DEFENDING INTRINSIC BIOLOGICAL ESSENTIALISM

Michael Devitt

ABSTRACT

In "Resurrecting Biological Essentialism" (2008) I went against the consensus in the philosophy of biology by arguing that a Linnaean taxon, including a species, has an essence that is, at least partly, an underlying intrinsic, mostly genetic, property: this intrinsic nature explains both the truth of generalizations about the phenotypic properties of the taxon and why being in the taxon is explanatory. The present paper is a response to two criticisms: that this intrinsic essentialism is at odds with certain biological variations; and that this talk of intrinsic essences is an uncalled for metaphysical addition to biology.

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ACKNOWLEDGMENTS

I am grateful to the following for helpful comments on drafts: Matthew Barker, John Dupré, Marc Ereshefsky, Peter Godfrey-Smith, Paul Griffiths, Joseph LaPorte, Antonella Mallozzi, Karen Neander, Makmiller Pedroso, Iakovos Vasiliou, Denis Walsh, and Andrea Woody.

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Philosophy of Science 88 (2021) pp. 1–16. 0031-8248/2021/8801-00XX$10.00

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1. Introduction

What is the "essence," "nature," or "identity" of a biological taxon? The commonsense answer, particularly about species, is that the essence consists, at least partly, in intrinsic, underlying, and mostly genetic properties. Let us call this answer, "*Intrinsic Biological Essentialism*" ("IBE"). It has been urged by the influential philosophers, Saul Kripke (1980), Hilary Putnam (1975), and David Wiggins (1980). Nonetheless the consensus in biology, especially in the philosophy of biology, is that the answer is quite wrong, smacking of "Aristotelian essentialism," and reflecting a naive and uninformed view of biology that is incompatible with Darwinism. Samir Okasha captures the consensus well:

> virtually all philosophers of biology agree that…it simply is not true that the groups of organisms that working biologists treat as con-specific share a set of common morphological, physiological or genetic traits which set them off from other species. (2002, 196).

On this matter, according to Sarah-Jane Leslie, "there is a degree of consensus among philosophers of biology (and indeed biologists) that is almost unprecedented in philosophy at large" (2013, 132).

Clearly, if a species has an essence at all and that essence is not intrinsic, then it must be relational. The consensus is indeed that the essence is relational: for an organism to be a member of a certain species, it must have a certain *history*. As Kim Sterelny and Paul Griffiths put it, there is "close to a consensus in thinking that species are identified by their histories" (1999, 8).

In "Resurrecting Biological Essentialism" (2008), I went along with the consensus in accepting that there is an historical *component* to the essence of a taxon, and have since argued for this (2018a). However, "Resurrecting" went sharply against the consensus, particularly over species, in arguing that there is *also* an underlying intrinsic component: I argued for IBE. This argument has received detailed and interesting criticisms from five knowledgeable philosophers: Matthew Barker (2010), Marc Ereshefsky (2010), Tim Lewens (2012), Sarah-Jane Leslie (2013), and Matthew Slater (2013). These authors have made many more criticisms than I can answer here. I have chosen to make detailed responses only where that advances the positive case for IBE rather than simply defends the case already made.[1]

2. Clarifications

We need to clarify two matters. First, what are the taxa in question? They are those that are thought to fall under the biological categories in the Linnaean hierarchy of kingdoms, phyla, classes, orders, families, genera, species, and even subspecies. I say "thought to fall" because I sympathize with the doubts of some about this hierarchy (see, for example, Ereshefsky 1999; Mishler 1999). Even if these doubts are appropriate, it is quite clear which groups are thought to fall under the categories. Those are the taxa that concern our essentialism issue. So the concern is *not* with some other biological groups; for example, *predators*, *parasites*, *females* (2008, 346).[2]

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[1] See *Biological Essentialism* (forthcoming) for detailed responses to other criticisms.
[2]. And the discussion may not apply to microbial kinds; see Ereshefsky and Reydon (2015).

There is an important further point: our working assumption should be that these taxa are very largely explanatory. Biologists are obviously striving for an explanatory classification and we should surely assume that they have mostly achieved this. We know from the history of taxonomy that mistakes have been made and they are surely still being made. Still, we should assume that our taxonomy is very largely good and explanatory. Thus, we should assume that *Canis* and *Canis familiaris* are explanatory notions whether or not it is appropriate to take them as being about a *genus* and a *species,* respectively.

Second, taxon essentialism is a *property* or *kind* essentialism. A property *P* is an *essential* property of being an *F* iff anything is an *F* partly in virtue of having *P*. A property *P* is the *essence* of being an *F* iff anything is an *F* in virtue of having *P*. The essence of being *F* is the sum of its essential properties. Essences can be fully intrinsic; for example, the essence of being gold is having atomic number 79. Essences can be partly intrinsic and partly extrinsic and relational; for example, the essence of being a pencil is partly being an instrument for writing, which an object has in virtue of its relation to human intentions, and partly having the sort of physical constitution that distinguishes it from a pen, which an object has intrinsically. Finally, essences can be fully relational and extrinsic; being Australian is probably an example because it seems that anything - Rupert Murdock, Phar Lap (a horse), the Sydney Opera House, a bottle of Penfolds' Grange, the expression "no worries mate," and so on - can have the property provided it stands in the right relation to Australia.

Some may resist any talk of "essence," thinking that the term smacks of Aristotelian metaphysics and has a scholastic air. But the term does not matter. What I am picking out with that term - also 'nature' and 'identity' - is the property *in virtue of which* an object is member of a certain kind; the property that *constitutes* its being a member or *makes* it a member. Those who find my terms for this property distasteful should choose another (2008, 347-8). Some may think that there *is* no such property. I think that they are very wrong, for a reason I shall give in section 5.

To say that kinds have essences (natures) is not to say, of course, that these essences are always worth investigating. Whether they are depends on how explanatory the kinds are. So the essence of being Australian is of little interest, that of biological taxa, of great interest, that of biological categories, of uncertain interest.[3]

3. Summary of the Argument for IBE

Why believe in IBE? My answer came in two parts (2008, 351-5). I summarize.

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[3] Michael Ghiselin (1974/1992) and David Hull (1978/1992) take their view that species are *individuals* and not kinds to be an antidote to essentialism. Ingo Brigandt claims that "most biologists and philosophers favor the idea that species are individuals rather than natural kinds" (2009: 77-8). In contrast, a recent survey of the opinions of 193 biologists from over 150 biology departments at universities in the US and the EU (Pušić et al 2017) found that, among biologists themselves, the position of individualism is "utterly marginal," only 2.94%. I agree with those like Okasha (2002, 193-94) who think that this individualism is a red herring to the essentialism issue (2008, 348).

The first part of my argument for IBE was that biological generalizations about the morphology, physiology, and behavior of a taxon require explanations that advert to an intrinsic underlying, probably largely genetic, property that is part of the essence of the taxon. If we put together each such underlying property that similarly explains a generalization about a taxon, then we have the intrinsic part of its essence.

The explanations in question here are "structural" ones about the underlying states in members of a taxon that, along with the environment, *make the generalizations true*. In urging this argument, I emphasize a crucial distinction, made by Ernst Mayr (1961) and renamed by Philip Kitcher (1984/2003), between these structural explanations and "historical" ones about how members of the taxon *evolved to have* such states.[4] My argument is that *structural* explanations demand (partly) intrinsic essences:

> There has to be something about the very nature of the group—a group that appears to be a species or taxon of some other sort—that, given its environment, determines the truth of the generalization. (2008, 352)

And that something can't be an historical/relational property. I was particularly harsh on the idea that the explanatory property might be that of standing in an appropriate relation to designated individuals, perhaps a "type specimen" (Hull 1978/1992, 311–12) or, "say, Brigham Young" (Ruse 1987/1992, 344). I called this idea "explanatorily hopeless" (2008, 363).

The second part of my argument is related to the first. Mohan Matthen points out that biologists think "that something is striped *because* it is a tiger" (1998, 115) (thus exemplifying the just-made point that biological classifications are explanatory). I put it like this: "the fact that an individual organism is a tiger, an Indian rhino, an ivy plant, or whatever, explains a whole lot about its morphology, physiology, and behavior" (2008, 352). *Why does it*? Because for an organism to be, say, a tiger, is for it to have the intrinsic underlying nature of tigers and that nature, in combination with the environment, causes its phenotypic features, its morphology, physiology, and behavior: the same underlying properties that make the organism a tiger cause it to be striped. That's why being a tiger is explanatory.

In sum, the intrinsic nature of a taxon explains both the truth of generalizations about its members and why being in the taxon is explanatory.

An important methodological point: Leslie claims plausibly that the traditional argument for IBE "makes critical use of intuitions" (2013, 109). As can be seen, my argument does not. It makes critical use of biological explanations.

What does the consensus have *against* IBE? "Resurrecting" identified two main objections. Okasha expresses one as follows: "On all modern species concepts (except the phenetic), the property in virtue of which a particular organism belongs to one species rather than another is a relational rather than an intrinsic property of the organism" (2002, 201). I argued (2008,

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[4] Mayr named the first sort of explanation "proximate," the second, "ultimate."

356-63, 366-70) that this objection is seriously mistaken, conflating another distinction due to Mayr, that between "the category problem" and "the taxon problem" (1982, 253-54). Mayr introduced the distinction for species, but we can generalize it. Then, the category problem is concerned with what it is for a taxon to be in any particular category, for example *genus*. The taxon problem is concerned with what it is for an organism to be in any particular taxon, for example *Canis*. The "species concepts" that Okasha is alluding to are theories that address the *category* problem *for species*: they say what it is for a taxon to be a species (rather than, say, a subspecies or genus). These concepts provide a *constraint* on an answer to the taxon problem *for any taxon that is a species* but fall far short of providing an answer even for those taxa. And the concepts *throw no light at all* on the problem for taxa that are not species. In contrast to these concepts, IBE is an answer to the *taxon* problem and has nothing to say about the category problem. So IBE and the concepts are answering different questions. The consensus, as expressed by Okasha, is simply wrong.

Mayr's distinction is well-established and yet, as I demonstrated (2008, 363-66), its significance has been standardly overlooked in discussing taxon essentialism. And it seems to me that my critics, Barker (2010, 76-7, 89), Ereshefsky (2010, 680-81), Lewens (2012, 752), and Leslie (2013, 138-42) continue this practice in mistakenly resting their case for an entirely relational view of a species' essence on the species concepts. My argument for this must await another time (forthcoming).

Why has the significance of this distinction been standardly overlooked in discussing essentialism? My tentative diagnosis was that it has been overlooked because of the appealing, but mistaken, idea that the species concepts imply relational answers to the *conspecificity* problem, the problem of what makes two organisms conspecific. For, if that idea was right, IBE could not be (2008, 363-66). In response, Barker claims that this idea about conspecificity is "common knowledge" among "species aficionados" (2010, 76). The idea is certainly present in my critics, Ereshefsky (2010, 681), Leslie (2013, 140-41), and perhaps Lewens (2012, 752). So, my diagnosis is no longer tentative. The substantive issue is, of course, whether the aficionados' idea is *right*. Is their "common knowledge" knowledge at all? My argument was that it is not. Barker disagrees (2010). My response to Barker must also await another time (forthcoming).

The other main objection to IBE is that it is at odds with the variation and change that are central to Darwinism. I responded to this at some length (2008: 370-78). I will say a lot more here.

Discussions of essentialism in biology are dominated by talk of species. This dominance is unfortunate in two ways. First, it leads to the almost total neglect of taxon problems for non-species; so answers are not comprehensive. For example, taxon problems for non-species go totally undiscussed by four of my critics.[5] Second, the domination muddles the essentialism issue, which arises equally for taxa of all categories, with the vexed issue, addressed by the species concepts, of what it is for a taxon to be in the species category rather than some other category. As noted, the

[5] Both Ereshefsky (2010, 674) and Leslie (2013, 111) mention these problems at the beginning of their papers but never discuss them. Neither Barker (2010) nor Lewens (2102) even mention them.

essentialism issue concerns the property an organism must have to be a member of some taxon that has been identified and named by biologists and that is standardly thought to have a certain place in the Linnaean hierarchy. Whether or not that taxon has that place, whether or not it counts as a subspecies, species, genus, or whatever, is an issue that is quite independent of taxon essentialism.

How are the intrinsic essences of taxa in a hierarchy related? Where taxon *T* is in a lower category than taxon *T**, and falls within *T**, the essence of *T* must include the essence of *T** but not vice versa. Thus, everything that has the essence of *Canis familiaris* has the essence of *Canis*, but not vice versa; some *Canis* are foxes.

I aim now to develop the case for IBE by responding to two sorts of criticisms. First, in section 4, I respond to criticisms that IBE is at odds with certain biological variations. Then, in section 5, I respond to criticisms of my answer in "Resurrecting" to the charge that IBE's talk of intrinsic essences is an uncalled for metaphysical addition to biology. I accept the criticism and offer another answer.

4. Variation

Leslie finds my argument from structural explanation to IBE "intuitively appealing, but not…ultimately successful" (2013, 133). Focusing on variation, she makes a number of good points about underlying states and phenotypic features. She thinks that these points count against IBE. Indeed, facts about variation are often thought to count against intrinsic essentialism: the idea "that variation among organisms is the crucial stuff of changing life and of life's progress" is thought to be "devastating to essentialism" (Hey 2001, 62). In contrast, I have argued that IBE accommodates variation nicely (2008, 370-8). Leslie's variation objections are interestingly different from the ones I have discussed and I shall address them in some detail. I think that IBE can handle them too.

(1) Leslie considers my example of the rhinoceros and comments:

> Devitt is, of course, indisputably correct that *each particular African rhino* has some intrinsic features that, in combination with the environment, are causally responsible for that individual's having horns. This does not entail, however, that those very same intrinsic features are also responsible for other African rhinos' having horns. Whether this is so is a substantive empirical hypothesis, not one whose truth can be intuited in advance. (2013, 134)

Leslie supports this with the case of jade. As is well-known, two different chemical compounds, jadeite and nepthrite, have been lumped together as jade. So no common intrinsic chemical structure explains the similar observable features of all samples of jade.

Leslie is right, of course, that the fact that some intrinsic feature causes one rhino to have horns does not *entail* that that feature causes other rhinos to have horns. But the argument for IBE does not rest on any such entailment. It rests on what, *as a matter of fact*, *explains* the (nonaccidental) fact that rhinos have horns. The claim that some one intrinsic feature of rhinos is responsible for this is, as Leslie says, "a substantive empirical hypothesis." But the modal force of the hypothesis arises from an "inference to the best explanation" not an entailment. The argument for IBE rests on fallible empirical hypotheses of just this sort. I claimed that such hypotheses, implicitly embraced even by little children (Keil 1989), are implicit in the practice of taxonomists (2008, 352-53). In particular, I now claim, these hypotheses are supported by the contemporary role of genetic analysis in *re*classification.[6]

Take tigers (*Panthera tigris*), for example. A paper begins with the following claim about what were thought to be the several subspecies of tiger: "available molecular evidence suggests that extant tigers are extremely similar genetically" (Cracraft et al 1998, 139). Nonetheless, using DNA sequencing techniques, the paper argues, *on the basis of genetic differences*, for a reclassification: the Sumatran tiger is not a subspecies of *Panthera tigris* but a distinct species (1998, 148).

Similarly, a paper on African elephants argues against "the consensus that all belong to the single species *Loxodonta africana*" largely on the basis of a "deep genetic division between the forest and savannah populations" (Roca et al 2001, 1473-4). The paper concludes that these populations form two species, the former, *Loxodonta cyclotis*, and the latter, *Loxodonta africana* (2001, 1476).

"Cryptic species" provide many examples of taxonomic reclassification on the discovery that phenotypic properties that were thought to be caused by the one underlying nature are actually caused by two:

> Cryptic species are defined as ''two or more distinct but morphologically similar species that were classified as a single species" (Pfenninger and Schwenk, 2007). Because of this morphological similarity, most cryptic species were initially considered to be a single species until genetic data and rigorous scrutiny of phenotypic characters indicated otherwise. Cryptic species are relatively common across a wide range of taxa and habitats. (Andrews et al 2016, 361)

What we see is a pattern of tying a taxon to an underlying genetic structure, a structure that causes its phenotypic properties, and of reclassifying whenever it is discovered that there is significant genetic difference among two populations within a taxon. And this is just what we should expect because the underlying structure makes the taxon explanatory (secs. 2-3).[7]

(2) Leslie claims that "a member of one species…may have more genetically in common with a member of another species…than with a member of its own species" (2013, 133). The argument for IBE requires that members of a species share a genetic structure that largely constitutes the nature of the species. It does not require that the rest of the genetic structures of these members be the same. So IBE is quite compatible with what Leslie claims. What matters to

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[6] Thanks to Derek Skillings for drawing my attention to some of this literature.

[7] Slater thinks that taxonomic errors of the sort illustrated here pose a problem for IBE (2013, 48), but they do not. Thus, in cases like the Sumatran tiger and the forest elephant, the one taxon with a certain underlying essence is simply moved from one Linnaean category (subspecies) to another (species).

IBE is not an overall similarity among taxon members, established by "counting genes," but a similarity in one particular part of the underlying structure, the part that causes (in its "normal" environment) the distinctive phenotypic features of the species.

(3) Leslie's next objection moves from genetic variation to phenotypic variation: "conspecificity is compatible with a great deal of variation in phenotype at a time, and even more dramatically over time" (2013, 134). The argument for IBE hypothesizes an underlying nature that causes the phenotypic properties that are *shared* among conspecifics. Conspecifics can of course share those phenotypic properties *while differing in many others*. This is very obvious when we think of male and female conspecifics which often differ dramatically. It is even more obvious when we think of the higher taxa (remember, IBE applies to them too); thus the primate taxon (an *order*, in Linnaean categories) includes lemurs as well as humans, which differ greatly in phenotype. The phenotype variations noted by Leslie do not undermine IBE.

Those variations are caused by intrinsic underlying differences within a taxon. But, of course, phenotypic variations can also be caused by environmental differences. Consider the standard example of air temperature determining the sex of fetuses in some turtles and reptiles; and the impact of temperature, soil, etc. on the properties of plants is obvious. From the perspective of IBE, there is no puzzle about this. The one underlying nature of a taxon causes its members to have certain features in one environment, others in another.

This raises an important general point: the underlying nature of a taxon is just as explanatory of the *atypical* phenotypic features of a member produced by an "abnormal" environment as of the *typical* features of members in their "normal" environment. The nature plays its causal role across *all* environments. (And, probably, no phenotypic feature is essential.) Finally, the generalizations about a taxon that its intrinsic essence is supposed to explain, like that zebras are striped, are implicitly restricted to the taxon's "normal" environment.[8]

Slater sees the causal role of the environment as generating a problem in taking "genetic structure" as the essence of a species just as we take chemical structure to be the essence of a chemical kind:

> there is an important contrast between…chemical structure and an organism's "genetic structure"….An organism's "genetic structure"…does not alone determine, or even strictly suffice to explain, facts about the organism's outward character or behavior in the absence of information about its actual environment. (2013, 44)

[8] I noted this (2008, 327 n. 50) but did not emphasize the related point that "abnormal" environments cause exceptions to the generalizations. (The implicit restriction does not imply any commitment to the Aristotelian "Natural State Model" (2008, 371-72)). Mutations can also cause apparent exceptions to these generalizations, of course: "monsters." Leslie seems to think that IBE cannot allow for these exceptions and hence is "highly revisionary" (2013, 140, n. 23). I argued that this is not so (2008, 375-76).

But there is no significant contrast. Change the environment of a chemical kind enough and its outward character or behavior may change; thus, zinc ceases to be malleable below 300 degrees Fahrenheit.

(4) Leslie points to variation in the causes of phenotypic features: “In general, within a given species, individuals who share a common phenotypic feature need not share intrinsic microstructural bases that gave rise to the feature” (2013, 136). A feature that is “genetically induced” in some individuals might be “environmentally induced” in others (2013, 137). And so it might.

(a) Consider environmentally induced phenotypic features first. We have just given some examples. Leslie provides a nice further one, the Himalayan rabbit, a breed of the Common Rabbit (*Oryctolagus cuniculus*):

> Himalayan rabbits, when raised in moderate temperatures, have white body fur with black tails, noses, and ears; if they are raised in cold temperatures, however, they develop wholly black fur. (2013, 137)

From the perspective of IBE, there is no puzzle about this. The one underlying nature of this breed causes its members to have certain features in moderate temperatures and certain other features in cold temperatures.

(b) Now consider genetically induced phenotypic features. Leslie points out that the black fur of a rabbit can arise not only from the environment, as with the Himalayan rabbit, but “relatively straightforwardly from a given rabbit’s genetic make-up—that is, as an inherited trait that manifests itself across various environments” (2013, 137). But note that such a rabbit’s black fur does *not* arise from the part of its genetic make-up that is essential to its being a rabbit (else rabbits would typically be black). So, the underlying nature of the taxon *rabbit* does not determine that fur color. So this phenomenon has no bearing on IBE.

In sum, the underlying nature of a taxon causes different phenotypic properties in different environments. Different genetic make-ups among organisms that share the common underlying nature of a taxon can lead to different phenotypic properties. Indeed, the latter point is obvious when we remember that both lemurs and humans are in the primate taxon.

(5) Leslie’s most interesting objection to IBE is to be found in her vivid discussion of cases demonstrating that the path to a particular phenotypic property is complicated and varied:

> Phenotypic traits are the upshot of complex biochemical processes controlled in most cases by a variety of genes. Differences in the genetic level need not translate into differences in the biochemical processes,…Canalization of a trait insures that the trait is stably expressed in the face of underlying genetic variation…we might say that phenotypic traits often exploit a certain *multiple realizability* at the microstructural level. …Every macroscopic phenotypic property depends on a massive number of biochemical reactions, originating with the genes themselves but continuing along the entire

> developmental pathway, at each point potentially subject to environmental influences, influences from other genes, and so on. (2013, 138)

We have just discussed the impact of the environment on the developmental pathway to a phenotypic property in species members and need say no more. But what about the impact of the genes?

(i) Leslie is emphasizing that the role of genes is *complicated*, involving "a variety of genes." *But it is no part of the argument for IBE that the paths from genes to phenotypic traits are simple*. Indeed, although for convenience I sometimes talk simply of a genetic essence, I am noncommittal on precisely what underlying properties constitute the essence of a taxon and hence cause its phenotypic properties:

> In sexual organisms the intrinsic underlying properties in question are to be found among the properties of zygotes; in asexual ones, among those of propagules and the like. For most organisms the essential intrinsic properties are probably largely, although not entirely, genetic. Sometimes those properties may not be genetic at all but in "the architecture of chromosomes," "developmental programs," or whatever (Kitcher 1984/2003, 123). (2008, 347)

Indeed, it would be foolhardy for IBE to have commitments on these empirical biological matters, and it has none. We already know enough, of course, to reject "the crude idea that there is, say, 'a tiger gene'" (2008, 371). Okasha thinks "that species are distinguished by clusters of covarying [chromosomal and genetic] properties" (2002, 197). So perhaps, I suggest (2008, 371), that is where we should look for the underlying essence. But perhaps not. Consider these interesting claims by Denis Walsh in the course of arguing for intrinsic essences:

> We have grown accustomed to thinking of genes working together as pathways. The development of each character can be traced back through a fairly discrete causal pathway to the actions of a small set of genes. This view of gene function, however, has recently been shown to be quite untenable, at least for an extremely large part of the organism's genome. Rather, genes are organised, not so much as pathways, but as complex, regulatory networks. Phenotypes are produced through the complex interactions among a number of gene-regulatory networks (inter alia). No phenotypic feature can be traced to any particular gene or subset of genes within the network. (2006, 436)[9]

The moral of this is that the wise philosophical essentialist leaves the details of essences to scientists; biological essences are to be discovered by biologists.

This point generalizes. Leslie criticizes Kripke-Putnam essentialism not only about *biological* kinds but also about *chemical* kinds (2013, 142-58). Her criticism draws heavily on Paul Needham's long-running campaign against Kripke-Putnam essentialism about water; see

[9] Walsh gives a subtle and interesting argument that intrinsic natures are needed to secure "the stability and mutability of individual organisms required for adaptive evolution" (2006, 436). And he concludes, provocatively, that these natures are *Aristotelian* essences (2006, 444)!

Needham (2011) and the earlier papers cited. This campaign, based on an illuminating presentation of the scientific facts about water, includes criticism of the Kripke-Putnam talk of "H2O" as the essence of water. But it is central to the positions of Kripke (1980: 119-29) and Putnam (1975: 224-5) that we should look to *empirical science*, not to philosophers like themselves to fulfil the supremely important task of *discovering* the essence of "natural kinds" like water. Needham shows convincingly that their talk of the essence of water being "H2O" is quite inadequate but this talk should be seen as nothing more than a philosopher's hand wave toward the scientific facts.

I conclude that a complicated role for genes in causing phenotypic traits is not a problem for IBE.

(ii) Leslie is also emphasizing something that may seem more problematic for IBE: the genetic component involved in the developmental pathway to a phenotypic property can vary among the members of a species. Thus, commenting on the cause of human female genitalia, Leslie says:

> For each individual infant, there will be a genetic component to the explanation (as well as an environmental component), but this genetic component need not be the same for each infant. (2013, 136)

She makes similar remarks about the three toes on the hind feet of guinea pigs:

> *having three toes on the hind feet* is a characteristic property of guinea pigs (*Cavia porcellus*). Possession of this phenotypic property is due to a flexible interaction-effect between a number of factors, both genetic and non-genetic—that is, there is a generous range of pathways, all of which lead to having three hind toes. Individual guinea pigs can differ significantly from each other with respect to these factors and yet each have three toes,…. (2013, 137)

See also her discussion of the number of eggs produced by female salamanders (2013, 137-38). The previous untroubling point was that many genes are involved in causing a phenotyptic property. The present point is that the ones involved can vary within a taxon: the posited essence of a taxon can cause a phenotypic property in more than one way. Walsh is illuminating on this. He continues the above passage on regulatory gene networks as follows:

> These networks, or circuits, are characterised by flexibility, redundancy and robustness (Salazar Ciudad et al. (2001); Szathmary (2002)). All of these features function to preserve the capacity of a gene network to produce its normal output in the face of perturbations and variations of initial conditions. For example, gene regulatory networks show a remarkable capacity to compensate for the 'knockout' of elements of the network. If a gene is removed, or 'knocked-out', of a regulatory network the network typically compensates and finds alternative ways of producing its characteristic output (Greenspan 2001)). (2006, 436-37)

Walsh does not see this variation in the effective genetic component as a problem for intrinsic essentialism. Quite the contrary. He takes the essence of a species to be “the causal capacities of its developmental systems” which realize its “phenotypic plasticity” (2006, 441). And that plasticity consists in just the sort of “flexibility, redundancy and robustness” illustrated by Leslie’s guinea pig. So, why does Leslie think that the variation is a problem?[10]

Leslie objects to the “disjunctive” nature of the underlying cause of a phenotypic property, its being *either* this genetic component *or* that one. Using the example of jade again, Leslie argues that IBE cannot allow this:

> the observable properties of the sample of jadeite are determined by its intrinsic chemical structure in conjunction with the environment, and similarly for the sample of nephrite. Yet there will be no common intrinsic chemical structure that explains the shared features of the two samples of jade.
> (There will, of course, be the non-explanatory disjunctive property of being composed of *either* NaAl(SiO3)2 *or* Ca2(MgFe)5Si8O22(OH)2. However, it is important to see that disjunctive properties cannot play the explanatory role that Devitt has in mind, or else the whole enterprise is trivialized. For example, let us suppose with Devitt that there is a common intrinsic property had by tigers that explains why they are striped. Let us also suppose that there is a different common property that explains why canna lilies are striped. If disjunctive properties are allowed to figure as common intrinsic explanatory properties in Devitt's sense, then there will be a further shared intrinsic property that explains why *this tiger* and *this lily* both have stripes. If disjunctive properties are countenanced in this endeavor, then *shared* properties become far too cheap to be of interest. Certainly, it would not then be a *biological* hypothesis that a common property explains why Indian rhinos have one horn—it would simply be a familiar point about the logic of disjunction.) (2013, 134)

Lewens makes a similar point, claiming that there might be various microstructural causes of stripyness in tigers and continuing:

> Of course, we will be able to identify a characteristic ‘pattern’ of genotypic properties across the species, simply by enumerating whichever genes actually cause the instances of species-typical phenotypic properties, but since this must be the case however unruly these underlying genes are, the sense of essence we salvage here is trivial. (2012, 753)

This is interesting. But the wrong moral to draw from it is that the one underlying nature cannot yield disjunctive explanations. The disjunctive nature of jade *does* explain its observable properties. It just doesn’t explain them well enough *for the purposes of science*. Kinds like jade, with extremely disjunctive underlying essences, are not scientifically acceptable because alluding to them does not provide *adequate* explanations; they do not “carve nature at its joints.” From the perspective of IBE, that is why a cryptic species is not a biologically acceptable species. And, for similar reasons, Sumatran tigers are no longer classified as a subspecies of tigers and forest elephants are no longer placed in the same species as savannah elephants. But none of this shows

[10] Leslie does not mention Walsh’s paper.

that the explanatory needs of biology cannot tolerate *any* disjunction in the way an underlying nature causes a taxon's phenotypic properties. Indeed, the moral of Leslie's cases of female genitalia, the guinea pig, and the salamander, and of Walsh's discussion, is precisely that biology embraces some disjunctions: the underlying nature may provide several different causal paths to a phenotypic property. Nonetheless, the argument for an intrinsic essence presented in section 3 holds: it is *because* of the underlying largely genetic essence of guinea pigs that they have three toes on their hind feet; and it is *because* a certain animal is a guinea pig, hence has the underlying nature of a guinea pig, that it has three toes on its hind feet. These are biologically sound explanations. But similar essentialist explanations of a phenotypic property of a cryptic species are not acceptable. And that's why cryptic species are not species.

I conclude that the disjunctive explanations that Leslie and Lewens are pointing to do not undermine IBE but rather throw an important light on a species' intrinsic explanatory essence. The explanations demonstrate that the essence of tigers is not only not "a tiger gene," it is a long way from a tiger gene. So, what sort of disjunctive explanation is scientifically acceptable in biology? What is the difference between the acceptable explanation of the guinea pig and the unacceptable one of a cryptic species? This is the sort of biological question on which, as just noted, it would be wise for a philosopher to be noncommittal. But, presumably, the acceptable disjunctive explanations, the ones that do not demand reclassification, must be ones arising from a certain underlying unity in what is flexibly responsible for phenotypic properties.

5. "The Added Metaphysical Claim"

Ereshefsky is quite unconvinced by my argument for IBE. He notes that the *structural* explanation of why zebras have stripes adverts to intrinsic properties whilst insisting that the *historical* explanation must advert to genealogical relations (2010, 680). So far, then, we are in agreement. Disagreement begins with the move I make from the structural explanation to IBE, to "the added metaphysical claim that the character cited in the explanans is essential to membership in a taxon" (2010, 680). This disagreement is reminiscent of a nice question put to me by Peter Godfrey-Smith. I expressed the question like this in "Resurrecting":

> It is of course the case that the truth of any…generalization [about the phenotypic properties of a taxon] must be explained by an intrinsic, probably largely genetic, property, but why does that property have to be an *essential* property of the kind in question? (2008, 354)

I attempted an answer to this question by appealing to the law-like nature of the generalizations and subjunctive conditionals (2008, 377-78). Both Lewens (2012, 755-56) and Slater (2013, 51-3) do a good job of showing that this answer is inadequate. So I hereby abandon it.

Implicit in my argument for IBE was another answer that I strangely overlooked. A clue to this answer was given in section 4: "The nature plays its causal role across *all* environments." Any organism in taxon *T* has certain phenotypic properties *because* it is in *T*; it is *because* an animal is a zebra that it is striped. So the property that *makes* something a zebra, whether we call that property an "essence," "nature," or whatever, must *cause* that zebra, in its environment, to have stripes; the essence of zebras must explain the place that certain organisms have in the causal nexus *just because they are zebras*.[11] Elliott Sober is getting at this with the following demand: an essence "must be *explanatory*…A species essence will be a causal mechanism that acts on each member of the species, making it the kind of thing that it is" (1980/1992, 250). The essence of *T* is whatever property, as a matter of actual fact, plays that causal role in an environment. This is the basis for the "added metaphysical claim."

So Lewens is right in supposing that I do not "wish to say that species have intrinsic essences just so long as there are clusters of properties common and peculiar to them" (2012, 755). But he is wrong to wonder whether my "essentialism amounts to nothing more than the unobjectionable assertion that…intrinsic properties of organisms are relevant to determining their species" (2012, 756). Intrinsic essences play a fundamental causal role.

6. Conclusion

My argument for *Intrinsic Biological Essentialism* in "Resurrecting," summarized in section 3, was that the intrinsic nature of a taxon explains both the truth of generalizations about its members and why being in the taxon is explanatory. This argument came in for interesting criticisms from Barker (2010), Ereshefsky (2010), Lewens (2012), Leslie (2013), and Slater (2013). My main aim in this paper has been to give a detailed response to criticisms where that response advances the positive case for IBE.

My first detailed response, in section 4, rejects the charge that IBE is at odds with certain biological variations. Thus, according to IBE, the underlying nature of a taxon causes different phenotypic properties in different environments; different genetic make-ups among organisms that share a taxon's underlying nature can lead to different phenotypic properties; the many genes involved in causing a phenotyptic property can vary within a taxon.

My second detailed response was to the charge that IBE's talk of intrinsic essences is an uncalled for metaphysical addition to biology. The response to this charge in "Resurrecting" has been shown to be inadequate. I now claim that the essence or nature of a taxon simply *is* whatever underlying state causes members of that taxon, in their environment, to have their phenotypic properties; the essence explains the place that those organisms have in the causal nexus *just because they are members of the taxon*.

It is a striking feature of the consensus, I argue elsewhere (2018a), that it has failed to produce a historical/relational answer to the taxon problem that is complete, plausible, and comprehensive. And, I would argue, none of my critics have rectified this situation. The absence of such an historical alternative to IBE is part of the case for IBE. That case increases if I am right that there is indeed an historical *component* to a taxon's essence but it is one that *demands an intrinsic component*. For, the relevant history of a taxon is of organisms of *a certain intrinsic kind*

[11] This view of essences generalizes to nonbiological kinds. Thus, the essence of gold causes instances of it to be malleable in its normal environment.

evolving into organisms of *a certain other intrinsic kind*, until we reach the taxon in question. (2018a).[12]

[12] I have argued similarly elsewhere that the essence of a biological *individual* has both an historical and an intrinsic component (2018b).

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