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5 **The Watson-Forbes biogeographical controversy untangled 170 years later**

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22

23 **Abstract.** Hewett Cottrell Watson and Edward Forbes were two naturalists of the Victorian age. They were protagonists
24 on a dispute that generated comment and serves as an illuminating case study of misunderstanding in priority issues.
25 Watson accused Forbes of having plagiarized his original classification of the British plants into groups on the basis of
26 their geographical distribution. This controversy originated mostly from a so-far-ignored basic difference in Watson's
27 and Forbes' ideas about biogeographical regionalization. Watson's classification of the British flora into groups of
28 species with similar distribution was probably the first application of the concept of “regional chorotype”. By contrast,
29 the biogeographical classification of the British flora proposed by Forbes belongs to the concept of “element”, because
30 it was based on assumed species history (i.e. colonization routes). The two approaches may produce similar outcomes,
31 but remain conceptually different. Although personal reasons may have contributed to exacerbate the Watson-Forbes
32 controversy, failure in recognizing this distinction by its actors and their contemporaries, such as Hooker and Darwin,
33 was the most important cause.

34

35 **Keywords:** Charles Darwin, Hewett Cottrell Watson, Edward Forbes, Joseph Dalton Hooker, Biogeography,
36 Botany

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Introduction

Hewett Cottrell Watson (1804–1881) and Edward Forbes (1815–1854) were two prominent naturalists of the Victorian age (Egerton, 2012; Jenkins, 2015). Watson is now mainly known for having introduced in *Cybele Britannica* (1847–1859) a subdivision of Britain into 112 recording areas called vicecounties. This subdivision (with various modifications) served as a recording unit system in botanical and zoological studies in Britain until the present day (Seddon, 1971; Vincent, 1990; Merritt et al., 1996; Stace et al., 2003; British Bryological Society, 2016) and represented an important step towards the introduction of statistical approaches into biogeography (Browne, 1983, 1992). Forbes is mainly known as one of the founders of the so-called extensionist paradigm¹ and for his pioneering study of marine biogeography (Egerton, 1972, 2010a; Mills, 1978, 1984, 2004; Rehbock, 1983; Vincent, 1990; Lomolino et al. 2010).

 Around 1846–47, Watson and Forbes were protagonists on a strong dispute testified by some Watson's publications and Darwin's correspondence. Specifically, Watson accused Forbes of having plagiarized his original classification of the British plants into groups on the basis of their geographical distribution (i.e. by placing species with similar ranges into the same distributional group). Thus, this controversy represents an early case of priority issues in biology. The priority rule has a central role in modern science (Strevens, 2003), but priority issues can date back to the XV, XVI, and XVII centuries in many fields, especially in mathematics and physics. We can remember, among others, the cases of Tartaglia vs. Cardano for the solving rule of cubic equations; Brahe vs. Raimarus for the introduction of the geo-heliocentric model; Galileo vs. Scheiner for sunspot observations; Newton vs. Leibnitz for the invention of calculus; or Hooke vs. Huygens for the invention of the balance spring watch. In fact, multiple independent discoveries or inventions are not rare in the history of science (Merton, 1973) and famous cases in biology are the independent discovery of the natural selection by Darwin and Wallace, the use of equations describing predator-prey cycles by Lotka and Volterra, the simultaneous rediscovery of Mendel's laws by de Vries, Correns and Tschermak, or the formulation of the equilibrium principle in population genetics by Hardy, Weinberg and Castle. In all these circumstances, however, multiple independent discoveries or inventions referred to really the same things. By contrast, as I will show in this paper, in the case of Watson and Forbes, the controversy originated from a misunderstanding, because the two authors dealt with conceptually similar and related, but in fact different issues.

 Another interesting aspect of this dispute is that it was apparently unilateral. While Watson attacked vigorously Forbes, we have no evidence of Forbes' reactions. Forbes' only public comment on Watson (apart citations of *Cybele Britannica*) appeared in an 1852 review of botanical works. In this comment Forbes described Watson as “indefatigable and deservedly illustrious in statistics, but grown misanthropic by working overmuch when in ill humor, [taking] a

68 melancholy pleasure in attributing evil motives to his fellow-labourers” (Rehbock, 1983, p. 178). According to Egerton
69 (2003, p. 233), Watson was afflicted by “a lifelong personality disorder” and both his personality and private life
70 exacerbated the virulence of his attack on Forbes, which was far beyond scientific justification. As noted by Egerton
71 (1979, p. 93), “Watson's botanical work was respected by his fellow British botanists, but since he often criticized their
72 work, he had few friends among them”. Actually, it seems that Watson was a sort of outsider in Victorian society
73 (Endersby, 2003). In particular, Watson felt resentment towards Forbes because the latter (who was a talented zoologist
74 and palaeontologist, but not an eminent botanist) had in 1842 beat Watson in a contest for the chair of botany at the
75 University of London. Other sources of their antipathies may be some disagreement over the degree of geographical
76 detail to be included on a map for recording plant species localities (a project promoted by the British Association for
77 the Advancement of Science around 1840) and a Forbes’ short paper satirizing phrenology appeared in 1834 (when both
78 were students; Watson was a supporter of phrenology and became the editor of the *Phrenological Journal*). Although
79 personal reasons had an important role in the controversy, it originated mostly from a so-far-ignored basic difference in
80 Watson's and Forbes' ideas about biogeographical regionalization (differences of which Watson and Forbes themselves
81 were unaware).

82 This Watson-Forbes controversy was important in their days, yet it is now forgotten and its role in the
83 development of biogeography is unknown. In fact, the controversy is briefly cited by Browne (1983) and discussed at
84 some length by Rehbock (1983). Browne writes that “Forbes had appropriated Watson’s scheme for the subdivision of
85 the British flora to back his highly speculative account of the geological history of the British isles” (p. 74), thus
86 supporting Watson’s claim of plagiarism. On the other hand, Browne also cites Forbes and Watson as representatives of
87 opposite views about the inclusion of historical inferences in biogeography: “But the transformation from
88 “topographical” to “historical” studies was not as clear-cut as it might seem. Several botanists, Hewett Cottrell Watson
89 among them, were not sure that they wanted to include the past in their analyses of the present, and they aid as much.
90 Others [...] adopted the historical approach [...]. Charles Lyell, Edward Forbes, and Charles Darwin attempted to link
91 ancient and modern distribution patterns together as a real sequence over time” (p. 86). These opposite views are at the
92 base of a substantial misunderstanding between Watson and Forbes. According to Rehbock (1983, p. 182), “There is no
93 question that Forbes had relied more heavily upon Watson’s Researches for his phytogeographic data than upon any
94 other source [...]. More substantive, however, was the disagreement between Forbes and Watson as to what it was in
95 Forbes’ paper (and the later essay) that was truly significant. Watson felt that the only important point was the division
96 of the British flora into geographical groupings [...]. By comparison Forbes’s geological hypotheses were to Watson
97 fanciful, disapprovable, and based on a misinterpretation of botanical evidence. To Forbes, however, the priority was just

the reverse.” These different perspectives were at the base of different biogeographical approaches in species grouping which both these two naturalists and following biogeographers failed to recognise as distinct.

Recent progresses in the concepts of biogeographical element and chorotype, and a reconstruction of how these concepts have been applied by different authors (Fattorini, 2015), allows now a better understanding of the origin of this controversy and its resolution by showing that the two authors referred to different things. To this end, I present here a detailed historical reconstruction of this controversy and explain how it originated from the fact that the authors failed in recognizing that their classification systems, although similar in final outcomes, were in fact based on very different concepts.

Origin of Watson's attack on Forbes

The four-volume book *Cybele Britannica* (1847–1859) can be considered the most important work of Watson. Although not well known at the time of publication (but see Rehbock, 1983 for examples of scientists that knew and commented on this work), this book had a subsequent high intellectual significance in the history of British botany, biogeography, ecology and evolution. The influence of Watson in the history of biology is for example testified by the role that his works had in development of Darwin's revolutionary ideas (see, for example, Egerton, 1979).

Watson was interested in many aspects of plant biology, but his most important contributions were on plant biogeography. Watson's biography and works have been already summarized by Boulger (1883) and discussed in details by Egerton (1976, 1979, 2003, 2004, 2010a, 2010b, 2012). The importance of Watson's works in the history of biogeography is discussed by Browne (1983) and Rehbock (1983). Thus, I summarize here only the most essential points useful to understand his attacks on Forbes. Watson's interest in the distribution of British plants started in 1831, when he participated to a contest for the best essay on the geographical distribution of plants. Watson's essay (which, incidentally, won the gold prize offered by Robert Graham, professor of Botany at Edinburgh) was an unpublished monograph currently preserved in the archives of the Royal Botanic Garden, Kew (Egerton, 2010b). The next year, Watson published his first book, titled *Outlines of the Geographical Distribution of British Plants*. The first part of *Outlines* provides general discussions, whereas the second part presents a brief indication of habitation, topographic range, and world-wide distribution for vascular species found in the British Isles. Here, in the section “Outline of the Topographical Distribution of British Plants”, Watson applied to the British Isles the ideas already expressed in his prize essay by dividing the British vegetation into three regions, each of which subdivided into two zones, for a total of six zones (Egerton 1979, p. 91; 2003, p. 35). Watson conceived this work as a guide to botanists interested in conducting local studies for more detailed understanding of British plant geography. In later works, he sometimes

129 modified, but never abandoned, this classification scheme. In 1845, when Edward Forbes proposed a classification of
130 the British vegetation into five zones rather than six (Egerton 2010a, pp. 187–188), Watson felt insulted.

131 Forbes was a naturalist interested in virtually all aspects of natural history (Egerton, 1972, 2010a, 2012).
132 Although his botanical production was rather limited (especially if compared with that of Watson), Forbes became
133 professor of botany at the University of London in 1842. Since Forbes' professorship did not pay enough to support a
134 family, he also joined the new Geological Survey of Great Britain as a palaeontologist.

135 Tacking advantage of working simultaneously as botanist and as palaeontologist, he decided to study how the
136 plants currently living in Britain reached this region by comparing fossil plants with the modern flora. As Egerton
137 (2010b, p. 301) says: "It was a fine project if he had realized how much data he needed to analyse before he could
138 publish his findings. Unfortunately, he rather quickly concluded that there were five sources of the British fossil plants,
139 and then he proceeded to partition the living flora into a comparable five regions. He summarized this scheme at the
140 annual meeting of the British Association for the Advancement of Science in 1845, and an abstract of his talk was soon
141 published in several periodicals, followed later by a long summary in the *Report of the BAAS* [...]. But since Watson
142 had already divided the British flora into six regions, he saw Forbes' scheme as a challenge to his competency by
143 someone unqualified to do so. He suspected that Forbes' crucial data came from his *Remarks on the Geographical*
144 *Distribution of British Plants* (1835), and he went to the library of the Linnean Society of London and found that Forbes
145 had checked out that book on 16 June, about a week before he read his paper (Watson 1847–1859, I:468, 472). Forbes
146 heard of Watson's unhappiness and attempted to make amends with a generous acknowledgement when he published
147 the full paper in 1846, but since he retained his fivefold division of British plants for Watson's six-fold division,
148 Watson was not mollified." In fact, although Watson is largely cited in Forbes' (1846) paper, such citations give the
149 impression that they were introduced not so much for acknowledging Watson as a source of the scheme, but for
150 implicitly suggesting that Watson's groups were incoherent and that his work was substantially only a detailed
151 compilation of data without any relevant theory beyond:

152
153 The zoological works of Fleming, Jenyns, Yarrell, Bell, and W. Thompson, have enumerated the species
154 and treated of the distribution of our indigenous animals, those of Smith, Hooker, Lindley, Babington,
155 Henslow, and especially Watson, have done the same service for our native flora; but the history of the
156 formation, if I may so say, of that fauna and flora, remains to be investigated. (p. 337)

157 This is the Atlantic type in Mr. Watson's arrangement of British types of vegetation. (p. 339)

158 They form part of Mr. Watson's second, or Germanic, and of his third, or English, type of British
159 vegetation. (p. 339)

The Highland, and part of the Scottish and Hebridean types of British vegetation, as defined by Mr. Watson, agree with the fourth flora, as defined above. In his Hebridean type he includes the rare *Eriocaulon septangulare*, a very remarkable plant, known in Europe only in the Hebrides, and Connamara, in the West of Ireland ; elsewhere it is an inhabitant of Boreal America, which is its true native country, and from whence, either by means of transport, now or anciently in action, it has, in all probability, been introduced naturally into the British Isles. [...]

Such of its members as are generally distributed compose the British type of Mr. Watson, whilst its more local species are distributed among, and form part of his Germanic, English, and Scottish types. (p. 341)

The essays of Mr. H. C. Watson may be cited as among the most remarkable, and to them I must refer geologists who would wish to learn more respecting our indigenous flora, than it is here necessary to state. (p. 342)

Thus, it is not very surprising that Watson was unsatisfied with these citations and, in 1847, he launched a very strong public attack on Forbes in the first volume of the *Cybele Britannica*:

In his communication to the British Association Mr. Forbes had made free (if unacknowledged) use of the former labours of the writer of this volume ; and in so doing he took to himself credit for results and generalisations which had truly originated with the author of the present work. But not wishing here to interrupt the regular course of his 'Explanations,' by the introduction of comments on the opinions or conduct of Mr. Forbes, he will add the view which he feels authorized and compelled to take, in reference to Mr. Forbes's publications in the matter, as an Appendix at the end of this volume. (p. 55)

In particular, in this appendix, Watson wrote:

In accordance with the intention intimated on page 55 of the present volume, the Author now proceeds to make some comments upon the similitude between his own 'types of distribution,' explained on pages 43-54, and certain so-called 'floras,' announced by Mr. Forbes for the basis of his hypothesis concerning the origin and migration of British plants. If, while so doing, he cannot avoid giving the matter an aspect unpleasant and unfavourable to Mr. Forbes, the latter must remember that his own neglect of the courtesy and justice usually shown to the rights of priority, among the cultivators of science, is the cause to which any such disagreeable results may be traced back. (p. 465)

191 And yet, in truth, the alleged 'floras' were little else than a garbled reproduction of the 'types of
192 distribution,' — taken from the ten-years-old volume without acknowledgement,— varied too probably
193 by sheer guess- work,—and then republished as if they had resulted from Mr. Forbes's own individual
194 investigations. (pp. 468–469)

195
196 In his attack on Forbes, Watson refers to reports of a Forbes' talk published before 1846 paper:

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198 The communication of Mr. Forbes is reported at considerable length in the volume for 1845, published in
199 the name of the British Association, in the Literary Gazette, in the Athenaeum, and in the Annals and
200 Magazine of Natural History. It is believed that no wrong will be committed by attributing all those
201 reports, directly or indirectly, to the pen of Mr. Forbes himself. There is great sameness among them. The
202 printing of such reports in the volumes published for the Association, is made conditional upon the
203 authors of communications themselves sending the reports of their own papers. And small doubt indeed
204 can exist respecting the authorship of the report in the Literary Gazette, where the communication of Mr.
205 Forbes is so highly complimented. Those various reports are worded in such manner, as unavoidably to
206 impress readers with an idea that Mr. Forbes himself had originated the arrangement of British plants into
207 the "floras." (p. 468)

208
209 As regards Forbes' 1846 paper, it is important to note that *Cybele Britannica* was published 1847, but, as
210 declared by Watson himself: "As the title-page of this volume bears the date of 1847, the year of its publication, it may
211 be proper to mention that two-thirds of the volume was printed in 1846." Thus, the volume was produced
212 simultaneously with Forbes' 1846 paper. Although Forbes' paper was probably not in circulation when Watson wrote
213 his tremendous attack, yet he knew the paper and referred to it in this passage, showing that he was completely
214 unsatisfied with how Forbes acknowledged his works (and, from the excerpts reported above, it is easy to understand
215 why):

216
217 Since the Cambridge meeting of the British Association Mr. Forbes has printed a second essay on the
218 same subject; and which, it is understood, will be published in a volume connected with the Geological
219 Survey. This time, there is so great a show of evidence adduced in seeming support of the hypothesis,
220 that it will doubtless appear very plausible in the eyes of those persons (not botanists) to whom the *ipse*
221 *dixit* of its proposer is a sufficient warranty for the accuracy of his facts. This time, also, Mr. Forbes does

refer to the previously published geographicobotanical arrangement of the 'Remarks'; although his passing allusions thereto are conveyed in terms which have more the effect of slurring over his own obligations to that work, than of admitting it to have been the source whence he took the idea of his 'floras;' and from the appendix to which he ascertained their comparative "magnitude as to species." (pp. 469–470)

Forbes heard of Watson's complaints, and wrote a letter to Hooker (31 October 1846, so before the violent attack presented in *Cybele Britannica*), where he stated that the novelty of his own work was to link biological and geological data and that he did not quote authorities in the abstract to save space (see Rehbock, 1983). Of course, the lack of space seems to be not a valid justification for not citing Watson's fundamental work. In a second letter to Hooker (22 April 1847), after the publication of *Cybele Britannica*, Forbes reiterated his position, but also added: "Moreover I do not put them [= distributional types] forward as any new things, but as expressions of facts well known to all British Botanists. What I claimed for myself was the explanation of the causes of them" (letter cited in Rehbock, 1984, p. 181). Resentment against Watson is more apparent in a letter to Lyell (27 June 1850): "Watson (who is curiously enough cited as the originator of views against which he has violently and malignantly protested, being crazy)" (cited in Rehbock, 1983, p. 187)

Watson's critique of Forbes' work and its echoes in Darwin's correspondence

Watson not only accused Forbes of plagiarism, but severely criticized him for his scarce knowledge of botany and for having presented hypotheses without sufficient support. These critiques were already briefly presented in 1846 among editorial comments in the journal *The Phytologist*, of which Watson was an important contributor (Anonymous, 1846):

The longest report is devoted to Mr. Forbes' communication "On the Endemic distribution of Plants." There is a good share of clever ingenuity manifested in the geologic hypothesis of the learned botanist of King's College, and as it is made to appear based upon botanical facts, it will doubtless be received by those who are slenderly acquainted with the actual present distribution of plants, and who are unprepared to separate what is true and borrowed from what is original and imaginary, in the botanical foundation of the paper. For our own individual part, we must confess a great distaste for ad captandum articles hastily got up after the manner of Mr. Forbes's paper. We can find nothing for extract. "What is new is not true : what is true is not new." This aphorismal jingle of sound was never more justly applied than in the

present instance. Still, unsound as we consider the paper to be, it is yet one which could never have emanated from an intellect of mediocre character. Regarded as a figment of the imagination, it is ingenious—as a contribution to science it is only vicious.

This anonymous editorial note was simply signed as “C.”, but it is virtually sure that the author was Watson, whose middle name was Cottrell². Then, Watson presented an articulated criticism in the Appendix to *Cybele Britannica*:

But it must be obvious enough that the mere repetition of an arrangement previously before the public, whether disguised or undisguised, could add nothing to existing knowledge respecting the distribution of plants in this country. Nor would a variation of that arrangement be likely to conduce to the advance of scientific knowledge, unless made by a competently informed botanist, and in such manner as to render it more exactly accordant with the truths of nature. Such a result, it is confidently believed, Mr. Forbes's individual knowledge of the distribution of British plants was very far from adequate to realize ; for, whatever may be his scientific merits in other respects, that gentleman's repute, as a botanist, is more academical than personal. Moreover, no real knowledge of vegetable geography was indicated in the reports. They conveyed only generalities, vaguely expressed, without the mention of a single plant in example of any of the so-called 'floras.' And the pervading character of the whole communication, as reported, was that of a hasty and untried fancy, for support of which the propounder intended to find his facts at some after time. (p. 469)

Easily may the result be guessed. Borrowed facts, misunderstood, and applied by a forgetive imagination, make up the botanical illustrations in favour of the hypothesis. [...] In short, considering the small number of the pages in Mr. Forbes's second essay, which are devoted to the botanical bearings of the subject, it absolutely teems with errors in its botany—inconclusive arguments, inconsequent logic, inept illustrations, and the guesswork of the imagination put forth ostensibly as the ascertained facts of science. (p. 471)

In presenting this criticism, Watson also accused Forbes of having used primary data from other works without acknowledgement and uncritically:

Accordingly, a few lists of species are given, evidently made up from the slight notices of habitats introduced into 'Babington's Manual of British Botany.' [...] Though Mr. Forbes has not acknowledged the special manner in which he was indebted to that useful work, [...] he has copied so blindly therefrom as to follow its imperfections uncorrected ; including a tell-tale error of the press, precisely of a kind to mislead and betray the copy-wright from home : while he has also, in other cases, substituted his own erroneous interpretations instead of the facts rightly, though not precisely, stated by the author of the Manual; through attempting to specialize and apply the general indications of the Manual, without first caring to make himself acquainted with the facts truly intended thereby. (p. 470)

However, it should be noted that Watson recognised, at the end, the merit of Forbes of linking botany and geology. In the last volume of *Cybele Britannica* (1859, pp. 8–9), Watson wrote: “Although blundering and false in its botanical illustrations, and perhaps not less untrue in some of its zoological assertions, that treatise by Edward Forbes may still be allowed to have evinced remarkably suggestive conceptions of science, larger and more comprehensive than those which have characterised the writings of English botanists, present or past, with extremely few exceptions. [...] Edward Forbes is no more. The Author of this *Cybele* will soon be the same. But now, on looking back to the Appendix at the end of the First Volume, he finds scarcely a word there which he wishes unprinted; because he fully believes that the manner in which the Essay of Mr. Forbes was got up is correctly stated there; and because its reckless hardihood of assertion, in regard to facts, was eminently calculated to mislead those scientific men, interested in the subject, who were not specially familiar with vegetable distribution in the British Islands and neighbouring portions of the Continent, and also with the climatal adaptations of the species cited.”

Since Forbes' extensionist ideas would have had important implications for evolutionary theory, Charles Darwin was strongly interested in Watson's attack on Forbes' work (see Browne, 1983). First, Darwin was worried about how to discuss Forbes' work without entering the fray. He wanted to maintain good relationships with both, but citing Forbes' work would raise Watson's hackles, whereas ignoring the extensionist ideas would create problems with Forbes. Second, Darwin was much concerned about the validity of Forbes' paper, because he felt he was not sufficiently expert in botany to assess if Watson's criticism was founded or not. Thus, Darwin turned to Hooker for a qualified opinion.

Darwin's correspondence clearly shows how important the Forbes-Watson dispute was for him. Darwin had already written to Forbes, apparently after reading the 1845 summary, about the existence of an ancient post-Miocene landmass extending from the Mediterranean to the Azores. Darwin's letter is missing, but we know Darwin's objections from Forbes' reply, dated 25 February 1846, thus before the full 1846 paper appeared³.

313 On 25 February 1846, Darwin wrote to Hooker to discuss with him Forbes' reply. This passage is also interesting
314 because Darwin's allusion to the lack of Forbes' reference to Lyell's work suggests that Forbes was careless in citing
315 other scientists⁴:

316

317 Forbes has been writing to me, about his subsidence doctrines; I wish I had heard his full details, but I
318 have expressed to him in my ignorance my objections, which rest merely on its too great hypothetical
319 basis; I shall be curious, when I meet him, to hear what he says— He is also speculating on the gulf-
320 weed. I confess I cannot appreciate his reasoning about his miocene continent, but I daresay it is from
321 want of knowledge.—

322 You allude to the Scicily-flora, not being peculiar, & this being caused by its recent elevation (well
323 established) in main part; you will find Lyell has put forward this very clearly & well.— The Appenines,
324 (which I was somewhere lately reading about) seems a very curious case.—

325 I think Forbes ought to allude a little to Lyell's work on nearly the very same subject as his speculations;
326 not that I mean that Forbes wishes to take the smallest credit from him or any man alive: no man, as far
327 as I see, likes so much to give credit to others, or more soars above the petty craving for self-celebrity.—
328 If you come to any more conclusions about polymorphism, I shd be very glad to hear the result; it is
329 delightful to have many points fermenting in one's brains, & your letters & conclusion always give one
330 plenty of this same fermentation. I wish I cd ever make any return for all your facts, views &
331 suggestions.

332

333 Darwin also sent Forbes' letter to Hooker⁵, and on 2 March 1846 Hooker wrote to Darwin his comments⁶. In his
334 reply, Hooker discusses several points in detail, showing that botanical evidence conflicted with parts of Forbes'
335 hypothesis, but supported others.

336 Thus, both Darwin and Hooker were quite sceptical about Forbes' extensionist ideas, but there is no indication,
337 in these letters, of Watson's attack on Forbes biogeographical groupings of the British plants. Watson's accusations
338 became discussed between March and August 1846, because in an undated letter (written however before 3 September
339 1846), Hooker wrote to Darwin⁷:

340

341 This probable fracas between the 2 Geographers distresses me, for they are almost the only 2 men who
342 have looked on British Flora with the eyes of philosophers. Watson in particular ranks in my opinion at
343 the very head of English Botanists, whether for knowledge of species or of their distribution; he first

wrote philosophically upon them & his works are of the highest order. Unfortunately he is touchy & very *severe* when first offended, though he never holds a grudge long.

In his response to this letter (dated 3 September) Darwin suggested a diplomatic exculpation for Forbes' fault in acknowledging Watson's works⁸:

I have not yet seen Forbe's memoir, but have ordered it, & will enjoy writing to you my opinion. I am very sorry to hear what you say about Watson's previous work; I feel sure that Forbe's own noble indifference to fame is the main cause of his not in some instances making proper acknowledgment.—Horner (*private*) tells me that he has just remonstrated with him, for not having mentioned Lyell's views on climatic changes, & his answer was,—“I shd as soon have thought it necessary to refer to Linnæus, as originator of specific characters”.—& I have no doubt this is the simple truth.— I cannot remember whether I have ever read (except a few papers) any of Watson's works: could you sometime lend me the chief? I shd much like to see them.

This letter also shows that the lack of reference to Lyell's work, already noted by Darwin in the 25 February letter, did not go unnoticed, but Lyell was evidently satisfied with Forbes' reply and did not enter into controversy with him. On the 28 September Hooker stopped the delay and finally formulated a clear criticism to Forbes' work by highlighting his inadequate knowledge of plant distribution, yet stressing his good faith⁹:

I have not seen Forbes since studying his paper & really do not know what to say when I do, for he will be sure to ask me about it, & most unfortunately he does not seem to know the Geographic Distrib. of the English Plants. I must confess to have taken his modification of Watson's types of vegetation as correct, & this for granted, but I had occasion to look closely at them the other day & find his S.E Flora, numbered III., to be altogether a fallacy: all or almost all the 20 species on whose supposed presence he founds it, being as common or more in the W. or N as in the E. or S. & some of them not existing in the S.E. at all! or if so as introduced species. I now see the cause for Watson's being so peculiarly savage & offering me proof that all that is correct is mere plagiarism. I still however quite acquit Forbes of any intentional piracy, he has long & early understood & appreciated Watson's views & has fancied that he has grounds for modifying them. I do all I can to appease W., but in vain, he threatens to denounce F. publicly & if he does I fear that it will read awkwardly for our friend. I need not ask you to say nothing

375 of this, except you can offer some way or means of keeping these, almost the only 2 Philosophical Brit.
376 Botanists, out of a broil, at which all the dirty species-mongers will chuckle.
377 I will send you one of Watson's works at once: you must judge it by what has been previously done, or
378 even done up to this time, by any other Brit. Bot.; more than by its own intrinsic merits, which however
379 are very high.

380
381 Darwin prudently avoided contacting Watson until after Forbes died in 1854, but he had an intense
382 correspondence with Watson from the end of November 1854. Even after Forbes' death, Watson persisted in
383 denouncing his misconduct and wrong conclusions.

384 In a letter to Darwin dated 11 October 1855, Watson wrote¹⁰:

385
386 In reference to a remark of your letter, closely following the name of E. Forbes. I claim no property in
387 what I may write during such a correspondence as ours. I simply comment on your ideas, & you are fully
388 at liberty to use or apply such comments in any way you like. You probably deem me touchy or
389 tenacious of aught that has been my own, because I fell foul of Forbes in regard to certain geographical
390 groupings of British plants.—Forbes did not re-examine these groupings,—ascertain their correctness,—
391 & then apply them to his further object. Had he done so, they would have become his also; & I should
392 have said nothing (or little) about his mere omission to state that such groupings were not *original with*
393 *him*. But he practised a *fraud* on the British Association & general public of science, by giving on his
394 sole personal authority the results of long investigations & comparisons which he had neither made, nor
395 repeated, nor imitated,—simply misappropriated, & without clearly understanding them. Not any
396 evidence drawn from the animal kingdom was given in his own printed reports of the communication to
397 the B.A. The “facts” afterwards found from the animal world, were found under water by a man not
398 remarkably conscientious, & to whom it had become a sort of necessity to find them. I shall believe them
399 when confirmed by some other naturalist, conscientious & uncommitted.

400
401 Watson discussed with Darwin the land-bridge hypothesis of Forbes in a letter to Darwin dated 10 June 1856¹¹:

402
403 I know not whether a few words in reference to Mr. Forbes's views about former expanses of land,
404 alluded to in your letter of 7th, will be worth your trouble in reading them. [...] Forbes was not under the
405 trammels of a very strict Conscience. He would be more likely to find “facts” to suit a conjecture, than

those calculated to test its soundness. I should deem any one unwise who adapted his theories to Forbes's statements of facts, unless those facts were verified afresh, or corroborated by others of like nature.

This accuse of speculation and construction of theories to which an *ad hoc* selection of facts is attached as a support, is not only a severe attack on Forbes, but it is also paradigmatic of Watson life-long insistence on the importance of collecting primary and accurate data before attempting any generalization. While Watson dedicated most of his scientific activity to the systematic collection of data, Forbes was more interested in using available data to formulate theories. These different attitudes are an important point to be taken into account to understand the controversy.

A resolution

Watson's (1832, 1835, 1847–1859) classification of the British flora into groups on the basis of species distribution in the British Isles is probably the first attempt to identify groups of species with “statistically” similar distributions *within* a certain region, what Fattorini (2015) calls “regional chorotypes”. In particular, in *Cybele Britannica*, Watson (1847) identified seven groups: (1) British type (species widely occurring in England and Scotland); (2) English type (species prevalently distributed in England); (3) Scottish type (species prevalently distributed in Scotland); (4) Highland type (species occurring in mountain areas of Wales, Scotland and England); (5) Germanic type (species occurring in eastern England); (6) Atlantic type (species prevalently distributed in the west and south-west Britain); and (7) local or doubtful type (species with scattered distribution):

In addition to their distribution by provinces and climatic zones, there is a third mode of indicating the geographical relations of plants, which may also require some explanation. It has been before observed that certain species are spread over the whole island, while others are limited to one, two, three or more of the provinces. The same holds true in the zones; some species occurring in all of them, others in one or more. Perhaps no two species have exactly the same distribution or relative frequency; and yet certain general similarities may be traced, by which the species may be grouped together under a few leading *Types of distribution*. In the small volume before alluded to, by the title of ‘Remarks,’ certain “geographic types” were indicated (pp. 86–89); and in the ‘Tabular Appendix’ to the same volume (115–184) the species were severally assigned to their peculiar types. This was simply an attempt to express, by a single term, the leading character of their distribution, with reference to geographical position and

climate. Six types of distribution were particularly mentioned; under one or other of which, it was thought, nearly all the species of plants indigenous in Britain might respectively be arranged. No attempt, however, was made to define the precise limits of the types geographically. Nor, indeed, could any exact boundary lines be traced on a map, without abruptly cutting asunder the fine gradations of Nature; for the types pass into each other without any hard or abrupt lines of distinction. In slightly describing the several types, in the former volume, a different order of succession was adopted, and consequently the nos. affixed to them were different also; but in other respects they were essentially the same as the following:—

1. *The British Type*. – In this group will be included those species which are found in all, or nearly all, of the eighteen provinces before explained; and which, moreover, are not so exclusively prevalent or predominant in any particular portion of the island, as to bring them clearly within one or other of the following types. Some of the species may be regarded as of universal occurrence in this country, growing in all the eighteen provinces, probably in every county, and even in all the six ascending zones of vegetation or climate also. Few species, however, even of this most general type, are so very general in their distribution. [...]

2. *The English Type*. – The plants of this geographic type are distinguished from those of the British type by having their chief prevalence in England, and particularly in its more southern provinces; whence they gradually become rare in a northern direction, and finally (with few peculiar exceptions) find an earlier northern limit or cessation than those of the preceding type. [...]

3. *The Scottish Type*. – This may be deemed the opposite of the English type; the distribution of the species referred hereto being characterized by a northern tendency, either by absolute limitation to Scotland or the north of England, or otherwise by a chief prevalence there and increased rarity southward. [...]

4. *Highland Type*. – This may be considered the boreal flora in a more intense degree, as respects climate, than that of the Scottish type. The species referred hereto are distinguished from those of the Scottish type by being more especially limited to the mountains or their immediate vicinity. [...]

5. *The Germanic Type*. – The distribution of several species which might otherwise be associated with those of the English type, is peculiarly characterized by a tendency to the eastern side of the island. [...]

6. *The Atlantic Type*. – Contrary to the peculiarity of distribution which constitutes the Germanic type, there is in that of other species a marked tendency towards the western and south-western coasts or counties. [...]

7. *A Local or doubtful Type*. – Interspersed about the island, there are some species whose distribution is restricted to single or few counties. [...]

This biogeographical classification was aimed at identifying groups of species that roughly occupy the same geographical units (e.g. regions or grid cells) within a certain area, without reference to their overall distribution and without any particular assumption about their history. This biogeographical approach has been championed in the 1970s when the use of computers allowed the application of automatic classification procedures to manage large matrices of species presence/absence across geographical units.

To indicate these groups of species Baroni Urbani et al. (1978) used the word “chorotype”. The word chorotype has been used with this meaning in a number of studies (see, for example, Marquez et al., 1997; Vergas et al., 1997; Sans-Fuentes & Ventura, 2000; Olivero et al., 2001; Ferrer-Castan & Vetaas, 2003; Gómez-González et al., 2004; Báez et al., 2005; Real et al., 2008) and operationally also corresponds to the concept of Hausdorf & Hennig’s (2003) biotic element. However, because the word chorotype is also used to indicate groups into which species with roughly similar overall ranges can be classified, Fattorini (2015) proposed to distinguish between “overall” and “regional” chorotypes.

While Watson’s works belongs to the history of the concept of chorotype, Forbes’ groups can be more correctly associated with the history of the concept of “element” *sensu* Passalacqua (2015), i.e. species groups based on biogeographical principles. The biogeographical classification of the British flora proposed by Forbes belongs to the concept of element because it is not merely based on species distribution (as Watson’s groups), but on species history. As observed by Rehbock (1983, p. 182): “The primary thrust of his [= Forbes’] research program was geological: to establish how and when the various components of the British flora [...] had arrived in Britain. [...] Where Watson’s floral types were based solely on the present distribution and association of species with Britain, Forbes regarded each of his floras as being part of a temporally dependent, European grouping.” Forbes himself, in a letter to Hooker (22 April 1847; reported in Rehbock, 1983), stressed what he considered a fundamental difference between his and Watson’s groupings: “I did not call “the five floras,” modifications of his – because I did not and cannot regard them as such: they being *types of external relations*, his types of *internal relations*. I never meant to substitute them for his, because both I looked upon as equally true – but as meaning different things.” In fact, the difference, completely overlooked by Watson and only partially understood by Forbes himself, is not limited to the scale (British vs. European), but involved a very different logic. Watson’s types were merely phenomenological, whereas Forbes attached an historical meaning to his groups.

Forbes recognized five groups of species on the basis of their presumed immigration time and routes into the British Isles: (1) Iberian (or Asturian) species, found in the north of Spain, and assumed to be the most ancient group;

499 (2) Armorican (or Gallian) species, found in the Channel Isles and western France, also assumed to be a very ancient
500 group, but less than the first one; (3) Kentish species, found in northern and north-eastern France, and assumed to have
501 colonized the British Isles through an ancient union between the two sites of the Channel; (4) Scandinavian (or Boreal)
502 species, found in the Arctic and sub-Arctic region, immigrated during the glacials; and (5) the Germanic species,
503 distributed in central and west-central Europe, immigrated after deglaciation (see Rehbock, 1983 for details).

504

505 The vegetation of the British Isles presents a union of five well-marked floras, four of which are
506 restricted to definite provinces, whilst the fifth, besides exclusively claiming a great part of the area,
507 overspreads and commingles with all the others.

508 I. Commencing with the smallest, we find the mountainous districts of the west and south-west of Ireland
509 characterized by botanical peculiarities which depend on the presence of a few prolific species belonging
510 to the families *Saxifragæ*, *Ericaceæ*, *Lentibulariæ*, and *Cruciferæ*. The nearest points of Europe where
511 these plants are native is the north of Spain. [...]

512 II. In the south-west of England and south-east of Ireland we find a flora which includes a number of
513 species not elsewhere seen in the British Isles, and which is intimately related to that of the Channel
514 Isles, and the neighbouring part of France. [...] This is the *Atlantic type* in Mr. Watson's arrangement of
515 British types of vegetation. [...]

516 III. In the south-east of England, where the rocks of the cretaceous system are chiefly developed, we find
517 the vegetation distinguished by the presence of a number of species common to this district and the
518 opposite coast of France. Here are the localities of the well-known *chalk* plants, much sought after by
519 botanists from the north. They form part of Mr. Watson's second, or Germanic, and of his third, or
520 English, type of British vegetation.

521 IV. The summits of our British Alps have always yielded to the botanist a rich harvest of plants which he
522 could not meet with elsewhere among these islands. The species of these mountain plants are most
523 numerous on the Scotch mountains [...]

524 V. The fifth and general flora of the British Islands—everywhere present, alone or in company with the
525 others—is identical as to species with the Flora of Central and Western Europe—that which may be
526 properly styled *Germanic*. [...]

527 During the post-pliocene epoch, over the elevated bed of the glacial sea, the great mass of the flora and
528 fauna of the British Isles migrated from the Germanic regions of the continent. The whole of the flora I
529 have numbered V., including the great mass of British plants, is Germanic. [...]

530 We have seen that the great Germanic and central British plains themselves were portions of the elevated
531 bed of a pre-existing sea, which sea [...] was the sea of the Glacial period [...].

532 Now it was during this epoch (the epoch of my IVth flora), that Scotland and Wales, and part of Ireland,
533 then groups of islands in this ice-bound sea, received their alpine flora and a small portion of their fauna.
534 [...]

535 As a general rule, we may regard the most southern floras to be oldest, especially when, as in these cases,
536 they are more and more fragmentary, and their character is more and more southern.

537 That which I have numbered III. is the most extensive, and from the number of species which are
538 exclusively or chiefly found in chalk districts in this country, I have called it the Kentish flora. [...] Still
539 more ancient appears to have been the flora numbered II., the peculiarities of which are seen more
540 especially in Cornwall and Devon, and in the south-east of Ireland. This flora—a relic of a larger—is
541 undoubtedly a part of that which we find in the Channel Isles, and in the adjacent provinces of France. It
542 is still more southern in character than No. III., exhibiting the features of the transition between the great
543 flora of central Europe and that of the southern or Mediterranean region. [...]

544 Whatever doubts may be entertained respecting the antiquity of the Kentish and Devon floras, there can
545 be none (if my premises be granted) respecting that which I have numbered I., and from which the
546 peculiar botanical character of the south-west and west of Ireland is derived.

547

548 My reconstruction of the history of this controversy illustrates how conceptually complex a seemingly
549 straightforward approach – that of grouping species according to the places they inhabit – can get. This complexity
550 cannot be resolved by reducing it to one concept, because it reflects two different research programmes, whose
551 distinction was not clear to Watson and Forbes and it is still matter of confusion among biogeographers (see Fattorini,
552 2015): one programme is devoted to group species with similar ranges into general distributional patterns for
553 descriptive purposes with no explicit assumption about how such patterns originated; the other is aimed at grouping
554 species according to their biogeographical affinities, thus assuming that a common history is reflected by similar
555 patterns. The protagonists did not know that they were using different conceptual schemes because the underlying
556 biogeographical concepts were very subtle, even if they may seem simple at first sight. Failure to recognize these
557 differences was one of the root causes of the controversy. It is difficult to imagine if the protagonists could realize that
558 they were dealing with actually different research programmes. At the time of the controversy, biogeography was at its
559 infancy and failure in recognizing this distinction until recent times (see Fattorini, 2015 for a discussion) suggests that
560 the misunderstanding was probably impossible to escape.

561 The conceptual difference between Watson's and Forbes' groups is a clear reflection of authors' different
562 perspectives and attitudes that turned into different, but difficult to distinguish, research programmes. Watson was a
563 botanist mainly amassing large quantities of data on species distribution, as detailed as possible. By contrast, Forbes
564 was a palaeontologist and thus introduced a temporal component in his classification scheme by attempting to define
565 groups based on common history.

566 The two approaches have obvious overlaps, because species that tend to have the same distribution also have a
567 similar history. Thus, the the two approaches may produce similar outcomes, but remain conceptually different.
568 Although personal reasons may have contributed to exacerbate the Watson-Forbes controversy, failure in recognizing
569 this distinction by its actors and their contemporaries, such as Hooker and Darwin, was the most important cause.

570

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¹ Extensionism refers to the hypothesis that currently disjunct distributions can be explained by assuming that landmasses had larger extent in the past. The idea that landmasses were formerly larger can be already found in Lyell (1830-1833), but became popular after Forbes' work and was largely supported by Hooker to explain biogeographical patterns. According to the extensionists, long-distance dispersal across barriers is a rare phenomenon and can be hardly evoked to explain cosmopolitan and disjunct distributions. Thus, they proposed that species used past transoceanic land-bridge connections and ancient continents that are now submerged. Darwin was adverse to the extensionism, being on the contrary a strong supporter of the hypothesis that cosmopolitan and disjunct distributions can be explained by current and past dispersal events with no need to suppose the existence of former landmasses. Yet, the extensionist hypothesis became an influential paradigm in historical biogeography throughout the latter decades of the nineteenth century (Browne, 1983; Rehbock, 1983).

² See editor's note on a letter of Hooker to Darwin, before 3 September 1846, DAR Letter 994. I use 'DAR' to refer to Darwin Correspondence Project Database, <http://www.darwinproject.ac.uk>. Accessed 27 Febr. 2016.

³ Forbes to Darwin, 25 February 1846. DAR letter 956.

⁴ Darwin to Hooker, 25 February 1846. DAR letter 955.

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