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The early history of chance in evolution

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ABSTRACT

Work throughout the history and philosophy of biology frequently employs 'chance', 'unpredictability', 'probability', and many similar terms. One common way of understanding how these concepts were introduced in evolution focuses on two central issues: the first use of statistical methods in evolution (Galton), and the first use of the concept of "objective chance" in evolution (Wright). I argue that while this approach has merit, it fails to fully capture interesting philosophical reflections on the role of chance expounded by two of Galton's students, Karl Pearson and W.F.R. Weldon. Considering a question more familiar from contemporary philosophy of biology—the relationship between our statistical theories of evolution and the processes in the world those theories describe—is, I claim, a more fruitful way to approach both these two historical actors and the broader development of chance in evolution.

1. Introduction

Our discussions of the history and philosophy of evolutionary biology continually make use of terms that may broadly be described as falling under the umbrella of 'chance': 'unpredictability', 'randomness', 'stochasticity', and 'probability' provide only a few examples. We find extensive discussion in the history of biology concerning the introduction of statistical *methods* in the life sciences (see, e.g., Porter, 1986; Sheynin, 1980). In the spirit of integrating the history and philosophy of science, however, it is notable that the corresponding question about these *concepts* often goes unanswered. How were the various notion of 'chance' now so prevalent in the biological literature introduced into evolutionary theorizing?

One of the only serious attempts to describe both facets of this historical transformation was advanced by Depew and Weber (1995), and has since been found in various places throughout the history and philosophy of biology. Their picture of the development of chance in evolution seeks to understand two crucial historical events. First, when and how did evolutionary theorizing become statistical? Second, when and how did such theories come to be taken to describe "genuinely chancy" processes in the world?[1]

Elucidating this standard view is the project of my second section. Francis Galton, it is generally recognized, is responsible for the first, methodological shift—it was Galton's work on the statistically derived law of ancestral heredity that introduced statistics into the study of evolution. The second, conceptual shift originates in Sewall Wright's shifting balance theory, which required a much more significant role for a chancy process of genetic drift than the theories which had come before it.

After introducing Depew and Weber's view, we will explore it in more detail. Section 3 will return to Darwin's own works, to establish the now-standard interpretation that Darwin believed evolution to be a *non*-statistical theory of *non*-objectively-chancy processes in the world. We then turn to Francis Galton in Section 4, where I describe his role in the development of the first statistical methods in the study of evolution. Rather than moving on to Wright, however, we will examine in Section 5 two of Galton's students at the end of the nineteenth century, Karl Pearson and

[1] The appropriate referent for "genuinely chancy" here is a very difficult problem, as various concepts of objective chance are often conflated in the (historical and present) literature on evolutionary theory. Thankfully, the point will not matter substantially for us, as I will not consider how the second question should be answered.

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W.F.R. Weldon. On Depew and Weber's view, these two would be minor characters.

Why, then, consider Pearson and Weldon at all? It is their work that will serve as our point of departure from considering the introduction of chance in terms of Depew and Weber's two focal historical moments. I will argue that if we are interested in the emergence of chance in evolution, Pearson and Weldon should indeed not be read as minor players. A vitally important distinction can be detected in Weldon and Pearson's writings on the philosophical justification for the use of concepts of chance. Suitably considered, that is, we can see Pearson and Weldon as innovators not merely in the use of statistical methodology, but in the philosophical grounding for the use of chance as well. If we focus only on the two events of the Depew and Weber view, we will entirely fail to recognize this aspect of their thought. We must look, then, for a new context for this historical development—a new driving question on which we are able to understand the eventual philosophical rift between Pearson and Weldon. I will argue that this distinction can be best exposed by considering the relationship between our *statistical theories* and the *processes* which those mathematical frameworks are intended to describe.

As regards this new question, then, a more mathematical, more positivist school of thought, with Pearson at its head, takes these statistics to be a tool for glossing over the (complex, possibly deterministic or indeterministic) causal details of biological systems. On the other side, a more empiricist, experimentally inclined school, with Weldon at its head, takes these statistics to be an essential way of grasping the full causal detail of biological systems. We can thus see here, I claim, a dramatic difference in the understanding of the connection between evolutionary theories and the evolutionary process, positions that are better comprehended not by way of the "reification" or "objectification" of chance, but by considering their differing views on the relationship between evolutionary theory and the biological world. And this question, as I will briefly argue in the conclusion, resonates strongly with contemporary work in the philosophy of biology.

2. Two focal events

We will begin, then, by discussing the view of the historical development of chance laid out in Depew and Weber's *Darwinism Evolving* (1995) and echoed throughout the subsequent literature in the history and philosophy of biology.[2] The second part of their book is devoted to describing the relationship between the advance of a new variety of Darwinism grounded in the developing science of genetics and what they call the "probability revolution"—the same broad historical process that Hacking called the "taming of chance" (Depew & Weber, 1995, p. 202). While they sometimes refer to this revolution as a singular event, they also helpfully subdivide it into two parts. The first is a "statistical revolution," the introduction of statistics as a tool "for collecting and analyzing quantifiable data," initially in the social and then in the scientific realm (Depew & Weber, 1995, p. 203). Later, with the addition of a robust probability theory, "the idea arose that probabilities [derived from these statistics] are based on objective propensities of real things" (Depew & Weber, 1995, p. 206). These two ingredients combined to make the probability revolution complete.

We see again, here, the distinction between the introduction of statistical methods into science and the corresponding introduction of the philosophical concepts that underlie these methods. Narrowing our view to the evolutionary realm, we are led to investigate the two historical events mentioned in the introduction: what was the first time that the statistical revolution was reflected in evolutionary theory (i.e., the first use of statistical methods), and what was the first time that probability in the genuine, objective sense was utilized (i.e., the first use of one particular philosophical conception of chance)?

Depew and Weber go on to describe what have come to be the standard explanations of these two events. For the first, they point to the work of Francis Galton. "Galton," they note, "contributed less to the continuity of the Darwinian tradition by his substantive views … than his conceptual and methodological ones" (Depew & Weber, 1995, p. 201). They make extensive use of the analysis of Hacking, who persuasively argued that Galton was the first not just to use a statistical law for the description of phenomena, but also as "autonomous," as a law "serviceable for explanation" of those phenomena by itself, without having to invoke a large array of supposed (but unobserved) underlying, small causes (Hacking, 1990, p. 186). Depew and Weber note that this, as well, is the first time that statistics is used in a positive manner for the support of Darwinian theory, rather than as a way to attack natural selection.[3]

In the case of the second event—the introduction of an objective, reified, or "genuine" notion of chance in evolution—Depew and Weber argue that "Sewall Wright opened up this Pandora's box" (1995, p. 287). Wright's turn toward chance, they write, was a way of enhancing the ability of the evolutionary process to create novelty, to provide "more openings for creative initiations" (Depew & Weber, 1995, p. 285). Wright, therefore, completes the probability revolution in the biological sciences. While Fisher, they argue, saw chance as merely a source of mathematical noise, a difficulty in theorizing which needed to be overcome and factored out, it was Wright who first argued that evolution invoked genuinely chancy processes—including random drift, the chanciness of which occasionally pushed organisms down an adaptive peak and enabled them to reach a higher neighboring optimum. On this view, we have a shift toward 'chance' precisely because chance is, for the first time, an active force which can be implicated in certain sorts of population change (namely, change which runs contrary to fitness gradients). The interpretation of Wright is, however, famously quite complicated (Hodge, 1992a, pp. 287–288), and for our purposes here I will leave the issue underdeveloped. As we will see, whether or not Wright was indeed the first to use an objective notion of chance is immaterial to my project.

Before continuing, I should note that by offering a new, third focus for our historical work on chance in evolution here, I do not at all intend to disparage either this pair of questions or the explanations offered for them. Indeed, both mark significant and important developments in the history of biology, ones which we are right to single out for extra scrutiny. I will argue, however, that if we restrict ourselves to only looking at the development of chance through these lenses, we run the risk of missing significant and important developments in the way that chance was

[2] Both questions are found, at least, in Hodge (1987) and Morrison (2002). Galton's role in the first shift has been discussed by Hacking (1990), as we will see later. Sheynin (1980) covers the first shift extensively as well. The second question is explored by Morizot (2012). Philosophically, many works—such as Brandon and Carson (1996), Millstein (2000), Rosenberg (2001), or Pence and Ramsey (2013)—implicitly rely on this distinction between the (assumed) statistical nature of evolutionary theory and the (contested) "chanciness" of biological processes.

[3] The same analysis is offered by Provine (1971, pp. 22–23), Gayon (1998, p. 105), Porter (1986, pp. 135, 284–285), and Radick (2011, p. 133).