Type Specimens and Reference[1]

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1. Introduction

In an ingenious and provocative paper, "Individualism, Type Specimens, and the Scrutability of Species Membership", Alex Levine argues that "species membership, by which I mean the relation that connects a given organism, *o*, with the species *S* of which it is part, is a fundamentally contingent matter" (2001, 333). He finds this contingency in conflict with the role of "type specimens" in biology. He points out that "naming a species requires collecting and preserving one, or at most a very few specimens of the species in question" (327). David Hull has the following view of this practice:

> The sole function of the type specimen is to be the name bearer for its species. No matter in which species the type specimen is placed, its name goes with it. (Hull 1982, 484)

Levine takes Hull's view, together with the "rigid designation" theory of reference, to entail that any organism selected as the type specimen for a species is necessarily a member of that species. This generates the conflict that Levine sums up neatly as follows: "*qua organism*, the type specimen belongs to its respective species contingently, while *qua type specimen*, it belongs necessarily"; he finds this "paradoxical" (Levine 2001, 334).

What precisely is Levine's necessity thesis about type specimens? Joseph LaPorte (2003) has clarified this question. He starts with the following statement of the thesis: "It is necessary that any species with a type specimen contains its type specimen". He points out that such statements have two readings:

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[1] A version of this paper appears as chapter 5 in Devitt 2023.

> The *de dicto* reading of the statement in question would typically be expressed thus: "Necessarily, any species with a type specimen contains its type specimen." The *de re* reading would be expressed: "Any species with a type specimen necessarily contains its type specimen". (LaPorte 2003, 586)

Laporte thinks that although the *de dicto* reading is true (2003, 587), the *de re* one is not, and this resolves the paradox. The first major concern of this paper is to argue that the *de dicto* reading, which I shall call "*Levine's Thesis*", is false. That is my conclusion *C1*.

LaPorte's response to Levine's alleged paradox was followed by several others: Matthew Haber (2012), Joeri Witteveen (2015), and Jerzy Brzozowski (2020). Haber argues that *Levine's Thesis* is false. Witteveen argues against Haber. Brzozowski defends Haber's position.

My argument for *C1* in section 3 appeals only to biology, with no mention of theories of reference. Indeed, I take the rejection of *Levine's Thesis* to be straightforwardly present in the words of biologists themselves. So why have some of these philosophers of biology accepted *Levine's Thesis* and all of them found the matter much more complicated? Answering that question is the other major concern of this paper. I shall argue that discussions of *Levine's Thesis*, whether for or against, have gone awry because of mistakes about language. One mistake is about the bearing of theories of reference on the assessment of a biological claim like *Levine's Thesis*. That is the subject of conclusion *C2*, argued in section 4. Another mistake is about reference itself. That is the subject of conclusion *C3*, argued in section 5. A final mistake is about the relation between linguistic decisions and the world. That is the subject of conclusion *C4*, argued in section 6. In sum, the engaging debate about *Levine's Thesis* has been misguided. In section 7, I consider some objections.

LaPorte's *de re* reading is not a major concern, but what about it? LaPorte thinks that it is false because of the possibility of the type specimen "never having been born" (2003, 587). I agree: no member is essential to a species. But he and Levine have another reason for thinking that the *de re* reading is false, one that LaPorte sets aside here (2003, 584). They both reject what LaPorte (1997) has aptly called "*Essential Membership*", the doctrine that an organism that belongs to a taxon does so essentially. If no organism is essentially a member of its species, then no type specimen is. So, even if the actual type specimen for a species *is* born in another possible world, it might not be a member of that very species in that world. I must reject this reasoning because I have argued elsewhere (Devitt 2018b) *for Essential Membership*. Still, I agree that no type specimen of a species is necessarily a member of that species because, as we shall see in section 3, what counts against the *de dicto* reading (*Levine's Thesis*) counts also against the *de re* one.

2. The causal theory of reference and *Levine's Thesis*

Let us consider Levine's path to his Thesis. It starts with David Hull's "compelling account of the role of type specimens in the practice of taxonomy" (Levine 2001, 325), an account Hull offers in urging individualism and anti-essentialism about species.[2] Michael Ghiselin, who shares those views, is led to say: "As species are individuals, there is but one rigorous way to define their names: ostensively, in a manner analogous to a christening" (Ghiselin 1966, 209). Levine remarks: "It is interesting that Ghiselin's analogy to christening predates the literature on the Kripke–Putnam theory of reference (Levine 2001, 336, n. 3). And Levine notes that Hull was "quick to recognize" a connection between his view of type specimens and the Kripke–Putnam theory of reference:

> the importance [Hull] ascribes to the collection of type specimens in the ostensive naming of a species is strongly reminiscent of the role played by acts of baptism or dubbing in the Kripke–Putnam theory of rigid designation. (Ibid., 328)

Others noted this too (LaPorte 2003, 584; Haber 2012, 770; Witteveen 2015, 570; Brzozowski 2020, 2).[3]

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[2] Ghiselin (1974) and David Hull (1978) take their view that species are *individuals* and not kinds to be an antidote to essentialism. I agree with those like Okasha (2002, 193–94) who think that this individualism is a red herring to the essentialism issue (Devitt 2008, 348).

[3] Devitt (2008, 2018a, 2018b) are among the papers cited by Brzozowski as offering "defenses of the causal-theoretical account of typification" (Brzozowski 2020, 7). This is very odd because there is no such defense in any of these papers. Indeed, their only mention of type specimens and the

Now I note first that the more usual, and much better, name for the Kripke–Putnam theory is "the causal theory of reference".[4] In any case, what was central and most novel about the Kripke–Putnam theory was not the appeal to dubbing, which we will consider in a moment, but the idea of epistemically undemanding *reference borrowing*: people who are very ignorant, even wrong, about the referent of a term, whether a proper name or a "natural kind" term, can nonetheless be competent users of the term simply in virtue of borrowing its reference from someone who was competent; there is a causal chain of such borrowings all the way back to the people who fixed the reference in a dubbing. This was a truly revolutionary idea. And Hull embraced that too:

> In rigid designation, a name is conferred in an initial baptismal act (possibly fictitious) and thereafter passed on in a link-to-link reference preserving chain. Regardless of the appropriateness of the Kripke–Putnam analysis in general, it accurately depicts the way in which systematists introduce the names of biological taxa. (Hull 1982, 491–492)

There was nothing novel, or particularly interesting, about drawing attention to dubbings as the typical way that proper names and some "natural kind" terms get their reference. Previous theorists of reference had not failed to notice the

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causal theory of reference together is in a footnote sentence (Devitt 2018b, 39, n. 3) that concerns something else: the sentence foreshadows the conclusion that the causal theory does not imply *Levine's Thesis* (section 4).

[4] (I) Kripke (1980) carefully defined "rigid designator" for singular terms for the purpose of arguing that standard description theories of the reference-determining meaning of proper names are false. But, as quickly became apparent, this argument is easily avoided by a *description* (not causal) theory of rigid designation: a name's meaning is expressed by a *rigidified* description (Devitt and Sterelny 1999, 53–54). (II) The name "rigid designation" is particularly infelicitous for the Kripke–Putnam theory of "natural kind" terms. For, though Kripke extended his talk of "rigid designator" to general terms he did not provide a definition of its use for general terms. Just what the "rigidity" of such a term amounts to, or should amount to, is unclear, as quite a large literature shows; see, for example: LaPorte 2000; Schwartz 2002; Devitt 2005.

obvious fact that the names of many entities—babies, pets, ships, newly discovered animals and substances, and so on—typically acquire reference-determining meanings at baptisms and the like. But *what* meaning and reference was thus acquired in a dubbing, and *how*? *That was the issue.* The established "description theories" all assumed that the resulting reference was determined by descriptions that all competent with the new term associated with it. The major novelty of the Kripke–Putnam causal theory was, first, to reject that theory and, second, to emphasize that reference is fixed by dubbers *who then pass on the benefits of dubbings to others who may know little or nothing about the referent*. But what did the Kripke–Putnam theory tell us about that reference fixing in a dubbing? Not very much. Thus Kripke, discussing proper names in *Naming and Necessity*, talks briefly of "fixing a reference by description, or ostension" (Kripke 1980, 97). Howard Wettstein thinks fixing by description was Kripke's "paradigm" (Wettstein 2012, 115). Putnam talks of an "ostensive definition", but one accompanied by a description (Putnam 1975, 225–229): as he emphasized later, "descriptions play a key role: the original dubber or dubbers identify or have the capacity to identify what they are talking about by definite descriptions" (Putnam 2001, 496–97).

Indeed, it was hard then, and is hard now, for anyone to say much about what goes on in reference fixing. Ostension always struck me as the right way to go, but then what determines that a particular object is the object of ostension? There have been description theories of that too (Reichenbach 1947; Schiffer 1978). I favored a causal theory: reference is fixed in an object, directly or indirectly, by the causal link between a person and the object when it is the focus of that person's perception. This is what I call a "grounding" (Devitt 1974, 1981a).

So, on this view of reference fixing, the original users have their ability to designate Aristotle by "Aristotle" in virtue of a certain causal link to him and then we inherited this ability to designate him by reference borrowing. Even if one goes along with these old discussions of reference fixing, much is left unexplained, as I summarized in a recent update (Devitt 2015b). Still, those discussions did include a development that is very relevant to *Levine's Thesis*, the idea of "multiple grounding". I will get to this in section 5.

Return to Hull and Levine. Given their individualism, they think that the name attached to a species by a type specimen is a *proper* name (Levine 2001, 329). They clearly reject the idea that the reference of that proper name is fixed by means of a description of the Aristotelian essence of the species. But then how do they think that reference *is* fixed? Levine has this to say:

> What allows such rigid designators to attach to their referents irrespective of the truth of any associated descriptions is that *they acquire their meanings in acts of dubbing or baptism*…. The similarity between the collection of type specimens, as understood by Hull, and such acts of baptism, should be evident. In the former, a biologist, in direct contact with a part of the target species (the specimen), attaches a name to a species without thereby proposing an Aristotelian definition. (Levine 2001, 328)

The theory of grounding that I have just described is clearly a "direct-contact" view of reference fixing and so it is not surprising that Levine (2001, 330–332) is sympathetic to it (and aware of some of its difficulties).

How do we get from this sort of causal theory to *Levine's Thesis*, "Necessarily, any species with a type specimen contains its type specimen"? The Thesis comes from the following view: "No matter in which species the type specimen is placed, its name goes with it" (Hull 1982, 484). Thus, the above-quoted passage, in which Hull likens the "rigid designation" theory's treatment of the "initial baptismal act" to the introduction of "the names of biological taxa", is followed by this:

> Both… require reference preservation. The respective terms cannot change their reference, although we can find out that we are mistaken about what we thought their reference was. (Hull 1982, 492)

This idea that the reference "cannot change" suggests to Levine that "the relation between a type specimen and the reference of its species name is… necessary" (Levine 2001, 334).

So Levine thinks that the causal theory applied to the species naming procedure implies *Levine's Thesis*. All his respondents agree. Now, anyone who accepts this implication and favors the causal theory might well be led to embrace *Levine's Thesis*. Indeed, that is clearly the path of Levine and LaPorte; it seems also to be the path of Witteveen, as we shall see (sec 6.2). Yet is it really appropriate to embrace a biological thesis like Levine's on the basis of a theory of reference? I think not. Semantics should not be dictating to biology. Rather, semantics should answer to biology. This claim reflects the methodology of "putting metaphysics first" that I have argued for in a book of that name:

> We should approach epistemology and semantics from a metaphysical perspective rather than vice versa. We should do this because we know much more about the way the world is than we do about how we know about, or refer to, that world. (Devitt 2010, 2)

It follows that it is a mistake to use *any* semantic thesis to assess *any* biological thesis; the direction of assessment should be the reverse. Applying this to our particular issue yields another one of my conclusions, *C2*: *it is a mistake to use a theory of reference to assess Levine's Thesis*. My argument for this is in section 4.

Still we are interested in semantics as well as biology and so we do need a theory of reference that is compatible with the biological facts including, according to *C1*, the falsity of *Levine's Thesis*. In section 5, I shall argue that the causal theory is compatible *once we take account of multiple grounding*; for multiple grounding allows reference to change. So, I think that Levine and his respondents are wrong to accept the above implication: *the causal theory of reference does not imply Levine's Thesis*. This is my conclusion *C3*, to be argued in section 5.

I turn now to an evaluation of *Levine's Thesis*, an evaluation that will, of course, make no appeal to theories of reference.

3. The falsity of *Levine's Thesis*; the case for *C1*

Haber came up with an excellent example which has appropriately been at the center of the discussions of *Levine's Thesis* and will be at the center of mine:

> In the late 1990s a minor taxonomic scuffle arose over the endangered San Francisco Garter Snake (*Thamnophis sirtalis tetrataenia*, Cope in Yarrow 1875), and the common California Red-Sided Garter Snake (*Thamnophis sirtalis infernalis*, de Blainville 1835). Researchers discovered that *T. s. infernalis'* type specimen belonged to *T. s. tetrataenia* (Boundy and Rossman 1995; Barry et al. 1996). Typically in such cases the taxa would be re-named. The codes of taxonomic nomenclature are clear on this, with rules specifying just how to handle such cases, e.g., the principles of priority and typification (ICZN 1999, Art. 23, 61). In this case, though, a petition was submitted to the International Commission on Zoological Nomenclature (ICZN) requesting that the names be conserved for each taxon in question. The case was published (Barry and Jennings 1998), commentary solicited (Smith 1999), and a ruling issued (ICZN 1999): Opinion 1961 of the ICZN stated that a new type specimen had been designated for *T. s. infernalis*, thus conserving prevailing usage of the names. (Haber 2012, 767–8)

This example is about the type specimen of a *sub*species whereas *Levine's Thesis* is explicitly about species. Still what goes for the type specimen of a species goes for that of a subspecies. So we should take *Levine's Thesis* as being implicitly about subspecies too.

The 1835 type specimen, or holotype, for *T. s. infernalis* (originally *Coluber infernalis*) is held in a museum in Paris and catalogued as "MNHN 846" (Boundy and Rossman 1995). *Levine's Thesis* is:

> Necessarily, any species with a type specimen contains its type specimen.

Applying this to the subspecies *T. s. infernalis*, we get:

> Necessarily, *T. s. infernalis* contains its type specimen.

Does it? The resounding answer from experts is "No". The experts we need are those who know most about the type specimens of garter snakes, biologists, particularly taxonomists. We shall see that some think that the type specimen of *infernalis*, MNHN 846, *is not* a *T. s. infernalis* and others think that it *may well not be*. There is no sign of any expert thinking that *it must be*. So, *Levine's Thesis* is false—conclusion *C1*—and there is no paradox.

It will help to show this if we identify two propositions that are entailed by the application of *Levine's Thesis* to this example. First, and most obviously:

> **HOLO**: MNHN 846, the type specimen for *T. s. infernalis*, is an *infernalis*.

Boundy and Rossman's claimed discovery that 846 is, in fact, from the snakes popularly known as San Francisco Peninsula garter snakes has not been contested. So let us assume it is so. Then, with that discovery, the application of *Levine's Thesis* entails that *T. s. infernalis* is (and always has been) the subspecies of those Peninsula snakes and not, as everyone has thought for decades, the subspecies of snakes popularly known as California coastal red-sided garter snakes. For, according to the discovery, 846, the type specimen of *T. s. infernalis*, is in the former subspecies not the latter. So:

> **INF** *T. s. infernalis* is the subspecies of San Francisco Peninsula garter snakes not the subspecies of California coastal red-sided garter snakes.

The very bad news for *Levine's Thesis* is simple: there is *no sign at all* of any expert endorsing either HOLO or INF and lots of signs of their not doing so.

Consider Boundy and Rossman 1995 on HOLO. They note that a 1941 review "restricted the name *infernalis* to the California coastal subspecies" and "revived the name *T. s. tetrataenia*" for "the San Francisco Peninsula populations" (Boundy and Rossman 1995, 236). As a result, at the time of their paper, as other biologists remark, "the taxonomy of the western subspecies of *Thamnophis sirtalis* has been resolved and well-accepted for 45 years" (Barry et al. 1996, 172). Boundy and Rossman have a detailed discussion of whether holotype MNHN 846 should be allocated to "either of the populations currently known as *T. s. infernalis* or *T. s. tetrataenia* or of an intermediate between the two" (Boundy and Rossman 1995, 237). They found that a certain

> combination of pattern elements on individual snakes is limited to the San Francisco Peninsula… within populations of typical

> *T. s. tetrataenia*. The geographic restriction of this pattern strongly indicates that the holotype of *C. infernalis* is assignable to those populations… The holotype belongs to a population(s) outside the geographic range and definition of *T. s. infernalis* as currently recognized. (Ibid., 238)

In other words, *MNHN 846 had been misidentified and is not an infernalis: HOLO is false*.

Now consider Barry and Jennings 1998. In their petition against Boundy and Rossman's proposal, they claim: "It is possible that the holotype of *T. s. infernalis* is a specimen of *T. s. tetrataenia*" (Barry and Jennings 1998, 224). In other words, *MNHN 846 might have been misidentified as an infernalis and HOLO might be false*. *Levine's Thesis* cannot allow this because it entails that 846 *cannot* be both a type specimen for *infernalis* and not an *infernalis*.

What about INF? Boundy and Rossman reject it also, but not so obviously. First, conspicuously, Boundy and Rossman do *not* say that, given their discovery about MNHN 846, we should embrace INF. Rather, their discussion of the "allocation" of 846 proceeds as if INF is not even under consideration. Thus, in making the comparisons that the allocation requires, they examined "approximately 200 specimens from within the range of *T. s. infernalis*". And their examination leads them to say that a certain marking on *Thamnophis sirtalis* "is reduced to irregular spotting, or re-placed by a broad, dark ventrolateral suffusion, in *T. s. infernalis*" (Boundy and Rossman 1995, 237). If INF were even a possibility given what Boundy and Rossman were revealing about 846, then rather than talk simply, as they do, of "*T. s. infernalis*", they should have said something like "the coastal snakes that *may have been wrongly identified as T. s. infernalis*". They are taking the falsity of INF for granted.

It's a similar story with Barry and Jennings (1998). As noted, they accept the possibility that MNHN 846 is not an *infernalis*. If *Levine's Thesis* were right, then this possibility would entail the possibility that INF is true. Barry and Jennings write as if this possibility has never occurred to them; Smith (1999), likewise. Thus, Barry and Jennings, after citing a large range of literature describing the Peninsula snakes as "*T. s. tetrataenia*", claim that "much of the same literature refers to *T. s. infernalis* as an allopatric form that does not occur on the San Francisco Peninsula" (Barry and Jennings 1998, 225–226). There is no airing of the idea that this literature might be wrong because, given the facts about MNHN 846, *infernalis* might be *tetrataenia* and so INF might be true. Rather, Barry and Jennings presume INF is false.

Boundy and Rossman's discovery about MNHN 846 does not even raise the issue, for taxonomists, of whether the coastal snakes are *T. s. infernalis*. The issue actually raised by the discovery is quite different and is indicated by Haber: "typically in such cases the taxa would be re-named" (Haber 2012, 768). The issue raised is simply *which official names to use for the subspecies of Thamnophis sirtalis in the future*. Nothing more, nothing less. Should taxonomists follow the "default" (ibid., 777), according to the ICZN code, *re*naming *tetrataenia* "*infernalis*" and assigning a new name to *infernalis*, as Boundy and Rossman propose? Or should both subspecies retain their old names, as Barry and Jennings successfully petitioned? *All* parties see the issue raised by the discovery as simply over future names. Thus, for Boundy and Rossman, it is an issue of "nomenclatural changes" (1995, 238); for Barry and Jennings, one of "the rearrangement of the subspecies names" (1998, 226); for commentator Smith, one of "the stability of usage of these names" (1998, 72); finally, for the Commission, ICZN itself, in opinion 1961, the issue is

> the conservation of the subspecific name of *Thamnophis sirtalis infernalis* (Blainville, 1835) for the California red-sided garter snake from the Californian coast, and of *T. s. tetrataenia* (Cope in Yarrow, 1875) for the San Francisco garter snake from the San Francisco Peninsula… (ICZN 2000, 191)

This common understanding of the issue raised by MNHN 846 is at odds with INF and hence with *Levine's Thesis*. For, if INF were correct, there could be no question of *conserving* "*T. s. infernalis*" for the coastal snake since it would already be the name for the Peninsula snake not the coastal snake. And there could be no question of *re*naming the Peninsula subspecies "*T. s. infernalis*" because it would already have that name (even though nobody realized that it had!). It would have that name because MNHN 846 is the type specimen for *T. s. infernalis* and 846 is a Peninsula snake. The possibility that INF might be true is not even contemplated.

I conclude that Boundy and Rossman's uncontested discoveries about the type specimen, MNHN 846, are taken by those who know most about the type specimens of garter snakes not to imply either HOLO or INF. Taxonomy is rife with controversies but this is not one of them. So the experts reject *Levine's Thesis*. So we should too: conclusion C1.

I noted in section 1 that *Levine's Thesis* is LaPorte's *de dicto* reading of a claim that also has the following *de re* reading: "Any species with a type specimen necessarily contains its type specimen" (2003, p. 586). This reading is not a main concern but it is worth noting that the present discussion counts against that reading too. MNHN 846 was the type specimen for *T. s. infernalis*. Bounty and Rossman's uncontested discovery was that 846 had been misidentified and was not an *infernalis*. So the *de re* reading is false. (Since I endorse *Essential Membership* (Devitt 2018b), I think that 846 was necessarily a member of its species, *T. s. tetrataenia*. That is of course consistent with 846 being contingently a member of the species for which it was the type specimen, *T. s. infernalis*. So it does not create a new paradox.)

4. "But what about the theory of reference?"; the case for *C*2

In section 2 I foreshadowed the conclusion *C*2, that "it is a mistake to use a theory of reference to assess *Levine's Thesis*". Rather, the direction of assessment should be from biological facts to the theory of reference. So, my discussion of HOLO and INF has proceeded without appeal to a theory of reference. But *why* is it a mistake to make such an appeal? Why should we not follow Levine and others and argue as follows? "Our favorite theory of reference for biological kind terms, TR, tells us that, given the nature of MNHN 846, the name '*T. s. infernalis*' refers to the Peninsula snake not the coastal snake. So HOLO, INF, and *Levine's Thesis*, are true after all!" Problem: *Why believe TR*? Why not prefer a rival theory that tells us that "*T. s. infernalis*" refers to the coastal snake, or even to nothing at all? The traditional answer has been that TR *matches our referential intuitions*. Thus, TR predicts, time and again, that the reference of a biological kind term *E* in real or imagined situations is *X* and it just seems intuitively to us philosophers that *E* does indeed refer to *X*. This methodology has been severely criticized in recent years. Many have argued that it is scientifically unsound and have insisted that theories of reference must be tested experimentally; see, for example, Machery et al. 2004; Machery et al 2009; Nichols et al 2016. Genoveva Martí (2009, 2012, 2014) and I (2011b, 2012a, 2012b, 2015a) have joined in the criticism and have gone on to argue that theories should be tested against *linguistic usage*.

This debate over methodology cannot of course be replayed here,[5] but I shall briefly apply the Martí–Devitt line to the present example. We should not accept any theory of reference for a term simply because its predictions conform to our intuitions about what the term refers to. Rather, we should test the theory against the usage of those competent with the term. So, TR needs to be tested against the usage of biologists particularly. Do these people *show by their usage* that they are referring to *X* by *E*? For example, does the taxonomists' use of "*T. s. infernalis*" show that they identify the Peninsula snake as its referent? Moral: *we need biologists opinion on the likes of INF in order to know whether TR is right*. Our only way now, perhaps ever, to determine whether a theory of reference for biological terms is right depends on our determination of the biological facts. The biologists' usage shows us that INF is false, as we have seen. So TR is false. That is the right direction of argument. No theory of reference has the evidential support to rule on INF and *Levine's Thesis*, contrary to what Levine and others presume. That is the case for *C2*.

Nonetheless, a theory of reference should be able to explain the linguistic usage demonstrated here, as anywhere. The causal theory mentioned in section 2, unlike TR, does explain that usage, once developed to include "multiple grounding".

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[5] See Devitt and Porter 2021 for a summary of the literature and some examples of testing usage.

5. The causal theory of multiple grounding; the case for C3

As noted, my theory of "grounding" is a theory of the sort of reference fixing by "direct contact" that Hull and Levine favor. The most obvious examples of such groundings are the ceremonial dubbings that they mention. But there can be groundings without any such dubbings. Thus consider the naming of the cat Nana, discussed by Levine (2001, 330–1). This naming was by a dubbing but it could have been simply the result of usage: someone looking at Nana might have just said "Nana is a striking looking kitten" and thereby started the practice of calling the kitten "Nana". Nicknames are often introduced in this way. I recently summed up the theory of grounding as follows:

> What is it about all these situations that ground the name in a certain object? It is the causal-perceptual link between the first users of the name and the object named. What made it the case that this particular object got named in such a situation was its unique place in the causal nexus in the grounding situation. (Devitt 2015b, 114)

This leads straightforwardly to the theory of *multiple* grounding.

> It is important to note that this sort of situation will typically arise many times in the history of an object after it has been initially named: names are typically *multiply grounded* in their bearers. These other situations are ones where the name is used as a result of a direct perceptual confrontation with its bearer. The social ceremony of introduction provides the most obvious examples: someone says, "This is Nana", demonstrating the kitten in question. Remarks prompted by observation of an object provide many others: thus, observing Nana's behavior, someone says, "Nana is skittish tonight". Such remarks are likely to happen countless times during Nana's life. All these uses of a name ground it in its bearer just as effectively as does a dubbing because they involve just the same reference-fixing causal-perceptual links between name and bearer…. Dubbings and other first uses of a name do not bear all the burden of linking a name to the world. (Ibid., 114)

I used this idea of multiple grounding, together with Hartry Field's (1973) idea of partial reference, to explain cases of reference *confusion* (Devitt 1974, 200–203). Thus, consider Kripke's famous leaf-raking example: "Two people see Smith in the distance and mistake him for Jones" (Kripke 1979, 14). Suppose one person comments to the other, "Jones is raking the leaves". I argued that this use of "Jones" has a semantic-referent, Jones, but no determinate speaker-referent; both Jones and Smith are *partial* speaker-referents because the use is grounded in both (Devitt 1981b, 512–516; 2015b, 118–121). Later (Devitt 1981a, 138–152; 2015b, 121–124), I applied the ideas to cases of reference *change* including another famous example, Gareth Evans' "Madagascar" (Evans 1973). The story goes that Marco Polo, on the basis of a hearsay report of Malay sailors, mistakenly took the name of a portion of the African mainland, "Madagascar", as the name of the great African island. And that island is now, of course, the semantic-referent of "Madagascar". So "Madagascar" changed its reference. The explanation, in brief, is that the reference of a name changes from *x* to *y* when *the pattern of its groundings changes* from being in *x* to being in *y*.[6] This discussion is particularly relevant to *Levine' Thesis* if we go along with the individualist view that a species name is a proper name.[7]

Appeal to multiple grounding is also vital in explaining reference change in "natural kind" terms (Devitt 1981a, 190–5). Arthur Fine (1975, 22–6) criticized Putnam's causal theory

[6] Nonetheless, the mistaken idea that cases of reference change are "decisive against the Causal Theory of Names" (Evans 1973, 195) persists (Searle 1983; Sullivan 2010; Dickie 2011). Kripke's own response to "Madagascar" is in "Addenda" to *Naming and Necessity* (1980, 163). As I note (2015b, p. 123, n. 33), the grounding theory can be seen as an explanation of Kripke's admittedly brief proposal (but doubtless not one he would accept).

[7] So, it is odd that Levine does not mention this theory of reference change. He devotes much attention (2001, 330–332) to a discussion of "the qua problem" in chapter 4 of Devitt and Sterelny 1999, a textbook presentation of the causal theory of reference. That presentation includes the theory of reference change (75–76). Indeed, in the 1987 first edition which Levine uses, the theory of reference change immediately precedes the discussion of the qua problem.

of these terms on the ground that it makes it impossible for a term to change its reference: its reference is fixed by the original dubbing. Yet such scientific terms quite obviously often do change their reference. I pointed out (Devitt 1981a, 291–92, n. 1) that Putnam could easily add multiple grounding to his theory. And later he did: "As Devitt rightly observes, such terms are typically 'multiply grounded'" (Putnam 2001, 497). Reference change can then be explained, as it was with proper names, as a *change in the pattern of groundings* (Devitt 1981a, 192–5). This discussion would be particularly relevant to *Levine' Thesis* if we do not accept individualism as, it seems, most biologists do not.[8]

This explanation of reference change is not an *ad hoc* addition to the causal theory to solve problems. It is a straightforward corollary of the causal theory of groundings:

> Groundings fix designation. From the causal-perceptual account of groundings we get the likelihood of multiple groundings. From multiple groundings we get the possibility of confusion through misidentification. From confusion we get the possibility of designation change through change in the pattern of groundings. (Devitt 2015b, 123–124)

It is a truism among theorists of language that an expression gets its meaning and reference from conventions of usage. These conventions sometimes start with stipulations—dubbings are examples—but they mostly come from regular usage. However a convention is established, *even if by stipulation*, it can change through regular usage. (Think of the sad fate of "beg the question".) The above theory of groundings is an explanation of change for some sorts of words.

We now apply this theory to the names used to refer to Haber's garter snakes. An expression's conventional reference is typically established by regular usage. There was clear

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[8] Ingo Brigandt claims that "most biologists and philosophers favor the idea that species are individuals rather than natural kinds" (2009, 77–8). Brigandt may be right about philosophers of biology—certainly the present debate provides evidence that he is—but a recent survey (Pušić et al 2017) shows he is quite wrong about biologists. The survey of 193 biologists from over 150 biology departments at universities in the US and the EU found that the position of individualism among biologists is "utterly marginal", only 2.94%.

consensus among taxonomists in the above debate that since 1951 there had been a stable usage of the name "*Thamnophis sirtalis infernalis*" to refer to California coastal red-sided garter snakes; see Barry and Jennings (1998), particularly. According to the causal theory this stability reflects *a pattern of groundings* of the name in those coastal snakes, a pattern of taxonomists (and others) using the name as a direct result of perceptual contact with those snakes. Doubtless in those decades, there were some groundings of the name in snakes of other kinds, particularly in MNHN 846, which is, after all, the type specimen for *T. s. infernalis* and yet is (we are assuming) a *tetrataenia*, not an *infernalis*. But these misidentifications pale into insignificance against the pattern of groundings in the coastal snake, *infernalis*. That pattern established and maintained the conventional use of the name "*Thamnophis sirtalis infernalis*" to refer to the coastal snake. And this is true whether we take the name to refer to an individual or to snakes of a certain kind.

According to Article 61 of the code, MNHN 846 should have provided "the objective standard of reference" (ICZN 1999) for "*Thamnophis sirtalis infernalis*": type specimens are supposed to stipulate a conventional usage. That is the thought behind Witteveen's claim: "If we baptize a specimen that belongs to some taxon as name-bearer, we thereby fix the name's reference to the taxon the specimen belongs to" (Witteveen 2015, p. 581). But the reference is thereby fixed only if all goes well for the stipulation. For, as just noted, stipulations can fail because expressions are not used as stipulated and different convention are established.[9] The consensus opinion about the usage of "*Thamnophis sirtalis infernalis*" shows that MNHN 846 is an example of such failure.

I emphasize that the Hullian idea that reference "cannot change" was *never* part of the Kripke–Putnam causal theory. Certainly the issue of reference change was not addressed in

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[9] A corollary is that the following claims are false: "taxonomists had always known (with a priori certainty) that the *infernalis* type specimen belonged to the *infernalis* taxon" (Witteveen 2015, 582); "Type specimens… can be known a priori to belong to [their respective species]" (LaPorte 2003, p. 583). Knowledge of referential facts, indeed knowledge of semantic facts in general, is always empirical (Devitt 2011a; Salmon 2020).

the earliest presentations of the theory. Still it was in later ones. That is the case for C3: the causal theory of reference does not imply *Levine's Thesis*, as Levine and others think.

C2 identified the mistake by Levine and others of using a theory of reference to determine a biological thesis (sec. 4). That mistake is compounded by using a theory that does not accommodate reference change.

C3's rejection of the inference from the causal theory to *Levine's Thesis* has consequences for what Haber and Brzozowski say about reference. Given their acceptance of the inference, they take their arguments against *Levine's Thesis* to count against the causal theory (semantics appropriately answering to biology; sec. 2).[10] Thus, Haber thinks that his argument "suggests that rigid designation and causal theory of reference may be more fragile than supposed" (2012, 768).[11] The argument presents "a serious challenge to philosophical accounts of proper names, or perhaps their applicability to biological taxonomy" (ibid., 781). Brzozowski is led to the view that taxon names have their reference fixed by descriptions and are "descriptive names". He thinks that this "account of taxon names is able to better account for the uses and misuses of taxon names when compared to the causal view" (Brzozowski 2020, 23). *C3* undermines these criticisms of the causal theory.

6. Philosophical evaluations of *Levine's Thesis*

I turn now to the evaluation of *Levine's Thesis* by other philosophers. These evaluations include some claims which, from the perspective I have presented, are dead right. But they include others that are dead wrong. Thus, on the right side,

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[10] If the rejection of *Levine's Thesis* poses a problem for the causal theory then, as LaPorte points out, it is "a general one": "it arises whether species are individuals or kinds, given the standard causal theory of reference" (LaPorte 2003, 586).

[11] Haber adds the following startlingly false claim: "Taxonomic theory is, in part, a theory of reference applied to biological nomenclature" (Haber 2012, 768). Taxonomic theory does specify *a practice for the stipulation of* a taxon name that will *cause* it to have a certain reference when all goes well, which it sometimes doesn't; but taxonomic theory is far from a *theory of* this reference.

Haber claims, contrary to HOLO, that "researchers discovered that *T. s. infernalis'* type specimen belonged to *T. s. tetrataenia*" (Haber 2012, 768) and goes on to reject *Levine's Thesis* and hence resolve the paradox. Brzozowski makes a similar claim (Brzozowski 2020, 10) and endorses Haber's rejection. Even Witteveen, who wrongly endorses *Levine's Thesis*, nonetheless apparently rejects INF in saying that Boundy and Rossman "discovered that taxonomists had been wrong about which taxon was [the *infernalis* type specimen's] taxon" (Witteveen 2015, 582).

But then there is the wrong side.

6.1 Haber; the case for C4

Haber's rejection of *Levine's Thesis* is strangely qualified: he thinks that the Thesis "only holds under idealized conditions" (Haber 2012, 782). This reflects a more serious problem: his reason for rejecting the Thesis confuses changing language with changing the world. This is the last of the "mistakes about language" that are a major concern of this paper.

My own reasons for rejecting *Levine's Thesis* arose from two related responses of taxonomist to the discovery about MNHN 846, the type specimen for the subspecies *T. s. infernalis*. These responses were contrary to what the Thesis demands. First, contrary to HOLO, these experts concluded that 846 had been, or might have been, misidentified as an *infernalis*, the California coastal red-sided garter snake; second, contrary to INF, these experts showed no sign of even entertaining the possibility that *infernalis* was not that coastal snake.

Now as noted in section 3, the discovery about MNHN 846 did demand a further response: taxonomists, particularly ICZN, had to make a decision about the future official names for the subspecies of *Thamnophis sirtalis*. *But the falsity of Levine's Thesis does not depend in any way on that decision about future usage*. Yet, as we shall see, Haber seems to think that it does. He seems to think that the Thesis *would be* true if ICZN always followed the code's "default" in such cases of misidentification, a default that would have been illustrated had ICZN accepted Boundy and Rossman's proposal that *tetrataenia* be renamed "*infernalis*" and a new name be assigned to *infernalis*.

Abraham Lincoln is said to have once pointed out that a person's calling a donkey's tail a "leg" does not make it a leg. Similarly, the ICZN's calling the Peninsula snake "*T. s. infernalis*" would not have made it *T. s. infernalis*. It was a worldly fact that the Peninsula snake was not *T. s. infernalis*, no matter what decisions ICZN, or anyone, makes about how to use language in the future. Contrary to what postmodernists, and sadly many others, seem to think, languages do not make worlds. This is not the place to argue this large issue (but see, for example, Devitt 1997, 235–258; 2010, 99–136).

The key discussion in Haber begins nicely:

> That a specimen was preserved and identified prior to careful study of a particular taxon does not mitigate that the type specimen may be wrongly hypothesized to belong to that taxon. (Haber 2012, 779)

But then Haber goes on:

> In a default case, the species identity of the type specimen does not change, it still belongs to the species it designates. (*ibid*)

Had ICZN responded to the discoveries about MNHN 846 by deciding to follow the default it would have renamed *tetrataenia* "*infernalis*". This would have changed the status of 846: before such a decision, 846 does not belong to the subspecies for which it was a type specimen because it does not belong to *infernalis*; after the decision, it would have belonged to the subspecies for which it was a type specimen because it belongs to *tetrataenia*. But it would not have been in virtue of this decision that 846 kept its "species identity"! 846 was a *tetrataenia* (we are assuming) misidentified as an *infernalis*, showing *Levine's Thesis* to be false, *whatever linguistic decision anyone made about future usage*. Haber continues:

> On successful active petition… the type specimen… is reassigned to a new species, and no longer belongs to the species it formerly designated (though other specimens might). (*Ibid.*)

As Witteveen points out, Haber is arguing that the decision by ICZN to accept the petition of Barry and Jennings "entails that a type specimen got misidentified" (Witteveen 2015, 575).

Yet, what ICZN actually did was decide to conserve the subspecific *names* of both *T. S. infernalis* and *T. s. tetrataenia* (ICZN 2000, 191), rather than follow the default. This decision did not reassign MNHN 846 "to a new species" or entail that 846 had been misidentified. On the contrary, the decision is totally irrelevant to what (sub)species 846 belongs to. 846 had been misidentified as an *infernalis*, independent of any linguistic decision: to repeat, languages don't make worlds. Finally, contrary to what Haber claims (2012, 780), it is not because of that decision, rather than the default one, that the "*de dicto* necessity [*Levine's Thesis*] fails to hold". It fails simply because type specimens can be misidentified, as 846 illustrates. The "species identity" of any type specimen, like that of any organism, is constituted by its nature not by a linguistic decision of ICZN.

In sum, *it is a mistake to make any inferences about species identity, and hence about Levine's Thesis, from decisions about nomenclature*. This is my conclusion *C4*.

6.2 Witteveen

Witteveen claims to resolve Levine's paradox by arguing that "there is no sense in which type specimens belong contingently to the species they name" (Witteveen 2015, 571). Well, if my argument against *Levine's Thesis* is right then there is at least one such sense. Set that aside for a minute. According to LaPorte, there is another sense: the contingency that arises from the rejection of the *de re* necessity, "Any species with a type specimen necessarily contains its type specimen"? I argued that the misidentification of MNHN 846 provides one reason against this necessity (sec. 4). And LaPorte rightly points out that we should reject the necessity because of the possibility of the type specimen "never having been born" (LaPorte 2003, 587). Furthermore, he thinks, though I do not (sec. 1), that we should also reject this necessity because *Essential Membership* is false. So, there are *several* potential reasons for the contingency that comes from rejecting LaPorte's *de re* necessity. How does Witteveen resist all of them in claiming that that "there is no sense in which type specimens belong contingently to the species they name"? Briefly, by confusing LaPorte's *de re* reading with his *de dicto* one (in a section called "Contingency confusion"!):

> Thus, it appears that in all possible worlds in which we find a species with a type specimen, it contains its type specimen. This means that the sentence "Any species with a type specimen necessarily contains its type specimen" is true after all. (Witteveen 2015, 576–7)

This is wrong. What appears to Witteveen to be so in his first sentence amounts to, "Necessarily any species with a type specimen contains its type specimen". This is LaPorte's *de dicto* reading, *Levine's Thesis*. This differs strikingly in the scope of its "necessarily" from what Witteveen takes the sentence to mean in his second sentence, namely, LaPorte's *de re* reading. And, the contingency we are considering is a rejection of the *de re* reading *not* the *de dicto* one. Witteveen has not addressed *that* "sense in which type specimens belong contingently to the species they name".

Return to Laporte's *de dicto* reading, *Levine's Thesis*. Witteveen's endorsement of this is, for our purposes, the key sense of contingency that he rejects. So, what is Witteveen's case for *Levine's Thesis*? It starts with criticism of Haber's case against. We have just rejected Haber's argument that the ICZN decision to accept Barry and Jennings' petition establishes that MNHN 846 was misidentified. Witteveen's criticisms are different. First, he claims:

> What Haber should have said" is that that ICZN decision "causes a specimen that formerly served as type specimen to stop belonging to the taxon for which it formerly anchored the taxon name. (Witteveen 2015, 580)

Now that decision *did* cause MNHN 846 to cease to be the type specimen of *infernalis*. But the decision *did not* cause 846 "to stop belonging to" *infernalis*: 846 never did belong. And no decision by ICZN could bear on the worldly fact of 846's subspecies membership; see conclusion *C4*. Witteveen's second criticism is better: he claims that the ICZN decision "does not show that *de dicto* necessity [*Levine's Thesis*] fails" (ibid., 581). No linguistic decision *could* show this. So Witteveen is right that Haber's case *against Levine's Thesis* fails. But what does Witteveen have to say *for Levine's Thesis*?

Only the passage we quoted and rejected earlier (sec. 5): "If we baptize a specimen that belongs to some taxon as name-bearer, we thereby fix the name's reference to the taxon the specimen belongs to" (ibid., 581). The problem was that attempts to stipulate usage can fail; reference can change (sec. 5). In any case, no thesis about language has the authority to settle a biological matter; see conclusion *C2*. To support *Levine's Thesis*, Witteveen needs to show that MNHN 846 was *not* misidentified as an *infernalis*, as taxonomists clearly think it (very likely) was. Witteveen has not done so.

6.3 Brzozowski

Brzozowski offers "a defense of Haber's (2012) position in response to Witteveen (2015)" (Brzozowski 2020, 4). Part of this defense is the rejection (ibid., 12) of a criticism of Haber that I have just emphatically endorsed: the charge that Haber takes *the ICZN decision* to entail that a type specimen got misidentified. In rejecting this criticism, Brzozowski points to a passage (Haber 2012, 778) like the one above that I labelled "on the right side". But the criticism is well-based in the cited passages "on the wrong side".

Brzozowski's discussion of this criticism, and his own remarks "on the right side" (Brzozowski 2020, 10), might suggest that he rightly thinks that the biological discovery that MNHN 846 had been misidentified alone shows that *Levine's Thesis* is false. But, in fact, he thinks that this discovery falsifies only a "metalinguistic" version of the thesis about "the reference of a species name" (ibid., 22). And this falsification depends on complicated semantic machinery, including the claim that names are descriptive (ibid., 14–23). This is a mistake: biology alone shows *Levine's Thesis* false. No semantics is needed; see conclusion *C2*.

I turn finally to some likely objections to my argument against *Levine's Thesis*.

7. Objections

I have a good basis for anticipating objections. For, the argument in this article has been presented before in a paper, "Type Specimens and Reference", that was rejected by two journals on the basis of some thoughtful reports from reviewers.[12] I found the objections from two of these reviewers particularly interesting. The reviewers rightly think that issues about language have been center stage in the discussion of *Levine's Thesis* and *they insist that these issues continue to be*. Indeed, they find it incomprehensible that linguistic issues should not be put center stage. So, the reviewers are insisting on precisely what my paper argues is a very mistaken methodology. I shall develop my argument in this section in responding to the objections. It seems that this linguistic methodology is much more entrenched in this area of the philosophy of biology than I had supposed.

7.1 Reviewer R1 and codes of nomenclature

The objections from *R1* do not seem to be about language to begin with. *R1* claims that my

> bold argument would have been very interesting if it had been supported by convincing empirical evidence that taxonomists agree unanimously that it is not necessary for type specimens to belong to their species… I expected that the author would present evidence from questionnaires with vignettes of the kind that are frequently encountered in contemporary experimental philosophy (particularly in the area of semantics).

Section 3 presents fairly overwhelming evidence that *all* the taxonomists involved in the case of MNHN 846, and the international body ICZN itself, agree that 846, which is indubitably the type specimen for *Thamnophis sirtalis infernalis*, is, or at least might be, nonetheless a *T. s. tetrataenia*. What they agree on is inconsistent with *Levine's Thesis*. Now it is always good to have more evidence. So, we could see what taxonomists say about other cases of apparently misidentified type specimens. And we could indeed do some "experimental philosophy" on taxonomists. But if we do, we should not ask the taxonomists their opinion about whether it is "necessary for type specimens to belong to their species" (*Levine's Thesis*): that sort of question asked of taxonomists is

[12] The journals were *Biology and Philosophy* and *History and Philosophy of the Life Sciences*.

far too abstract and "philosophical" to provide good evidence. Rather, we should ask taxonomists about actual or imagined cases of apparently misidentified type specimens. This would provide good and direct evidence for or against *Levine's Thesis* of just the same sort as I provided. Indeed, we could present taxonomists with a vignette about MNHN 846 itself and ask them whether it is a *T. s. infernalis* or a *T. s. tetrataenia*; we could ask them about HOLO. But do we really need any of this extra evidence? Thus, given the *actual* discussion of 846 that I cited, we can surely be confident about their answer: 846, the type specimen for *T. s. infernalis* is, or at least might be, a *T. s. tetrataenia*.

This can't be *R1's* real worry about evidence and it soon becomes apparent that it isn't. The real worry is that the evidence that I provide from that actual discussion is "not viewed in the context of the debate" of Haber, Witteveen, and Brzozowski. What context is that? *A context that is largely about language*. Thus *R1* demands

> a close analysis of how this [rejection of *Levine's Thesis*] is supported by the wording of codes of nomenclature (ICZN, ICN and others) that taxonomists have devised and follow in their nomenclatural practices.

R1 charges that I do not "attend to the role of codes of nomenclature in taxonomic practice". *R1* finds this

> really quite baffling, since these codes—and their role in taxonomic practice—have been at the center of discussion in recent contributions to the "type specimen debate". By failing to consider the content and application of the codes in taxonomic practice, the author misses entirely what this type specimen debate has been about.

R1 is, of course, right that the debate over *Levine's Thesis* has centered on such linguistic matters. Indeed, I emphasized this at the very beginning of my discussion. So, I haven't *missed* it. Rather, I have emphatically *rejected* it: a "major concern" of the paper, and this article, is to argue that the debate has "gone awry because of mistakes about language" (sec.1).

How *might* a nomenclatural practice bear on *Levine's Thesis*? Here's a way. In section 4, I noted that a theory of reference, *TR*, could be brought to bear by telling us that, "given the nature of MNHN 846, the name '*T. s. infernalis*' refers to the Peninsula snakes not the coastal snakes", thus supporting *Levine's Thesis*. Now suppose that *TR* tells us this about the name "*T. s. infernalis*" *because TR* takes the nomenclatural practice of stipulating a meaning for a taxon name via a type specimen to be what constitutes that reference to the Peninsula snakes. Then, clearly, the nomenclatural practice would provide evidence for *Levine's Thesis*. But, also clearly, the practice does so only if *TR* is *right* to give this role to the practice. And the problem is that *TR* is *not right* to. How do we know? Well, for "*T. s. infernalis*" to refer to the Peninsula snakes, there would have to be a convention of using it to so refer. That's a truism. And the usage by biologists shows that there is no such convention. Indeed, biologists had for decades been identifying the *coastal* snakes, not the Peninsula ones, as *T. s. infernalis*. *It is these identifications by biologists that provide the evidence for or against any theory of reference of "T. s. infernalis"* (Devitt and Porter 2021, 2023). Those identifications are what *TR* has to be tested against, and it fails.

But the moral of this tale is deeper. To assess *Levine's Thesis*, we need to know whether MNHN 846, the type specimen for *T. s. infernalis*, is a *T. s. infernalis* (HOLO). The deep moral is that it was a mistake to bring a theory of reference to bear on this question from the start (sec. 4). For, any theory of the reference of "*T. s. infernalis*" has to be tested against the term's usage. And the usage in question is that of taxonomists in identifying snakes as *T. s. infernalis* or not. So, to assess *Levine's Thesis*, we should simply check what biologists *do* identify as *T. s. infernalis* or not and skip the detour into the theory of reference. And that is what I did in section 3.

No application of a nomenclatural code constitutes the reference of "T. s. infernalis". That's a fact from the theory of language. There is no call for *R1* to be baffled by my inattention "to the role of codes of nomenclature in taxonomic practice". I attend to the only role played by these codes that is *relevant* to the reference of "*T. s. infernalis*". That role, I argue (sec. 5), is a *causal* not constitutive one. The application of a code is an obvious attempt to *stipulate* a term's reference, for important scientific purposes. And, of course, those attempts are mostly successful: they establish a convention, thus causing the term to *have* that very reference. But, as the case of "*T. s. infernalis*" shows, sometimes stipulations fail because usage establishes different conventions. In sum, when all goes well for an authoritative body like ICZN, its stipulation that *E* is to refer to *S* will cause *E* to refer to *S*, but it never constitutes it so referring. That *E* refers to *S* is constituted by dispositions among *E*'s users (Devitt 2021, 75–81).

Despite the irrelevance of theories of reference to the assessment of *Levine's Thesis*, we do of course need a theory of reference that is compatible with the biological facts of the matter. I offered a causal theory of multiple grounding (sec.5). *R1* is not impressed, accusing me of failing "to see that taxonomists have agreed on the convention that only type designations 'ground' formal taxonomic names". Not guilty! Rather, what *R1* has failed to see is that *conventions agreed on may not be followed*; Geneva Conventions provide one example; "*T. s. infernalis*", another. *R1* continues: "One could in fact argue that one of the main purposes of the type method is to formally forbid 'multiple groundings' of taxon names". One could, but multiple groundings are a fact of linguistic life. So, it would be more plausible to argue that "one of the main purposes of the type method is to formally forbid" *groundings in any organism that is not in the same taxon as the type specimen*. That's plausible because the type method is a stipulation and stipulations indicate what people want. But, sadly, wanting something to be so, doesn't make it so. Thus, despite the Geneva Conventions, people got tortured. Similarly, despite the ICZN code, "*T. s. infernalis*" got multiply grounded in the coastal snake. So, the term *actually* refers to that snake. And *actual* reference matters to the theory of reference, not what the ICZN, or anyone, wants.

One might put my main point in response to *R1* as follows. The empirical methodology for the theory of reference, discussed in detail in the many works cited in section 4, and briefly described in that section and above, shows that the linguistic "context of the debate" over *Levine's Thesis* is mistaken. *R1* insists on that context without any recognition of that empirical methodology.

7.2 Reviewer R2 and the linguistic turn

R2 characterizes my methodology as follows: "we should simply ask experts (i.e., taxonomists) about whether *Levine's Thesis* holds". That's not quite right. My refutation of *Levine's Thesis* rests entirely on what taxonomists had to say about certain snakes, organisms that taxonomists know a lot about. The refutation does not rest at all on what taxonomists think about *Levine's Thesis*, a philosophical thesis that they might well find quite puzzling. In any case, *R2* objects:

> This methodology needs further motivation, since it is far from clear… that the taxonomists actually draw the conclusion that the Author claims they do. In particular, the Author will need to consider that the taxonomists he cites recognize the difference between the *usage* of names and their *valid* designation…. it is not evident that the taxonomists think that the valid name for a taxon can refer to a taxon that doesn't include the type for that name…. the Author appears to be holding the taxonomists to unreasonably high philosophical standards of precision in talking about naming and reference.… We can't expect taxonomists to neatly distinguish between these kinds of reference in their writings.

The opinions of taxonomists about snakes that I cite, including about type specimen MNHN 846, are inconsistent with *Levine's Thesis*. That is why we should reject *Levine's Thesis*. *R2* objects that we shouldn't reject it until we know what taxonomists think about *the names* of those snakes, until we have established that taxonomists have certain quite subtle *semantic* views. But, I responded to *R2*'s review, it was a central theme of my paper that views about language should *not* be used to assess a biological thesis like *Levine's Thesis*; see *C2* (sec .4) Any views about language, even ones held by expert semanticists, let alone by taxonomists, should not count against the views of expert taxonomists *about organisms*.

R2 was hugely unimpressed with this response, insisting that semantics *must* play a role. In particular *R2* finds it "really quite puzzling" how I "*could* think" that *Levine's Thesis* "is a purely biological thesis". For,

> a type specimen (a holotype or neotype) is nothing other than a specimen that serves as the bearer of a species name. So, we could rewrite [*Levine's Thesis*] as: "Necessarily, any species with a specimen that serves as the bearer of that species' name contains that specimen".[13] Is this a "purely biological" thesis? Surely not! It has semantics written all over it! Just consider a simple question this thesis invites: which is the species that the name-bearing specimen belongs to? Is it the name's semantic referent?

A consequence of *C2* is that this move to a semantic question is uncalled for and mistaken. Take our case of MNHN 846. Everyone agrees that 846 is the type specimen that serves as the bearer of the name for the species *T. s. infernalis*. Then *R2's* "simple question", applied to this case, is: "Does MNHN 846 belong to the semantic referent of '*T. s. infernalis*'?" But the question that should concern *Levine's Thesis* is not this partly semantic one but rather the entirely nonsemantic, "Is MNHN 846 a *T. s. infernalis*?" (cf. HOLO). And the resounding answer from people who know a lot about snakes, but probably very little about semantics, is "No (or probably not)". *That* is the judgment that refutes *Levine's Thesis*. *R2*'s insistence on bringing in semantics (without even addressing my argument that we should not) is very revealing of just how entrenched this "linguistic turn" is in this area of the philosophy of biology.

There is no sign that biologists involved in this case ever entertain *Levine's Thesis*, but they show by their practices that they reject it. So, they are not bothered by the problem allegedly posed by the Thesis. And they are right not to be. The alleged problem is a philosophical illusion, a misguided attempt by philosophers, driven by mistaken ideas about the relevance of views about language, to impose a problem on biology.

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[13] *R2* actually proposed the following rewrite: "Necessarily, any species with a specimen that serves as the bearer of a species name belongs to the species of which it bears the name." But this must be a slip as it is clearly not a rewrite of *Levine's Thesis*. I have made corresponding adjustments in what follows the slip.

8. Conclusion

Levine (2001) sees a conflict between the contingency of species membership and a view of the role of type specimens that he takes from Hull: "*qua organism*, the type specimen belongs to its respective species contingently, while *qua type specimen*, it belongs necessarily"; he finds this "paradoxical" (ibid., 334). My concern has been with the thesis about type specimens which, following LaPorte, I take to be the *de dicto* necessity, "Necessarily, any species with a type specimen contains its type specimen" (LaPorte 2003, 586). I called this "*Levine's Thesis*". I have used Haber's lovely example of MNHN 846, the type specimen for *Thamnophis sirtalis infernalis*, to argue for conclusion *C1*: *Levine's Thesis* is false (sec. 3). For, the uncontested discovery by two taxonomists, Boundy and Rossman (1995), is that 846 is not a *T. s. infernalis* but a *T. s. tetrataenia*.

The alleged paradox has led to papers not only one from LaPorte but also from Haber (2012), Witteveen (2015), and Brzozowski (2020). My argument for *C1* appealed only to biology, with no mention of theories of language. In this respect it differs from other arguments about *Levine's Thesis*, whether for it or against it. A major concern of this paper has been to show that these arguments have gone awry because of mistakes about language.

First, Levine's path to *Levine's Thesis* rests on a causal theory of reference which he takes from Kripke and Putnam. My conclusion *C2* was that it was a mistake for Levine to use a theory of reference to assess *Levine's Thesis*; the direction of assessment should be from biological facts to the theory of reference (sec. 4). This criticism applied also to LaPorte's and Witteveen's arguments for *Levine's Thesis* and to Brzozowski's argument against.

Still we are interested in semantics as well as biology and so need a theory of reference compatible with the biological facts. So, we need a theory that does not imply *Levine's Thesis*. I argued against the received view that the causal theory does imply this: that's my conclusion C3 (sec. 5). A causal theory that includes multiple groundings can explain reference change and accommodate the falsity of *Levine's Thesis*.

The final mistake is about the relation between linguistic decisions and the world (sec.6). Haber rightly rejects *Levine's Thesis*, but he does so for the wrong reason. In response to Barry and Jennings' (1998) petition about the MNHN 846 discovery, ICZN (2000) decided to conserve the subspecific names of both *T. S. infernalis* and *T. s. tetrataenia*. Haber thinks that it was this decision that made it the case that 846 had been misidentified as an *infernalis*, hence establishing the falsity of *Levine's Thesis*. Witteveen, who accepts *Levine's Thesis*, has a different view of what that decision achieved: it caused 846 to stop belonging to *infernalis*. It followed from my conclusion *C4* that both these views are wrong: it is a mistake to make any inferences about species identity, and hence about *Levine's Thesis*, from decisions about nomenclature; changing languages does not change worlds. Whether or not 846 is an *infernalis* or a *tetrataenia* and hence has been misidentified is a biological fact that does not depend in any way on a linguistic decision.

I ended my discussion by responding to some objections taken from a couple of unfavorable reviews (sec.7). These reviewers wrongly insist on putting linguistic issues center stage in discussing *Levine's Thesis*, despite my argument that this is a mistake (C2).

Levine's Thesis is false. So, there would be no paradox even if *Essential Membership* were not true. But it is true (Devitt 2018b).[14] This does not yield a new paradox. According to *Essential Membership*, MNHN 846 is necessarily a member of its species, *T. s. tetrataenia*. That is quite consistent with the falsity of *Levine's Thesis*: it is consistent with 846 not necessarily being a member of *T. s. infernalis*, the species for which it is a type specimen; indeed, with it not being a member of that species at all.[15]

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[14] In the version of this paper that appears as ch. 5 in my book, *Biological Essentialism*, the "not" in this sentence was mistakenly moved to the next sentence leading to the false claim that *Essential Membership* "is not true" (Devitt 2023, 156).

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