

Title

Constitutive elements in science beyond physics: The case of the Hardy-Weinberg principle

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Abstract

In this paper, I present a new framework supporting the claim that some elements in science play a *constitutive* function, with the aim of overcoming some limitations of Friedman's (2001) account. More precisely, I focus on what I consider to be the gradualism implicit in Friedman's interpretation of the constitutive *a priori*, that is, the fact that it seems to allow for degrees of 'constitutivity'. I tease out such gradualism by showing that the constitutive character Friedman aims to track can be captured by three features – namely, quasi-axiomaticity (QA), generative potential (GP), and empirical shielding (ES) – which are exhibited to a maximal degree by the examples Friedman deploys, particularly in his analysis of Newtonian mechanics.

I argue that not all varieties of 'constitutivity' can be captured by the kind of gradualism implicit in Friedman's view, although developing the gradualism itself might provide useful insights. To show this, I analyse the function of the Hardy-Weinberg principle (HWP) in population genetics in terms of its QA, GP, and ES. Whereas the HWP does not count as constitutive in classical philosophical interpretations (Sober 1984), nor does it within Friedman's framework, it does nonetheless perform a *minimally* constitutive function. By means of historical details and considerations on the prospects of replacing the HWP, I show that the HWP is minimally constitutive by being a counterfactual instantiation of a paradigmatically constitutive stability principle, where the latter might itself be regarded as an enabling condition for a variety of modelling practices across the sciences.

Keywords

Constitutive principles in science, Hardy-Weinberg principle, population genetics, equilibrium state, scientific theorising, Michael Friedman

1.

1.1 Introduction

Recent literature has been flourishing around the idea that some elements in science perform a special, namely 'constitutive', function. One overarching account based on this idea has been developed by Michael Friedman.[1] In his (2001), in fact, he argues that physical theories contain principles that play a peculiar role, in that they provide fundamental conceptual elements required to formulate and empirically test the theory itself. In other terms, as he explains, they establish a coordination between our mathematical representations and the concrete phenomena to be represented. At the same time, these principles cannot be themselves tested from within their own theory. Thus, they count as *a priori*, even though they can be endowed with empirical content (Friedman 2001, pp. 39-40).

Friedman's approach certainly allows for more fine-grained epistemological analyses than Quine's (1951) holism, especially as far as understanding scientific change is concerned (Stump 2015), because it accounts for fundamental epistemic asymmetries internal to physical theories which cannot be captured by Quine's notion of entrenchment. However, Friedman's position seems to adapt well only to some episodes of radical conceptual change in the history of physical theories, and his interpretation of the constitutive function seems hardly applicable to many other scientific domains, such as, for instance, that of the life sciences. This can be explained, in my view, by the fact that he seems to regard the performing of a constitutive function as an *all-or-nothing* feature while, in fact, his account already presents an implicit gradualism, albeit in a limited sense. This limitation is mainly due to his interpretation of the constitutive *a priori*, which is based on his analysis of the structure of space-time theories.

This paper aims at developing a framework in which this constitutive function is construed as a matter of degree, so that it can be extended to domains beyond mathematical physics, and therefore accommodate perspectives on the constitutive function of non-theoretical components that have emerged in the literature after Friedman. I will show the fruitfulness of my framework by focusing on a case study, that is, the epistemic role of the Hardy-Weinberg principle (HWP) in population genetics.

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[1] In this paper, I consider Friedman's view as the 'default' position in the debate, for two main reasons. Firstly, Friedman's view established itself as a major reference point in the literature on constitutive principles in science. Secondly, it develops a *general* notion of constitutive principles, which impacts on various philosophical and scientific issues, such as the role of philosophy with respect to the sciences, or the issue of conceptual change across scientific revolutions. Nonetheless, much work has been done (and is still being done) on more context-sensitive analyses of the constitutive role of some principles, particularly in modern physics, which do not appeal to Friedman's analysis (cf. Bitbol et al. 2009, Castellani 1998).

1.2 The Hardy-Weinberg principle

The HWP is a theoretical tool deployed in a wide range of modelling practices implemented by population geneticists (Gillespie 1998; Hartl and Clark 2007; Russell 2010). Its simplest formulation assumes the presence of only two variants of the same allele, dominant and recessive. It states that, in the absence of evolutionary forces (natural selection, mutation, migration), in an infinite population of sexually reproducing diploid organisms which mate randomly, the genotypic frequencies of the offspring generation will be distributed according to the following frequencies:

$$AA: p^2 \qquad\qquad Aa: 2pq \qquad\qquad aa: q^2$$

where A and a are the two allelic variants which determine the three possible genotypes (AA, Aa, and aa), whereas p and q are the allele frequencies in the parent generation.

With the establishment of the Darwinian synthesis in the 1930s, the use of the HWP became customary, in that it described the *equilibrium state* of (at least a class of) genetic populations, which is often interpreted as the conditions under which no external *force* acts on a system (Stephens 2004; Hartl and Clark 2007). Given this role, genetics textbooks and papers generally characterise the HWP as a *zero-force model*, and analogies between such role and that of the first law of motion in Newtonian mechanics are frequent (Mayo 2008; Ruse 1971; Sober 1984). Nonetheless, sceptical attitudes towards the HWP as a zero-force state, and towards the understanding of evolution as a theory of *forces* in general, have emerged (Earnshaw 2015; Matthen and Ariew 2002, 2009; Walsh 2007). In addition, some researchers have emphasised how certain assumptions of the HWP are problematic for its role of zero-force state (Li 1988; Stark 2006a, 2006b, 2007; Stark and Seneta 2013). Consequently, alternative directions have been explored, with the aim of overcoming the limitations of the HWP, by either reconceptualising the notion of equilibrium state in a dynamical, rather than a mechanical, sense (Bosco et al. 2012), or providing a characterisation of the zero-force condition for all evolutionary systems (McShea and Brandon 2010).

A prominent understanding of the HWP characterises it as an *a priori* element of population genetics, when suitably formulated (Sober 1984). My argument that the HWP performs a minimally constitutive function is mainly aimed at showing the explanatory potential of my framework for constitutive elements in contrast with Friedman's (2001). The HWP makes for a good case study because population genetics is a highly mathematised science, thus allowing for a convenient comparison with the case of Newtonian mechanics analysed by Friedman. At the same time, the HWP demonstrates the advantages of a gradual understanding of the constitutive function outside the domain of mathematical physics. Nonetheless, given that Sober's and Friedman's analyses do not overlap, since they are based on different assumptions concerning the *a priori*, this paper may be regarded as an attempt to bridge the discussion about the constitutive function of some elements and the one about the role and status of the HWP in population genetics. Therefore, I will provide grounds to claim that my interpretation of the HWP as minimally constitutive is not in principle incompatible with Sober's view on the HWP.

1.3 Outline of the paper

In section 2, I outline the core of Friedman's perspective, and focus on the main criticisms raised against his view. I also clarify in which respects I depart from his view and draw elements from others, more specifically from William Wimsatt. In section 3, I introduce the three parameters – that is, quasi-axiomaticity (QA), generative potential (GP), and empirical shielding (ES) – through which I show the gradualism implicit in Friedman's view. These features also allow me to develop a framework to understand the constitutive function as a matter of degree and context. Given such framework, in section 4 I examine the epistemic function of the HWP as minimally constitutive, in comparison with the paradigmatic case of the laws of motion in Newtonian mechanics, as analysed by Friedman (2001). In the light of such analysis, I argue that the HWP plays a constitutive function in that it works as equilibrium principle for a class of genetic populations, thus performing a different function than what Friedman focuses on, but nonetheless constitutive to a minimal extent. As part of showing that the HWP represents a minimal case, I also point to what may count as a paradigmatically constitutive component in its background. In fact, the HWP's function might be considered as one of the possible concrete instantiations of a more general and formal 'stability' principle, which might be understood as an enabling condition to implement our modelling practices across the sciences. Due to the limited scope of this paper, the point concerning the stability principle will only be presented as a tentative suggestion, in need of further scrutiny. In section 5, I summarise my conclusions.

2.

2.1 Whither *constitutive* principles?

Recently, there have been several reappraisals of the role of the *a priori* in philosophical analyses of science.[2] These reappraisals move away from the epistemological holism that emerged from Quine's (1951) famous rejection of the logical empiricists' analytic/synthetic divide, which leaves no room for any *a priori*. Still, they do not have the specific aim of defending the existence of *a priori truths* independent of experience. Rather, they usually focus on the interpretation of the *a priori*

[2] Cf., for instance, Chang (2008), Friedman (2000), Philström and Siitonen (2005), Stump (2015), and Suárez (2012).

from a *transcendental* standpoint, conceived in terms of the epistemic *function* of certain elements as enabling conditions for science.

By a transcendental stance, it is usually meant the suspension of the belief that the world discloses itself to us independently of the ways we engage with it, to focus on the *conditions of possibility* for us to investigate and represent the world scientifically. This does not necessarily entail a sharp contrast with epistemological naturalism, i.e. the view that "the study of science is itself a scientific enterprise" (Giere 1988, p. 12).[3] A transcendental stance should be conceived as the temporary bracketing of existence claims about what we normally consider as scientifically explainable, to focus on *how it is possible* that it can be explained scientifically. To answer the questions: "How is it possible that *x* can be accounted for scientifically?" and "What is required for *x* to be explained scientifically?", indeed recourse can and should be made to empirical data (cognitive, historical, sociological, etc.). However, the mode of organisation and justification of such empirical input, the emphasis on framing the *explanans* as a *condition of possibility*, and the act of 'bracketing' itself are certainly philosophical in nature. They are aspects of the investigation and reflection *on* science as object of inquiry, that allow such reflection to elevate itself at a meta-level with respect to the science they want to account for.

Given these premises, some recent views have focused on the *a priori* with respect to its *constitutive* function (Friedman 2001, Stump 2015). By 'constitutive', it is meant that some components of a certain scientific framework can count as *a priori* in virtue of the fact that they constitute the object of knowledge.[4] That is, these components provide fundamental *conceptual* structures that are required by a certain scientific framework to frame the empirical phenomena of interest and, thus, enable the problem-solving activities taking place within the framework. However, those peculiar conceptual structures are not themselves merely *derived* from experience, even though they are not necessarily devoid of empirical content. According to these authors, the constitutive function is performed by certain principles internal to the structure of at least some (physical) theories. These principles have a special status in virtue of their distinctive epistemic function within those theories, but they are not *a priori* in an absolute and fixed sense since they can be subject to replacement. In the next subsection, I will outline the key features of Michael Friedman's view, which will be my main critical target for the remaining part of section 2.

2.2 Michael Friedman's 'relativised *a priori*' and its critics

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[3] Cf. Callebaut (1993, pp. 1-5) on the varieties of naturalism and their relationship with the transcendental standpoint.

[4] Cf. the classic passage from Reichenbach (1920/1965, pp. 48-60).

According to Friedman, the problem-solving activities of science-making take place within Kuhn-like scientific paradigms or frameworks. By focusing chiefly on the history and structure of space-time theories, Friedman argues that physical theoretical frameworks are comprised of three 'levels', a mathematical, a mechanical, and an empirical one, where the mechanical part establishes a coordination between the mathematical and the empirical parts. For instance, in Newtonian mechanics, the three laws of motion are the mechanical part, in that they establish the connection between the mathematical part, supplied by the mathematics of the infinitesimal calculus, and the empirical part, which is the domain of phenomena described by the law of universal gravitation. Within the context of a theory, the mathematical part and the mechanical part are together *constitutive* of the empirical part. The mathematical and the mechanical parts are therefore *a priori*, but only in the sense that they are constitutive of the fundamental theoretical concepts of a theory, since they provide basic elements of the conceptual machinery that is required to formulate and test the law of universal gravitation. Nonetheless, these constitutive principles change during profound conceptual revolutions, which bring about a novel paradigm. The transition from Newtonian mechanics to relativistic physics is marked by a major conceptual shift, which is determined by a change in the paradigm's constitutive (or *relativised a priori*) principles. The paradigm of classical mechanics, framed by three-dimensional Euclidean space and the Newtonian laws of motion, is substituted by special relativity, where the constitutive elements are the four-dimensional geometry of Minkowski space-time and the light principle, and by general relativity, where the constitutive function is performed by the geometry of (semi-)Riemannian space-time manifolds and by the principle of equivalence.

Friedman's view has received several criticisms, which may be grouped according to the categories of internal and external critiques. While the former engages with the pitfalls and flaws of Friedman's view based on his own examples, the latter criticises his framework for its limited applicability or lack of consistency with further broadly shared considerations about science. I focus on the external critique, given that the aim of this paper is that of developing a notion of constitutive principles which may be a useful epistemic tool across scientific fields. It should be evident, even from the succinct sketch presented above, that Friedman's interpretation of the constitutive *a priori* relies on his analysis of the structure of physical theories, coupled with a Kuhnian picture of scientific development. As it is widely known, for Kuhn (1962/1970) scientific revolutions produce fundamental changes with respect to the problem-solving activities and conceptual underpinnings of a scientific field of inquiry, in that they bring about new paradigms, that is, new agreed-upon standards of investigation that come to characterise the state of 'normal' science within that field. In Friedman's view, the passage from one scientific paradigm to the following – i.e., from one set of constitutive elements to the following – happens through a series of 'natural transformations'. That is, when a new framework supersedes the previous one, the latter maintains its validity only as limiting case (Friedman 2001, p. 63). Indeed, this analysis does not seem to do justice to the actual state of most science, given that such natural transformations seem quite rare in the history of the sciences, and are often more chaotic than allowed by Friedman's framework. What is more, the coexistence and sometimes the competition of a plurality of frameworks in a field seems a rather common feature across scientific fields.[5] In addition, Friedman's 'purified' understanding of Kuhnian paradigms as uniquely composed of *relativised a priori* elements, that is, as being purely conceptual in nature, rests on a strong idealisation of Kuhn's view, leaving out crucial aspects of scientific practice in constituting a paradigm, to the advantage of an overly theory-centric perspective (Mormann 2012). Finally, critics have focused on another assumption made by Friedman. His interpretation of certain principles as constituting empirical laws in space-time theories seems to entail that these principles should count as fundamental assumptions for *all* empirical inquiries across all sciences (Chang 2008, p. 116), while this is far from being uncontroversial.[6]

2.3 Beyond Friedman's interpretation of the constitutive *a priori*

The criticisms I briefly rehearsed in the previous subsection press Friedman's perspective with respect to its bias towards mathematical physics and to its theory-centrism, given that it assumes that science is a collection of theories superseding one another, thus downplaying the role of scientific practice. These limitations can be better understood – I believe – by taking a closer look at Friedman's interpretation of the constitutive *a priori*, and at its own limits.

Friedman embraces a *material* interpretation of the *a priori*, according to which those elements that perform a constitutive function, and thus count as *a priori*, can be endowed with empirical content (such as in the example of the Newtonian laws of motion). These principles, according to Friedman, are components that contribute in making up the structure of the physical theory under scrutiny. They are constitutive in that they provide fundamental concepts and the conditions of

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[5] This is certainly the case for the life sciences, where synchronic epistemic pluralism is the norm (Leonelli 2009), but also in electrochemistry and atomic chemistry (Chang 2012). Even in the case of space-time theories, it is often noted that Newtonian mechanics is still commonly taught and used for a wide variety of purposes, and that scientists often work across frameworks. A common example is that of GIS systems, which combine elements from Newtonian mechanics, relativistic physics, and quantum physics (cf. Chang (2012) and Winther (2015)).

[6] Cf., for instance, Stump's (2015) criticism of Friedman on this point, based on Norton's (2010, 2014) account of induction. Rather than in very broad assumptions on nature in general, scientific inquiries are usually grounded in specific presuppositions (cf. also Padovani 2017). In the words of Love (2013, p. 335): "Changes in concepts due to empirical advances are localized and therefore tend to exhibit distinctly regional affects in scientific theorizing, and empirical testing, because tests are localized to a particular MIS [material inferential structure] such that positive or negative results do not echo into every other theory structure".

testability of other empirical statements, while they cannot be themselves empirically tested from within their own framework. Therefore, these principles constitute, that is, conceptually determine, the first-order empirical laws, such as the law of universal gravitation, where 'first-order' refers to those lawful empirical generalisations which directly structure the concrete empirical phenomena.

It is worth highlighting that Friedman's material interpretation of the constitutive *a priori* is not the only one available. The constitutive *a priori* can also be subject to a *formal* interpretation, according to which the *a priori* is completely devoid of content, but still performs a constitutive function,[7] and it can be understood in *pragmatic* terms, thus not changing in function of the growth of scientific knowledge, but of pragmatic choices.[8] In addition, Friedman's material interpretation in terms of theory-relative principles, which determine conceptually certain lawful empirical generalisations, can only account for one among the many 'phenomena' in science where a constitutive function may be at play, i.e. how lawful empirical generalisations expressed in mathematical terms can say something about the world of concrete phenomena.[9]

As emphasised by the label 'first-order' applied to empirical laws, Friedman's material interpretation of the *a priori* is supported by his analysis of the structure of physical theories in three levels (mathematical, mechanical, empirical). This analysis allows him to accommodate the material interpretation of constitutive *a priori* elements as working between the level of mathematical structures and that of concrete phenomena. At the same time, it solves the problem of coordination i.e. how to coordinate abstract mathematical structures empty of content with empirical phenomena, so that the former can describe the latter. The coordination is established by the constitutive elements at the mechanical level.

As pointed out by Jonathan Tsou (2010, footnote 5) Friedman's distinction of three levels internal to physical theories seems to allow for a distinction between different degrees of apriority or *constitutivity*. The mathematical principles are, in fact, *a priori tout court*, in that they cannot be directly tested or disconfirmed in the light of experience. The mechanical (coordinating) principles are, in a sense, *less a priori* than the mathematical ones, in that they partake of empirical content. Following the same direction, the infinitesimal calculus seems 'more constitutive' than the laws of motion, since it provides the concepts to express the laws of motions themselves, while both the calculus and the laws of motion are required to formulate and test the law of universal gravitation.

[7] Examples can be found in Buzzoni (2013) and Stuart (2017).
[8] Chang (2008, 2009) and Stump (2015) go in this direction.
[9] This is the so-called 'problem of coordination'. Cf. Van Fraassen (2008, chap. 3).

This gradualism, however, is not spelled out explicitly by Friedman. Indeed, the gradualism resulting from tying the interpretation of the constitutive *a priori* to a specific analysis of the structure of physical theories might be hard to defend in other contexts. In fact, this gradualism might at least take different forms in different scientific frameworks, which are not well-described by the three-level structure Friedman identifies in space-time theories.

Furthermore, it has been argued that there are components of science-making that play a constitutive function but are not to be searched at the level of theories, as in the case of measurement procedures (Van Fraassen 2008). Consequently, even the issue of coordinating our mathematics with empirical phenomena might not be solved only by means of the theoretical principles Friedman allows for (Padovani 2015, 2017), even if we embrace the three-level analysis.

Overall, Friedman's material interpretation within the context of space-time theories allows for a gradualism that, plausibly, may be peculiar only to highly mathematised physical sciences, while its capacity of solving the issue of coordination is questionable. Indeed, his view on the constitutive *a priori* has been criticised even with respect to some of his own examples in space-time physics.[10] Still, my aim here is not that of defending Friedman's view, but rather using it as a springboard for further reflection on the very notion of constitutive principles. The possibility of an analysis of the constitutive function with respect to scientific practice or fields other than mathematical physics finds from the very start a limited tool in Friedman's interpretation and in his understanding of 'constitutive' merely as 'constitutive of first-order empirical (physical) laws'.

Given these considerations, I believe that resources can be found to elaborate an account of constitutive elements that, in the first place, acknowledges and pushes the boundaries of the gradualism implicit in Friedman. In the next section, I will provide a framework that, on the one hand, makes evident and develops the gradualism of Friedman's view while, on the other hand, it allows for accommodating constitutive elements different from those Friedman focuses on, by decoupling the gradualism from his analysis of the structure of physical theories. To do this, I will first introduce two notions I borrow from William Wimsatt's account of scientific change, which will provide some crucial underpinnings of my framework.

Wimsatt (1987, 1999, 2007) developed a model of scientific change that could also replace the analytic/synthetic distinction. He established an analogy between evolving systems in the realm of biology and science as characterised by an organic development. Within this development, certain

[10] These criticisms are what I labelled 'internal' critique in section 2.2. Howard (2010) and Pitts (2017) convincingly argue against Friedman's characterisation of the equivalence principle as constitutively *a priori* within the general theory of relativity. I thank an anonymous referee for directing me to their criticisms. See also Everett (2015).

generative systems have a role in providing developmental constraints along the path of scientific progress. Some elements are characterised by a high degree of *generative entrenchment*, which is proportional to the number of downstream features which depend upon them (Wimsatt 1987). When applied to the analysis of scientific knowledge, this notion of entrenchment is not identical to that presupposed by Quine's holistic web, because in Wimsatt's entrenchment picture the organisation and interactions between scientific theories and disciplines are characterised nonetheless by *quasi-independence*, which is essential for evolution to proceed at a reasonable rate. This means, each scientific 'niche' preserves a significant degree of autonomy from the development of other disciplines, fields, and theories. Without this independence, scientific theories, sub-disciplines, etc. would have to bear too many constraints to evolve as fast as they do. The notion of 'generativity', albeit decoupled by the feature of entrenchment, and that of 'quasi-independence' will be key features of the framework I develop in the next section.

3.

3.1 Towards a new framework for understanding the constitutive function

The aim of this section is that of developing a framework that accounts for the fact that a constitutive function may be performed by different epistemic elements in numerous ways. As a first step towards my own framework, I develop the gradualism already implicit in Friedman's view by starting with a rather broad understanding of the constitutive function and describing its most salient features. Then, in section 4, I turn to a case study, to show how the gradualism can help making sense of the constitutive function beyond the limits set by Friedman's interpretation.

Here is my general understanding of the constitutive function. Depending on the scientific framework of inquiry under scrutiny, some elements of it are constitutive in that they are preconditions that enable the epistemic activities required by the framework to operate, with respect to their capacity to determine conceptual categories that allow us to distinguish between relevant and irrelevant patterns of regularities of phenomena. In other words, these elements play a crucial role in the development of fundamental conceptual tools by which a certain scientific framework works, and which, at the same time, constrain the ways in which that scientific framework characterises conceptually its object of inquiry. Rather than being features merely inferred from empirical reality, such elements are better characterised as features of our epistemic frameworks that enable our scientific representation of – and engagement with – reality. This characterisation might include instances of several types of epistemic components, at different levels of generality (e.g. theoretical principles, measurement procedures, scientific instruments, etc.). In fact, these elements may perform distinct *specific* functions, and even be hierarchically organised. A powerful framework should be able to allow for the specific differences between them, but it is by adding contextualised and historically-situated analyses of specific cases that such differences should be accounted for.

3.2 Central features of the framework

The typical features that I take constitutive elements to share, also on the grounds of both Friedman's and Wimsatt's accounts, are the following:

Quasi-axiomaticity (QA): certain components of scientific frameworks show a feature which is akin to that exhibited by conventions or analytic truths, in that they introduce standards and constraints for describing and investigating a domain by means of specific concepts (or procedures). They are held constant, while allowing the other variables that comprise the framework (often, but not always, a theory) to be tested or changed. Their quasi-axiomaticity depends on the extent to which they are fixed elements within a framework of inquiry, the prefix 'quasi' denoting the absence of a strict axiomaticity requirement, i.e. the requirement that such elements should be rigorously and explicitly defined or standardised.[11]

Generative potential (GP): Generative potential captures the amount of dependence-relations that certain elements can support with respect to other components of their scientific framework. Since constitutive elements allow the emergence of fundamental *conceptual* resources for investigating and representing an empirical domain, more domain-specific components (e.g. empirical laws, empirical models, models of data, explanations, complex observational protocols etc.) will themselves partially depend on such elements. This means, the most paradigmatic cases of constitutive elements will present a high degree of GP, in that they support a large amount of dependencies which ground other conceptual resources employed in a framework of inquiry and allow the emergence of a large enough conceptual space to generate further empirical developments (hence the term 'potential').[12]

Empirical shielding (ES): On the one hand, the extent to which some element can be discarded and replaced, given the empirical evidence which can be obtained from within its framework, is in

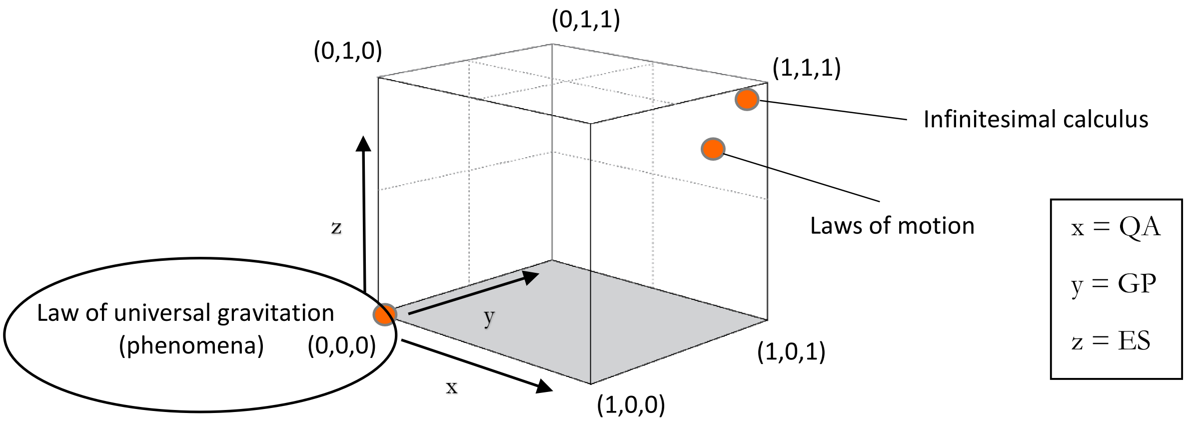
[11] Despite the lack of the requirement of strict axiomaticity, one of the primary tools that allows fixing some element certainly is formalisation. Formalised languages (especially, but not exclusively, mathematics) and procedures, which extend beyond the mere use of symbols to, e.g., graphic representation, or standardisation of measurement procedures and tools, allow the fixation of such elements.

[12] In the case of constitutive elements of propositional nature, these dependence-relations do not hold in virtue of the *truth* of these elements since this would already presuppose a one-to-one correspondence between such conceptual elements and the respective (classes of) objects of empirical inquiry.

part dependent, at least in the case of propositional elements, on the amount of empirical detail characterising them and, therefore, to their degree of abstraction. In the case of theories, once some empirical generalisation, or representation of phenomena, has been provided a general formulation, particularly in the case of highly symbolic ones, such formulation, rather than being subject to empirical disconfirmation, might be better understood as inapplicable to certain types of phenomena, while applicable to others. On the other hand, the generality of the framework of inquiry in which they are embedded, be it a paradigm, a theory, an experimental system, etc., has an impact on the possibility to empirically test them.[13] The broader the framework, the more difficult will it be to empirically test its constitutive principles. Testability is, therefore, inversely proportional to the degree of generality of the framework of inquiry, and to the degree of abstraction that 'shields' the element from being empirically tested. I call the combination of such factors the degree of *empirical shielding* of an element, that is, the extent to which some theoretical element *cannot* be empirically tested from within the framework of inquiry in which it is embedded.[14]

3.3 Reassessing Friedman's view and its limitations: the example of Newtonian mechanics

Now that my main analytic tools have been introduced, I can show how Friedman's interpretation of the constitutive *a priori* can be deconstructed, to highlight the gradualism underlying his framework. Fig. 1 provides an attempt to represent graphically the paradigmatic example of Newtonian mechanics, quickly introduced in section 2, in terms of my own parameters.



[13] Certainly, other contextual and contingent factors may limit the possibility of testing certain components of a framework (e.g. limited technological advancements and further practical constraints).
[14] Mathematical and logical principles – one could say - are devoid of empirical content and, therefore, should be applicable in every domain or framework. However, as Wimsatt (1987, p. 25) points out, even in the case of mathematical and logical theorems "there seem to be antecedent conditions which must be met for the relevant theorems to apply. Where these antecedent conditions are explicitly specified, they are often regarded as a part of the apparatus, [...] which does not apply everywhere". In my own terminology, albeit they have high degrees of ES, many mathematical and logical principles may have limited degrees of GP, thus lacking a crucial feature characterising the constitutive function.

fig. 1: Graphic representation of the components of Friedman's (2001) analysis of Newtonian mechanics in terms of quasi-axiomaticity (QA), generative potential (GP), and empirical shielding (ES). In a circle, the domain of phenomena represented by the law of universal gravitation.

Once we frame the domain of phenomena represented by the law of universal gravitation as our *explanandum*, by attributing to it value 0 for all our parameters, we have that the values of QA, GP, and ES for those elements that are considered 'constitutive' of the law of universal gravitation in Friedman's analysis increase.[15] It must be highlighted that the laws of motion are less shielded than the calculus, since a mathematical *language* cannot be disconfirmed in the same way as empirical laws, such as by means of experimental procedures directly appealing to empirical facts.[16] They also have less GP, because they are themselves formulated by means of conceptual structures made available by the calculus, as I pointed out in section 2.2. As a result, the calculus might be said to be 'more constitutive' than the laws of motion within the context of Newtonian mechanics.

Some clarifications about the role of my three parameters and the relationships between them are due. GP is certainly the most salient feature, since it captures the core of the constitutive function in the general sense I outlined, that is, the capacity of some elements to generate further, more specific conceptual tools for empirical inquiry and thus, at the same time, to constrain the workings of a scientific framework. According to Friedman, as in most of both traditional and contemporary interpretations, these elements are themselves conceptual structures, and they are often related to the workings of our scientific *theories*, but this need not be the only case, as I mentioned earlier. What is important here is that, by following Friedman's interpretation, only those elements count as constitutive, which approximate a maximal degree of GP within a framework. This requirement is motivated by Friedman's attempt to establish a universal and sharp dichotomy between constitutive and non-constitutive components, but, in my view, it is neither a necessary nor a desirable feature of a general account of the constitutive function. What is more, Friedman's relativised *a priori* show degrees of QA and ES which also tend to the maximum. QA guarantees the fixity of the principles, that is the constancy of their role with respect to the constituted element.

[15] The expression 'domain of phenomena' already refers to a *representation* of these phenomena, in this case by means of the law of universal gravitation. I must stress that the relationship between the law of gravitation, the laws of motion and the calculus is partially determined by the framing of the domain of phenomena captured by the law of gravitation as our explanandum. Thus, the 'distance' between the law of gravitation (phenomena) and the laws of motion appears as larger than what it might look like in Friedman's three-level account of theories, because we have framed the former as explanandum by attributing to it, somehow artificially, the values (0,0,0), that is, by assuming that they are not constitutive with respect to anything else, which is, of course, an idealisation. The latter move, which is essential to develop the gradual account, implies that we could frame a different epistemic component as our explanandum, even within the same theory. I thank an anonymous referee for asking me to clarify this point.

[16] This does not mean that a mathematical language cannot be empirically tested *at all*. As rightly emphasised by an anonymous reviewer, the expressive adequacy of a mathematical language can be evaluated based on its capacity to express an empirically adequate theory.

In the example of Newtonian mechanics, for instance, we have that the laws of motion are *always* presupposed when the law of universal gravitation is deployed (e.g. for providing explanations, making predictions, etc.) or tested. Finally, ES is the measure that captures the degree of shielding of some elements from empirical testing and disconfirmation. As I already pointed out, the calculus approximates a maximal ES, in that it is a mathematical language, while the laws of motion have high ES, since they cannot be tested from within the framework of classical mechanics, but they are, in a sense, closer to empirical phenomena than the calculus. As it appears, Friedman's material interpretation of the constitutive *a priori*, modelled on his analysis of the structure of space-time theories on the one hand, requires maximal or close to maximal degrees of QA, GP, and ES. On the other, it requires a gradualism, which is nonetheless constrained by his three-level interpretation of the structure of physical theories.

I believe that Friedman's view does not have the resources to accommodate other varieties of constitutive elements, but that decoupling the gradualism from his specific analysis of the structure of scientific theories might provide useful insights. Indeed, certain elements which perform a constitutive function might show non-maximal degrees of some parameters. For instance, while Friedman's view appears to require constitutive elements to approach a maximal degree of QA, this does not seem to be required in the historical process by which measurement procedures come to constitute the quantifiable theoretical terms used in most physical theories.[17] Allowing for degrees also in the case of the other features, ES and GP, could make it possible to analyse under the same framework elements that exhibit a constitutive function, albeit not to the same extent, and of the same kind, of those Friedman focuses on. Nonetheless, a relatively *high* GP will plausibly be a central feature of all elements exhibiting a significant constitutive character, while a very *low* ES would hardly be a feature of highly constitutive elements. In fact, the possibility of a certain element to be tested empirically in a variety of ways would be in contrast with it being a feature that depends significantly on the scientific framework under analysis, rather than it simply being a feature dependent on a real system.

In general, the gradual framework is meant to provide a tool for comparing the constitutive function of different elements within scientific frameworks.[18] The difficulties of being precise about the measure of the parameters, also given the different sorts of constitutive function they perform,

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[17] Padovani, personal communication.

[18] The possibility of making meaningful comparisons *across* scientific frameworks introduces many complications. For one, it would require a sufficient degree of similarity between the explananda of the different frameworks, e.g. in the case of theories dealing with (roughly) the same domain, or when both explananda have a similar form (e.g. when both are laws expressed in mathematical form). I thank an anonymous reviewer for helping me see this point.

increase the importance of case studies to demonstrate the fruitfulness of a gradual understanding of the constitutive function based on QA, GP, and ES, as characteristics which come in degrees and are context-sensitive. My gradual framework is not supposed to account just for what might be taken as paradigmatic cases, like that of Newtonian mechanics as analysed by Friedman. Given a certain scientific framework, there will be some elements which, for any such framework, exhibit different degrees of ES, QA, and GP, and, therefore, their characterisation as 'constitutive' will apply to a certain extent.

Indeed, the gradualism of the general framework I suggest does allow for a differentiation from Quine's entrenchment picture. Cases of, for instance, well-established empirical constants, such as Avogadro's number – which would have to count as highly entrenched from the point of view of Quine's web-of-belief, since their robustness has been tested in many different ways – would hardly count as constitutive, given their very low ES.[19] Finally, the framework I suggest diverges from Wimsatt's view, because it does not require the character of generativity to strictly correlate with the persistence, along the historical development of science, of the elements playing a constitutive function. Thus, it can account also for cases of rather radical scientific change, which are instead well-accounted by Friedman's view.

4.

4.1 The HWP as a case study

As I highlighted in the introduction, the HWP is usually understood as a zero-force model capturing the equilibrium state of genetic populations. In actual practice, the HWP – and its more specific variants – provides the proportions to which allele frequencies in real populations (obtained, for instance, via 'gene counting') are tested against by means of statistical methods, then allowing to formulate hypotheses to explain the extent and reason of any departure from Hardy-Weinberg proportions. It is for this reason that the HWP is often addressed as a zero-force model, which determines the equilibrium state of a genetic pool holding under certain conditions. By adding further conditions to the general formulation of the HWP, i.e. by introducing parameters through which, for example, rate and frequency of mutation in a real population can be modelled, more realistic models can be developed to allow more precise investigations.

A classic philosophical interpretation claims that the HWP, when suitably formulated, is *a priori* (Sober 1984). According to this view, the nature of zero-force model of the HWP entails that it is

[19] Tsou (2010) makes the case for Avogadro's law being entrenched in the Quinean sense, but not constitutive.

devoid of empirical content and, thus, that it cannot be empirically refuted. This interpretation supports the view that there are laws in biology, even if they are not empirical laws (Elgin 2003). In this section, I am not contributing to the debate on the nature of biological laws, nor to the one on the legitimacy of an empirical requirement for scientific laws. Rather, my aim is that of assessing the epistemic status and function of the HWP in the historically-situated development of population genetics, and according to the fact that it provides fundamental conceptual resources for the current workings of population genetics. Considering my characterisation of the constitutive function in section 3.1, there is no risk of it collapsing on the notion of *a priori* in the purely definitional sense endorsed by Sober. In the last subsection I will tease out why my analysis is compatible with Sober's.

Another clarification is required, concerning my comparison between the case of Newtonian mechanics, as analysed by Friedman and reinterpreted within my framework, and my analysis of the HWP in population genetics. This comparison is only a tool to emphasise the similarities and differences in the epistemic role of certain components within these two frameworks. It is not meant to support or refute the claim that certain laws *qua* laws are empirical, on the basis, for instance, of the analogy between the law of inertia and the HWP as zero-force models. What I would like to show is, on the one hand, that *if* we take the case of Newtonian mechanics as a paradigmatic term of comparison, the HWP will count as a minimal case of constitutive element. On the other, I will claim that the constitutive function performed by the HWP is of a different sort compared to the one Friedman focuses on. I will defend these two claims against the fact that the HWP could not count as constitutive in Friedman's view, in that it does not constitute any 'first-order' empirical law by coordinating abstract mathematical representations with empirical phenomena. Nor would it count as constitutive simply by embracing its *a priori* status, given that, although it is foundational for population genetics, several parts of population genetics do not require the HWP for their formulation and testing. As I will argue, it is in virtue of the gradual and contextual interpretation of the constitutive function I defend, that the HWP can be said to perform such a function to a minimal extent.

4.2 A cognitive-historical analysis of the epistemic function of the HWP

As historical accounts of the HWP usually go, its explicit formulation came about as a rather simple mathematical derivation from the Mendelian scheme (Diaconis 2002, Edwards 2008).[20] During a lecture delivered by R. C. Punnett on "Mendelism in relation to disease" to the Royal Society of Medicine in 1908 in London, G. U. Yule pointed out that, if brachydactyly is a dominant character

[20] See also Provine (1971, pp. 131-136).

in man, by assuming random mating we should expect a proportion of 3:1 brachydactyly:normal, which was not what the evidence showed. Yule seemed to assume, as he did already in a 1902 paper, that the frequency of the two characters *A* and *a* in a population was of one half, inevitably leading to the 3:1 proportion in the offspring generation.[21] In the light of such remark, Punnett asked his Cambridge colleague, the mathematician G. H. Hardy, whether there was any intrinsic reason to expect a dominant character to continually increase in frequency over time, to the detriment of the recessive. Hardy (1908) demonstrated that, assuming a large population of randomly mating individuals, if the parental genotypic proportions for the three genotypes *AA*, *Aa*, *aa* were p:2q:r respectively, then they would be $(p + q)^2 : 2(p + q)(q + r) : (q + r)^2$ among the offspring.[22] He highlighted that the condition for the distribution to remain the same from parent to offspring generation was that $q^2=pr$, and that, since such relation between q, p, and r remains stable independently of the values assumed by p, q and r, the distribution will be stable after the second generation, provided no changes in the conditions occur. The German physician Wilhelm Weinberg independently obtained the same result, although his work went largely unnoticed until 1943.[23]

4.2.1 Quasi-axiomaticity

Even though neither Hardy nor Weinberg regarded the formulation of the principle as a significant contribution (Crow 1999), and at first its importance was not univocally acknowledged,[24] the HWP quickly gained a crucial role for the work of the founding fathers of population genetics and the modern synthesis. The use of Hardy-Weinberg proportions as equilibrium principle was clearly a central feature of the early work of those scientists who conjugated Mendelism with Darwinism

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[21] Ironically, by demonstrating that under the condition of random mating and with the assumption of complete dominance, the offspring generation of hybrid parents would preserve the proportions of 3:1 dominants:recessives, Yule had himself reached the first, albeit restricted, formulation of the Hardy-Weinberg principle (Edwards 2008). Nonetheless, he did not examine any further such result in that 1902 paper, nor the assumption of random mating which he himself introduced.

[22] Hardy assumed *a* and *A* as the only allelic variants in the idealised parent population. This assumption, I believe, might motivate the lack of an explicit statement of the normalisation constraint $p+2q+r=1$, which imposes that the ratios of the genotypes present in the population add up to one (i.e. the totality of individuals in the population). This consideration is certainly worth further scrutiny, with respect to the epistemic function and status of this normalisation constraint. I thank an anonymous referee for directing my attention to this point.

[23] For an appreciation of the value of Weinberg's contributions beyond his derivation of the HWP, see Crow (1999). Edwards (2008) highlights that Pearson (1904) is sometimes mentioned as the first paper which showed the Hardy-Weinberg proportions for the special case of gene frequency of one half. In 1909, eventually Pearson explicitly derived the HWP and referred it to the work of Hardy. Castle (1903) is also often indicated as a precursor of the Hardy-Weinberg principle because, as Provine (1971, p. 133) points out, he expressed in non-mathematical terms the equivalent of the equilibrium principle for a single locus with two alleles (see Edwards 2008, p. 1149 for an alternative view on Castle's contribution).

[24] For instance, in Bateson's (1909) account of Mendelism there is no recognition of Hardy's law (see Edwards 2008: p. 1147).

and with the statistical 'style' introduced by the biometricians. In what follows, I will briefly rehearse some well-known pieces of historical evidence, in support of the claim that the HWP played a crucial epistemic role during the phase in which population genetics was brought to full life.

In the first place, Fisher (1918) devised a statistical method to account for those characteristics in which many Mendelian factors contribute to the phenotypic outcome. Despite his use of biometrical results, he rejected some fundamental assumptions made by Pearson (1904). He ignored Pearson's rejection of Mendelian dominance, and assumed that, in the combination of a large number of factors, the heterozygote could assume any value between the recessive and the dominant (thus rejecting complete dominance). As Mayo (2008, p. 252) highlights, in the 1918 paper, the HWP "was the basis for Fisher's derivation of correlations between related individuals under Mendelian inheritance". Even though he did not reference Hardy, also in his 1922 paper: *On the dominance ratio*, Fisher assumed that the distribution of the frequency ratio for different Mendelian factors could be determined from the stability of the distribution in the absence of selection, random survival effects, etc..[25] The development of Fisher's concept of 'balanced polymorphism' also required the assumption of the equilibrium proportions (Mayo 2008). In the second place, already in his early studies on the inheritance of coat colour in mammals, Wright (1917, 1918) assumed the HWP as equilibrium principle – even though he was not aware, at that time, of the work of Hardy and Weinberg – because such theoretical tool allowed him to formulate the appropriate genetical hypotheses about the genotypic distributions in populations. As noticed by Provine, such assumption should have seemed rather self-evident to Wright, since he referred to it as to "the well known formula for a Mendelian population in equilibrium" (Wright 1917, p. 522, as cited in Provine 1971, p. 156). Thirdly, Haldane (1924) used the HWP as the basis for all his important early work on selection in populations. He derived the recurrence relationship for the approach to equilibrium of gene and genotypic frequencies of an X-linked diallelic gene. Finally, Dobzhansky (1998) points out that Hardy's derivation played a fundamental function not only in the Anglo-American context, but also in the population genetics of the former Soviet Union, with specific reference to the work of Chetverikov.

In the light of these considerations, it seems fair to say that the HWP was held 'fixed' across different empirical inquiries and theoretical developments during the early days of population genetics, as the equilibrium state for genetic populations. Such fixity allowed the framing of the relevant questions to be asked and the testing of other theoretical components under development, especially in the absence of a common framework of inquiry shared by the scientists. All these

[25] See Morrison (2007, pp. 320-329) for a synthetic but comprehensive account of the trajectory that Fisher followed from the 1918 paper to the 1922 paper.

elements indicate a high degree of quasi-axiomaticity of the HWP already in the early days of population genetics, which further consolidated with its establishment as a discipline.

4.2.2 Generative potential

At a first glance, the HWP may appear as a sheer consequence of Mendel's laws of inheritance. Punnett's question to Hardy already encapsulates the connection of the HWP to Mendel's laws: why is it the case that the dominants do not eliminate the recessives in the long run? According to Edwards (2008, p. 1144):

> The answer is immediate from Mendel's first law. Segregation is independent of the segregants. Dominance has nothing to do with it […] it is an immediate consequence of Mendel's law of segregation that the expected frequencies of the genes among the offspring of two parents are equal to the frequencies of those genes in the parents themselves.

Nonetheless, the HWP had a significant impact, such that the occurrence of its first explicit formulation is often made to coincide with the origins of population genetics (Crow 1988). The HWP clarified that segregation is independent of dominance and, thus, that it has nothing to do with the phenotypic expression of the genotype. In addition, the innovation brought about by the HWP consisted also in generalising Mendel's experimental results, showing their independence from any assumption of a specific initial distribution of gametes: "While Mendel conceived the independent binomial sampling of gametes from parents and hence could be regarded as the first to have considered a population–genetical example […] the generalization to arbitrary gene frequencies to give HWE was the true foundation of population genetics" (Mayo 2008, p. 254). Rather than being just an 'empty law' or a mathematical derivation which greatly simplified the mathematical treatment of the distribution of genetic variability, the history of the HWP shows that it performed a distinctive epistemic role. It demonstrated something about the preservation of genetic variability in a population, that is, the fact that there is no intrinsic tendency for genetic variation (in its simplest form, of the three different genotypes, one heterozygote and two homozygotes) to disappear in the long run (Morrison 2007).

Still, it might be argued that its epistemic role might be easily characterised as that of a definition resulting from a mathematical demonstration and, as such, *a priori*, rather than attributing any conceptual generativity to the HWP. In addition, the HWP explicated certain relationships between assumptions of the Mendelian explanatory scheme, explication which was *historically* functional to enhance the development of population genetics, while the conceptual burden remained on Mendel's laws. Furthermore, it might be added that the HWP does not have the role of providing a conceptual determination of an empirical law, as in the case of the three laws of motion with respect to the law of gravitation.

Certainly, the HWP is *not* an empirical law or generalisation. However, it might be seen as functioning like a "contrafactual ideal situation" (Rheinberger 2013, p. 488), where any deviation from the ratios it expresses points to hidden interactions in need of explanation. In other words, being an instance of counterfactual reasoning, the HWP posits an 'impossible' world or scenario to which real situations can be tested against. At the same time, such counterfactual situation represents a *(re)conceptualisation* of certain relationships between Mendelian assumptions under certain conditions as capturing the equilibrium state of (diploid and sexually mating) genetic populations *in general*. In this sense, the HWP came to represent the *enabling condition* to establish which real situations need an explanation, in that they deviate from that ideal situation conceptually determined as equilibrium state. As an upshot, it might be said that the HWP has a fairly high GP, but it does not approach a maximal degree, given that its generativity within the domain of population genetics is restricted only to its character of equilibrium principle, which does not apply to all genetic populations.

4.2.3 Empirical shielding

I highlighted in the previous subsection that the HWP is not an empirical law or generalisation. As it is evident from the formulation of the HWP provided in the introduction, certain explicit assumptions enter the definition of the HWP, and determine its domain of application. Indeed, the conditions under which a Hardy-Weinberg distribution holds are extremely rare in nature, and Hardy-Weinberg proportions can indeed hold even under the influence of evolutionary causes (e.g. when mutation and selection compensate one another), thus the binomial distribution it establishes under its explicit conditions cannot be tested directly. This is at least in part a consequence of the idealised nature of the HWP. For instance, the requirement that the population be infinite for Hardy-Weinberg proportions to hold is motivated by the necessity to rule out statistical error, which approximates zero the more a population tends to infinity.[26] Clearly, simplifying assumptions such as that of an infinite population are unrealistic, in that they posit 'impossible' worlds in which certain conditions might hold. Still, the gain in simplicity brought by the HWP as a theoretical tool is greatly advantageous from a pragmatic point of view: "[…] such models are useful because they strip a process to its essence and allow scientists to test particular attributes of a system in isolation" (Russell 2010, p. 604). Formulating hypotheses about the evolutionary causes determining the

[26] In actual scientific practice, this requirement holds looser because statistical error is considered irrelevant for very large populations (usually $n > 500$).

different patches of genetic variation within and between populations seems to require the assumption of a state under which no such forces are active. However, additional assumptions beyond the absence of forces are also required, on the one hand, to model such equilibrium state according to the specific domain under investigation (for instance, assuming infinitely large populations reduces the risk of sampling error); on the other hand, to test for departures from equilibrium proportions (for instance, it is assumed that small departures from the assumptions lead to small departures from Hardy-Weinberg equilibrium, which is not trivial).[27] In sum, these considerations indicate that the HWP has a high degree of empirical shielding.

4.3 Discussion: The HWP as a minimal case?

In the light of the analysis I provided in the last three sections, the HWP's epistemic function within population genetics could be indicatively represented in terms of its ES, QA, and GP, as depicted in fig. 2:

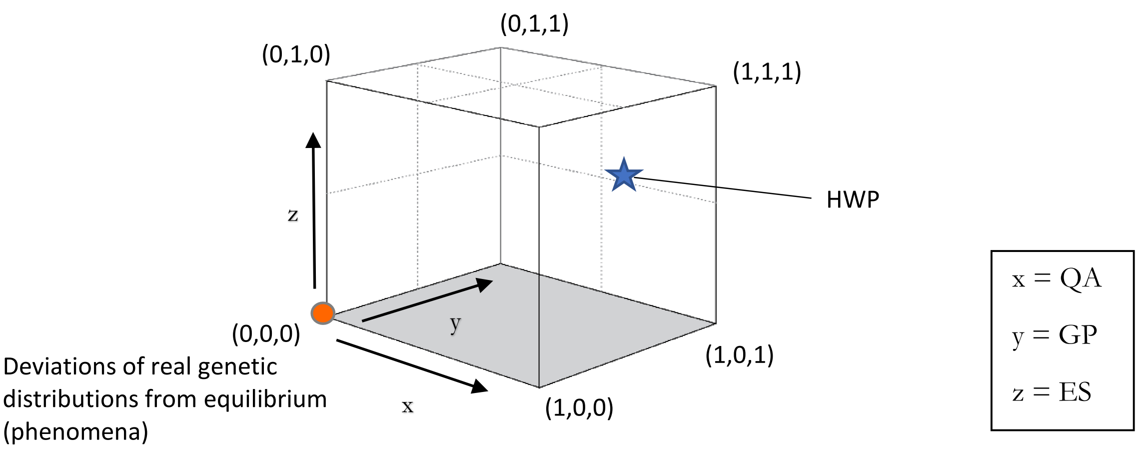

fig. 2: Graphic representation of the approximate position of the HWP within population genetics with respect to its epistemic function described in terms of empirical shielding (ES), quasi-axiomaticity (QA), and generative potential (GP).

The graphic representation in fig. 2 certainly does not aim at capturing the structure of population genetics. Indeed, it leaves out many theoretical and conceptual components relevant to population genetics, for instance Mendel's laws and the statistical tools deployed by population geneticists. My aim is merely to display what I take to be the constituted element, i.e. those real distributions considered to deviate from equilibrium conditions, together with what I claim to be an element which has a role in constituting them, that is, the HWP. Of course, a real distribution is *not* constituted in and of itself, as it is obtained, for instance, through gene counting. But its

[27] The latter assumption is an instance of what Van Fraassen (2008: p. 52) labels 'the Principle of Approximation' and defines as follows: "If certain conditions follow from the ideal case, then approximately those conditions will follow from an approximation to the ideal case."

characterisation as a deviation from equilibrium, and the outcomes of the explanatory practices implemented to account for such a deviation, are determined by what we take to be the equilibrium condition.

The parameters characterising the HWP might seem to show certain differences and similarities with what I took as the paradigmatic case in section 3, that is the laws of motion in Newtonian mechanics. Of course, this comparison can only be gestured at, given the problems intrinsic to inter-framework comparisons, and must be taken only as a tool to exhibit how certain differences in intra-framework epistemic function and history of certain principles result in certain degrees of QA, GP, and ES, and in a certain position in their respective cubes.[28] Their position with respect to their QA is similar, since both are held fixed within their framework, to fulfil the epistemic functions under analysis, i.e. that of providing conceptual tools and testability conditions for the law of universal gravitation in the case of the laws of motion; that of providing the equilibrium state against which real genetic distributions are compared, in the case of the HWP. In terms of their position in the cube with respect to their GP, the HWP seems to have a lower position, compared to the laws of motion. As highlighted above, the GP of the HWP is tied to its role of providing equilibrium conditions for a certain class of phenomena. It does so by being an instance of counterfactual reasoning, in that it provides a counterfactual ideal situation against which real distributions are tested. Finally, another similarity is that they both seem to have high ES, but not approximating the maximal degree. Why, in the HWP case, this cannot simply be explained away by its interpretation as *a priori*, in Sober's sense? Even though the HWP may be definitionally *a priori*, this does not make it empirically shielded at the same level of, say, a mathematical language or a generative grammar, in that it still has the purpose of maintaining some connection with empirical phenomena, even if it is highly shielded itself.

These considerations about the GP and ES of the HWP can, I believe, receive support by some recent literature. Some developments among both biologists and philosophers have shown the presence of plausible alternatives to the HWP (Bosco et al. 2012; McShea and Brandon 2010). Certainly, the presence of empirical irregularities can constitute a reason to promote such alternatives.[29] Still, their emergence would not lead to an outright *disconfirmation* of the HWP, which even in case of replacement would still work in relevant domains and for a wide variety of purposes.

[28] Cf. footnote 18.
[29] Following up on the work of Li (1988), Stark (2006a, 2006b, 2007) showed that the assumption of random mating is sufficient but not necessary for Hardy-Weinberg proportions to hold in a population, but they can be reached also with non-random mating. Thus, even though experience has shown the fruitfulness of the HWP as a basis for hypothesis formulation and testing, this does not constitute a good reason for retaining the assumption of random mating (Stark and Seneta 2013).

The possibility of replacing the HWP, in fact, does not depend on any empirical test of the HWP itself, as its high ES exhibits. It rather depends on the presence of competitive counterfactual alternatives which can fulfil the same epistemic function, but conceptually determine the domain of interest (or a larger one) in a different way. For instance, Bosco et al. (2012) argue that the very notion of 'equilibrium' state (in a mechanical sense) as identified by the HWP would only be attainable in the infinite time horizon, and not by looking at isolated observations on a genetic population, which can, at most, capture 'stable' states. Consequently, they call for a reconceptualisation of the notion of equilibrium in population genetics and argue that the equilibrium state of genetic systems should be defined in terms of dynamical systems, by integrating a time variable to account for the evolution of the allele frequencies of the system over time. Another proposal is that of McShea and Brandon's (2010) ZFEL (zero force evolutionary law), which identifies the equilibrium state of evolutionary systems with their increasing state of diversity and complexity, rather than with a no-change state as the one captured by the HWP. These alternatives to the HWP fulfil its same epistemic function, i.e. that of providing the equilibrium state for a class of systems. However, by providing a *different sort of* equilibrium state, they constitute alternative representations of the systems themselves, since which parameters – and relationships between them – within the system of interest are considered salient in the alternative representations is at least partially dependent on how the equilibrium state is characterised. Consequently, such aspect bears on what type of entities scientists look for when they develop their explanations, what further parameters they introduce for their modelling practices, etc. For instance, an equilibrium state characterised in terms of dynamical systems, as envisioned by Bosco et al. (2012), configures the system of interest (a certain genotypic distribution) as an entity developing over time, subject to many perturbations, and whose equilibrium condition can only be determined (asymptotically) in the long run. On the other hand, McShea and Brandon's attempt to identify an equilibrium state that could apply to all evolutionary systems, in contrast with the HWP's restrictive conditions, also has substantial consequences on the way the system of interest is modelled, and explanations are developed. In fact, "[a] biologist armed with the ZFEL looks to stasis, rather than change, as the sure indicator that an evolutionary force is at work" (Gouvea 2015, p. 369).

To sum up, I believe these considerations highlight some aspects which cannot be fully captured by understanding the HWP simply as *a priori* in Sober's sense, even though it can be compatible with it. Certainly, the HWP *defines* the equilibrium conditions of genetic populations. But it does so by presenting a counterfactual ideal situation, which exhibits a constitutive function in that it represents an instance of counterfactual reasoning, rather than conceptually constituting a first-order empirical law, as in Friedman's case. Indeed, the HWP provides expected frequencies, against which real gene distributions are tested, thus it does not constitute the real gene distributions themselves, of course. Nonetheless, as I highlighted above, their characterisation as deviations from equilibrium, and the outcomes of the explanatory practices implemented to account for such a deviation, are determined by what we take to be the equilibrium condition.

Once we abstract away from the type of generativity that characterises the HWP, it seems clear that it is not maximal within population genetics, given the restricted domain to which it applies (diploid organisms, sexually mating), thus, *if* we take the laws of motion in Newtonian mechanics as paradigmatic standard, it can count only as a minimal case. Still, given its epistemic function of providing equilibrium or stability conditions for (at least a class of) genetic populations, it may be interpreted as an instance of a more general 'stability principle', which might itself be a paradigmatic constitutive requirement for modelling practices across the sciences. These last points certainly deserve further scrutiny.

Conclusion

I have argued that the HWP performs a constitutive epistemic function to a minimal extent, when compared to a paradigmatic case such as that of the laws of motion with respect to the law of gravitation in Newtonian mechanics. Such conclusion is not simply a consequence of the fact that the laws of motion are constitutive in the sense that they are required for formulating a first-order empirical law, whereas the HWP does not constitute another empirical law but is directly deployed in modelling practices. In fact, under the assumption of blurring the distinction between theory and practice, also the HWP can be understood as providing fundamental conceptual determinations to the empirical material of the domain of interest, without being itself directly testable within its own framework of inquiry. The HWP performs a constitutive function in that it conceptually determines the equilibrium state for genetic populations under certain conditions, by providing a counterfactual idealised scenario to which real situations can be compared. By instantiating a (specific) counterfactual ideal situation, it opens a restricted set of possibilities within which empirical inquiry on (at least a class of) genetic populations in general can take place, thus determining, for instance, certain relationships between the parameters of interest, and which entities are salient when developing explanations. However, once the HWP has been subject to analysis in terms of the framework I provided, it shows the typical characteristics of constitutive elements to a lesser extent than paradigmatic cases. Finally, the emergence of alternative theoretical elements which can perform the same function of equilibrium condition as that of the HWP, albeit with different epistemic purposes and outcomes, indicates that the HWP might be understood as one of the concrete instantiations of a general and formal (i.e. empirically contentless) stability principle, that might itself be considered as a (paradigmatic) constitutive precondition for our modelling practices in general.

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