

Francis Galton's Theory of Inheritance and the Problem of Unconceived Alternatives¹

Abstract

Elsewhere I have argued that the most significant threat to scientific realism arises from what I call the problem of unconceived alternatives: the repeated failure of past scientists and scientific communities to even conceive of alternatives to extant scientific theories, even when such alternatives were both (1) well-confirmed by the evidence available at the time and (2) sufficiently scientifically serious as to be actually embraced in the course of further investigation. In this paper I explore Francis Galton's development and defense of his "stirp" theory of inheritance and conclude that this particular historical example offers impressive support for the challenge posed by the problem of unconceived alternatives while simultaneously showing how we can make that challenge deeper and sharper.

1. Introduction

In recent and forthcoming work (2001, 2006) I have argued that scientific realism is actually threatened most substantially neither by traditional underdetermination arguments nor by the traditional pessimistic induction, but by a quite different sort of historical pattern. What should actually give realists pause, I suggest, is our repeated failure to even conceive of alternatives to our scientific theories that were both well-confirmed by the evidence available at the time and sufficiently serious as to be ultimately accepted by some actual scientific community in the course of further inquiry. If the historical evidence confirms that past practitioners have routinely failed to conceive of (and therefore failed to consider) such alternatives when they existed, we have every reason to believe that there are similarly serious and well-confirmed unconceived alternatives to contemporary scientific theories, even if we cannot specify or describe them further. Perhaps the most important difference between this and the traditional pessimistic induction, I suggest, is that it concerns the theorists rather than the theories of past science: even if today's theories are sometimes supported by varieties or degrees of empirical evidence that many past theories did not enjoy, we have no reason to think that today's theorists are any better at exhausting the full range of theoretical possibilities well-confirmed by this evidence than were the greatest scientific minds of the past.

As I noted in my original presentation of the challenge (Stanford 2001), a theory need not accommodate all of the evidence available at a given time in order to count as well-confirmed, for theories are permitted to have evidential anomalies. Nor does the challenge require us to ignore the phenomenon of explanatory losses in the transition

from an earlier theory to a later one: a theory need not explain everything a competitor explains in order to count as well-confirmed by the available evidence, since competing theories may simply have different evidential anomalies and/or explanatory achievements. Thus, the point of the challenge is simply that we have repeatedly found ourselves able to conceive of only one or a few theories that were well-confirmed by the totality of the evidence available at a given time in a given natural domain, when alternatives that were also reasonably well-confirmed by that same body of evidence indeed existed unconceived by us.

My original presentation of this challenge also gestured towards any number of what I claimed were at least prima facie examples of past successions of scientific theories exemplifying this historical pattern, in which the theories that would actually be accepted by later scientific communities were not even conceived of by adherents of an earlier theory or theoretical tradition despite being reasonably well-supported by the evidence available at the time. I also acknowledged, however, that any convincing case for the importance of this problem of unconceived alternatives would have to stand or fall with a close analysis of the details of the historical record in these cases. In this paper, I hope to at least start making good on this promise by examining just one such example in the requisite detail: Francis Galton's "stirp" theory of inheritance. I will argue that a close analysis of the details of Galton's development and defense of this theory demonstrate his failure to conceive of scientifically serious alternative lines of theorizing about inheritance that were well-confirmed by the evidence available to him, including those that would actually be accepted by later scientists and scientific communities. Thus, the case of Galton's stirp theory offers substantial support for the claim that the problem of

unconceived alternatives poses a clear and present danger to scientific realism.

2. Darwin, the Rabbits, and Galton's 'Stirp' Theory of Inheritance

The idea of living or material units or particles as the substrate of inheritance that is developmentally continuous with our own is traditionally traced back to the “physiological units” introduced by Herbert Spencer in his Principles of Biology (1864) and most importantly to the “gemmules” of Charles Darwin’s theory of pangenesis, first proposed in his Variation of Animals and Plants Under Domestication (hereafter VAP) in 1868. There Darwin offered a long list of phenomena of inheritance and generation that he suggested were “at present left disconnected by any efficient cause” (VAP ii 433), but for which his own theory could provide a convincing explanation:

How it is possible for a character possessed by some remote ancestor suddenly to reappear in the offspring; how the effects of increased or decreased use of a limb can be transmitted to the child; how the male sexual element can act not solely on the ovules, but occasionally on the mother-form [under this heading Darwin also later includes its effect on the offspring of later matings]; how a hybrid can be produced by the union of the cellular tissue of two plants independently of the organs of generation; how a limb can be reproduced on the exact line of amputation, with neither too much nor too little added; how the same organism may be produced by such widely different processes, as budding and true seminal generation; and lastly, how of two allied forms, one passes in the course of its development through the most complex metamorphoses, and the other

does not do so, though when mature both are alike in every detail of structure. (VAP ii 432-433)

The fundamental mechanism of inheritance that Darwin saw as capable of explaining these phenomena both individually and collectively was that the various developed tissues or parts of an organism individually “throw off minute granules which are dispersed throughout the whole system” and that these granules or “gemmules” are aggregated in the new buds, sex cells, or any cells from which an entire new organism may be produced (VAP ii 457). Thus, pangenesis held that “an organism does not generate its kind as a whole but each separate unit generates its kind” (VAP ii 490) and that “every separate part of the whole organization reproduces itself. So that ovules, spermatozoa, and pollen-grains,—the fertilized egg or seed, as well as buds,—include and consist of a multitude of germs thrown off from each separate part or unit” (VAP ii 433).

Galton was intrigued by this proposal, and moreover, thought he saw a way to subject it to a convincing empirical test. Darwin’s discussion at least suggested that the gemmules circulated throughout the body in the bloodstream. Thus, Galton reasoned, if he mated purebred “silver grey” rabbits after transfusing blood from various other breeds into them, the gemmules acquired in these transfusions would be passed to the sex cells of the silver greys and produce in their offspring some form of “mongrelism”: variations in traits or features that were uncharacteristic of the pure breed. Despite sustained efforts repeated for years over multiple generations using rabbits with higher and higher proportions of transfused blood, however, these hopes were disappointed, and Galton ultimately concluded that pangenesis had failed his experimental test.

Following this episode, Galton returned to an idea of his own about inheritance originally broached in 1865: that the tissues of the parents were not the immediate progenitors of similar tissues in the offspring, but that both were instead the products of a shared germ-line. The phenomenon described as the “distant reversion” of an organism to characteristics found in earlier ancestors but not its own parents had led even Darwin to suggest that gemmules might sometimes remain latent and be transmitted from an ancestor to a later descendent without being expressed in the intervening generation, but it remained to Galton to seize upon this same possibility and elaborate it into a systematic general mechanism of inheritance and hereditary resemblance. On Galton’s account, the developed tissues and characteristics of the offspring resembled those of the parents not because the latter made any direct material or causal contribution to the former, but because both were the effects of a common cause (i.e. the shared germ-line that had produced them both). That is, he retained Darwin’s suggestion that the tissues of the offspring develop from gemmules or hereditary particles passed through the bodies of their parents, but suggested that the ultimate source of these hereditary particles was not the parent’s own tissues but instead a “stirp” (from the Latin ‘stirpes’, a root) of shared germinal materials from which the parent’s own tissues had themselves also developed.²

Galton offered the most detailed development of this “stirp” theory of inheritance in his articles “On Blood Relationship (hereafter BR) and “A Theory of Heredity” (hereafter TH).³ There Galton defined the stirp as “the sum-total of the germs, gemmules, or whatever they may be called, which are to be found, according to every theory of organic units, in the newly fertilized ovum” (TH 330). Following fertilization, he proposed, this stirp of hereditary materials was subject to a process he called “Class

Representation”, which selected a small number of hereditary elements destined to become ‘patent’ and develop into the tissues of the organism from a much larger ‘residue’ of ‘latent’ elements instead remaining undeveloped. These latent elements were then multiplied, after which a further process of “Family Representation” would separate those destined to die with the organism from those destined to be passed on and to form the stirp of that organism’s own offspring, either in buds or (after uniting with those of another parent in fertilization) in sex cells. Either method would produce a stirp of equal size to that with which the process had begun in the parent organism, after which the cycle would begin again with the process of Class Representation in the offspring. By appeal to this mechanism of inheritance, Galton sought to explain such phenomena as the resemblance of offspring to parents and siblings, individual variation, and distant reversion to ancestral characteristics in a manner analogous to the effects of statistical sampling from a large population.

In fact, Galton was able to retain the explanations of phenomena Darwin had offered as the heart of his case for pangenesis simply by allowing that the processes of generation, inheritance, growth, development, and repair were mediated by hereditary particles distributed throughout the body, even while insisting that the ultimate source of such hereditary particles was a continuous germ line passed (in a variety of possible ways) from parents to offspring rather than the developed tissues of the parent organism itself. In this way, he could simply appropriate virtually all of the explanatory accomplishments of pangenesis itself, including Darwin’s pangenetical explanations (cf. VAP ii 467-88) of reversion, of bud-variation, of graft-hybrids, of parthenogenesis, of the development of complex tissues, of the processes of repair (and their precision), of the

continuity between various forms of reproduction, of the possibility of producing identical organisms by both budding and seminal generation and with or without complex metamorphoses, and even of phenomena whose existence Darwin accepted but that we deny, like the direct influence of the “male sexual element” on the tissues of the mother plant (later called xenia or metaxenia) and on later progeny of the same female animal by different males (telegony). Galton’s account even managed to accommodate Darwin’s famous commitment (especially late in life) to the inheritance of acquired characteristics by allowing for the possibility that events during the life of an organism could affect the composition of the stirp of germinal materials passed along to its offspring: that is, by recognizing that a shared germ-line need not be an isolated germ-line.⁴ Thus, his theory offered equally plausible and convincing explanations of the phenomena for which Darwin invoked pangenesis to account, including especially those phenomena most strongly supported by the available evidence, and of course, the surprising negative result of the transfusion experiments as well. As we will now see, however, Galton himself failed even to conceive of alternative lines of theorizing that would be accepted by later scientists and scientific communities and that were also well-confirmed by the very phenomena he invoked the stirp theory to explain.

3. Galton’s Maturational, Invariant Conception of Inheritance and his Failure to Conceive of Directive or Contextual Alternatives

It is clearly difficult to find convincing historical evidence for the claim that a given thinker genuinely failed to conceive of a given theoretical alternative or line of theorizing, rather than finding it uninteresting, regarding it as unpromising, or any of the

innumerable other reasons a scientist might have for neglecting to discuss or develop a particular theoretical approach to a particular set of phenomena. But the precise manner in which he develops his case for the stirp theory offers an especially powerful source of such evidence in Galton's case, for he repeatedly insists that several features of the stirp theory are simply unavoidable.

For example, Galton opens "A Theory of Heredity" by asserting that there is a consensus among his contemporaries in favor of Darwin's view that "the body consists of a multitude of 'organic units', each of which possesses its own proper attributes, and is to a certain extent independent of all the others" (TH 329; this is quoted by Galton from VAP ii 453). He goes on to insist that this hypothesis "and all that such an hypothesis implies, must lie at the foundation of the science of heredity" (TH 329). Most importantly, however, he then goes on to describe what he calls "four postulates that seem to be almost necessarily implied by any hypothesis of organic units" (TH 331):

The first is, that each of the enormous number of quasi-independent units of which the body consists, has a separate origin, or germ. The second is, that the stirp contains a host of germs, much greater in number and variety than the organic units of the bodily structure that is about to be derived from them; so that comparatively few individuals out of the host of germs, achieve development. Thirdly, that the undeveloped germs retain their vitality: that they propagate themselves while still in a latent state, and contribute to form the stirps of the offspring. Fourthly, that organisation wholly depends on the mutual affinities and repulsions of the separate germs; first in their earliest stirpal stage, and subsequently during all the

processes of their development.

Immediately following his presentation of these postulates, Galton remarks that “We must also bear in mind that the alternative hypothesis of a general plastic force resembles that of other mystic conceptions current in the early stages of many branches of physical science, all of which yielded to molecular views, as knowledge increased” (TH 332). That is, Galton treats the hypothesis of a “general plastic force” as the only serious alternative to any “molecular” hypothesis of “organic units” (which must obey his four postulates). And he concludes the paper by insisting again that these postulates appear to be the “necessary consequences” of any attempt to theorize about heredity on the basis of organic units.

Galton’s repeated insistence that any theory of organic units must satisfy the postulates he lays out offers important evidence of any number of respects in which he failed to appreciate serious and equally well-confirmed alternatives to the line of theorizing embodied by the stirp theory. Galton seems, for example, to have shared with Darwin what we might call a “maturational” rather than a “directive” account of inheritance: that is, one on which the effects of inherited germs on the resulting organisms must be achieved by the germs themselves growing into or becoming the constituent parts of that organism, rather than simply directing the growth and/or development of its constituent physiological units. This maturational conception is reflected in the first of Galton’s postulates: that each of the “quasi-independent units” of which an organism’s body consists must be produced by a separate germ. This seems to ignore the possibility that inherited germinal units might produce the characteristics of organisms without the transmission of a single discrete and isolable hereditary unit

specific to each distinguishable organic unit or each constituent of an organism's body. That is, Galton seems not to recognize the possibility that inherited germinal particles could produce the constituents of the body without any one-to-one correspondence between particular germinal and organic units, or (as we might say it now) without a distinct hereditary unit specific to each cell of the body, each tissue, or each phenotypic trait. The maturational conception is also reflected in Galton's fourth consequence, that the organization or structure of an organism's body "wholly depends on the mutual affinities and repulsions of the separate germs." Here Galton simply assumes that if hereditary particles produce the organic units making up an organism's body, then a global spatial or physical organization among those inherited germs must be the mechanism whereby the structure or organization of the resulting body is achieved.

By contrast, the directive conception of hereditary influence seems not even to suggest, much less to require, either that each trait, tissue, or physical constituent of the body must have a separate germinal source (Galton's first consequence) or that the organization or structure of an organism must be achieved by a corresponding organization among the germinal materials that are destined to develop into that organism, whether by "mutual affinities and repulsions of the separate germs" (Galton's fourth consequence) or in some other way. Thus, it would seem that Galton can rest confident in the necessity of his first and fourth consequences only because he fails to conceive of even the possibility of any directive alternative to the maturational conception of particulate heredity that he and Darwin shared.

Even more fundamentally, however, Galton's commitment to the necessity of the second and third of his postulates illustrates his failure to conceive of any alternative to

what we might call a “contextual” rather than “invariant” account of inheritance. On Galton’s invariant conception, each active or developed hereditary element exerts a specific, recognizable effect no matter what others are inherited along with it and no matter the context in which it occurs. On a contextual alternative, hereditary materials might be present in a fully ‘developed’ or ‘active’ form, but simply have different causal consequences in the context of a different suite of inherited materials or against a different environmental background. Galton’s failure to conceive of any such contextual alternative is reflected in his insistence that the reversion of organisms to characteristics not present in either of their parents simply requires any hypothesis of organic units to suppose that there must be many undeveloped germs which retain their vitality and are passed on to offspring (the second and third of the “necessary consequences”). This connection is particularly clear and explicit in Galton’s more detailed argument for the second necessary consequence:

That the stirp contains a much greater variety of germs than achieve development, is proved by the fact that a person is capable of transmitting a variety of ancestral peculiarities to his children, that he did not himself possess. But since everything that reached him from his ancestors must have been packed in his own stirp, it follows that his stirp contained in addition to such peculiarities as were developed in his own bodily structure, those numerous other ancestral peculiarities of which he was personally destitute, but which he bequeathed to one or more of his descendants. Therefore, every stirp must be held to contain a great variety of germs in addition to those that may achieve development in the person

who grows out of that stirp. (TH 331-2)

Of course, Galton's insistence here that the phenomenon of distant reversion requires us to postulate latent or undeveloped germinal materials follows only on the invariant conception's assumption that an inherited germinal unit must always have precisely the same effect that it did in every ancestor when it "achieve[s] development" at all, or is 'patent' rather than 'latent' or 'dormant'. A contextual version of Galton's own theory could account for reversion equally well by assuming the intermittent recurrence of those (internal or external) causal conditions that enabled a particular germinal element to produce a particular characteristic in a given ancestor. This sort of possibility also clearly characterizes contemporary molecular genetics, of course, where differences in the phenotypic expression of a given gene are characteristically regarded as a matter of causal context and interaction, and even classical Mendelian relations of dominance and recessiveness seem most naturally considered as interactions between alleles rather than latency, dormancy, or some kind of undeveloped state of a gene itself.⁵ Thus, Galton's confidence in the necessity of the second and third of his postulates seems to reflect his failure to conceive of even the possibility of a contextual rather than invariant account of particulate inheritance.

Although they are especially noteworthy in connection with his insistence on the necessity of his four postulates, Galton's failures to conceive of alternatives to his own maturational and invariant conception of heredity are in evidence throughout his central writings on generation and inheritance. He argues in "On Blood Relationship", for instance, that "because ancestral qualities indicated in early life frequently disappear and yield place to others" the organism "must receive supplementary contributions derived

from their contemporary latent elements” (BR 396). But this consequence follows on the invariant conception’s distinctive presumption about the range of expression of any single developed hereditary element. And this same failure to conceive of any alternative to invariant inheritance is even more strongly in evidence elsewhere in “On Blood Relationship”, where Galton argues that

From the well-known circumstance that an individual may transmit to his descendants ancestral qualities which he does not himself possess, we are assured that they could not have been altogether destroyed in him, but must have maintained their existence in a latent form. Therefore each individual may properly be conceived as consisting of two parts, one of which is latent and only known to us by its effects on his posterity, while the other is patent and constitutes the person manifest to our senses. (BR 394)

The most significant aspect of this argument for our purposes is Galton’s unselfconscious slide from the claim that recurring ancestral characteristics must have persisted in some latent form to the claim that organisms must therefore consist of distinct latent and patent parts. Perhaps there is room within the abstract notion of latent ‘form’ to accommodate a contextual interpretation, but such a possibility is simply ruled out by the further insistence that such latency must be manifested by physically distinct latent parts of an ancestor transmitting characteristics that are not themselves expressed in that ancestor.

This is by no means simply a momentary lapse or passing thought on Galton’s part, for throughout his writings on inheritance and generation he repeatedly treats the patent and latent hereditary elements found in any organism as disjoint sets of material

components. He insists, for instance, that “the latent elements must be greatly more varied than those that are personal or patent” (BR 395) and that it follows from his arguments “that for each place among the personal elements there may exist, and probably often does exist, a great variety of latent elements that formerly competed to fill it” (BR 395). And the insistence that distant reversion requires a division of the organism into distinct latent and patent parts is similarly explicit in the much later work Natural Inheritance (hereafter NI):

The existence in some latent form of an unused portion is proved by his power, already alluded to, of transmitting ancestral characters that he did not personally exhibit. Therefore the organised structure of each individual....is the coherent and more or less stable development of what is no more than an imperfect sample of a large variety of elements” (18/318).

Of course, this passage also reflects Galton’s unquestioned commitment to the maturational conception of inheritance, as it insists that inherited germs themselves become or develop into the constituent parts of the body of an individual organism. Thus, the inferential footprints of the maturational conception are spread throughout Galton’s writings no less than those of the invariant conception; in “On Blood Relationship” he writes that “The embryonic elements are developed into the adult person” (395, original emphasis) and that “the embryonic elements are...developed (a) into the visible adult individual” (396, original emphasis). Even the terminology of this latter work reflects Galton’s exclusive commitment to the maturational conception, for he coins distinct terms for the two distinct developmental processes by which a latent germ ultimately becomes part of the stirp passed to the next generation and by which a patent germ

becomes part of the adult organism's body ("development b" and "development a", respectively).

Galton's failures to conceive of the relevant alternatives to his maturational and invariant conception of inheritance are also both in evidence elsewhere in "A Theory of Inheritance", for example in the course of the attack on Darwinian pangenesis offered in its original version:⁶

[t]he germs that become developed into structure, are relatively too few to exert much hereditary influence, and when fully developed they would be somewhat passive and sterile. I argue, that as fertility resides somewhere, it must have been vested in the non-developed residue of the stirp, or rather in its progeny and representatives (whatever, or however numerous, they may be) at the time when the individual has reached adult life. (TH [1875] 88)

Here again Galton presupposes that germs affecting future generations must be in a "non-developed state", reflecting his failure to conceive of any alternative to the invariant conception of inheritance, and that the formation of an organism is a matter of the inherited germs themselves becoming "developed into structure" and reaching a "fully developed" state, reflecting his failure to conceive of any alternative to the maturational conception of inheritance. Thus, throughout his central writings on generation and heredity Galton is consistently willing to make claims and draw inferences that signal his failure to conceive of either a directive or a contextual alternative to his own account of the process of inheritance.

As I go on to show in detail in (Stanford 2006, Ch. 4), these failures to conceive

of serious and well-confirmed alternatives to (or alternative versions of) his own account persist even when Galton is most concerned to explain what (and how much) remains to be learned about the process of inheritance. But perhaps most important among the further details of this case that limitations of space prevent me from discussing fully at present are two salient points at which it seems that Galton's writings must countenance contextual and/or directive alternatives to his maturational and invariant account of inheritance. In light of the importance of these two potential sources of contrary evidence for my historical thesis, I will here briefly summarize the substance of the longer discussion pursued in my more detailed treatment of this case.

The first is that Galton might seem to make room for a contextual account of inheritance in acknowledging that we are largely ignorant about “whether the result of the elections at one place may or may not influence those at another (on the principle of correlation)” (BR 395). As his subsequent discussion of this principle of correlation makes clear, however, Galton here intends to leave room simply for the possibility that the selection of one germ for development into a part of the organism might affect which other germs are also chosen to become developed and not for the genuinely contextualist possibility that the development of a given germ could affect what another germ would become, produce, or grow into. Likewise, although Galton elsewhere recognizes a “large number of variable influences” and “an incalculable number of...unknown circumstances” (NI 9/309) at work in development, he consistently sees these simply as affecting which germs are chosen for development into the organism and not the form into which those germs might develop once selected or the character of that development.

The second important point concerns Galton's analysis of the case of the

inheritance of skin color in the case of “negro blood”, for there he argues that the intermediate color of a mulatto or quadroon is produced by a small number of germs (“present very obviously or not at all” (TH 335)), which would seem to suggest first that a small number of germs affect the color of all those which develop into skin cells (a contextual possibility) and second that at least some discrete germs thus affect the characteristics of the resulting organism in some way besides developing into one of its constituent parts (a directive possibility). That this analysis seems to modern eyes to cry out for the recognition of alternatives to the invariant and maturational conception of inheritance renders it all the more telling that Galton himself did not see matters in this way. In response to an objection pressed to him by Darwin in correspondence, Galton argues that the color of a hybrid organism does not appear as a result of uniform coloration among its various constituent physiological units, but is instead produced by a pattern of variously colored discrete individual cells (or smaller physiological units) developed from germs with distinct origins. That is, Galton argues that a given degree of darker or lighter coloration is produced in an organism by the inheritance of a larger or smaller proportion of gemmules identical to those that became colored cells (or smaller physiological units) in its parent(s), and that these colored units must be distributed throughout the relevant part of the body in a mosaic pattern too fine for the individual colored physiological constituents to be resolved.⁷ Perhaps most importantly of all, in his letter to Darwin (Dec. 18, 1875; in Pearson, vol II 189) Galton again claims, just as he did in the case of his four ‘necessary consequences’, that this explanation is the one that “any theory of organic molecules” would give.

4. Conclusion

Despite first appearances, then, even those aspects of Galton's writings that seem to leave room for or require contextual and/or directive accounts of particulate inheritance turn out on closer analysis to provide compelling further evidence of his failure to even conceive of such alternatives instead. Nonetheless, some later directive and/or contextual theories of inheritance, such as Weismann's theory of the germ-plasm, Mendelian genetics, and contemporary molecular genetics, would offer equally powerful explanations of the most fundamental phenomena for which Galton had invoked the stirp theory to account, especially for those phenomena whose existence was supported most strongly by the available evidence (see Section 2, above). But we need not rest content with the claim that these influential successors were also well-confirmed by the evidence available at the time, for we have also seen that Galton failed to conceive of any directive and/or contextual version of his own theory, despite the fact that the available evidence and the phenomena of particular interest to him supported such an alternative equally well. And there seems no more room to doubt the scientific seriousness or legitimacy of a directive, contextual version of Galton's own stirp theory than of those directive and/or contextual accounts that would ultimately come to be actually accepted by later scientists and scientific communities.

It might seem natural enough, however, to worry about the fact that Galton's stirp theory itself never came to dominate the scientific practice of its day. After all, the idea of a continuous germ-line is usually associated with the name of Weismann rather than Galton, who would ultimately complain that "all along I seemed to have spoken to empty air" and that his views on inheritance represented "a minority of one" (1894, cited in

Provine 1971 24). But in evaluating the threat posed by the problem of unconceived alternatives, what matters is the character and status of the various alternatives of which we have failed to conceive, not that of the theories to which they were alternatives in the first place. After all, the worry is that there might be scientifically serious unconceived alternatives to even the best of our own scientific theories that are well-confirmed by the evidence available to us. And the inability of past theorists to exhaust the space of serious, well-confirmed alternatives to their own theories provides equally compelling evidence for the view that this is our general predicament in scientific theorizing whether those same theorists managed to convince their contemporaries of the merits of the proposals they did manage to conceive of or not.⁸

Moreover, Galton's development and defense of the stirp theory suggests at least one important way in which we might extend and/or deepen the challenge posed by the problem of unconceived alternatives itself. As we have seen, Galton's failures to recognize important alternative theoretical possibilities seem to have a nested or hierarchical structure: it is because Galton cannot conceive of any alternative to the maturational conception of heredity, for instance, that he finds himself forced to insist that each of the separate constituent parts of an organism must be produced by a separate germ and that the physical organization of the body must reflect a corresponding physical organization in the germ. Likewise, it is because Galton fails to conceive of any contextual alternative to his invariant conception of heredity that he is forced to conclude that reversion to ancestral characteristics requires the existence of latent or undeveloped hereditary materials that are passed from ancestor to offspring. And the nested structure of these inferences shows why the challenge posed by unconceived alternatives does not,

as it might have initially seemed to, require that the relevant alternative theoretical possibilities were never conceived of at any time or in any way by the members of a given scientific community. Even if we discover that Galton and/or one of his contemporaries caught sight for a moment of one of the possibilities we have regarded as neglected, these possibilities were nonetheless left out of account when it really mattered: that is, at the time Galton was making inferences and drawing conclusions about what the processes of inheritance and generation had to be like. And the threat posed to scientific realism by the problem of unconceived alternatives persists so long as the relevant theoretical alternatives are not considered at such crucial inferential junctures.

Of course, in the final analysis it is in no way surprising that Galton failed to conceive of all possible accounts of particulate inheritance, for scientists neither do nor claim to proceed generally by surveying all possible theories before trying to confirm one that has occurred to them against the existing alternatives. Nor is it fair to suggest that Galton somehow behaved recklessly in drawing inferences from what seemed to him, in light of the evidence, the only possible forms that such an account might take. Instead, the point is that Galton's case provides evidence in favor of the quite general challenge to scientific realism posed by the problem of unconceived alternatives: that there typically have been alternatives to even the best scientific theories of the past that remained unconceived by us at the time despite being well-supported by the available evidence, and thus that we have every reason to believe that there are similarly serious and well-confirmed unconceived alternatives to even the best scientific theories of the present day.

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² I argue that Darwin never conceived of this possibility, or indeed understood it when Galton presented it to him, in (Stanford 2006). There I develop the case for the problem of unconceived alternatives much more fully, and I consider the case of Galton's stirp theory (alongside those of Darwin's pangenesis and Weismann's theory of the germ-plasm) in substantially greater depth and detail than available space will permit me to do here.

³ Except where otherwise specified, all references in the text are to the revised version of "A Theory of Heredity" published in 1876.

⁴ Indeed, as I argue in (Stanford 2006 Ch. 3), this proposal actually offered a better explanation than pangenesis' own for those cases of the inheritance of acquired characteristics that Darwin took most seriously: those in which amputation or mutilation was accompanied by disease. Galton himself was skeptical of the inheritance of acquired characteristics, however, and assigned this mechanism an explicitly "supplementary and subordinate" (TH 330) and "unimportant" (BR 400) role in inheritance.

⁵ The closest analogue in contemporary molecular genetics to Galton's latent, dormant, or undeveloped state of inherited particulate germinal material would be a gene's failure to be "turned on" by the appropriate regulatory gene, but even this would seem to be a matter of interaction between inherited germinal units and not a difference of development between them: indeed, the action of regulatory genes constitutes one of the ways in which inherited particulate germinal materials can fail to do what they did in an ancestor without themselves being inherited in a latent state or undeveloped form at all. And of course, many other differences in the phenotypic expression of a gene are not caused by differences in the operation of such regulatory genetic machinery in any case.

⁶ This attack is expanded in the 1876 revision of Galton's paper (1876 339).

⁷ Galton explicitly extends this general analysis of hybrid color to the case of "negro blood" at NI 12/312. On this account, distinctive coloration in an organism would have to be "present very obviously or not at all" as a consequence of Galton's "principle of correlation", above: that is, a minimum proportion of gemmules producing colored physiological units would have to be inherited together or not at all. Galton describes a possible physiological mechanism for achieving such correlation (on the assumption that the bearers of coloration are physiological units smaller than individual cells) in his exchange of correspondence with Darwin.

⁸ The relatively minimal influence exerted by Galton's stirp theory might be worrying in another way, however: we might suppose that conceiving of alternatives to a given theory is something that a community of scientists is much better at doing than is any single scientist acting alone. This suggestion ought not be dismissed out of hand, but in (Stanford 2006) I discuss further cases in which we have equally strong evidence for the importance of the problem of unconceived alternatives in the case of theories that were indeed widely influential in their own day.