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Wright's path analysis: Causal inference in the early twentieth century

(El análisis de caminos de Wright: inferencia causal a principios del siglo xx)

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ABSTRACT: Despite being a milestone in the history of statistical causal inference, Sewall Wright's 1918 invention of path analysis did not receive much immediate attention from the statistical and scientific community. Through a careful historical analysis, this paper reveals some previously overlooked philosophical issues concerning the history of causal inference. Placing the invention of path analysis in a broader historical and intellectual context, I portray the scientific community's initial lack of interest in the method as a natural consequence of relevant scientific and philosophical conditions. In addition to Karl Pearson's positivist refutation of causation, I contend that the acceptance of path analysis faced several other challenges, including the introduction of a new formalism, conceptual barriers to causal inference, and the lack of model-based statistical thinking. The presence of these challenges shows that the delayed progress in causal inference in the early twentieth century was inevitable.

KEYWORDS: Sewall Wright, path analysis, Karl Pearson, causal inference, history of statistics.

RESUMEN: *Pese a suponer un hito en la historia de la inferencia causal estadística, la invención de Sewall Wright del método de caminos en 1918 no recibió demasiada atención inmediata por parte de la comunidad científica y estadística. Mediante un análisis histórico cuidadoso, este artículo muestra algunas cuestiones filosóficas previamente desatendidas acerca de la historia de la inferencia causal. Situando la invención del análisis de caminos en un contexto histórico e intelectual más amplio, presento la falta inicial de interés de la comunidad científica como una consecuencia natural de factores científicos y filosóficos relevantes. Además de la refutación positivista de la causación por parte de Karl Pearson, defiendo que la aceptación del análisis de caminos se enfrentaba a otros retos, incluyendo la introducción de nuevos formalismos, barreras conceptuales a la inferencia causal, y la ausencia de un pensamiento estadístico basado en modelos. La existencia de estos retos muestra que el retraso en el desarrollo de la inferencia causal a principios del siglo xx era inevitable.*

PALABRAS CLAVE: Sewall Wright, análisis de caminos, Karl Pearson, inferencia causal, historia de la estadística.

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1. Introduction

A challenge faced by scientists in many domains is answering causal questions from statistical data, especially when randomized experiments are unavailable or impractical. In the last few decades, numerous innovations have been made in statistical causal inference, such as graphical causal models (Spirtes *et al.,* 1993; Pearl, 2000; Hitchcock, 2023) and the potential outcomes framework (Rubin, 1974).[1] The application of these methods in biomedical and social sciences has proven fruitful (see, e.g., Pearl, 1995; Imbens & Rubin, 2015). This is strong evidence that the notion of causation is indispensable for statistical sciences in both experimental and nonexperimental studies. This may sound like a cliché to many; however, looking back at the history of statistics, causation was not always a welcomed idea. In fact, when modern statistics was born in the hands of Francis Galton and Karl Pearson at the turn of the 20th century, analysis of correlation rather than causation was the central theme.[2] It was in this historical context the population geneticist Sewall Wright introduced path analysis —now widely recognized as the earliest precursor to graphical causal modelling (for the history of path analysis, see Provine, 1989; Shipley, 2016; Pearl & Mackenzie, 2018).

Realizing that Pearson's correlation analysis could not answer causal questions in genetics, Wright designed path analysis to infer new causal knowledge by combining correlational data with prior causal knowledge. Despite being an important innovation, the method did not attract much immediate attention from the statistical community.[3] Even negative responses (e.g., Niles, 1922, 1923) were sparse. Several decades later, path analysis was rediscovered in the social sciences (see, e.g., Duncan, 1966), followed by the emergence of various causal inference methods, including the abovementioned potential outcomes and graphical modelling frameworks. For those who have observed the success of causal inference in contemporary statistics, the question of why the invention of path analysis received little immediate attention naturally arises. As a philosopher of science, however, my interest in this paper is not merely historical. Through a careful historical analysis, this paper will unveil a range of philosophically intriguing issues regarding path analysis and statistical causal inference that have been overlooked or underexplored in prior discussions.[4]

It is well-known that Pearson strongly opposed the idea of causation. One may, therefore, be tempted to blame Pearson (and his followers) for impeding progress in causal inference.[5] While Pearsonians' aversion to causation is a salient explanatory factor that we

[1] It is worth mentioning that the potential-outcomes approach to causal inference also has a precursor in the early 20th century, namely, Neyman (1923/1990).

[2] For a brief introduction to the history of modern statistics, see Otsuka (2022, chapter 1).

[3] Path analysis didn't make a notable impact during the first half of the 20th century, except that a couple of psychologists (Burks, 1928; Engelhart, 1936) found its applications in educational psychology.

[4] For philosophical discussions on path analysis, see Irzik (1986), Griesemer (1990, 1991), and Irzik and Meyer (1987), although they didn't devote much space to its history.

[5] I do not intend to attribute this simple-minded explanation to any author, nor is it essential for my paper that anyone has seriously made this proposal. That said, Pearl and Mackenzie (2018, p. 67) seem to have this idea in mind when they claim that "the death of causation" was the casualty of Galton and Pearson's invention of correlation: "When it comes to explaining the expulsion of causality from

should not ignore, it would be a mistake to claim that Pearsonians should bear the sole or primary responsibility for the delayed progress in causal inference. To begin with, it was not a coincidence that path analysis came two decades after Galton and Pearson discovered correlation: correlation coefficients are an essential component of path analysis. For this reason, Galton and Pearson's discovery of correlation should be seen as a precondition rather than a hindrance for Wright's path analysis. In this paper, I suggest that the delayed acceptance of path analysis is a multifactorial event with several important contributing factors. By depicting a fuller picture of the history of path analysis, I hope to show that the slow progress in causal inference in the early twentieth century was, in fact, a natural consequence of relevant scientific and philosophical conditions. It was just too challenging to make real progress in statistical causal inference back in the early twentieth century.

The paper is structured as follows. In section 2, I provide an exposition of the method of path analysis using Wright's example and then summarize a few key features of the method. Section 3 discusses Pearson's positivist philosophy of science and Henry Niles's Pearsonian objections to Wright's path analysis. This tension between Wright and Pearsonians, however, is only a small part of the story. In section 4, I place the invention of path analysis in a broader historical context, featuring the paradigm shift from Pearson's model-free descriptive statistics to Fisher's model-based inferential statistics. Within this context, I identify a few factors that can explain the delayed acceptance of path analysis. First, I argue in section 5 that Wright's contemporaries were not yet ready to appreciate the newly introduced path diagram. In addition to being an unconventional formalism, the analytic power of the path diagram was underestimated because its power had not been demonstrated on rigorous grounds. Second, I show in section 6 that the success of classical microphysics at the turn of the 20th century had a deep influence on people's thinking of causation. This created conceptual barriers to progress in causal inference in nonphysical sciences in which a different notion of causation was needed. Third, in section 7, I suggest that there could have been (implicit) concerns regarding the empirical validity of (causal and statistical) assumptions made by path analysis. These concerns, however, could not be properly formulated, not to mention addressed, before Fisher's model-based statistical thinking was well-entrenched. Once we take all these factors into consideration, it becomes clear that the delayed progress in causal inference was an unavoidable consequence of relevant scientific, historical, and philosophical conditions. Section 8 concludes with a quick remark on the necessity of an integrated history and philosophy of science (integrated HPS) approach to the history of causal inference.

statistics … There simply is no other way to understand how statistics became a model-blind data-reduction enterprise, except by putting on our causal lenses and retelling the stories of Galton and Pearson in the light of the new science of cause and effect. In fact, by so doing, I rectify the distortions introduced by mainstream historians who … marvel at the invention of correlation and fail to note its casualty —the death of causation."

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2. *Path Analysis and Its Key Features*

In the early 20th century, statistically minded scientists such as biometricians and population geneticists started to realize the need for a tool to analyze nonexperimental data in hopes of solving causal problems concerning, for instance, the mechanism of inheritance. An example of such kind of causal query is to estimate the *relative causal importance* of different factors in determining variations in animal traits. Realizing the limitations of Pearson's correlation analysis, Wright invented path analysis in 1918.[6] Wright suggested two uses of his method: to estimate the relative strengths of known causal relations or to test whether a causal hypothesis is true or false. I will only demonstrate how path analysis is used in the first way using an example from Wright (1920).

Wright (1920, p. 321) raises the following causal question: what is the comparative importance of the influence of heredity (i.e., genetic constitutions) and environmental factors on the pattern of coat colours in guinea pigs? This question clearly could not be answered using controlled experiments. Nor could it be solved using correlation analysis alone. The reason for the latter is that "correlation between two variables ... gives merely the resultant of all connecting paths of influence" (Wright, 1921, p. 557), but to answer Wright's question, a decomposition of correlation along different causal paths is needed. This prompted Wright to invent path analysis. The method begins by positing causal relations among variables used to characterize the target system. These *causal postulates* come from Wright's knowledge of the mechanism of inheritance in guinea pigs and can be depicted in a *path diagram* —a primitive form of a graphical causal model (see Figure 1). A path diagram is conducive and essential to causal inference, as we shall see in section 5.

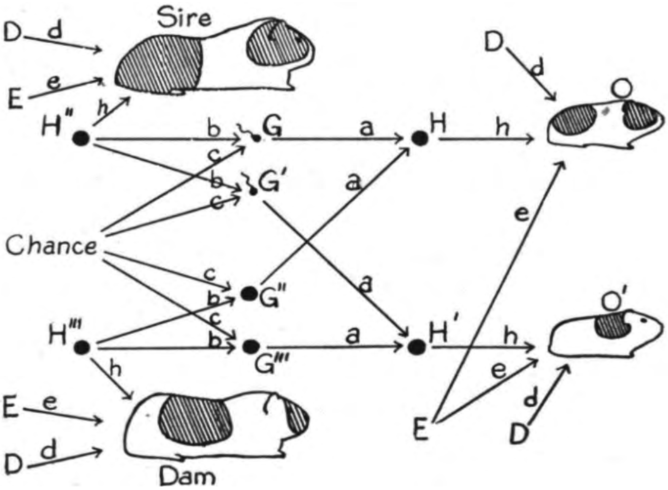

Figure 1

Wright's (1920) path diagram,
showing how variations in coat colour in guinea pigs can be causally explained

[6] The method was further developed by Wright in 1920, 1921, 1923, and 1934.

In this diagram, an arrow connecting two variables represents direct causation between them. An arrowhead indicates the direction of causation. Variables *H*, *H'*, *H''*, and *H'''* represent the genetic constitutions of the four individual guinea pigs. *G*, *G'*, *G''*, and *G'''* represent the genetic constitutions of the four germ cells. *E* represents environmental factors, which are the same for the four guinea pigs that were born in the same litter. *D* represents other factors, largely developmental irregularity. Lowercase letters a, b, ..., alongside those arrows represent *path coefficients*. A path coefficient measures the strength or importance of a direct causal relation (relative to other direct causes of the same effect). When an effect variable has multiple causes, calculating the path coefficients of these causal relations involves decomposing the variability of the effect variable in proportion to each cause's contribution to this variability.[7] As Wright (1920, p. 329) explains,

> The path coefficient, measuring the importance of a given path of influence from cause to effect, is defined as the ratio of the variability of the effect to be found when all causes are constant except the one in question, the variability of which is kept unchanged, to the total variability. Variability is measured by the standard deviation ... It can be shown that the squares of the path coefficients measure the degree of determination by each cause.

Now, to determine whether the genetic factor (*H*) or the environmental factor (*E*) plays a greater role in coat colour patterns, we need to compare the relative causal contributions of *H* and *E*, which, in turn, is to compare the values of path coefficients h and e. The problem, however, is that these path coefficients cannot be directly determined from the data or background knowledge. To solve this problem, Wright wrote down a series of linear equations about these path coefficients based on some general principles about causation and correlation (details below); since some of the quantities in the equations could be known from data or background knowledge, we could solve these equations and infer the unknown. The question is: based on what principles can we obtain the equations?

First, according to Wright (1920, p. 329), "the squares of the path coefficients measure the degree of determination by each cause. If the causes are independent of each other, the sum of the squared path coefficients is unity".[8] That is, our first principle is that the causal

[7] For example, for the path from *E* to *O'*, its path coefficient can be denoted as $p_{O'.E}$, whose value is labelled 'e' on the diagram. The value of $p_{O'.E}$ can be obtained by first calculating the total variability of the effect variable *O'* (denoted as $\sigma_{O'}$; measured by the standard deviation of *O'*) from data and then calculating the variability of *O'* when holding constant the values of the other direct causes of *O'* (i.e., *H'* and *D*). This conditional variability can be denoted as $\sigma_{O'.E}$. Finally, we obtain the path coefficient: $p_{O'.E} = \sigma_{O'.E} / \sigma_{O'}$. What we are doing here is basically proportionally attributing the variability of *O'* to the three causal paths going into *O'*.

[8] This implies that the values of path coefficients depend on the assumed path diagram. If the diagram is drawn differently —for instance, by changing the direction of a certain causal path— the path coefficients might end up different. More specifically, if we reverse the direction of causation between *D* and *O'*, values of h, e, d will be different: in this case, only *H'* and *E* are direct causes of *O'*, so Equation (1) would become $h^2 + e^2 = 1$.

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impact of all (direct) causes on an effect variable should add up to unity.[9] For example, for the effect *O'* and its three (direct) causes *H'*, *E*, and *D*, we have (p. 330):

$$h^2 + e^2 + d^2 = 1 \tag{1}$$

Second, according to Wright (1920, p. 330), "the correlation between two variables can be shown to equal the sum of the products of the chains of path coefficients along all of the paths by which they are connected". Therefore, our second principle is that the correlation between any two variables should be completely explained by the causal paths linking them (disregarding random errors due to chance). For example, the correlation between *O* and *O'* (i.e., $r_{OO'}$) can be explained by three causal paths, namely *O-H-G-H"-G'-H'-O'*, *O-H-G"-H'"-G'"-H'-O'*, and *O-E-O'*.[10] Wright assumed that the causal relations were linear so that we could multiply the path coefficients along a directed causal path, and the causal influence from distinct causal paths was additive. With these assumptions, we then obtain (Wright, 1920, p. 330):

$$r_{OO'} = habbah + habbah + ee \tag{2}$$

By following a similar procedure, we can obtain more equations in addition to (1) and (2). In these equations, correlation coefficients such as $r_{OO'}$ can be inferred from data. Wright relied on Mendel's laws of inheritance (e.g., the law of segregation and the law of dominance) to determine path coefficients a and b (see Wright, 1920, p. 331). By solving the equations, Wright was able to obtain the values of h and e that were needed to answer the causal question he raised earlier.[11]

The above illustration reveals several key features of Wright's method:

1. Path analysis remains *noncommittal* to any philosophical conception or definition of causation but relies only on a few *intuitively* plausible principles about causation (e.g., causation is distinct from correlation; the correlation between two variables can be explained by causal connections between them).
2. Path analysis requires sufficient and reliable background *causal knowledge* about the target system. Such substantive and subject-specific knowledge may come from several sources (e.g., theoretical hypotheses or laws, previous experiments or observations, or common sense).

9 In constructing a path diagram, we need to make sure that we have included all the direct causes of an effect variable (so that the system is "closed"). Note, however, that this strong condition is not required in modern graphical causal modelling, which adopts a different method to quantify causal effects.

10 Not all paths linking two variables can contribute to the correlation of the two variables. When the path diagram is more complex, it is often difficult to decide which paths contribute to the correlation. So, Wright discovered some graphical rules to figure out which causal path counted. I will discuss this later in section 5.

11 Wright's (1920) result is that in a group of guinea pigs where mating was random, variations in guinea pigs' coat colour patterns were determined 42.2% by genetic factors, and 57.5% by irregularity in development, while environmental factors played a relatively minor role.

3. Path analysis uses *path diagrams* to represent domain-specific causal knowledge and make inferences about causal effects.[12] A path diagram, as an integral part of the method, helps us obtain the correct equations we need.
4. Path analysis requires trustworthy *correlational data* from which the needed correlation coefficients can be reliably inferred. Additionally, it is worth noting that path analysis cannot be used to derive causal relations from purely correlational data.[13]
5. Path analysis makes simplifying and *idealizing assumptions* when representing and quantifying causation: the target causal system is linear and additive, it contains no causal feedback or loops, and direct causes of an effect are independent (i.e., noninteractive).

Unfortunately, the invention of path analysis did not obtain much immediate attention. This may not sound particularly surprising to some; after all, events of this type are not uncommon in the history of science. Still, the delayed acceptance of path analysis calls for an explanation. More importantly, as we shall see, through a careful historical analysis, several intriguing but previously unexplored philosophical issues about path analysis will be revealed. This also motivates the historical framing of this paper.

As a first attempt, one may appeal to Pearson's (and his follower, Niles's) aversion to causation to explain the initial lack of enthusiasm for path analysis: as positivists, Pearson and Niles insisted that the idea of causation was philosophically unfounded, and therefore, scientifically meaningless. Pearson was not an ordinary statistician: as one of the pioneers of modern statistics, Pearson retained a dominant presence in the field for several decades. For instance, he served as the editor of a major journal in statistics, *Biometrika*, until his death in 1936 (see Aldrich, 2013). This paper recognizes that the Pearsonian's overt hostility to causation played an important role in the delayed acceptance of path analysis; in the next section, I will go through and analyze this role in detail. However, this factor alone cannot provide an adequate explanation; moreover, putting too much emphasis on this factor risks creating an unnecessary opposition between causal and correlational analysis, as well as leading to the negligence of other equally or even more important explanatory factors. In sections 4-7, I endeavour to provide a fuller and more faithful depiction of the status of path analysis in the early 20th century.

3. Pearsonians' Resistance to Causation and Path Analysis

As classical physics achieved its mature form at the end of the 19th century, the positivist view of causation —the idea that "causation" should be eliminated from mature science and replaced by something more "scientific"— became exceedingly popular among scientists. For example, for Ernst Mach, the prevalence of functional relations in physics showed

[12] From Fisher's statistical modelling perspective, path diagrams can be seen as a kind of *explanatory* model; according to Lehmann (1990, p. 959), "an explanatory model … requires detailed knowledge and understanding of the substantive situation that the model is to represent."

[13] This task of discovering causal structures from purely observational data —when certain conditions are satisfied— is made possible by Spirtes *et al.* (1993).

that the "old-fashioned idea" of causality was "a little clumsy" and should be abandoned (Mach, 1886, as cited in Heidelberger, 2010). Influenced by Mach, in *The Grammar of Science*, Pearson rejects the idea of causation and argues that causation is just a special case of correlation, namely *perfect correlation* (a correlation is perfect when its Pearson coefficient is 1). However, Pearson argues, there is *no* such thing as perfect correlation in the universe; it is merely a conceptual limit or statistical approximation, not an idea based on our experience (Pearson, 1911, p. 156f). "No phenomena are causal", Pearson (p. 174) claims. Consequently, "[i]t is this conception of correlation between two occurrences embracing all relationship from absolute independence to complete dependence, which is the wider category by which we have to *replace the old idea of causation*" (p. 157; emphasis added).

Between 1896 and 1911, three editions of *The Grammar* were published. Given Pearson's influence, Wright must have heard of Pearson's arguments against causation before he published path analysis in 1918. Wright, however, did not seem to be bothered by these arguments. His works show no sign of interest in philosophical disputes over causation. Since 1918, he had been using the concept of causation in an intuitive and pragmatic way. In particular, when Wright introduces the method of path analysis in his 1921 paper, he says: "the method depends on the combination of knowledge of the degrees of correlation among the variables in a system with such knowledge as may be possessed of the causal relations" (Wright, 1921, p. 557). Here, Wright bluntly assumes that there is a distinction between "causation" and "correlation" without offering any explanation or justification. For Pearson and his follower Niles, Wright's indifference to the philosophical basis of causation was unacceptable. This is why Niles (1923, p. 256) complains that Wright "spends comparatively little time dealing with the philosophic basis of his theory" (we will return to Niles later).

The Pearsonian also objected to the method on the basis that it required the pre-specification of a set of causal postulates about the target system; such causal postulates belong to what are broadly known as data-generating processes (which are typically unobservable). This contrasts with Pearson's method of correlation analysis, which does not require the postulation of any unobservables. Given that today it is standard practice to posit data-generating processes behind data, one may wonder why the Pearsonian statistics did not adopt this approach. An important reason can be found in *The Grammar*, in which Pearson develops a positivist epistemology of science. Pearson (1911, p. 86; emphasis added) argues that science can only acquire knowledge from our "sense impressions", namely, the data we can directly access: "Law in the scientific sense is thus essentially a product of the human mind and *has no meaning apart from man*." Presumably, here "scientific laws" also include those laws of correlation —that is, statistical regularities— we find in statistical data.

Given that for Pearson all scientific knowledge is contained in observed data, a statistical method should not and need not presuppose any data-generating mechanism beyond the reach of our sense impressions. Any reliable knowledge should be directly inferred from data in a mathematically valid way without postulating anything beyond what is observed. That is, what scientists should be after are laws of correlation inferred from data rather than laws of causation out there in the world. Therefore, from a positivist point of view, whatever a path diagram represents, it goes beyond our sense impressions and should be rejected *a priori*.

Henry Niles, one of Pearson's close followers, was strongly opposed to Wright's path analysis. His (1922, 1923) objections to Wright, published in *Genetics*, seem to be the only noteworthy criticisms of path analysis in the first half of the twentieth century. These objections closely follow Pearson's positivist philosophy; therefore, although Pearson did not directly respond to the invention of path analysis, by reading Niles we can still obtain a good grasp of what Pearson would have said about path analysis.

Niles's first objection to Wright is that there is no point in distinguishing between causation and correlation since there is no real difference between them. The argument, basically a reiteration of Pearson's positivist refutation of causation, goes as follows. There could be a real difference between causation and correlation only if there was some kind of "inherent necessity" in causation that was lacking in correlation. However, according to Niles (1922, p. 259), "in no case has it been proved that there is an *inherent necessity* in the laws of nature." Setting aside the philosophical plausibility of this argument, it is somewhat surprising that in 1922, a purely philosophical objection to a new scientific method could be published in *Genetics* (a journal over which Pearson had no direct control). This suggests that Pearson's positivist philosophy of science did have a considerable impact on the scientific community during the 1920s.

The second objection is more intriguing: "[t]he basic fallacy of [Wright's] method appears to be the assumption that it is possible to set up *a priori* a comparatively simple graphic system which will truly represent the lines of action of several variables upon each other" (Niles, 1922, p. 261). Niles's worry here is how we could guarantee that the "simple graphic system" (the path diagram) used in path analysis "truly represent[s]" causal reality. As he explains, "[i]f we put into our system all important causes we know of, and all the important causes of these [variables], and so on back, we would cover the whole universe and even then find no logical stopping place" (Niles, 1922, p. 262). It seems that again, this argument was inspired by Pearson (1911, p. 131) who says that "the causes of any individual thing thus widen out into the unmanageable history of the universe."

It seems to me that Niles's (as well as Pearson's) worry was primarily *metaphysical* or conceptual (at least on a literal reading).[14] Essentially, his concern is that a true causal representation of any particular system of interest must include the entire causal history of the system, which is something that a simple and limited path diagram cannot do. Therefore, to justify the use of path diagrams, one needs to show the grounds on which we can isolate a causal system from its "unmanageable" causal history. Wright (1923, p. 25), however, merely asserts that such isolation of limited systems works satisfactorily in practice, without giving any further explanation for why the isolation is justified; as he says, "[i]n practice we find that we can satisfactorily isolate a portion of the universe and deal with causation relative to this limited system."

Thanks to recent developments in causal modelling, we now know that the key to the justification is the statistical independence of exogenous variables in a causal model; a variable is exogenous as opposed to endogenous if it does not have causes in the specified causal model. To isolate a causal system from its "unmanageable" history, we only need to ensure that the exogenous variables specified in our causal model are effectively statistically independent —a requirement that can often be met in practice. The underlying rationale is also known as screening-off or the causal Markov condition: conditioning on those (statistically

[14] That is, the worry here is not about the *empirical* validity of the path model. I will discuss (potential) empirical concerns about path analysis later in section 7. Although nothing was preventing Niles from having empirical concerns about path analysis, these empirical concerns were at best implicit in Niles's objections.

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independent) exogenous variables of a causal model, further knowing the causal history of these exogenous variables becomes irrelevant for explaining or predicting the values of endogenous variables in that model. This justification of causal isolation, however, was not available to Wright. This means that even if Wright was right that in practice, the isolation of local causal systems was justified, he nevertheless was not able to provide a systematic and rigorous ground for the isolation. In this sense, Niles's second objection did pose a genuine challenge to the validity of Wright's path analysis.[15]

4. *A Fuller Picture: Paradigm Shifts in Modern Statistics*

In this section, I will situate the invention of path analysis in a broader historical context and paint a fuller (and hopefully also more faithful) picture of the status of causal inference in the early 20th century. A key to this historical picture is Fisher's recasting of Pearson's model-free descriptive statistics into model-based inferential statistics. Before that, the paradigm in statistics was analysis of correlation (without assuming any data-generating processes). From the perspective of the model-free paradigm, Wright's introduction of path analysis —which explicitly posits a data-generating process— was a notable departure from this dominating paradigm.[16]

Modern statistics did not start out being dominated by analysis of correlation. In fact, in 1877, Galton tried to find a causal explanation for the phenomenon of regression to the mean in human inheritance (the phenomenon that human heights tended to "revert to mediocrity"). It was his failure to find a causal explanation that made him believe that causal analysis did not apply to this problem, so he turned to correlation analysis around 1889 (Ariew *et al.,* 2017). Galton's turn from causation to correlation had a significant impact on Pearson. Pearson was deeply impressed by Galton's discovery of correlation and its applications in biometry (aka biometrics). He followed Galton and began to work on mathematical analysis of correlation at the end of the 19th century.

In the early 20th century, with the efforts of Pearson and others, analysis of correlation started to become the dominating paradigm in statistics, especially after the foundation of *Biometrika* in 1901 (see Table 1). The success of correlation analysis in biometry convinced Pearson that its scope of application could be expanded to all special sciences in the future. In commenting on Galton's contribution to statistics, Pearson (2011/1930, p. 1) claims that "[u]p to 1889 men of science had thought only in terms of causation, in future they were to admit another working category, that of correlation, and thus open to quantitative analysis wide fields of medical, psychology and sociological research." Pearson's vision for the future of correlation was not without its grounds. Even if statistics today has gone far beyond what Pearson could imagine, Pearson correlation coefficients remain a basic and essential tool across a broad range of scientific domains.

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[15] I thank Kino Zhao and Jun Otsuka for helping me realize the significance of Niles's second objection.

[16] See Spanos (2022) for a more detailed discussion of major paradigm shifts in statistics. Following Spanos, I adopt the well-known concept of "paradigm" to describe progress in statistics (although my adoption of this term doesn't imply that I agree with Thomas Kuhn's original view on scientific change).

Table 1. Major events in the history of statistics and causal inference

Year	Event
1889	Galton invented the notion of correlation.
1892	Pearson published 1st edition of *The Grammar of Science.*
1896	Pearson published first paper on correlation.
1901	Galton, Weldon, Pearson founded *Biometrika.*
1911	Pearson published 3rd edition of *The Grammar.*
1918	Wright published his first paper on path analysis.
1922	(1) Fisher laid the foundations for model-based statistics.
	(2) Niles criticized path analysis.
1925	Fisher published *Statistical Methods for Research Workers.*
	…
1966	Path analysis was rediscovered by sociologist Duncan.
1975	Li published *Path Analysis: A Primer*.
	…
1993	Spirtes *et al.* published *Causation, Prediction, and Search*.
2000	Pearl published *Causality*.

After achieving methodological successes in correlation analysis, Pearson seemed to come to the realization that a good way to defend the paradigm of correlation analysis was to take issue with "the old idea of causation" and show that it could be replaced with correlation. This defence was provided in *The Grammar* where Pearson critically examined the philosophical basis of causation. Notably, in the first two editions of *The Grammar* published in 1892 and 1900, Pearson had not yet explicitly formulated the idea that causation should be replaced by correlation, even if he had been working on correlation since 1896. That is, he did not propose those provoking arguments against the role of causation in statistics until the third edition of *The Grammar* (published in 1911), in which Pearson added a new chapter titled "Contingency and correlation: the insufficiency of causation". His skepticism about causation reached its peak in this new chapter, especially in his famous claim that "we have to replace the old idea of causation [with correlation]." This suggests that Pearson's arguments against causation in the 1911 edition were more like afterthoughts or reflections on his earlier works on correlation. It is therefore fair to say these arguments were invoked to further defend and entrench the paradigm of correlation analysis.

Going against the established Pearsonian paradigm may at least partially explain the initial lack of enthusiasm for Wright's path analysis. Wright was too ahead of his time. His method requires the postulation of a data-generating mechanism whereas the shift from

Pearson's positivist paradigm to Fisher's (1922, 1925) model-based paradigm had not yet been initiated, not to mention established.[17] Indeed, the merit of path analysis would have been better appreciated had model-based statistical thinking already been well-entrenched when path analysis was invented. In contrast to Pearson's positivist statistics, Fisher's model-based statistics permits the postulation of unobservable data-generating processes, specified by probabilistic models; importantly, such probabilistic models incorporate both domain-specific substantive information (including causal information) and domain-general statistical information (including probabilistic assumptions about the stochastic process generating the data) (see Spanos, 2006, 2022).[18] Of course, some statisticians may insist that Fisher's model-based statistics does not directly lend support to the legitimacy of path analysis. Still, it is undeniable that model-based statistics provides a more suitable framework for path analysis than does Pearson's positivist statistics since the latter rejects the postulation of unobservables altogether.

However, at the same time, by postulating a data-generating mechanism behind observed data, the model-based paradigm faced the challenge of statistical model specification (which we shall return later in the paper). Naturally, the idea of statistical model specification was not introduced to statistics until Fisher (1922) (cf. Lehmann, 1990). In hindsight, it is clear that in adopting a model-based perspective, Wright's path analysis faced challenges regarding the (empirical) validity of path models. These challenges, however, were not evident to Pearsonians precisely because the latter lacked this model-based perspective. However, I shall argue in section 7 that, behind Pearson's philosophical skepticism about data-generating mechanisms, there could have been *implicit* empirical concerns about the validity of path models, especially regarding those idealizing assumptions made by the models.

In the following three sections, I will discuss three interrelated factors that may have contributed to the delayed progress in causal inference in the early 20th century:

— Causal methods require *unconventional formalisms* (e.g., path diagrams). The representational and inferential power of the path diagram was underestimated by Wright's contemporaries. This underestimation was not only because statisticians were not yet ready to adopt a model-based perspective on statistics but also because Wright's path diagrams lacked mathematical rigour.

— Classical deterministic microphysics had shaped the philosophical notion of causation at that time. But statistical causal inference in higher-level sciences requires a quite different notion of causation (e.g., causation is directional; causes make partial contributions to an effect). Unfortunately, the *domain-specificity* of causation was not yet well-aware by 20th-century scientists and philosophers.

[17] I do not deny that model-based statistical thinking existed to some extent before Fisher (1922). The idea of a statistical population can already be found, for instance, in Student's (1908) t-Tests paper. But it is fair to say that it was Fisher (especially Fisher, 1925) who laid a solid and rigourous foundation for model-based statistical inference.

[18] As Spanos (2006, p. 99) puts it, empirical modelling in the sciences often "involves an intricate blending of substantive subject matter and statistical information"; importantly, the assessment of substantive adequacy may often "take the form of applying statistical procedures within an embedding statistical model".

— In addition to philosophical objections to data-generating mechanisms, there could also have been (implicit) concerns about the *empirical validity* of the (probabilistic and causal) assumptions made in specifying these mechanisms. These empirical concerns (which can be articulated and addressed only from a model-based perspective) may have prompted the Pearsonian to reject data-generating mechanisms altogether and opted for "assumption-free" statistics.

5. *The Analytical Power of Path Diagrams*

The formalization of causation and causal inference is challenging, not because it requires advanced mathematics but because it requires new formal apparatuses that cannot be directly found in mathematical textbooks. Furthermore, traditionally, scientists and philosophers of science assumed that scientific theories and inferences should take sentential forms (Griesemer, 1991). The role of non-sentential scientific languages, such as diagrams, has been underestimated (cf. Abrahamsen & Bechtel, 2015). However, for causal inference, conventional formalisms such as algebraic equations and probability calculus alone just cannot do the job; in particular, it is difficult to represent the direction of causation using algebraic equations and probability calculus without introducing causal arrows.

Notably, unlike diagrams used in descriptive statistics (such as histograms), the path diagram, as a type of *causal/explanatory* model, does much more than visualizing what has already been contained in data or equations. As Griesemer (1991) puts it, path diagrams "add analytical power to path analysis beyond what is supplied by linear equations". Pearl and Mackenzie (2018, p. 77) explain that the path diagram "is a powerful computational device because the rule for computing correlations involves tracing the paths that connect two variables to each other and multiplying the coefficients encountered along the way." Wright's new formalism, therefore, marks a stark departure from Pearson's descriptive, model-free statistics. One may contend that the path diagram can simply be interpreted "as a descriptive device to summarize observed correlational patterns" rather than as a causal model.[19] This interpretation, however, cannot explain why Wright used one-direction arrows in path diagrams. Given that correlations have no direction, such arrows are not an economic way of summarizing correlational patterns. Interestingly, as we shall see below, Niles omitted all *arrowheads* in the "path diagrams" he drew in his paper, which indicates that Niles had (mistakenly) interpreted path diagrams as a mere descriptive device, thinking that these arrowheads were dispensable.

How is the path diagram "a powerful computational device"? Essentially, the function of a path diagram is to help scientists determine how correlations in the data can be explained by causal patterns in the target system so as to derive the equations needed for further inference. More specifically, the relationship between causal patterns encoded on the diagram and correlational patterns in statistical data can be captured through a few graphical rules —what Wright calls *tracing rules*.[20] Earlier in section 2, we saw that to explain the

[19] I thank Jun Otsuka for suggesting this point to me.

[20] For example, Wright (1934, p. 163) formulates the tracing rules as follows: "In tracing connecting paths it is obvious that one may trace back along the arrows and then forward as well as directly from one variable to the other (perhaps through intervening variables) but never forward and then back."

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correlation between two variables, we had to trace all relevant causal paths responsible for the correlation. This task can be challenging if we are dealing with a large causal network; tracing rules helps ease the problem. In fact, these tracing rules play a similar role as those used in graphical causal modelling for adjusting for confounding (such as the backdoor and front-door criteria; see Pearl, 2000). Thanks to the theory of graphical causal models developed by Spirtes *et al.* (1993) and Pearl (2000), we now see why the path diagram (or similar formalisms) was a powerful analytical tool.

Nevertheless, in the early 20th century, graph theory had not yet been applied to empirical sciences, so Wright could not have known that the path diagram could be transformed into a rigorous mathematical formalism (i.e., directed acyclic graph). Instead, he introduced and used path diagrams mainly on *intuitive* grounds. His path diagram as we saw earlier (in Figure 1) is clearly not yet a serious mathematical model but looks more like a picture used to aid the reader's understanding of the topic. Wright probably thought that an intuitive and informal explanation of how path diagrams work sufficed to justify their usefulness. But Wright's contemporaries did not seem to share his intuition. The lack of a solid mathematical foundation may have made it difficult for others to see the distinct value of his new formalism. Given also that Pearson and his followers believed that statistical analysis did not require the specification of a data-generating mechanism, people likely thought these path diagrams were merely descriptive devices (just like histograms). This further prevented others from appreciating the power of path diagrams.

Without seeing path diagrams as an integral part of path analysis, one cannot genuinely understand the logic of the method. This led to some intriguing misunderstandings of path analysis. In Niles's (1922) reply to Wright, Niles gave some counterexamples to path analysis by "demonstrating" that, following Wright's method, we might obtain mathematically impossible values of path coefficients. The problem with Niles's "demonstration" is that in these counterexamples, Niles carelessly —or maybe even intentionally— omitted all the *arrowheads* in the path diagrams he drew, as if the arrowheads were dispensable. However, without these arrowheads, a path diagram no longer represents a *causal* structure, and we can no longer correctly write down the equations and calculate the path coefficients. This was why Niles got absurd values for path coefficients from his calculation (see Niles, 1922, pp. 269-271). What makes the story even more interesting is that in a later response to Wright, Niles (1923, p. 257) explains that "the omission of arrowheads in the diagrams in my [1922] paper was a draughtsman's error". This excuse is not very convincing. Niles's calculation unambiguously shows that he did not understand how the diagram worked, how those equations were supposed to be derived from the diagram by applying tracing rules, and especially, what role the arrows played in the diagram. That is, Niles failed to see that path diagrams played a substantial, analytic, and explanatory role in statistical analysis.

A similar misunderstanding also happened to Henry Wallace. Despite his positive attitude towards path analysis and Wright's careful explanations in the correspondence, Wallace failed to see how the method was different from multivariate regression (see Provine, 1989, p. 148). A reasonable explanation for this confusion is that, similar to Niles, Wallace also failed to see that the path diagram was an essential part of path analysis and that it played an explanatory rather than merely descriptive role in path analysis. Had he understood the use of path diagrams, it would be immediately clear to him how the two methods differ: one involves a pre-specification of a causal model, whereas the other can be done by fitting equations to data without bothering to think about the data-generating mechanism.

It must be emphasized that I am not blaming Niles or Wallace for failing to understand path diagrams. I am merely stating that Wright's contemporaries were not yet prepared to recognize the analytic and explanatory power of path diagrams as a new type of explanatory statistical model (in conjunction with simultaneous equations). This is understandable: although Wright rightly pointed out the importance of substantive subject matter knowledge in statistical inference using path diagrams, Pearson and his followers were not yet ready to accept this stark departure from their own model-free paradigm.

6. Conceptual Barriers to Statistical Causal Inference

In addition to Pearson's positivist refutation of causation, there were also other conceptual or ontological barriers to causal inference back then; I shall discuss two in this section. One concerns the direction of causation, and the other concerns causal interpretations of imperfect correlations. Both have to do with the dominance of classical physics in shaping scientists' and philosophers' thinking about causation back in the early 20th century.

In the previous section, I argue that scientists in Wright's time were not ready to appreciate the power of path diagrams; this is shown in the omission of arrowheads in path diagrams drawn by Niles. There is yet another potential reason, albeit a *conceptual* (or ontological) one, why the Pearsonian might have disliked the path diagram: for Pearsonians, there was simply no such thing as the direction of causation. Recall that in Pearson's positivist philosophy, causation is just perfect correlation, and correlation has no direction; neither does causation. More importantly, the direction of causation derives from the direction of time. But classical (micro)physics has "proven" that time is symmetric which points to the conclusion that the direction of causation is an illusion; the arrowheads in the path diagram represent nothing in the world. Regarding this skepticism towards the direction of causation and time, Wright (1934, pp. 175-176; emphasis added) gives the following brief but intriguing response:

> Some authors (Pearson, Niles) ... [hold the view] that direction in time is of no significance, and indeed G. N. Lewis has recently argued for the complete symmetry of the physicist's time. *The common sense view that direction in time is a basic perception is not without support, however.*

Unlike Pearson and Niles, Wright adopts a commonsensical view of the direction of time and causation. This echoes my earlier claim that Wright's path analysis, including the way he uses path diagrams, relies on an intuitive and pragmatic understanding of causation. On this pragmatic view, even if classical physics shows that time has no direction at the microscopic level, this does not prove that the idea of time direction in common sense is a mere illusion. For macroscopic sciences such as genetics (or even in macroscopic physics), time direction can still be in some sense real.

Nevertheless, we should be cautious not to (mis)interpret history through a contemporary lens. Wright's view about causal and temporal directions was far from being a consensus among philosophers in the early 20th century. Back then, philosophers and philosophically minded scientists were more likely to defer to (microscopic) physics without being aware of the disunity of science. In particular, in *The Grammar*, Pearson explicitly supported the unity of science —specifically, the continuity of physical and biological sciences. Pearson (1911, p. 116) claims that "[t]he difference between the two branches of science is rather quantitative than qualitative; that is, the descriptions of mechanics are simpler and more general than those of biology." If this is the case, there is no reason to treat causation in physics and causation in special sciences as conceptually different. Therefore, the idea that causation in special sciences is a different notion than causation in physics (cf. Hitchcock, 2007) is very unlikely to be anticipated by the Pearsonian. For Pearson, if the direction of time had been proven to be illusory in microphysics, this would imply that the direction of time and causation had lost its scientific grounds altogether; the direction of time and causation, despite being useful in common sense, became unscientific.

Let us now turn to another important conceptual disagreement between Pearson and Wright on the notion of causation: Pearson takes the cause-effect relationship to be a *deterministic* relation, with an earlier state of a system completely determining a later state, whereas Wright understands the relationship between a cause and an effect as partial and statistical (whose strength/intensity is measured by path coefficients).[21] Recall that Pearson thought that causation was, by definition, *perfect* correlation, with a correlation coefficient of 1. Moreover, in *The Grammar* (1911, p. 174), he contrasts causation with contingency: causation, by definition, implies some sort of "necessity".

In comparison, the concept of causation presupposed in path analysis is "imperfect" or partial. An effect variable Y may have multiple (partial) causes $X_1, X_2, ..., X_n$, each of which makes partial contributions to the effect (e.g., in Wright's guinea-pigs example, we saw that the pattern of coat colours had three distinct contributing causes: genetic constitutions, environmental factors, and developmental irregularity). On Wright's use of the term, we are allowed to say that "X_1 is a *cause* of Y" even if X_1 alone does not determine the value of Y. For Pearson, the relationships between each of these factors and the coat colour pattern should be described not as causal relations but as (imperfect) correlations. He failed to see that we could causally interpret these imperfect correlations, given his preoccupied notion of causation as determination or necessitation. Note that one could, of course, designate a collection of causes $\mathbf{X} = \{X_1, X_2, ..., X_n\}$ as a (deterministic) cause of Y since there is now a perfect correlation between $\mathbf{X}$ and Y (for the sake of argument, let us assume that $\mathbf{X}$ is a well-defined cause-variable). But Pearson would simply respond that labelling this relationship as "causal" adds no substantive insight into the relationship.

Causally interpreting imperfect correlations is no news today, but it was so a century ago. In fact, other popular philosophical theories of causation at that time, such as Aristotelianism (i.e., causation consists in "inherent necessity" or "power") and Humeanism (i.e., causation is just constant conjunction or regularity), also presupposed some sort of determinism or necessitation. The deterministic conception of causation was also grounded in the success of classical physics, which describes the causal structure of the world in terms of deterministic dynamical laws expressed by mathematical functional relations (e.g., Ha-

21 Note that we are talking about the relationship between *one* cause and its effect, which is represented by an arrow in a path diagram. Wright, of course, assumed that if we took *all* the causes of an effect together, they would fully determine that effect.

miltonian equations). In this picture, the earlier state of a (closed) system completely determines a later state. The idea of a partially contributing cause plays no role in classical microphysics.

It is worth noting that the Pearsonians were not alone in failing to anticipate that special sciences could make good use of a notion of causation that differed significantly from that in microphysics. During that era, many great thinkers made similar presumptions about a unified notion of causation in the sciences, without even realizing that this was a substantial assumption. For example, Russell (1912) argues that because the concept of causation was rarely used in mature sciences such as gravitational astronomy, the idea of causation is "a relic of a bygone age." Russell does not distinguish between causation in physical and special sciences in his argument; he is talking about "science" in general. Russell was not alone. Norman Robert Campbell and Pierre Duhem, both physicists and philosophers of science, also rejected the notion of causation as unscientific, following a similar reasoning. Given this ideological background, it was indeed challenging for any thinker at that time to undertake *a conceptual shift* from a necessitation understanding of causation implied in classical microphysics to a statistical notion of causation suitable for higher-level sciences.

7. The Empirical Validity of Path Models

When scientists choose a statistical method, in addition to its mathematical rigour and conceptual tenability, they are also concerned with whether the method is *empirically* valid and reliable. In this section, I argue that the empirical reliability of path analysis could have been questioned —albeit implicitly. Recall that path analysis requires causal postulates and idealizing assumptions; these postulates and assumptions can be difficult to verify, especially in complex biological and social systems. Concerns of this type may have discouraged scientists from embracing path analysis; such concerns nevertheless could not be made explicit, for reasons I shall explain soon. To be clear, arguments in this section are to some extent speculative since it is difficult to find textual evidence of any scientists raising these concerns. However, as we shall see, concerns of this type have been more or less implied in Niles's worries about the legitimacy of path diagrams.

Generally speaking, it is not uncommon in the history of science that philosophical or ontological concerns raised by scientists were entangled with empirical ones. For instance, behind philosophical objections to chemical atomism, we find latent empirical concerns about the lack of evidence for the existence of atoms. Once such empirical concerns were alleviated, scientists started to embrace the existence of atoms.[22] Retrospectively, we may conjecture that many scientists rejected chemical atomism not (just) on philosophical grounds but (also) because of the lack of empirical evidence for atomism. It is reasonable to suspect that something similar happened to the initial lack of enthusiasm for path analysis: behind Pearsonians' positivist skepticism about path analysis, there might have been empirical concerns about the reliability of the new method as well.

[22] According to Chalmers (2019, Introduction section, para. 4), "[a]ny opposition from scientists that remained was removed by Jean Perrin's experimental investigations of Brownian motion."

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Causal relations in high-level sciences can often be complex and obscure. Wright (1921, p. 557) himself acknowledges the complexity of biological causation at the very beginning of his paper: "In the biological sciences, especially, one often has to deal with a group of characteristics or conditions which are correlated because of a complex of interacting, uncontrollable, and often obscure causes." Therefore, granted that it makes sense to talk about causal mechanisms behind data, scientists back then were still entitled to worry about the empirical validity of the path models, including both the validity of the posited causal structures and that of the statistical models (i.e., linear equations) in which the causal structures are embedded (for a contemporary formulation of this concern, see Spanos, 2022, sect. 5.1).

We saw in section 3 that Niles was skeptical about using a path diagram to "truly represent" the causal mechanism behind data; there, I emphasized that his concern was philosophical or conceptual rather than empirical. Here, I want to suggest that this does not prevent Niles from having implicit empirical objections to path analysis in his mind. That is, the Pearsonian could have also objected to Wright by taking a step back and arguing that even if it made sense to talk about such a causal mechanism and isolate it from its "unmanageable" causal history, still, it was too difficult to guarantee that we had reliable knowledge about the underlying causal mechanism. Nor did we have ways of assessing whether the statistical properties of such causal mechanisms were adequately captured by the equations Wright used. All of this, of course, is speculative; the Pearsonian did not actually pose empirical objections to Wright. But it is instructive to consider why they did not.

One possible reason why the Pearsonian did not explicitly bring up empirical objections to Wright is that these empirical concerns might have been pre-empted and overshadowed by their philosophical objections to path analysis: if philosophically speaking, there is no point in talking about the actual "data-generating mechanisms" behind the data, then no further empirical questions would arise. By rejecting the commitment to any "data-generating mechanisms", Pearson no longer needs to worry about the empirical validity of the assumptions made in path analysis. More importantly, these empirical concerns could not be clearly spelled out, let alone properly addressed, before the emergence of Fisher's model-based statistics which explicitly raised the problem of *model specification*. According to Spanos (2006, p. 100; emphasis original), "statistical model specification refers to the choice of a model (parameterization) arising from the probabilistic structure of a stochastic process ... that would render the data in question ... a *truly typical realization* thereof." The misspecification of path models can lead to erroneous causal inferences; however, for understandable reasons, Wright did not provide a means of addressing or assessing this problem.

We might even speculate that Pearsonians' adoption of a positivist philosophy of statistics was a strategic move to circumvent empirical concerns about the specification of data-generating models, given that a better way of addressing these concerns was unavailable. Indeed, even today, causal complexity poses serious trouble for statistical model specification. By avoiding causation and sticking to summaries of "sense impressions" (i.e., correlations in observed data), the Pearsonian could steer clear of the trouble of causal complexities and the challenge of characterizing them statistically. In particular, note that Pearson's positivist philosophy exempted Pearsonians from justifying the simplifying assumptions (e.g., linearity) in a model-free correlation analysis. For positivists, these assumptions need not be veridical, and therefore, they do not even idealize anything in the world. After all, in Pearson's (1911, p. 112) view, a scientific law is just "a brief description in mental shorthand of as wide a range as possible of the sequences of our sense-impressions." In this case, simplifying assumptions in descriptive statistics could be justified *instrumentally* on the grounds of their ability to usefully summarize data. This is surely not a permanent solution to the problem of causal complexity and may sound like ostrichism; however, I would like to emphasize again that we should avoid interpreting history through a present-centred perspective.

In comparison, Wright's bold choice of positing a data-generating model would have raised the following concern: were those causal postulates and model assumptions in path analysis empirically reliable? Recall that in the coat colour pattern example, Wright assumed background knowledge about the mechanism of inheritance, which came from his understanding of Mendelian genetics. But was Mendelian genetics considered reliable background knowledge in the early twentieth century? Not everyone would agree. In fact, Pearson was highly skeptical of Mendelian genetics, not only because he found the use of unobservable entities such as 'factors' (genes) unacceptable but also because Mendelism was considered inconsistent with Darwinism whereas Pearson himself was a convinced Darwinist.[23] Wright also believed that Mendelian inheritance was linear (see Provine, 1989, p. 139), which has been found not to be the case as additional complexities in the mechanism of inheritance have been revealed. Moreover, in Wright's path diagram, genetic factors (H) and environmental factors (E) are independent or noninteractive causes of coat colour (O), which means that the effect of H on O would remain the same when the environment changes. The existence of gene-environment interaction implies that this is wrong; in different environments, the effect of H on O might be different (see Ottman, 1996). When gene-environment interaction is considered, Wright's diagram is no longer an accurate representation of the underlying causal mechanism.

To be clear, my point is not that Wright should be accused of not foreseeing things that were discovered decades after his invention of path analysis. Instead, my point is that, as a matter of fact, it was very difficult for Wright to guarantee the empirical reliability of path analysis because of the lack of reliable background causal knowledge back in the 1920s. This is also in line with Fisher's (1925) later insistence on the necessity of randomized controlled experiments in making causal inference; the consideration here is that the reliability of randomized controlled experiments can be guaranteed —without relying on substantive subject matter knowledge— as long as the researcher conducts the experiments properly. This has motivated Fisher to see "randomisation and experimental control as the only reliable way of obtaining causal knowledge" (Shipley, 2016, sect. 3.2). Again, this shows that there could have been legitimate empirical concerns about the validity and reliability of path analysis because of the method's heavy reliance on reliable background causal knowledge. This point has been well made by Shipley (2016), so I will not reiterate it here.

[23] As a Darwinist, Pearson believed that evolution was continuous, while Mendelian genetics at his time held that evolution was discontinuous. For more, see Provine (2001, p. 33ff.).

8. Conclusion

In an attempt to provide a rational explanation for the delayed appreciation of path analysis, this paper uncovers some philosophically intriguing issues concerning the method and its history. This study is a demonstration that the history and philosophy of statistical causal inference are so intertwined that it is impossible to discuss one adequately in separation from the other. In view of this, I suggest that we take an integrated HPS approach to the history of causal inference. As this paper endeavours to show, taking an integrated HPS approach will help historians better understand the history of causal inference, especially regarding conceptual issues in causal inference. At the same time, I believe that a close examination of the history of causal inference will also teach philosophers of causation valuable lessons. While this paper does not demonstrate the latter in detail, here is a quick example of how this might work: the history of path analysis can make an excellent case study for philosophers of science to investigate the role of ontological or philosophical assumptions in scientific and methodological practices.

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