubling you—
- 37 www.darwinproject.ac.uk/letter/entry-4752 It seems to me extremely clever like every thing that I have read of his; but I am not shaken; perhaps you will say that neither gods nor men could shake me. I demur to the Duke reiterating his objection that the brilliant plumage of the male humming bird could not have been acquired through selection, at the same time entirely ignoring my discussion (p. 93 3rd Edition) on beautiful plumage being acquired thro' sexual selection. The Duke may think this insufficient, but that is another question. All analogy makes me quite disagree with the Duke that the differences in the beak, wing & tail are not of importance to the several species. In the only two species which I have watched, the difference in flight & in the use of the tail was conspicuously great.
- 38 www.darwinproject.ac.uk/letter/entry-5167 It is quite new & very interesting to me what you say about the endemic plants being in so large a proportion rare Species.
- 39 www.darwinproject.ac.uk/letter/entry-5193 I received a few days ago a sheet of your new work, & have read it with great interest. You confer on my book, the "Origin of Species", the most magnificent eulogium which it has ever received, & I am most truly gratified, but I fear if this part of your work is ever criticized, your reviewer will say that you have spoken much too strongly. Your abstract seems to me wonderfully clear & good; & one little fact shews me how clearly you understand my views, namely your bringing prominently forward, which no one else has ever done, the fact & the cause of Divergence of Character. Oddly enough, as it now appears to me, it was many years before I clearly saw the necessity of admitting a tendency to divergence of character, & some more years until I could see the explanation.
- 40 www.darwinproject.ac.uk/letter/entry-5216 I have been much interested by what you say on seeds which adhere to the valves being rendered conspicuous: you will see in the new Edit. of the origin why I have alluded to the beauty & bright colours of fruit; after writing this, it troubled me that I remembered to have seen brilliantly coloured seed, & your view occurred to me.
- 41 www.darwinproject.ac.uk/letter/entry-5351 The consequence of this is that all the tap roots of our trees are destroyed—all depends on a series of horizontally spreading surface roots—thus rendering them infinitely less able to cope with the storms to which they are so subject here— I assure you, I do not exaggerate the character of our grounds when I say that throughout the rainy season they are fit only for the cultivation of rice!
- 42 www.darwinproject.ac.uk/letter/entry-5447 you must excuse me not having thanked you before for sending me the copies of my notice on *Corydalis*, but I was waiting for a little treatise of mine to be finished to send you a copy of it. I have ventured to show how right you are when you say that nature abhors perpetual self-fertilisation. I heard that there has appeared a new edition of your "Origin of Species", perhaps you have said in it more about selffertilisation and intercrossing than in

- the first, but I suppose that you did not enter in details and I hope that my little book will not appear quite useless for the public, though I do not believe that you will find much in it that you have not known before. I cannot send you as yet the paper on *Aristolochia*, it is very annoying that Pringsheim goes on so slowly with his *Jahrbücher*.
- 43 www.darwinproject.ac.uk/letter/entry-5554 I quite agree with what you say on the extreme interest of attempting to affiliate extinct & existing Species. The Duke of Argyll's book is very fair & manly. He cannot agree with you, but he writhes about under you as one who feels himself likely to be beat. What he says about the humming birds is his weakest part.
- 44 www.darwinproject.ac.uk/letter/entry-5640 www.darwinproject.ac.uk/letter/entry-5642 I hope that you will not think me presumptuous if I cannot resist the pleasure of telling you how much I admire your argument of the origin of species in the *Transact. of the Victoria Institute*. The whole case strikes me as placed in the clearest & most spirited light; & I have no where seen so good an abstract. I quite agree with your Chairman that you have put the whole argument better than I have done. But I disagree with you, & it is the only point on which I do disagree, when you say that there is nothing in your article original. As I am writing I will ask you two questions, but if you cannot answer them easily, pray do not take any trouble on the subject; Firstly. Where have you seen an account of inherited baldness & deficient nails; & 2ndly of the case of the plane which sent up an evergreen sucker or shoot.
- 45 www.darwinproject.ac.uk/letter/entry-5772 What you say about the ocelli is exactly what I want, viz the greatest range of variation within the limits of the same species,—greater than in the Meadow Brown if that be possible.
- 46 www.darwinproject.ac.uk/letter/entry-5937 This proves that male fish do fight, and if one large fish meets with another, “then comes the tug of war”, which may account for the numbers of large-sized dead male fish found in our rivers at the end of the Spawning Season.— As to the attachment of the fish, the only inference we can draw is that the first couple are the selected, but from what we have seen going on, they seem to have no more mutual attachment to each other than those of the canine species. I know little or nothing about other fishes. You are welcome to take notice on my authority of what I have said on the Salmon and if I can give you any further assistance on that point, I shall be happy to do so.—
- 47 www.darwinproject.ac.uk/letter/entry-5997 I agree with what Dr Wallace says about collecting larvæ and as those of many species of Macro-Lepidoptera differ very much in size—the females being much larger than the males—they are more conspicuous and consequently are more often collected than the smaller ones which produce males— I do not suppose Mr Stainton has ever reared whole broods of any of the Micro-Lepidoptera from the eggs—it is the only way to ascertain the numbers of each sex—
- 48 www.darwinproject.ac.uk/letter/entry-6116 What he says about the males in these cases being always species with pectinated or branched antennæ is worth notice; because Réaumur and others have contended that the sense of smell resides in those organs.
- 49 www.darwinproject.ac.uk/letter/entry-6133 You must remember that I have been breeding Lepidoptera from the egg—partly because I found this a fascinating recreation (just as others take to gardening or fishing) & partly because I had formed an intimate friendship with Mr Buckler, who was desirous of figuring the larvæ of our Macrolepidopteræ; I have not therefore—as I told Mr Stainton—tried to rear a number of specimens— 6 or 7 of a species contented me; but when I say that I dont mean that in a general way I had the chance of selecting 6 or 7 larvæ out of a large number; I simply collected as many as I wanted (that is if I could get them) & then stopt; or if offered eggs—I told my friend to send me a few; besides I think that if in the case of such species as *Eriogaster lanestris* or *Bombyx neustria* I may have selected a few larvæ out of a nest, had I taken the finest I should have expected rather they would have produced male moths—as being more advanced than their fellows, & therefore likely to be the first to emerge from pupa,—& certainly the ♂s as a rule do appear before the ♀s in the perfect state. I cannot get over my amazement at what you say about my Botanical work. By Jove, as far as my memory goes, you have strengthened instead of weakened some of the expressions. What is far more important, than anything personal, is the conviction which I feel that you will have immensely advanced the belief in the evolution of species.

50 www.darwinproject.ac.uk/letter/entry-6427 I entirely agree with what you say about each species varying according to its own peculiar laws; but at the same time it must, I think, be admitted, that the variations of most species have in the lapse of ages been extremely diversified; for I do not see how it can be otherwise explained that so many forms have acquired analogous structures for the same general object, independently, of descent.

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