"Fleming Leapt on the Unusual Like a Weasel on a Vole"[1]

Challenging the Paradigm of Serendipity in Science

Abstract

This paper describes and offers a corrective for problematic implications of classic paradigms of serendipitous discovery in science, such as the narrative that Fleming discovered penicillin. As usually told, an individual (Fleming) makes an isolated observation by chance (of mould in his petri dish) which leads inevitably to a major discovery. Such stories leave out important interactions—emerging networks—that were equally important. Further, they perpetuate the mistaken belief that the epistemology of discovery is mysterious. By reforming the paradigm, I provide a social-epistemological grounding for the role of chance in science, and for the development of a skill-based epistemology of discovery.

1. Introduction

What is the role of chance in scientific discovery? And, more to the point, if chance plays a key role in scientific discovery, what room is left for reason? These are grounding questions in the debates, for instance, over whether there is a distinction to be made between discovery and justification in science, and whether innate genius must play a role in discovery or if there exists some method that can (in principle) be taught to anyone (Nickles 2009). While the role of chance has been discussed throughout the history of science, it has resurfaced in recent debates over how science should be funded, and particularly in the field of biomedical research. The word serendipity, for example, has become increasingly prevalent in the literature of discovery and innovation, as a way of categorizing unplanned and unanticipated yet often especially valuable discoveries. Empirical work is being done to define what

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[1] Quoted from an article by Rob Dunn on the website Smithsonian.com, "Painting with Penicillin: Alexander Fleming's Germ Art", published July 11, 2010 (see references list for link).

kinds of discoveries count as serendipitous, how often these kinds of discoveries occur, and how they contribute to the progress of research and science (eg. Sampat 2015; Yaqub 2018). In the field of innovation management, new theories about the kinds of organizations, institutions and networks that give rise to serendipity most often and effectively have emerged from empirical studies of past successes and failures (eg. Garud 2018). These studies have shown that serendipity takes many forms and that a network is required to recognise the value of potentially serendipitous observations and to take them up into a process of discovery (Copeland 2017).

Despite these new tools, however, a more classic paradigm of serendipitous discovery continues to prevail—that chance discoveries are best depicted as a specific observation, often made by a single individual, who happens upon something that is later revealed to be especially valuable. The dominance of this paradigm is captured by the almost universal appeal to Alexander Fleming's discovery of penicillin as exemplifying this kind of discovery. For instance, in a recent editorial in *Nature* announcing the launch of a European Research Council five year grant to measure the rate of serendipity in research, a picture of Fleming, alone in his lab and looking carefully at a specimen in a petri dish, looms large ("The Serendipity Test", 2018). While a considerable body of work in the history and social studies of science, as well as the recent empirical work in diverse fields mentioned above, demonstrate that this image of discovery as occurring in a singular moment and made by a single individual is rare, if not simply false, that paradigm continues to act as a touchpoint in discussions about the role of chance in the progress of science. This paper seeks to call attention to certain implications of the continuing use of that paradigm that are not yet looked to by the empirical or historical analyses with due attention. I argue that the continuing popularity of the isolated individual/singular event paradigm of serendipitous discovery tends to obscure the epistemology of discovery and to impede discussion about the importance of diffusing epistemic credit for discovery among members of the contributing network.

Serendipity, as defined by the inventor of the term, Horace Walpole, represents an integration of both “accidents and sagacity” in the moment of observation (Walpole, 1960, p. 408); that is, it includes both chance *and* wisdom. I focus in my analysis on the common narrative, that Fleming serendipitously discovered penicillin, primarily because it is mentioned in practically every instance scientific discovery is discussed.[2] Fleming’s discovery is remarkable, both for its humble beginnings in a ‘lucky’ observation, and for its famously progressive end, the alteration of medical practice for the better across the board with the introduction of a life-saving and readily available therapeutic option, the antibiotic. It is frequently offered as an exemplar for the inherent role that chance, or luck, plays in scientific discovery. However, Fleming was not awarded a share of a Nobel Prize for being merely lucky, nor has he been lauded as a great discoverer in history for merely being in the right place at the right time. Rather, the use of this example is meant to convey that serendipitous discoveries require both the wisdom of a particular individual as well as a chance occurrence. However, by giving primary, or even sole credit to Fleming as the discoverer of penicillin, in its role as a paradigm of this kind of discovery, I argue, it leads to the neglect of both the epistemic skills needed for the recognition of potential value and the networks required for such discoveries to occur in science.

One common source of the belief in the importance of serendipity to science has been Vennevar Bush and his 1945 report to President Roosevelt, *Science, the Endless Frontier*. In that report, Bush claimed that “Discoveries pertinent to medical progress have often come from remote and unexpected sources, and it is certain that this will be true in the future” and that “Progress in the war against disease results from discoveries in remote and unexpected fields of medicine and the underlying sciences.” Because serendipity often leads to such valuable, practical outcomes, then, Bush argues that “Scientific

[2] Note that within the scope of this paper I cannot hope to give an exhaustive, historically precise account of this discovery process or even of Fleming’s role. For interested readers, there exists an extensive and thorough literature on the matter, with several biographies, histories and polemics that cover all possible angles of the case. The references listed at the end of this paper represent a good starting point.

progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown." In other words, individual scientists must not be hindered by government or funding bodies that seek to direct their work. Rather, the natural outcome of curiosity-driven science is discoveries. Further, scientists who are allowed to freely pursue "subjects of their own choice" can (individually) follow up on the unexpected to make the kinds of serendipitous discoveries that have value to society as a whole. Thus, the role assigned to chance, in this case a legitimate driver of scientific curiosity, has direct implications for how science ought to be organized—and funded.

But the implied dichotomy between curiosity-driven and applied science, in terms of their susceptibility to chance discovery, has been demonstrated to be false (e.g. Sampat 2012; Ruphy & Bedessem 2016)[3]. Further, serendipity seems to offer a category that straddles the distinction between reason and chance, insofar as its use as a category is meant to highlight the need for wisdom as well as luck for so-called discoveries to occur. However, through the use of paradigmatic examples such as Fleming in his lab with his petri dish, a false dichotomy is nevertheless perpetuated. The wisdom—or sagacity in Walpole's definition—at hand tends to be seen as epistemologically impenetrable. Like the context of discovery itself has been, it is seen as the territory of psychology or creativity-theory, rather than philosophy. Thus, the particular wisdom of discoverers such as Fleming, who is said to have seen a potentially valuable discovery in a chance observation that no one else did or would have, is classified as 'genius' rather than the outcome of intentionally developed, epistemic skills.[4] The distinction, then, between such discoverers and others, many of whom may have played an equally essential role as

[3] For a direct response to Bush that called into question the dichotomy between basic and applied research more generally, see Stokes (1996). For an extended discussion of the interaction between categories such as 'pure' and 'applied' science in history, see Roberts, Schaffer and Dear (2007).
[4] The reader may here recall the famous dictum from Louis Pasteur, often quoted as "chance favours the prepared mind"—later in this paper I address the problem of what counts as preparedness, and distinguish between such preparation and what I consider to be epistemic skills.

members of the network that enabled the discovery process to follow that observation, is read as an epistemic talent, given to an individual. The resulting individualization of the discovery itself in turn centralizes the credit given for that discovery, to the detriment of both our understanding of how the discovery occurred and of who contributed what to its occurrence.

2. The Usual Story

Definitions of serendipity are notoriously ambiguous (p. Merton & Barber 2004; Catellin & Loty 2013, p. 33). Walpole himself refused to give a concrete definition, but instead preferred to offer the fairy tale of the Princes of Serendip as illustration of the concept in use in his letter to Horace Mann, writing, “you will understand it better by the derivation than by the definition” (Walpole, [1754] 1960, p. 408). In some accounts, the role of the intellect is emphasised, and serendipity is described as an individual’s ‘capacity’ or ‘ability’ for discovering things they were not actively searching for (Austin 2003; de Rond 2014). In other cases, serendipity is defined primarily by the presence of chance, and its frequency illustrates that even the most skilled of scientists can credit some of their greatest discoveries to a fortunate turn of events (Campanario 1996; Friedel 2001). Still others see serendipity as having numerous forms, arising as easily from jokes and play as from accidents in the lab (van Andel 1994). In most cases, however, the tendency is to depict serendipitous discoveries as occurring at a specific moment in time, and to be the direct result of someone’s perception or intellect.

This focus on the individual and the event lies in contrast to a considerable body of work on the nature of discovery in science that instead emphasises the role of the social and epistemic context of a discovery process over and above the unique moment in time labelled its beginning. Simon Schaffer (1996), for instance, calls our attention to the fact that the ‘heroic’ model of discovery is a retrospective affair: the author of a discovery is only recognized as such *post hoc*, once the scientific community has judged both them and the discovery they are being credited for as worthy of such recognition. In

Thomas Nickles' terms, "by telescoping historical development, scientists whiggishly invest these charmed cases with far more meaning than they originally possessed" (Nickles 1997, p. 128). The Fleming example is no exception. It is well known that, rather than the story of a singular, private, moment in time, the discovery of penicillin actually involved a considerable number of researchers, and a process of investigation extending over time and across continents (e.g. Bud 2009).

And yet, the narrative persists: that *Fleming* discovered penicillin in his lab one September afternoon, after returning from vacation to tidy up some petri dishes he had left to fester, and upon luckily and wisely recognizing the value of a contaminated sample that—were he another person, or were it another day—would otherwise have been discarded. Textbooks frequently assign the credit for the discovery of penicillin to Fleming alone, despite the efforts of not only his co-winners of the Nobel Prize for the discovery, Howard Florey and Ernst Chain, but many others (Slowiczek & Peters, n.d.). Similarly, popular references to the discovery (of which there are countless) generally elevate the moment of Fleming's observation to a key moment in the history of medicine and science, lauding Fleming for having the right personal traits—including his supposed sloppiness—necessary for the discovery to occur at all. For instance, consider the following versions of the tale:

> Sir Alexander Fleming 1881-1955 Discovered PENICILLIN in the Second Storey Room Above This Plaque (The plaque outside St. Mary's in London)

> For thousands of years men looked at the cryptogamic mould called Penicillium notatum, but Dr. Fleming was the first to see its cryptic meaning. His discernment, restoring to science the creative vision which it has sometimes been held to lack, also restored health to millions of men living and unborn. (n.a., TIME Magazine in 1944, in the issue Fleming was depicted on the cover)

> Although penicillin was given to the world as a therapeutic remedy by the brilliant work of Florey and his colleagues at Oxford, Fleming's name will always be associated with its discovery, and it was the discovery of a man working alone. (n.a., 1955 obituary, British Medical Journal, p. 734)

> The circle of goop was a fungus. In that chance moment, Fleming discovered the antibiotic properties of penicillin, properties that would change the world…Fleming leapt on the unusual like a weasel on a vole and in doing so discovered what others had walked right past or even thrown, disgusted, into the trash. (Dunn, Smithsonian.com, 2010)

> How Being a Slob Helped Alexander Fleming Discover Penicillin (Latson, TIME, 2015)

This narrative is problematic for several reasons. First, as I have already suggested, it conveys a problematic depiction of serendipitous discovery. Second, it conveys an incorrect description of this particular discovery—as others have also argued, Fleming did not by himself discover the therapeutic intervention, penicillin the antibiotic, in the moment of observation nor in his subsequent (brief) investigation into the properties of the mould in his petri dish. And third, as I will expand upon in later sections, the use of this particular narrative of this discovery as a paradigm of serendipitous discovery in science and medicine has specific implications. Narratives about scientific discoveries ought not to be taken lightly: they have an impact on how we as a society perceive both the practice of science and the nature of 'great' scientists, and consequently on how we assign credit to (and reward) the discoverers that we do recognize.

The narrative illustrated by the quotations about Fleming and penicillin above reflects a classic approach to discovery in science (Chen 1992, p. 245). Steve Woolgar captures this classical approach well: "The metaphor of scientific discovery, the idea of dis-covering, is precisely that of uncovering and revealing something which had been there all along. One removes the covers and thereby exposes the thing for what it is; one pulls back the curtains on the facts" (Woolgar 1988, p. 55). Indeed, Fleming often deferred the responsibility for his own observation to fate or even to penicillin itself, saying, for example, in the speech he gave at the banquet held in honour of his receiving the Nobel Prize, “I am here because of penicillin” (Fleming 1945). Serendipitous discoveries, or discoveries that are ‘unsought,’ are a particularly dramatic case of the world imposing itself upon the investigator. Robert Merton formulated this as the “serendipity pattern,” which occurs when an “unanticipated, anomalous and strategic datum exerts a pressure for initiating theory” (Merton 1948, p. 506). In this formulation, the datum is what drives the discovery. Merton does allow for the observer to bring something “to the datum,” as he puts it, insofar as it is the observer who makes the strategic connection between that datum and what it means in relation to prevailing theory (p. 507). The datum, however, actively “exerts pressure upon the investigator” to take up a “new direction of inquiry” in response (p. 507). Like the classical approach, this concept of serendipity suggests that the world is revealed by the discoverer, whose principal contribution is her wise perception of the ‘true nature’ of an unexpected observation or event as a discovery (see also Verhoeven 2016, p. 14-15).

But while the mould (P. notatum) may have forced its way into Fleming’s laboratory through a stairwell from the asthma lab on the floor below, “All the same, the spores didn’t just stand up on the agar and say ‘I produce an antibiotic, you know’” (Fleming, quoted in Maurois 1959, p. 131). A distinction can be made between the content of Fleming’s chance observation, the potential value that Fleming saw in that observation that he deemed worthy of following up with investigations and a paper, and the content and value of the discovery with which he is being credited when someone says “Fleming discovered penicillin.” Rather than a moment of ‘eureka’ upon making what has been deemed a serendipitous observation in light of the exceptional value it is now known to have had, Fleming is reported to have said something more like “That’s funny” (Hare 1970 p. 65). Further, he was not alone in the lab, but shared the moment with at least one other person and then proceeded to discuss the interesting find with others around him. So the picture of Fleming, standing alone at his bench and gazing into a petri dish with profundity, leaves out both the content and the context of the moment as it happened.[5] As with other instances of serendipitous discovery, the point of origin consisted of an uncertain intuition about potential value (rather than certain insight into ultimate value), and involved several people in discussion.

What caught Fleming's attention was not the therapeutic potential of the mould in his petri dish, but more likely its similarity to a previous subject of interest, lysozyme, and its potential as a medium for isolating the bacteria he was already working with (Chen 1992). As Merton suggests, the strategic aspect of the serendipitous datum is contributed by the observing scientist: without the knowledge of the scientist who notices its relationship to prevailing theory, the datum would not exert the pressure toward new inquiry that it does. The category of serendipity specifically picks out situations in which the world and the scientist jointly act to make a discovery. But the strategic value of the chance observation of the petri dish recognized by Fleming was not the same as the value we retrospectively assign that observation, in relation to the impact of penicillin upon our world. Fleming saw the value of a substance with antibiotic properties in terms of its relation to his past research with lysozyme, and in relation to its usefulness for his future research, as a tool for isolating bacteria for which he wished to develop vaccines.

Thus, like other scientific discoveries, the 'discovery of penicillin' could be broken down into multiple steps or even distinct discoveries, each of a different kind and content. As Thomas Kuhn (1962) argues, discovery involves recognition both *that* something is, and *what* it is, and these may be two distinct stages in a complex discovery process (p. 762). Norwood Russell Hanson (1967) goes further, to break down the concept of discovery into several categories. As Hanson points out, emphasising the role of chance tends to have the effect of making discovery seem more mysterious than it is, and giving a

[5] The phrase 'rational reconstruction', normally used to describe what happens when scientists rewrite the story of their investigation into logical steps for publication, may be apt here.

monolithic description of discovery fails to encourage analysis or understanding (Hanson 1967 p. 322). In a footnote, Hanson calls for further conceptual clarity about the complex nature of discovery as a concept in light of debates about who should get the credit as discoverer. He gives the example of the discovery of the positron—"was it the theoretician, Dirac—or was it the experimentalist Anderson?" (Hanson 1967 p. 338). So long as it is assumed that a single discovery was made, and that credit must be assigned to a single individual, the complexities of discovery processes will remain underappreciated.[6]

2. No Discoverer is an Island

In the case of penicillin, it has been pointed to in many histories and biographies that more than just Fleming were involved, that many others played equally essential roles, and that the discovery process extended across time and place. These authors have sought to understand the complexity of this process by noting the distinct stages, steps and interactions that together led to what has been subsumed under the narrative of the discovery of penicillin. A recent example is the historical context provided by Robert Bud (2009), who describes the origin and evolution of penicillin as a brand. Bud places Fleming's observation within the broader context of his social scientific network, for instance by pointing out how Fleming distributed samples of the active substance he was able to isolate from the mould to labs around Europe and beyond—one of these labs being at Oxford, where it was later picked up by Florey and Chain (Bud 2009 p. 26-27). Wai Chen's (1992) chapter gives a narrower but no less contextual description of Fleming's laboratory, pointing to the role that his boss, Almoth Wright, played in redirecting Fleming's research away from further investigation into the therapeutic potential of penicillin. Bud (2009) also takes a step back from Fleming and describes in his book the stages of the

[6] Woolgar points out that this tendency to associate discovery with "instantaneous revelation or sudden perception" affects not only the popular narratives but the narratives that those who were involved in the discovery itself will tell, upon reflection on the events. "Participants who use the term in this metaphorical sense appear to become committed to discussion of a point event in time, rather than a process" (Woolgar 1976, p. 417). Woolgar laments that this makes the work of sociologists of science difficult, insofar as they then have to take a critical position toward personal accounts influenced by such assumptions about discovery.

discovery process that followed from Fleming, giving detail to the roles played not only by Florey and Chain, but by countless others such as Andrew Moyer, who made the essential change to corn steep liquor as a medium, thereby enabling the mass production of penicillin to take off in America. As Bud explains, to a great degree Fleming's fame as the discoverer of penicillin began during these early days of heady excitement over the therapeutic value of penicillin as an antibiotic drug and was the result of political ambition on the part of those who created and popularized the narrative in the first place.[7] Fleming's heroic role as discoverer of penicillin benefitted many who told the narrative, including those who had an interest in the financial future of St. Mary's, the hospital housing Fleming's lab at the time he made his now famous observation.

However, despite our awareness of the complexity of the discovery process, the numerous people involved in bringing Fleming's observation to fruition as a medical intervention, and the politicized origins of the narrative that credits him with the discovery as a whole, the paradigm, as I pointed out above, persists. This tendency toward giving credit to isolated individuals goes deeper than the commonly held 'priority rule', by which individual scientists compete to be recognized as the first to have made a discovery, and by which only one person or laboratory deserves the title of discoverer in each case. For one, the priority rule itself does not necessarily limit us to recognizing a single person in every case: the Nobel Prize, for instance, was shared among Fleming, Florey and Chain, who worked at different laboratories and who contributed to the discovery process of penicillin at different times. But the ideal of finding a single person to whom the credit for a discovery should go pervades the philosophy and epistemology of scientific discovery as well. Consider, for instance, Robert Hudson's (2001) critique of Kuhn's and Woolgar's historical, process-based approaches to scientific discovery.

[7] Thanks go to an anonymous reviewer for referral to this invaluable resource, the book by Bud. The same reviewer also led me to the chapter by María Jesús Santesmases (2010), in which she further illuminates the political nature of Fleming's historical role as a hero of modern medicine.

Hudson argues that Kuhn and Woolgar share a problematic approach to scientific discovery. That is, they deny that historians can pinpoint a single discoverer in many cases because discovery itself is an historical and social process and, thus, there is no single moment, nor single individual, to whom a discovery can be credited. For Hudson, this problematic assumption derives from the mistaken belief that what must be conceived at the moment of discovery is the very object that constitutes the discovery itself. This leads to controversy over who was the first to produce the correct conception of, for example, oxygen. As Hudson suggests, "Indeed, we could probably rule out everyone as the discoverer of oxygen, on this approach, since everyone, even today, has some false views about oxygen" (Hudson 2001, p. 76). Models of discovery that see the construction of a concept as taking place over time, then, will disagree about who made a discovery because they disagree about whether a particular individual had in their mind the correct concept. Thus, for Hudson, the solution is to offer a substantial account of what kind of conceptualization counts, when it comes to discovering.

I agree with Hudson that such approaches make it difficult, if not impossible, to delineate exactly which person should be credited with the discovery of a particular scientific object, theory, or technique. In contrast with Hudson, however, I argue that this is not where the problem lies: Hudson has misidentified the false dichotomy we need to tackle, as between being able or not to pinpoint the person and moment when a discovery is sufficiently conceived. The problem lies, rather, with the assumption that the discovery need be credited to a single individual at a particular time at all. We could, instead, conceive of discoveries as being credited to emerging networks of participants in the process. This way of assigning credit is only problematic if we insist on holding on to an 'aristocratic' view of discovery. In other words, if we believe that discoverers have a unique and inherited characteristic that marks them as distinct—wiser, in some way—than others.

Simply put, the isolated individual/singular event version of the discovery gives Fleming too much epistemic credit. Since a network including multiple individuals were involved in the discovery, to say that the discovery was made by "a man working alone" is patently false. Further, despite the fact that the political reasons for originally constructing the narrative of Fleming as discoverer have gone by the wayside, he is still lauded in the popular press as having distinct characteristics—his sagacity…or sloppiness—that mark him as a notable historical character and as exemplary of discoverers. The problem here isn't that Fleming in particular has been granted too many awards for his work as a scientist—that is a debate for the biographers, and one in which they have already engaged. I want to draw a broader point about the role of our paradigms of discovery as rhetoric, and their impact on our conception of scientific discovery and scientific practice. These conceptions in turn directly influence how we assign epistemic credit to discoverers in science.

The perpetuation of the narrative that Fleming alone discovered penicillin legitimizes a depiction of scientific discovery as the work of isolated individuals, who experience eureka moments of discovery, for which they deserve full epistemic credit. Serendipity stories belong to the category of "rhetoric of effortlessness" in science, as James McAllister (2016) describes it. This rhetoric is used by scientists to "heighten the objectivity and credibility of their findings" (p. 145). It does this by strategically suggesting that a discovery occurred without requiring considerable effort on the part of the scientist. The discovery thereby gains in perceived objectivity, as the narrative conveys the impression that the discovery was self-evident—discovered (or, 'dis-covered'), rather than constructed. The scientist, in turn, gains credibility for being so wise to see it (McAllister 2016, pp. 148*ff*). And, because it is unexpected, a serendipitous observation requires "a particularly acute observer" (McAllister 2016, p. 150).

Because this is a rhetorical strategy does not mean that it is necessarily manipulative or fails to tell the truth: Fleming did indeed make an unexpected observation, and he indeed deserves epistemic credit for it. But to focus on Fleming's observation as the key to the discovery of penicillin is to buy into the rhetoric of effortlessness as legitimizing the narrative's role as paradigmatic of scientific discovery. And, as a paradigm of scientific discovery, this kind of narrative has a problematic effect on how we conceive of scientific discovery generally speaking. To quote McAllister (2016, p. 151):

> It propagates a seductive image of science as an immaterial, ethereal, leisurely, even at times aristocratic endeavor, achieved by scientists with easy grace and naturalness…it tends to render science an opaque, unanalyzable activity, making it difficult to understand the origin of findings.

I call attention to two especially problematic aspects of this image of science—that science is "aristocratic" in nature and that it is "an opaque, unanalyzable activity." Conceiving discovery in terms of rhetoric that leads to depicting science in this way also hinders further investigation into the skills and structures that enable serendipitous discovery in science.

3. A Different Story

Consider further that discoveries can always be found to have precedence, either in the life of the particular observer at hand and/or in the surrounding epistemic context of her education and work. Even seemingly out-of-the-blue discoveries have been shown, upon closer examination, to have a genealogy made up of "unintended interactions or applications" of "silent resources" (Holton, Chang & Jurkowitz, 1996, pp. 370, 373-4). Aharon Kantorovich's (1993) evolutionary approach to serendipity and scientific discovery goes further, to suggest that scientific progress is the result of natural selection-type responses to unanticipated observations.[8] Selection takes place at the point of uptake by first the individual and then her community, insofar as acceptance entails being able to 'fit' the new idea or datum into the broader, normative, epistemological environment. Thus, even when an observation is

[8] In some aspects the evolutionary approach goes too far. For example, Kantorovich tends to displace intentionality altogether, by deferring agency to the community instead of the individual. This has implications that I explore in more detail elsewhere.

unexpected, it takes place within a broader context of precedence and the individual observer is thereby 'nudged' to perceive its potential value.

The discovery of penicillin took place in such a context, beyond the personal interests of Fleming and his experience with lysozyme. For instance, it was already widely known that certain substances had antibacterial properties.[9] Fleming remarked in his Nobel Prize speech that the "inhibition of one microbe by another was commonplace," and that bacteriologists of his generation were both taught about such inhibitions and observed them regularly in practice (Fleming, [1945] 1964, p. 83). In fact, Fleming denied explicitly that he did anything other than act as any good bacteriologist would (Fleming 1964). According to Mark de Rond and Raymond-Alain Thietart (2007), "at least seven scientists prior to Fleming had noted the effectiveness of penicillin in inhibiting bacterial growth," and Fleming's "contributions [to the discovery of penicillin] seem to have never exceeded those of a French doctoral student [Duchesnes] some 30 years before" (2007, p. 548).

Fleming himself even pointed to a contemporary as an alternative source of the discovery. He is quoted as saying on a Belgian radio show in 1946 that, "but for circumstance, [Gratia] might well have been the discoverer of *Penicillin*." André Gratia, a friend and colleague of Fleming's, had noticed the antibacterial properties of a mould that was likely P. notatum, but had failed to preserve his culture (de Scoville, De Brower, Dujardin, 1999, p. 258). It seems the chance observation made by Fleming was not as necessary to the discovery of penicillin as the narrative makes it seem—someone else's observations *could have* played the same role.

[9] Described by Pasteur and Joubert first in 1877 (Chain 1971, p. 297). Diggins (1999) argues that despite these precedent observations of antibiotic moulds, the properties of P. notatum had not yet been discovered; rather, the focus of previous studies were *other* kinds of mould. While this detail may make a difference if my purpose were to describe the objective truth of the discovery and its history, as is Diggins' purpose, it does not affect my arguments here.

On the other hand, no one else's observation *did* play such a role—but Fleming's did. Even so, the contribution Fleming did in fact make to the discovery for which he, Florey and Chain were awarded the Nobel Prize was contingent.[10] For instance, the paper Fleming had written was indeed read by Chain, who came across it in a literature search. By Chain's own admission, "the possibility that penicillin could have practical use in clinical medicine did not enter our minds when we started our work on penicillin" (Chain 1971, p. 301). Rather, Chain chose to work with penicillin because he thought it might serve his current scientific interest in isolating the active substrate he hypothesised it had in common with lysozyme (p. 300). By "a curious coincidence," a sample of Fleming's mould was part of the collection of bacterial cultures held in the very School of Pathology in which Chain was working. This coincidence was due to an earlier request for a sample from a bacteriologist who believed Fleming had discovered a bacteriophage (i.e., who was on a different track altogether) (Chain 1971, p. 301). The first real hint of the therapeutic power of this substance came by surprise, when a routine toxicity test failed to kill a mouse as predicted (Chain 1971, p. 303). Chain's rising curiosity, which had led to further testing, was due to its being a "chemically very unusual substance, and thus it was of obvious interest to continue the work" (Chain 1971, p. 303). Therefore, contrary to the narrative, there was no direct line from an insight of Fleming's at the moment of his observation into the unique and clinically important properties of penicillin to their discovery.[11]

[10] There are other ways to think of the contingency involved that I do not address here. For instance, the epistemic climate was more amenable to the possibility that an antibiotic may be found with clinical efficacy at the time of Florey and Chain than at the time of Fleming's observation, due to the success of the sulphanomides.

[11] It is not that Fleming made neither note nor mention of possible clinical benefits. His 1929 publication on penicillin notes at the end that penicillin might be useful as a topical antiseptic, for instance (Fleming [1929] 1980, p. 139). I repeat, however, that this does not allow us to draw a direct line from this publication to Chain, who picked up penicillin for other reasons, by his own attestation. Further, this clinical possibility was neither the focus of this paper nor of Fleming's own future work with penicillin, which focussed on its usefulness as a means for isolating other bacteria in experiments. As Chen (1992) points out, the bulk of the paper was devoted to this rather different use (p. 289).

Fleming's observation was not necessary to the discovery of penicillin, neither in the sense that only he could have made that observation, nor in the sense that his observation led inevitably to the discovery of the curative penicillin. In contrast to the narrative in which Fleming made the discovery of penicillin alone, and that Fleming alone could have made this discovery, we can see that it was in large part the broader context that enabled him to play that role, rather than any particular features of himself or even of his famous observation. The isolated individual/singular moment account sets off on the wrong track for understanding the nature of Fleming's wisdom, which had much to do with what came before, and for understanding the nature of the discovery process itself, which 'began' with Fleming's observation for multiple reasons beyond the observation itself.

2. The Nature of Sagacity

So, what kind of action is the 'sagacity' that goes along with the accident in serendipitous discoveries? Or, what kind of wisdom enables the observer to act together with, or respond appropriately to, the world when it presents an unexpected observation? Consider the oft-quoted remark of Louis Pasteur, that "chance favours the prepared mind." Indeed, the "prepared mind" is vaguely described: exactly what can prepare a mind for the observation of something unexpected and yet valuable? Merton's serendipity pattern requires an investigator "steeped in data" and "theoretically sensitized" to recognize the strategic nature of the datum that imposes upon her (1948, pp. 506 & 507). Generally speaking, one must know a theory in order to know if such a datum might have an effect upon that theory. One must also have an idea about what must be expected, if one is to recognize the unexpected as such (Harnad, 2006, p. 164).

Whether chance played a role in this discovery is not in question. In fact, the major figures involved in the process, such as Fleming and Chain, think the discovery of penicillin is hardly unusual for this: "as in all scientific discoveries," noted Chain, "luck has played a very important role" (Chain 1971, p. 294).

Further, the chance observation Fleming made of the petri dish before it was discarded was not, in itself, the only nor the most important instance of chance in this story. Fleming's former assistant Ronald Hare offers numerous points at which chance was a factor in enabling the observation to happen in the first place. For example, temperature fluctuations during the time Fleming was away had to be just right and the mould had to have floated up a stairwell and onto a culture plate that had been seeded differently than any of the others, so that the mould and the bacteria could grow at the right times in relation to one another to have the visual effect Fleming observed (Hare 1970, pp. 60-87). The sheer number of coincidences that led to the observation caused Hare to declare that penicillin was, "surely, the supreme example in all scientific history, of the part that luck may play in the advancement of knowledge" (Hare 1970, p. 87).

However, I suggest that to reduce Fleming's wisdom to having a perfectly prepared mind is to do him a disservice as both a scientist and as contributor to this discovery. Previous investigations into lysozyme were more than a matter of aggregating the correct knowledge, needed to perceive the value of P. notatum when he saw it. That work and other contributions to science made by Fleming were valuable in their own right (seeHare 1970, p. 59-60; Diggins 1999). And if we see Fleming's mind principally in terms of its 'preparedness' it seems we reduce him, counterintuitively, to being merely lucky. Consider the counterfactual: what if he had not been fortunate enough to take vacation just when he did, or to be the very person who saw the petri dish before it had been discarded or cleaned? All that 'preparation' would have been for naught, insufficient for the discovery.

One counter to this has been to assert that, rather than personal experience, some innate creativity or intellectual prowess (genius, if you will) is what enabled Fleming to make his insightful observation when no one else could have. This approach, however, suggests the same questionably sharp distinction between the context of discovery and the context of justification that has marked philosophical approaches to science for much of the last century. Miriam Solomon points out that this positivist approach to the creativity involved in scientific discovery can be described as "anything goes"—in other words, it is insubstantial, and fails to give either constraints or recommendations on what kinds of reasoning processes might be involved (Solomon 2009). As such, it leaves us without recourse when trying to sort out the observation's epistemic nature, or how it counts as a scientific discovery.[12]

If we commit to the 'genius' narrative of discovery, however, we commit to the assertion that only that specific individual, in that time and place, would be capable of playing the role of discoverer. We have seen already that this is not the case—others had come before or at the same time, and it is due to contingencies rather than anything unique about the observation or observer that resulted in Fleming, rather than they being known as the discoverer. I suggest it can be true both that Fleming's preparedness was insufficient for the discovery of penicillin and yet that preparation was necessary for his recognition of the potential value of P. notatum. Consequently, it can be neither true that Fleming's observation is reducible to the inevitable outcome of his knowledge coupled with a fortunate turn of events, nor must it be elevated to the impenetrable insight of a genius in the face of chance.

Further, and importantly, Fleming is not the only one who demonstrated the ability to recognize potential value in the unexpected in the complex, complicated narrative of the process of the discovery of penicillin. The coincidences, that is, did not end at the moment of his observation. Chain's confessions quoted above show that the presence of Fleming's very mould in the hands of the Oxford team was lucky also. By the time research into penicillin had moved from Oxford to Peoria, a town in the United States, efforts had turned toward mass production of the antibiotic. Coincidentally, the best medium found for producing the mould was discovered to be corn-steep liquor—a by-product of cornstarch production, which happened to be a key industry in Peoria (Hendersen 1997, p. 686). The Peoria market,

[12] An earlier hint to the emptiness of this approach comes from Augustine Brannigan, who points out that theorists of scientific discovery tend to resort to the 'genius' factor when their own theories fail to otherwise explain an historical case (Brannigan 1981, pp. 153-154).

of all places, provided the cantaloupe upon which was found a better strain of mould for producing penicillin, despite the fact that the armed forces had been collecting samples from around the world in an effort to find one (Hendersen 1997, p. 686). And the list of fortunate accidents goes on.

Not all of these cases of luck, fortune and happenstance necessarily count as serendipitous occurrences. For one thing, they didn't all require someone to observe them in order for them to play a role—many of them are mere coincidences or instances of luck. However, even when observation was necessary, and presumably also the wisdom to recognize potential value similar to that exercised by Fleming, not every observer got the credit for their wisdom (and even fewer obtained a Prize). One example is the lab technician who picked up the mouldy cantaloupe from the Peoria market and brought it back to the lab. Mary Hunt, for her keen eye in recognizing the potential value of this mould, and for the valuable outcome her actions produced (without this strain of mould, mass production of penicillin would not have gotten off the ground in time for D-day, for instance), earned only a nickname—Moldy Mary—hardly a knighthood.

Just as the process of discovery can be broken down into several moments, independent discoveries, and even of different types, so can the contributions of various individuals as having recognized the potential value of an unexpected occurrence be added to the roster of serendipitous discoverers involved in this process. Chain's curiosity, Florey's drive, Moyer's creativity, Hunt's attentiveness, and the characteristics and skills of likely many others played an equal role in enabling the progress from initial observation to the ultimate discovery of penicillin as we know it today. Rather than a passive recipient of a chance observation—that is, rather than isolating him and his observation into a singular moment in time and presence of mind—then, it is better to see Fleming as acting within a personal and historical context as one, albeit significant, contributor to a process of discovery constituted by the efforts of many. Furthermore, having thorough knowledge about one's own field and the ability to compare new observations to what is already known would not have been features unique to Fleming. The discovery of penicillin can remain a paradigm of discovery, but described more accurately as the result of a network of participants, many of whom responded well to unexpected events. Multiple people each contributed to the process with their unique perspective, or 'prepared mind.' Epistemic credit, then, should be diffused across the network rather than concentrated in one individual.

We can thus break down the individual contribution each person makes to a discovery process, taking away some of the mystery. There is no need, as Hudson laments, to give up "exactness" in identifying discoverers (Hudson 2001, p. 86); connecting numerous points can still draw an accurate picture of how a discovery occurred. In fact, a *more* accurate picture will be so drawn. Like Fleming, each contributor brought to the plate her own perspective and experience, and each contributor was necessary to the process in some way. The discovery of penicillin would have not come about, in other words, without this *diversity* of contributors.

Consider the case of the floppy-eared rabbits, described in detail by Bernard Barber and Renée Fox (1958). Barber and Fox describe two complementary paths taken by two scientists, both of whom had made the same intriguing observation and had been equally wise to perceive its potential value—they had seen the ears of rabbits in their lab flop in response to an injection of the enzyme papain. Only one of these scientists, Dr. Thomas, however, brought the observation to fruition in a discovery about the nature of rabbit ear cartilage, years later. Part of the reason he was able to make the discovery in the end was that he had multiple opportunities, as a teacher and in discussion with colleagues, to revisit the problem. As Barber and Fox note, "As so often happens in science, an unsolved puzzle was kept in mind for eventual solution through informal exchanges between scientists" (1958, p. 132). Dr. Kellner, in contrast, experienced 'serendipity lost' because rather than continue seeing the observation as a problem, he incorporated the floppy eared response as a method in further experiments. It was not a subject of discussion, but rather part of the background for what he found to be more interesting experiments (Barber and Fox 1958, p. 135). Whereas Dr. Thomas had put the problem to test in a variety of contexts and by presenting it to several others, Dr Kellner had not. Both scientists had observed the floppy ears initially in the same way, with surprise, and both had followed it up with further tests. But it was the exposure of this unexpected observation to diverse context and expertise that led to a serendipitous discovery.

So sagacity, it seems, is less a feature of the original observation and someone's immediate response, and more about how the problem is responded to over time—importantly, this includes exposing the observation itself to multiple others and in a variety of contexts. Fleming's dispersal of his sample is the key to his sagacity, then, as much or more than his keen perception of the petri dish as 'funny' enough to investigate further.

The social nature of this sagacity or wisdom can itself lead to serendipity lost, however, when met badly. For instance, in a hierarchical organization of scientists, it can happen that the work of those in the lab with less prestige is either suppressed or taken up, depending on how much they are in favour with those above them on the ladder. Toby Sommer (2001) calls the cruel squashing of ambitions in science 'bahramdipity', a term he invented[13] to mark those cases where serendipity was usurped, rather than lost. Sommer gives several cases in which prestigious scientists exercised unfair control over the acceptance or dismissal of others' work—one such case was Beatrix Potter, whose paper on lichen biology was only granted an audience because an influential uncle intervened with the powers that be in the London Linnaean Society of the time (Sommer 2001, p. 85).

Sommer's examples offer a contrast to another famous story of serendipitous discovery, of the bacterial cause of stomach ulcers by Robin Warren and Barry Marshall. Lauded for his perseverance,

[13] Sommer names his concept after another character in the fairytale referred to by Walpole, *The Princes of Serendip.* Bahram V Gur was the King of Persia, and played the role of foil to the princes in the fairytale.

Marshall's story is often told as a narrative of personal success: he prevailed against common gastroenterological wisdom by fighting with tenacity for the credibility of his claims, the results of research originating in chance observations and events (eg. Meyers 2006, p. 113). As Paul Thagard reports, however, this success was much more than personal, but also required the support and cooperation of many others who performed supporting experiments and weighed in with their own influence until Marshall's claims were deemed credible by the wider scientific community (Thagard 1998). Thus we see that the same wisdom in observation can result in both serendipity gained and serendipity lost, depending on whether the observer is deemed a credible witness to the unexpected.

According to McAllister (2016), scientists, historians of science and others specifically use the rhetoric of effortlessness to heighten the credibility of the work being described, its results, and of the scientists themselves. This is the work that such rhetorical strategies do in science; they provide justification for believing the claims being made by describing characteristics of the work or of the scientists who have done the work that grant the results credibility. For instance, as I pointed to above, the rhetoric of effortlessness is used when a discovery is described as occurring naturally, or, as in the case of serendipity, even in despite of the aims of the scientist at the time. This naturalness of the observation gives credibility to it—it is more credible, suggests McAllister, than a result that could only come from a manipulated context, which has a higher probability of being an artifact of that manipulation or in error, than a direct and simply made observation of the world (McAllister 2016, p. 148). In turn, the scientist expends little effort in making the observation—she has only to be properly prepared, and her innate sagacity will allow her to perceive the potential value of the unexpected when it occurs. Having been credited with the perception of value, despite the chancy circumstances, leads to a "burst in reputation" via the rhetoric of effortlessness, as McAllister puts it (2016, p. 150). This gain in reputation in turn lends further credibility to the observation itself, having been the observation of a credible scientist.

McAllister focusses in his article on the tendency of this rhetorical strategy to diminish the importance of the intellectual work done before the finding is made; claiming effortlessness elides the great amount of effort that normally goes into scientific work, whether or not it is obvious in the moment of discovery. To quote, “Even so-called flashes of insight and instances of serendipity usually rest on much incubation or preparation. The rhetoric of effortlessness thus consists partly in concealing or downplaying the effort exerted in scientific work” (McAllister 2016, p. 150). However, it is not only the work of the isolated individual that is concealed when the focus is on a singular moment and with the aim of demonstrating how little effort was expended. Such rhetorical strategies, as I have tried to point out here, often elide the work of many others who contributed to the discovery process as a whole. In telling narratives captured by images such as Fleming pondering alone in his lab, we rhetorically ignore the social and temporal dimensions of a serendipitous discovery.

In turn, appeals to rhetorical strategies such as the rhetoric of effortlessness, or the isolated individual/singular moment depiction of serendipitous discovery, are also appeals to ‘aristocratic’ understandings of science that undermine efforts to understand both the epistemology of discovery and observation and the impact of hierarchies on our depictions of scientific practice. Such efforts have been undertaken with aplomb by historians of science. Steve Shapin’s (1989) exposure of the epistemological contributions of Robert Boyle’s technicians, despite their near-elision in favour of recognizing the results of the experiments they enabled as those of Boyle alone, provides an apt example. As Shapin notes,

> At a basic level, the individualistic bias of much of Western culture has historically expressed itself in views of how proper knowledge is made. In science, as in art and literature, the prevalent model places a solitary individual in contact with reality or with sources of inspiration (Becker 1982) [sic]. If we think of solitude as the proper situation for securing genuine insight, then we will regard a collective enterprise as doomed to yield at best mediocre or conventional knowledge. Moreover, a revelatory understanding of scientific discovery tends to stress momentary flashes of individual

insight as opposed to extended trains of collective work. In fact, there is much anecdotal evidence that such individualistic and revelatory models of scientific activity persist—even in the modern age of Big Science—constituting a general cultural basis for the invisibility of technicians and other support personnel, and for our tendency to see science predominantly as thought rather than as work. (Shapin 1989, p. 561)

Shapin focusses on the tendency to dismiss the role of technicians in this quotation, but in this paper I want to highlight how the very same rhetoric tends to also generate a "general cultural basis for the invisibility" of contributors to serendipitous discovery other than the one being given all the credit.

That is, there is a tendency toward giving epistemic credit for discovery not only to single individuals but also to *certain* individuals. As McAllister points out at the end of his paper, not every scientist can use the rhetoric of effortlessness to effect (McAllister 2016, p. 163). Rather, as Schaffer (1996) argues, communities determine retrospectively who deserves the epistemic credit for a discovery process and, thereby, which fortunate observation will be seen as the originating moment. Such determinations are often marked by social and political bias.[14] Consequently, while the rhetoric of effortlessness suggests that those who gain the credit for a discovery deserve it because of their acuteness in observation, the credit they ultimately obtain when recognized as discoverer may elevate them above others involved in the process for very different reasons. In turn, the assumption that the sagacity associated with serendipity is intuitive, a momentary flash of insight only possible in the mind of a unique individual, is the result of a conflation between credit due and credit given. That is, when the label of serendipity is applied to a discovery, it is meant to also imply that the observer exhibited wisdom—was not merely lucky—in making the observation at hand. But this is independent from the credit—read, authority, as

[14] Including, as one reviewer of this paper insightfully noted, racial, cultural and class bias, such as may have been the case when Fleming's fame overrode the roles of Florey and Chain in the popularization of penicillin's discovery as a paradigmatic narrative of scientific practice and progress.

Sommer does—given some of these observers in retrospect by their community when they are recognized as discoverers.

The close relationship pointed to above between credibility, prestige and the categorization of a discovery as serendipitous is brought together in the continuing promotion of Fleming's serendipitous discovery of penicillin as the work of an isolated individual and a narrative narrowly focussed on a singular event. As we see with Boyle in Shapin's analysis, and as suggested above, Fleming was given the credit, and allowed the privilege of effortless yet credible discovery through this narrative of serendipitous discovery, for social and political rather than epistemological reasons.

One solution is to see the sagacity involved in serendipity as a fairly common, epistemic phenomenon. A property belonging also to lesser known individuals, including Mary Hunt, rather than being an "opaque, unanalyzable" property that marks certain people as great discoverers. The importance of chance in science, once the temporal and social aspects of serendipitous discovery are taken into consideration, becomes clear: not because it leads to singular, significant, world-altering moments, but because of the way it interacts with and influences even well designed experiments and the best laid plans. Scientists frequently respond well to unexpected observations and opportunities, and those responses become part of the processes of scientific discovery on a fairly regular basis (Copeland 2017, p. 5).

3. Changing the Script

The paradigm we have of discovery makes a difference to what we can say about the reasoning processes that enable discovery as well as the practices that lead to the granting of epistemic credit. A narrative that serendipity is the quality of discoveries made by isolated individuals and described as singular events misdirects investigation into the relevant epistemic and social contexts, by configuring their significance fully in relation to how they 'prepare' the individual or how they attest to that individual's unique 'genius.' Such a narrow focus elevates the status of the individual and the chance occurrence that intersect in that moment and provides a very limited understanding of serendipity and the sagacity involved. In contrast, a paradigm of discovery that embraces the complexity of the network of individuals, perspectives and moments of chance that intertwine to lead to a discovery gives a more realistic view of discovery, how it happens, and why and when we give credit to discoverers when we do. When we change the narrative, new options for analysing the epistemology of observation leading to discovery open up. This in turn opens up two other possibilities: first, to ask better questions about why and how credit is and should be assigned and second, to imagine an alternative way of both depicting and organizing the practice of science that diminish rather than encourage the potential for bias in assigning that credit.

For example, instead of a passive, fully prepared mind, lying in wait for fate to present a valuable datum to it, I propose that the sagacity of serendipity is a specific type of wisdom or even a skill, related to the ability to recognize potential value in chance events. A skill-based epistemology of unexpected discoveries complements the idea that multiple individuals are able to make equally important, yet distinct, contributions to discovery processes, even those marked by chance. Because skills can be intentionally obtained or refined, unexpected discoveries are not limited to the elect. Further, they are explicable in epistemological terms, as methods for knowledge production. However, because such skills are nonetheless exercised in unexpected ways and in keeping with the unique perspectives and experiences of the individuals who exercise them, they do not amount to a 'logic' of discovery, nor to a method that could be repeated in any given context by any given person. Thus, we avoid the false dichotomy presented by the assumption that discovery is either methodological or mysterious.

I have raised the possibility of a skill-based approach to serendipity in science in previous work (Copeland 2017), suggesting there is a set of skills some individuals have intentionally developed in order to better perceive value in unexpected observations and to utilize those observations strategically.

I based my argument there on empirical work done in the information sciences and elsewhere, where evidence affirms that some individuals experience more serendipity than others and that those who do experience more serendipity attribute that fact to the exercise of specific types of perception and attention, which can in turn be cultivated. A skill-based approach to sagacity raises the possibility of an epistemological understanding of how observers are able to identify value in the unexpected, and how and when such perception leads to knowledge production.[15] While there is still much work to be done on the matter of what kinds of skills are relevant, and how they might be cultivated, the possibility of such an epistemology opens up an important philosophical discussion.

Further, a change of paradigm allows for the possibility of changing the norms of discovery held by scientific institutions: perhaps it is time that the Mary Hunts were given as much credit as the Alexander Flemings for their role in valuable discoveries such as penicillin. This is not to say, of course, that everyone involved in a valuable discovery deserves a Nobel Prize. But we could indeed look closer at the relationship between social authority and epistemic credit when it comes to giving out such prizes—for instance, a glance at the history of Nobel Prize winners and laureates reveals a distinct racial and gendered pattern (Halton 2017). Similarly, the recent debate over priority in the patenting of CRISPR technology has raised questions about how we assign credit for individual contributions, and how many people ought to be included in the narratives we tell about discoveries (Marcus 2017). To recall the influence of idealizations of socially significant science as the natural result of a purely curiosity-driven, individualistic endeavour, such as promoted by Bush and by the continuing impact of the Fleming narrative, this conception and approach has direct implications for how science is funded and organized.

4. Concluding Thoughts

[15] This approach is similar to the approach that Dustin Olson (2015) takes to epistemic agency, where he suggests that agency is exercised insofar as individuals intentionally develop skills that lead them to have qualitatively better beliefs.

Despite the classic tale of discovery, Fleming was not a passive recipient of a chance observation that revealed penicillin's true value, nor was he a unique individual in the sense of being the only one capable of recognizing it, but rather contingency and experience enabled his involvement in this discovery. The truth is more complex than either an internal, psychological and individualized or a fully external and social depiction of discovery can adequately describe or explain on their own. Rather, it seems that Fleming had epistemic worth in seeing the chance observation as valuable and also that he was contingently permitted to play the role he did, suggesting that both chance and wisdom were indeed involved, but in complex ways and throughout the process of discovery. Therefore, insofar as this discovery retains its paradigmatic status, we need to look at the bigger picture in which serendipity occurs rather than at the isolated individual or singular moment of the observation itself to understand what sagacity is and what role chance plays in discovery.

The importance of getting the narrative right is playing out now through the empirical investigations into the prevalence and impact of serendipitous discovery on the progress of science. A look to history suggests, as Chain does, that were it not for Fleming's observation, "The development of the antibiotics field might have been delayed by a few years, but it would, inevitably, have taken place with the same final results which we have now" (Chain 1971, p. 302). As I point out above, Fleming did not make the 'discovery of penicillin' alone and in that moment in time, despite the familiar narrative. Rather, in order to understand Fleming's role as the originator of that discovery, and to describe the sagacity he exercised in making his famous observation, we need to be able to look both at that particular moment in time and at the broader context that enabled him to make that epistemic contribution.

In their seminal analysis of the history of the word 'serendipity', Merton and Barber show how the epistemic credit associated with unexpected discoveries differs across time and communities (2004, esp. Chapter 9). In some scientific communities, such as archeology and astronomy, where observation is the principal method, serendipity is regularly recognized as a legitimate source of discovery, and the observers in such cases readily obtain epistemic credit for their role. In other communities, such as those of the experimental sciences, scientists are more reluctant to place the responsibility for their discoveries on the shoulders of fate (see also Campanario 1996, p. 5). A more fruitful investigation than into what kinds of methods (observational or experimental, for instance) tend to allow room for chance, given the conclusions drawn here, would be into the differences between the social and epistemic norms regarding who deserves epistemic credit in scientific communities. This paper takes a first step, by breaking down the assumptions inherent in our very paradigms of serendipitous discovery, and how they determine the roles of some individuals as compared to others in our understanding of the progress of science.

I have argued here against the tendency toward assigning epistemic credit to certain, isolated individuals, for discoveries that are better described as processes involving networks of contributing scientists. Indeed, this tendency is a reflection of the continuing belief that discovery is mysterious and that chance interferes with, rather than shapes, the normal practice of science. Rather, the intersection of chance and wisdom that is recognized when we categorize certain discoveries as serendipitous gives philosophers of science opportunities to analyse what kinds of epistemic skills enable observers to pick out valuable anomalies. Such opportunities should not be missed for the sake of telling a ‘good’ story about a scientific hero.

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