Reassessing Discovery: Rosalind Franklin, Scientific Visualization, and the Structure of DNA*

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Philosophers have traditionally conceived of discovery in terms of internal cognitive acts. Close consideration of Rosalind Franklin's role in the discovery of the DNA double helix, however, reveals some problems with this traditional conception. This article argues that defining discovery in terms of mental operations entails problematic conclusions and excludes acts that should fall within the domain of discovery. It proposes that discovery be expanded to include external acts of making visible. Doing so allows for a reevaluation of Franklin's role in the discovery of the structure of DNA.

1. Introduction. When James Watson and Francis Crick published their famous paper "Molecular Structure of Nucleic Acids: A Structure for Deoxyribose Nucleic Acid" in 1953, they made public a discovery that was undoubtedly one of the most important scientific achievements of the twentieth century. The discovery of the structure of DNA paved the way for modern genetics and a scientific understanding of how traits are passed between generations. Watson and Crick were widely hailed for their work, and no small measure of fame and accolades fell their way. As is often the case in science, however, discovery brings with it disputes concerning the proper appropriation of credit. The explication of the structure of DNA has been the subject of such debate. Rosalind Franklin was a crystallographer who produced diffraction images of DNA, which Watson

*Received April 2011; revised June 2011.

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‡I would like to thank John Lyne and Kenneth Schaffner for their very helpful comments on an earlier version of this article.

Philosophy of Science, 79 (January 2012) pp. 63–80. 0031-8248/2012/7901-0004$10.00
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https://doi.org/10.1086/663241 Published online by Cambridge University Press

and Crick used in their formulation of models of DNA's structure. Some argue that Franklin did not receive due recognition for her work and played a more significant role in the discovery than is often acknowledged (e.g., Sayre 1975; Maddox 2002, 2003; Rapoport 2002). The issue of how much credit Franklin deserves for the discovery of the structure of DNA raises interesting philosophical questions about the nature of discovery. Arguments about her contribution, however, generally do not address philosophical perspectives on discovery, instead focusing on gender politics and an unscrupulous act.

Sexism and deceit certainly played a role in this episode and must be acknowledged. Though Franklin did not work in the same institution as Watson and Crick, she belonged to the same lab as a friend of theirs, Maurice Wilkins, and given that both labs were working on DNA, they had reason to interact frequently. As evident in *The Double Helix*, Watson's book-length account of the discovery of the structure of DNA, sexism seemed to underlie Watson and Crick's regard for and interactions with Franklin. In a now-infamous passage, Watson described Franklin in a trivializing manner, scornfully criticizing her fashion sense (1968/1996, 17). There was a good deal of bad feeling between Franklin and Wilkins, which Watson discussed in a manner that similarly derided Franklin, observing that "clearly Rosy had to go or be put in her place" (17). Finally, and perhaps worst of all, Watson suggested that "the best home for a feminist was in another person's lab" (20). Wilkins eventually, and unscrupulously, showed his friends Watson and Crick some of Franklin's important DNA work without her permission. When "A Structure for Deoxyribose Nucleic Acid" appeared in *Nature*, Franklin herself was not aware of how much her research had contributed to Watson and Crick's discovery. Indeed, this did not become widely known until Watson published *The Double Helix*, by which point Franklin was dead. Thereafter, her role in the discovery of DNA's structure gradually gained wider recognition, raising questions about whether or not she was properly credited, and Franklin eventually became a symbol of sexism in science.[1] The backlash against Watson's sexist portrayal of Franklin, however, has in some ways led to oversimplification. The question of how much credit she rightly deserves is not solely a matter of gender politics and inappropriate access to her research. It is also quite importantly a philosophical question that has to do with conceptions of discovery.

One's evaluation of Franklin's contribution to the discovery of the structure of DNA hinges on the ideas one holds about discovery. Within

1. In 2002, the British government posthumously honored Franklin and created a Franklin Medal as part of a larger movement to combat sexism in science. Michel Morange, for one, objects to this revisionist history of Franklin's role (1998, 115–16).

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the context of most philosophical accounts, her contribution would be regarded as the production of useful data. However, this article will argue that upon close consideration, Franklin's crystallography work poses a problem for such conceptions of discovery by leading them to problematic conclusions. This does not damn the philosophical analysis of discovery but rather opens up new avenues of pursuit, particularly with regard to scientific visualization. Before I get to the question of discovery, however, it is first necessary to understand Franklin's crystallography work.

2. Rosalind Franklin and Crystallography. X-ray crystallography, a term that is often used synonymously with x-ray diffraction and chemical crystallography, takes advantage of a relatively simple phenomenon.[2] As Max von Laue found in 1912, when an x-ray beam is passed through a crystallized substance, it diffracts in a regular pattern. When x-rays are directed at a molecule with a photographic plate behind, the plate records the pattern and provides evidence of the arrangement of the molecules, or structure, of the type of matter being analyzed. Getting a good image and interpreting the evidence, however, can both be quite difficult. Materials can be tricky to work with, and analysis is quite complex (Kendrew 1966). When W. L. Bragg first used crystallography to determine a crystal structure, he used trial and error; that is, he kept trying different models until he found one that would produce the given diffraction pattern. He and his father later developed more efficient mathematical methods for interpreting the diffraction patterns, which worked well for inorganic compounds. However, in the 1950s, many biological molecules were simply too complex for crystallographic analysis. Luckily, however, DNA was not (Ewald 1962; Lima-De-Faria 1990).

Shortly before Rosalind Franklin's arrival there, King's College had obtained a prototype of a new type of fine-focus x-ray tube, which enabled x-ray beams to be highly concentrated. Franklin was responsible for getting this equipment ready for use in an x-ray machine, which was not an easy job. Rosalind and her student, Raymond Gosling, had to design the camera, and many of its parts were fashioned by hand in their institution's workshop (Maddox 2002, 130). Maurice Wilkins had gotten some important results through his earlier x-ray analysis. He had obtained some prized samples of DNA from Rudolf Signer that were particularly good for x-ray analysis and had produced images that indicated DNA had a regular, and therefore potentially mappable, crystalline structure. Before Franklin, William Astbury's lab had produced and published some images

2. For an explanation of x-ray diffraction techniques, see Kendrew (1966). For a history of crystallography up until 1962, see Ewald (1962). For a history that covers more recent years, see Lima-De-Faria (1990).

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of reasonable quality. But the new fine-focus tube allowed for the analysis of a single fiber of DNA, and Franklin, who was highly skilled at preparing samples, found a way to extract exceptionally thin fibers and mount them for analysis. She had also designed a microcamera that allowed her to expose fibers to the x-ray beam at a number of different angles (see Bernal 1958; Klug 1968). Furthermore, Franklin developed a means of hydrating and dehydrating the DNA fiber while exposing it to the x-ray beam by using salt solutions to control the humidity (Maddox 2002, 144).

Once Franklin got the crystallography machine up and running, she produced exceptionally good x-ray pictures of DNA. She was the only person getting x-ray diffraction images of DNA at high humidity (Maddox 2002, 153). She found that humidity changed the structure of DNA and was able to distinguish between two forms, a dry A form, which was shorter, and a wet B form, which was longer and thinner. She realized that previous images of DNA had been confusing because they were a blur of both types. Franklin's images, however, were clear and sharp. One of her images in particular was superb, the famous Photograph 51. It was of the B form of DNA, and its unambiguous X clearly signaled a helical structure. In an effort to ease tensions, the lab's director had assigned Franklin to work on the A form and Wilkins to the B form, so Franklin put this image aside and continued her work on the A form. Unfortunately, she got some aberrant asymmetrical results from one of the DNA fibers she analyzed, which led her on an errant path of inquiry regarding whether or not the A form of DNA was truly helical (Judson 1996, 112).[3]

In 1951, James Watson heard Franklin give a lecture on her DNA work. Watson was not trained in x-ray diffraction and may not have caught all the details, but he described what he understood of the lecture to Crick, who had a better background to appreciate it.[4] Soon after, Watson and Crick devised and built a model of DNA and invited Wilkins and Franklin to see it. Their model, however, had phosphates on the inside and bases on the outside and was based on an erroneous understanding of the water content, and Franklin curtly informed them of their errors (Olby 1974, 362). It was in 1953 when Watson visited the lab at King's College while Franklin was not present that Wilkins showed Watson some of Franklin's best pictures, including Photograph 51. As Watson wrote in *The Double Helix*, "The instant I saw the picture my mouth fell

3. Franklin and Gosling published three papers on their DNA work in *Acta Crystallographica* and two in *Nature*, one of which was published on April 25, 1953, alongside Watson and Crick's famous paper.

4. Klug (2003, 8) explains that even if Watson did not pass on this information at the time of the lecture, he and Crick later found this same information in a King's College committee report to which they had access.

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open and my pulse began to race" (1968/1996, 167). Watson and Crick did not use Franklin's crystallographic findings alone to derive their model, but her images quite clearly showed a number of the important features of DNA's structure. The question at the heart of this article is what role such images play in the process of discovery.

3. Frameworks of Discovery. Philosophical analysis of discovery traditionally begins with the distinction between "contexts of justification" and "contexts of discovery." Reichenbach (1938) introduced these terms in *Experience and Prediction*, identifying justification as "the form in which thinking processes are communicated to other persons" in comparison to discovery, which is "the form in which they are subjectively performed" (6). By most interpretations, *Experience and Prediction* suggests that the context of discovery is an inscrutably irrational part of the scientific process, and one that is not open to philosophical analysis (e.g., Schaffner 1980; Lamb and Easton 1984). Reichenbach is said to claim the context of justification as the proper subject of philosophy. Whether this reading of Reichenbach is correct, and some argue that it is not (e.g., Curd 1980; Nickles 1980b), the idea itself has had strong currency in the philosophical tradition. Others before Reichenbach, such as William Whewell, Charles Sanders Peirce, and F. C. S. Schiller, had also made similar suggestions, and there have been numerous discussions of forerunners to Reichenbach (e.g., Laudan 1980; Hoyningen-Huene 1987; Peckhaus 2010). A good many philosophers have conceived of the moment of discovery as an unfathomable leap of insight. Karl Popper, for instance, argued that "the initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man—whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge" (1959, 31). The positivists held that science exists not in the generation of theories but in results, and their considerable influence during the twentieth century led to the neglect of discovery, the study of which was said to properly belong to psychology and sociology, not philosophy (Nickles 1980b). According to such a position, the issue of Franklin's contribution to the discovery of the structure of DNA is not the concern of philosophers.

Not all philosophers in the twentieth century, however, abandoned discovery to the nefarious regions of psychology and sociology. There are, as Gary Gutting has called them, "friends of discovery" (see Nickles 1980a, xiii). N. R. Hanson is frequently credited with reigniting interest in discovery in the twentieth century (Schaffner 1980, 173). In his *Patterns of Discovery*, Hanson claimed that "the initial suggestion of an hypothesis

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is very often a reasonable affair. It is not so often affected by intuition, insight, hunches, or other imponderables as biographers or scientists suggest. Disciples of the H-D account often dismiss the dawning of an hypothesis as being of psychological interest only, or else claim it to be the province solely of genius and not of logic. They are wrong. If establishing an hypothesis through its predictions has a logic, so has the conceiving of an hypothesis" (1958, 71). Some of discovery's friends accepted the distinction between contexts of discovery and contexts of justification but argued that there is a distinct logic of discovery (Schaffner 1980). Some later modified this position, claiming that discovery is not necessarily logical, though it is, just as importantly, rational. Indeed, Nickles suggests that this is the most common position taken by friends of discovery (1980b, 6–7). Still others posed modified epistemological schemas, such as Richard Blackwell's (1980) three-part temporal, compositional, and meaning model. Of course these do not exhaust the philosophical positions various friends of discovery have taken, which are, importantly, united by the view that discovery is amenable to philosophical analysis.

Kenneth Schaffner, a friend of discovery, has written specifically about the discovery of the structure of DNA (1974). His view is that similar reasoning processes are at work in both discovery and justification. According to Schaffner, speculation is rational when it is constrained by certain factors, such as empirical adequacy and the unity of biological processes, among others. Indeed, Schaffner describes Watson and Crick's discovery of the structure of DNA as just this sort of speculation, which he calls "controlled speculation." He describes some of the constraints on Watson and Crick's speculations, which included "John Griffith's early theoretical constraint involving the approximate calculation of attractive forces between the stacking pairings of like and unlike nucleotide bases. In addition, Erwin Chargaff's 1 : 1 ratios (or 'rules') of the complementary nucleotide bases were known and exercised an important empirical constraint, as did the available X-ray crystallography findings" (1974, 383 n. 74). In other words, Franklin's images acted as one of many constraints on Watson and Crick's speculations, enabling them to remain within the realm of the rational. In contrast to Watson and Crick, Franklin focused more narrowly on crystallographic data, and according to Schaffner's conception of discovery, we might explain that she did not engage in controlled speculation as adeptly as Watson and Crick did. Because of this, Watson and Crick were able to discover the structure of DNA ahead of her.

As Simon Schaffer (1986) describes, models of discovery presume an inspired genius in whose mind discoveries take place. Both enemies and friends of discovery generally conceive of discovery as an internal cognitive act. The difference between the two positions concerns the type of

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cognitive processing involved, and it is this issue, the precise nature of the cognitive process, that remains the crux of debates about discovery. Gone, much less scrutinized, is the underlying presumption that discovery is necessarily an internal cognitive act. I argue that close consideration of Franklin's role in the discovery of the structure of DNA reveals problems for philosophical accounts that restrict discovery to the mind and suggests that discovery might also be located elsewhere.

This article's reconceptualization of discovery has import for our understanding of the role scientific images play in the discovery process. Recent years have seen growing philosophical interest in images (e.g., Perini 2005; Delehanty 2007; Goodwin 2009; Rescorla 2009; Rosenberger 2011). Some work even considers matters of discovery, examining how scientific images facilitate reasoning and theory development and/or addressing how images contribute to scientific discovery (e.g., Bechtel and Abrahamsen 2005; Bechtel 2006; Nersessian 2008). This article, however, goes a step further. It argues that image making can itself constitute a mode of discovery.

4. Rosalind Franklin and the Challenge to Friends of Discovery. Rosalind Franklin's research strategy was to avoid exactly the sort of speculation that Watson and Crick freely engaged in. It seems that Franklin wanted to remove guesswork from the research process and instead follow a path of gradual disclosure. As Brenda Maddox describes, Franklin wanted "to find the structure directly from the data, not from guesswork" (2002, 184). Robert Olby similarly explains that Franklin "was a professional structural crystallographer who distrusted intuitive guessing, and who wanted to solve the structure by direct methods, i.e., without introducing assumptions in the form of hypothetical structures" (1974, 374). Of course, Franklin did more than create crystallography images. She used a variety of techniques to work with those images in order to draw information from them. She was strongly committed to calculation of the Patterson function, a then labor-intensive way of transforming the information in the x-ray pattern into a form more useful for discerning structure. Significantly, in terms of Franklin's investment in direct methods, "it embodies no assumptions, using nothing other than the observed intensities" (Klug 1968, 810). In other words, Franklin wanted to put together the pieces of the puzzle by producing more and better x-ray diffraction images and by drawing out their properties using nonspeculative means.

Franklin's desire to avoid intuitive leaps obviously does not mean that she wanted to avoid discovery. And here we have run into the very problem at the heart of conceiving of discovery solely in terms of an internal cognitive act. Claiming that something is a discovery only if it comes by means of an intuitive leap seems to mistake the means for the thing itself.

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By this view, elucidating the structure of DNA would count as discovery if accomplished by Watson and Crick's speculative model-building methodology. However, Franklin's method of putting the pieces together based on what information was available to her in x-ray diffraction images would not be considered discovery, even if it had resulted in a complete picture of the structure. Therefore, by this view, the structure of DNA might never have been "discovered," even though we would have learned what it is!

Regardless of what was in Franklin's head, there was much in her x-ray pictures. Her famous Photograph 51, for instance, contains a wealth of information about the structure of DNA. Photograph 51 shows a clear central X, which unambiguously indicates a helical structure. Diamond patterns around the X suggest that the helix continues in both directions. From the details of the X and the space surrounding it, one can tell that the sugar phosphate backbones are on the outside of the molecule and the bases are on the inside. What look like smears on the X indicate the distance between various parts of the structure, and their symmetrical organization suggests that the helix twists in a regular fashion. What appear as "missing smears" on the four corners of the X are places where strands of the helices cancel each other out, and they point toward the molecule's double-helical structure. Furthermore, from this image one can calculate DNA's most important dimensions.[5] Franklin was not aware of all of these interpretations, or even entirely certain of those of which she was aware (see Klug 1968, 1974). It is much easier to determine what information is available in the image today, when we are sure of DNA's structure, than it was when there were still so many uncertainties. Therefore, while some of this was "in her head," some was only on the page.

The question of whether or not Franklin would have eventually arrived at a full picture of the structure of DNA through crystallographic methods is open to debate. However, it is at least conceivable that she would have. Gunther Stent (1972, 90), for one, argues that without Watson and Crick, the structure of DNA would have been discovered in a slower and less striking manner. For philosophical purposes, however, it is not necessary to answer this question, and one need not arrive at a conclusion about what Franklin may or may not have been able to accomplish by means of x-ray crystallography images. Indeed, it is helpful to posit an entirely hypothetical scenario to more clearly reveal some of the issues at stake. Let us imagine that Franklin had been able to develop a more powerful

5. The Public Broadcasting System produced a special that focused on Photograph 51, and its website includes an interactive feature that details all the information available in the image (http://www.pbs.org/wgbh/nova/photo51/). See also Judson (1996, 113).

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method for capturing images of DNA's structure than x-ray crystallography, so powerful in fact that she was able to produce an image that showed in crystal-clear detail all the features of the DNA double helix. We could say that insight plays no part in Franklin's interpretation of this image because the structure is unambiguously revealed such that it needs no more interpretation than looking at a photograph of a person and thereby knowing what he or she looks like. Let us then go one step further. Suppose that Franklin had suddenly died just as she pushed the button on her new image-making device to snap the perfect picture of DNA's structure. She did not ever see the image of DNA's structure, but others in her lab walked in to find her dead body and the perfectly clear image. Knowing she was working on producing pictures of DNA, her colleagues were able to publish the image, which made public for the first time the details of DNA's structure. In both of these hypothetical cases, Franklin is fully responsible for revealing the structure of DNA by means of a process that does not involve a cognitive leap of insight. Indeed in the second case, Franklin herself would never even have known what the structure was. I argue that theories that conceive of discovery as taking place by means of cognitive processes cannot account for this type of image production within the framework of discovery. Let us examine this point in more detail by considering the perspectives of two friends of discovery.

Howard Gruber disagrees with the view that discovery consists of sudden, rare moments of insight, "eureka experiences" that are infrequent and happen almost instantaneously. Gruber instead suggests that people often experience transformations in their thought processes as part of normal, stable cognitive functioning. Scientific insight is a protracted and relatively common affair. Gruber uses his work on Charles Darwin to illustrate this point. On the basis of his analyses of Darwin's letters, notebooks, and such, he estimates that, per day, Darwin had at least one insight, or "structural change in cognitive organization, entailing a feeling of increased clarity" (Gruber 1980, 128).

Gruber's position, assuming as it does the cognitive basis of discovery, does not offer a satisfactory means to account for Franklin's crystal-clear photograph of the structure of DNA. In our hypothetical case, all the insightful transformations of thought would have occurred in the development of the techniques for capturing the image. Once the device was in place, the role for insight had ended. Therefore, one could, it seems, shift focus and examine the development of the camera as discovery such that one would credit Franklin with the discovery of the technology for producing images of DNA. It is in the discovery of the camera that one finds the insight and is where one therefore locates discovery. This, however, seems to be a case of begging the question: one defines discovery as

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insight and then looks for the insight and calls that discovery. Anything that cannot be explained in terms of cognitive transformation is dismissed as something other than discovery. Furthermore, since only the development of the image-making technology is regarded as discovery, one still arrives at the troublesome conclusion that the structure of DNA was never discovered, though we now know what it is. In addition, it is also possible to posit another hypothetical situation in which the development of the ability to produce images was laborious and did not involve the insightful development of a new camera so much as sustained attention and perseverance. For this case, cognitivist theories of discovery offer no explanatory framework at all.

Nickles writes that "following current usage, by 'discovery' I shall mean the original generation or conception of an idea or theory, a particular historical process rather than a final, warranted achievement" (1985, 178). Because Nickles too defines discovery in terms of cognitive processes, the implications of his views regarding discovery are similar to Gruber's. Nickles's explicit emphasis on the original generation or conception of ideas as opposed to final achievement brings to our attention another issue. By Nickles's definition, a poorly formed idea would fall under the purview of discovery, whereas Franklin's production of a spectacular image revealing the structure of DNA in all of its fine detail would not. A similar entailment follows from a simple variation on the previously discussed hypothetical scenario supposing that Franklin died before seeing the image she had produced of the structure of DNA. Imagine that instead of her colleagues knowing what she was working on, Franklin was an extremely secretive researcher. When her colleagues came upon her image of DNA after her death, they at first had absolutely no idea what it showed. It took weeks of sorting through her lab notes (written in code to prevent snooping), reverse engineering her image-production device, performing tests on the samples, and so forth, before they came up with the theory that the image showed DNA's structure. According to Nickles's view, Franklin's colleagues were engaging in discovery whereas Franklin's production of the image does not qualify as such.

It would appear that those who hold traditional cognitivist conceptions of discovery must either (1) conclude that the revelation of the structure of DNA by image-making technologies is not discovery, which is problematic for the reasons described above, or (2) acknowledge that such conceptions are incomplete and do not account for all types of discovery. Producing an image of DNA is neither a fluke nor a stroke of luck. Rather, x-ray crystallography and other image-making technologies play a systematic role in science. Equating discovery with particular cognitive acts causes one to preemptively exclude such image-making processes from consideration, even when one can use them to arrive at the same results

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as insight. It seems that therefore we have ended up with theories of insight rather than theories of discovery. This is not to suggest that discovery should be defined in terms of results rather than processes but rather that the processes do not necessarily have to involve insight. This also does not necessarily negate what friends of discovery have written about insight. Rather this article suggests that, as they stand, traditional conceptions of discovery are incomplete and that an adequate framework of discovery must address image making as a potential mode of discovery.

5. Discovery Reconsidered. One might propose two different paths toward discovery. On the one hand, one can take something that is already visible and identify it, define it, or think about it in a different way. This type of discovery invariably involves insight and transformative cognitive processing. Kuhn's discussion of Uranus in *The Structure of Scientific Revolutions* concerns this type of discovery, as do many of the case studies introduced by the friends of discovery. Another path concerns directed attempts to visualize something that is already known to exist. It is the second type of discovery that has been the focus of this article. This path may involve insight, as in Watson and Crick's theorizations about the structure of DNA. However, it might also involve the directed application of technologies, as in Rosalind Franklin's x-ray crystallography. Of course, the two are not mutually exclusive, and a given researcher may utilize both strategies. In fact, despite Franklin's fierce commitment to revealing DNA's structure via direct methods, her notebooks contain evidence of her speculations on the subject, which included using models to try out various possibilities (Klug 1968, 844). Nonetheless, the directed application of technologies in order to produce diffraction images was a major component of Franklin's research program, and her success at producing exceedingly high-quality images constituted one of her key accomplishments.

This article suggests that theories of discovery that do not account for the use of image-making technologies are inadequate. I argue that the philosophy of discovery should recognize image making as a type of discovery I will characterize as "external" since it takes place apart from internal cognitive processes. When one engages in external modes of discovery, one uses (often technological) strategies to capture an imprint(s) of some facet(s) of the world and/or its processes that are otherwise not sensible. That is, agency is employed to cause an unseen phenomenon to affect the world in some manner such that it then becomes available to the visual, auditory, tactile, or olfactory sense. Franklin's discovery is specifically an act of making visible. If the DNA model had been readily available to sight, its structure would not have needed discovering; it already would have been known. No one need discover that a baseball

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has a spherical structure because it is already available to sight. We discover those structures that are naturally hidden. Therefore, there is something essentially visual about the discovery of the structure of DNA, though making visible is only one possible mode of external discovery. It does not matter if one understands the details of what has been made visible; what is important for attribution of discovery is the directed act of making sensible, which often involves a good deal of labor and skill.

The attainment of solutions through direct modes of visualization is an epistemologically significant aspect of science. It is an important means by which scientists gain knowledge about the world. Modes of visualization are not a matter of luck or happy accident, nor are they psychological and therefore do not belong within the realm of psychology. But the question remains, are they rational? The question of rationality is central given this article's position that the noncognitive act of making visible is a proper subject for philosophical accounts of discovery. It was precisely because of its presumed lack of rationality that Reichenbach, Popper, and others banished the discovery process from the realm of philosophy. A friend of discovery might therefore concede that making visible is an act of discovery, and one that is not properly acknowledged in standard definitions of discovery, but maintain that it is not a philosophically interesting mode of discovery. We can therefore simply acknowledge it, dismiss it, and move on. According to this perspective, only insight-driven discovery is potentially rational and therefore is the only mode of discovery that is amenable to philosophical analysis. However, Laura Perini's work provides a way of thinking about images as rational, thereby suggesting a means by which discovery by image making might be viewed as a proper subject for analysis, even within a philosophical system strictly delimited by rationality.

In "The Truth in Pictures," Perini argues that visual representations in science can be understood as components of logical arguments with the capacity to bear truth. She defines the truth of a system as "an appropriately systematic relationship between the form of its symbols and the states of affairs they refer to" (2005, 280). Perini explains that many pictorial systems have a syntax and semantics that allow one to articulate a precise definition of their truth conditions in a straightforward manner in terms of linguistic expressions. Other types of images, such as electron micrograph images, are less precisely specifiable, though no less truth bearing. As she describes, "the visible form of the representation is correlated with its content in a way that is difficult to describe with precision. It is like black and white photography in that gradations in light/darkness in both horizontal and vertical dimensions are relevant throughout the image" (279). As Perini notes, though there is no systematic way to precisely translate such images into linguistic expressions and thereby define

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their truth, there is a systematic relationship between the form of the image and its referent; it is just not localizable in discrete units. As a result, we can arrive at an "informal description" of the definition of truth for the system. For the micrograph the truth condition is as follows: "an electron micrograph is true IFF the shape of the micrograph is a geometric projection of the shape of the sample scanned in producing the micrograph" (281). Perini claims that our ability to precisely express an image's truth conditions is not a precondition for its ability to bear truth. Therefore, we can reasonably suggest that images like the micrograph are logical bearers of truth claims.

In Perini's account, the capacity to bear truth is interwoven with the act of interpretation. Images are logical bearers of argument because of a relationship between "symbol form and its *interpreted* content" (2005, 281; my emphasis). Of course this article's argument regarding certain acts of making visible as a form of discovery proposes that credit is due even when the image is not in fact interpreted. It has argued that Franklin's image still constitutes discovery even when a given interpreter (or interpretive community) lacks the resources necessary to elaborate its content-bearing features. Therefore, an external account of discovery would want to modify Perini's account by specifying that to be a bearer of truth, an image must simply be interpretable. This allows us to afford logical, rational status to an image that through some accident of circumstance fails to be fully or even partially interpreted.

Given Perini's account and this article's revision of it, we might generate an account of the crystallography image as rational. In a coarse-grained sense, x-ray crystallography images are similar to micrographs in that gradations of light provide structural information. Moreover, though the techniques of production and interpretation differ quite markedly, the crystallography image is like the micrograph in that it too is characterized by a systematic relationship between form and interpretable content. Therefore, by the definition established above, the x-ray crystallography image has the capacity to bear truth. Consequently, we might suggest that in the process of creating her x-ray crystallography images, Franklin produced logical arguments about the structure of DNA. These hypotheses were open to refutation; one might suggest, for instance, that an image is an inaccurate representation of DNA's structure because the sample was not correctly prepared for imaging. Or it is also possible that alternative methods might contradict the claims inherent in an x-ray crystallography image. We can see, therefore, how image making might begin to be interrogated as a logical part of the discovery process. Furthermore, we see how it can be situated within a hypothetico-deductive vision of science. It is beyond the scope of this article to provide a full account of the rationality of the scientific image. Moreover, Perini's work is merely

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a starting point for thinking about how to do so, and other frameworks might ultimately prove preferable.[6] This article instead modestly suggests that if accounting for such images as logical truth claims is conceivable, then reconciling the position that discovery is a rational process with the view that image making constitutes a form of discovery is at least possible.

This article's externalist account of discovery has implications beyond crystallography and the case of Franklin. Recent years have seen the development of a growing number of imaging technologies that produce images of what cannot be seen by the naked eye. Therefore, in its potential to revise accounts regarding visual dimensions of science, an externalist account of discovery not only is of philosophical import but also has significance for social and historical accounts as well.

6. Rosalind Franklin's Role Reconsidered. Rosalind Franklin produced exceptional x-ray images of DNA in part because of her technical skill in the preparation of samples for x-ray analysis and her ability to design and use experimental imaging apparatus. Making visible is often a difficult and even intellectually demanding feat. An externalist conception of discovery that recognizes making visible as an act of discovery enables the valuation of these skills as primary in the scientific endeavor. Franklin's x-ray images are no longer constraints that guide discovery but rather are discovery itself. As Gruber (1980) argues, discovery is not always the work of a heroic moment of insight but occurs frequently and often on a small scale. The externalist conception of discovery extends his point further to recognize more than just insight as a part of discovery. Franklin may never have experienced a eureka moment, but she made a number of discoveries as she revealed parts of the structure of DNA.

Watson and Crick seemed to have subscribed to a view of science that valued heroic insight above all else. Indeed, while it is true that Watson made disparaging and sexist remarks about Franklin, it is also the case that he made dismissive remarks about her mode of investigation, experimental crystallography. While these comments do not draw attention in the way that his statements about her appearance do, they are equally important for understanding why Franklin may not have been given appropriate credit for her contributions to the discovery of the structure of DNA. As Watson describes in *The Double Helix*, he thought that x-ray crystallography was the key to genetics for a time but found the study of crystallography quite boring. He describes how he was almost relieved by his failure at crystallizing myoglobin molecules because if he had been successful, his supervisor "might have put me onto taking X-ray photographs" (1968/1996, 50). He was also appreciative of Crick's willingness

6. See Goodwin (2009) for a critique of Perini.

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to share crystallographic facts with him because they were "ordinarily available only through the painful reading of professional journals" (50). But, as he explains, he soon learned that Linus Pauling did not discover the alpha-helix "by only staring at X-ray pictures"; Pauling instead went about solving the structural problem by means of model building (50). And with that, Watson happily realized that he could leave the details of crystallography to others. It was true that Crick was a trained crystallographer and was working on a thesis concerning x-ray diffraction and proteins. But he was not of the usual ilk. As Watson explains, his theories ranged far outside crystallography. Furthermore, he was essentially drawn to and was adept at theoretical interpretation, often providing interpretations of other people's data, and often to their chagrin. Watson himself characterized Crick as being somewhere between a theoretical crystallographer and an experimentalist. He explains how Crick would sometimes do experiments but get bored and turn to theory, only to realize that his theory did not work and go back to experimentation again (8–10).

Watson and Crick both seemed to view crystallography as a form of necessary drudgery. As Watson described, x-ray images of DNA were useful because they narrowed down the possibilities for theoretical model building. In *The Double Helix*, Watson stated that "inspection of the DNA X-ray picture should prevent a number of false starts" (1968/1996, 54). And they were not alone in the view that crystallography can be tedious. As Maureen Julian describes, crystallography has sometimes been considered a service rather than a discipline and was even referred to as a form of "intellectual knitting" (1990, 335). The term "intellectual knitting" carries the negative implication that crystallography is a matter of rote skill rather than a site of scientific innovation. It seems, therefore, that Franklin's role in the discovery of DNA was relegated to the shadows while the spotlight fell squarely on Watson and Crick's work at least in part because of the common conception, shared by Watson and Crick, that discovery consists in cognitive acts of insight. Even historians discuss the discovery in such terms.

Much of the discussion of whether or not Franklin received due credit for her role in the discovery of the structure of DNA focuses on what she knew, as we can surmise through her writings, lectures, notes, and conversations. Two related questions often arise in such discussions: (1) to what extent she accurately interpreted her x-ray images and (2) how many correct beliefs she had regarding DNA's structure. Historians also address Franklin's failure to solve the puzzle of DNA's structure before Watson and Crick in terms of cognitive factors. Horace Judson, author of *The Eighth Day of Creation*, found her lacking in her overall grasp of the structure: "it is easy to feel great sympathy with Franklin. The fact remains that she never *made the inductive leap*" (1996, 148; my emphasis).

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Klug similarly explains, "Franklin hadn't *understood* the two-fold symmetry at this point. She says the structure is believed to have two chains per unit cell, and she knew those two chains were anti-parallel in the A form, but *she had not made the transition* to say that the two chains in the B form must also be anti-parallel. But she was poised to *make that move*" (2003, 7; my emphasis). In the foreword to Olby's *The Path to the Double Helix*, Crick, perhaps more generous in his views on Franklin than Watson, says that she was two steps away from discovering the structure of DNA. As he describes, "she needed to *realize* that the two chains were anti-parallel and to discover the base-pairing" (1974, vi; my emphasis). With their focus on what Franklin knew, such conversations are based on the presumption that discovery is an act of cognitive insight.

The question of whether credit was properly appropriated for the discovery of the structure of DNA cannot be addressed solely by pointing to instances of sexism and scientific dishonesty. These are important concerns but not the only ones. The appropriation of credit for any discovery fundamentally involves conceptions of discovery, and most discussions of Franklin's role presume that discovery necessarily involves insight. This article's contention is that we do not necessarily need to know what was in Franklin's head in order to assess her role in the discovery of the structure of DNA. Furthermore, the devaluation of her role seems to have had as much to do with traditional perspectives on discovery, with their high esteem of intuitive and speculative leaps and their lower regard for empirical crystallography research, as it had to do with Watson and Crick's attitude toward Franklin and their inappropriate access to her work. I argue that we can conceive of Franklin's research as being a form of discovery, just one that does not fit with traditional conceptions of discovery as an internal cognitive act. Furthermore, it seems likely that other discoveries could also be reinterpreted in this light.

7. Conclusion. An externalist sense of discovery introduces a new avenue of pursuit for the so-called friends of discovery. But while it enables us to acknowledge Rosalind Franklin's work as discovery, it sets us on a path toward possibly accepting innumerable others as discoverers of DNA's structure. For example, researchers in William Astbury's lab had produced good images of DNA but never thought to publish them, and they were not shared. Raymond Gosling, as Franklin's student and collaborator, obviously participated in her acts of making visible. Furthermore, Rudolf Signer, who extracted the prized DNA that Franklin used for her images, also played a role in making visible. Do we consider all these people discoverers in the externalist sense? Where do we draw the line? That is not a question for which I can provide an answer here. Recognizing external discovery might inevitably result in a diffuse sense

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of discovery and discoverers. And perhaps that is not a bad thing. Lamb and Easton (1984) have argued for a less individual, more multitudinous sense of discovery. The encasement of the human skull as an inevitable limit to the attribution of discovery seems to be part of the attraction of an internalist conception of discovery. But at the same time, perhaps we want to avoid the admittedly attractive, but perhaps misleading, myth of science as the domain of the heroic genius who discovers a new idea. While romantic, perhaps it is less faithful to the true process of scientific discovery, which involves many people, each contributing some, often small, part to the process. So in conclusion, I would like to suggest that while it is true that Watson and Crick discovered the structure of DNA, it is also true that others, such as Franklin, did as well.

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