The Social Construction of Facts in Psychology

Philosophy of a Specific Discipline Bachelor Thesis

Words: 9723

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Main study: Bachelor in Psychology
Date: 15th of June 2021

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> [T]he question of knowing if human thought is able to reach an objective truth is not a theoretical question but a practical question. It is by practice that man ought to prove the truth, that is the reality and the power of the something beyond his thought.
>
> Marx (1870)

The notion of objective truth has always been a controversial topic. A pragmatic approach, like the one mentioned above by Marx,[1] focuses on practice as a way to bridge the gap between theory and reality. What is problematic about this perspective, however, is the fact that, consciously or unconsciously, practice is also affected by the theories that we endorse. Moreover, the theory ladenness inherent in our observations may cast doubt on their objectivity. Nonetheless, empiricism and its principles still have quite some authority when it comes to scientific discovery nowadays. The Enlightenment period had a major impact on the way science was integrated within our society. Francis Bacon's perspective of nature as something that we must conquer and control is perhaps one of the most important views of that time, which still resonates with scientific praxis nowadays. Seeing science as disclosing an image of truth promoted the idea that there is such a thing as a truth out there to be discovered by us.[2]

Four centuries later, and more than four decades ago, Latour and Woolgar challenged this notion of science and truth, and depicted facts as socially constructed rather than waiting to be discovered. In their book, *Laboratory Life*,[3] the two embark on an anthropological analysis of how research is done in the laboratory. What they aim to show is that despite our attempts to be objective and follow empirical processes and values, human biases and methodological limitations indicate that the process of scientific discovery is nothing more than the outcome of an intermingle between contingent and historical factors (e.g., social or economic). As their stance seems quite radical, it is worthwhile to consider it in more detail. Although there currently are clear methodological guidelines and regulations that researchers should follow, these are not always successful in preventing fraud, misconduct, and the interference of commercial interests or ideological convictions with scientific inquiry. Latour and Woolgar might argue, in this sense, that even if we would work to establish stricter rules and norms for scientific discovery, these rules and norms themselves would be subjected to our biases and will never be sufficiently objective to eliminate the interference of these so called 'non-scientific' factors. Because of this,

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[1] Bruno Latour, Steven Woolgar, and Jonas Salk, *Laboratory Life: The Construction of Scientific Facts* (New Jersey: Princeton University Press, 1979), 179.
[2] Jürgen Klein, "Francis Bacon," *Stanford Encyclopedia of Philosophy*, December 7, 2012, https://plato.stanford.edu/entries/francis-bacon/.
[3] Latour, Woolgar, and Salk, *Laboratory Life*.

the two disagree with calling such factors 'non-scientific' or 'social', since they are as important as technical and methodological factors when it comes to the construction of facts.

A field where this debate seems particularly relevant is psychology, especially in view of the current problem of the replication crisis.[4] The numerous replication studies which fail to reproduce the previously claimed findings reveal a systemic problem within the field. Social priming, ego depletion, and the facial feedback hypothesis which were previously believed to be well researched effects have been recently investigated and proven to lack the necessary empirical support. It became ubiquitous for systematic reviews and meta-analysis to perform statistical analyses in order to untangle the publication bias that is prevalent among this large majority of scientific papers. Researchers' confirmation bias unconsciously influences the data, as it nudges them to only seek positive and significant results. Consequently, papers which disconfirm researchers' hypotheses never make it to publication journals. The need for recognition or funding is perhaps the main reason for the appearance of this phenomenon, although there are also selfless reasons for which a scientist might have such inclinations. The case of former Dutch professor Stapel, who was suspended for manipulating and fabricating data for almost 60 research publications, is a good example of how factors outside the scientific laboratory interfere with scientific findings. Although he emphasized it was not selfish reasons that steered him to take these actions, the impact of his deeds was not limited to the faulty scientific facts which many other papers might have referenced or based their studies upon, but it cast a negative light on social psychology as a science. This is, among other examples, a case in which the process of scientific discovery is steered by an array of social, economic, individual, technical, and political factors. This may harm the integrity of scientific 'truths' and in more severe cases lead to erroneous practical applications of such facts.

In psychology, the more rigorous guidelines and strict methods we try to implement in research, the more we realize that previous studies are methodologically inadequate or at least questionable. Even more, attempts of replicating such studies bring forward disappointing results and reveal that well established theories and frameworks are not actually empirically sound. This may, upon first sight, imply that our solution is to find objective criteria that allow us to draw reliable and valid conclusions. In practice, this may seem like the only way of approaching this problem. However, how can we guarantee that these 'objective' guidelines are not corrupt by the same factors which altered findings in the first place? Having all these in mind, this thesis aims to determine whether we can prevent such contingent factors from interfering with the process of scientific discovery in psychology. In doing so, it will adopt Latour and Woolgar's social

[4] Ed Yong, "Psychology's Replication Crisis Is Running Out of Excuses," *The Atlantic*, November 19, 2018, https://www.theatlantic.com/science/archive/2018/11/psychologys-replication-crisis-real/576223/.

constructivist view to analyze psychology as a science and explain the emergence of the replication crisis.

In exploring this question, this thesis will first highlight what distinguishes Latour and Woolgar's anthropological approach from previous sociologists' attempts. Next, the social-technical distinction is evaluated according to the beliefs of the two authors, followed by a description of their anthropological method. The next chapter details the social constructivist perspective of how scientific facts come about, how mere claims adopt a fact-like status, and how microprocesses taking place in the laboratory influence this transformation. The third chapter delves into the development of psychology as a science over time as well as the current issues that limit its development. Lastly, considerations for practice based on Latour and Woolgar's account are evaluated.

Chapter 1: The Anthropological Approach

1.1 Previous Sociological Approaches Studying Scientific Activities

Previous sociological and economic studies of scientific activity, according to Latour and Woolgar, failed to address the actual content and processes underlying scientific work, and instead focus on less common events such as extreme cases of fraud or out of the ordinary cases of success and innovation.[5] The topics they address are almost always on a macro level, often related to funding, the trend of scientific growth in general, or political interference.[6] Moreover, the views of other fellow scientists tend to monopolize the existing literature on scientific activity, as their opinions are preferred over those of "outsiders".[7] Consequently, rather than making science more tangible, the use of highly specialized concepts by social scientists amplified the already existing gap between science and the public.[8] Analyzing citations, publication numbers, and the way funds are invested for research reveals little regarding the substance of scientific papers.[9] Most importantly, such approaches are often heavily biased as they take the products of science for granted and overlook the account of their initial production. In addition to focusing only on surface aspects of science, previous sociological studies also neglect their own "methods and conditions of appraisal".[10] This may seem rather ironic, as social scientists wishing to uncover the methodological shortcomings of other researchers overlook the methodological shortcomings of their own studies. This lack of reflexivity, as Latour and

[5] Latour, Woolgar, and Salk, *Laboratory Life*, 11.
[6] Latour, Woolgar, and Salk, *Laboratory Life*, 17.
[7] Latour, Woolgar, and Salk, *Laboratory Life*, 11.
[8] Latour, Woolgar, and Salk, *Laboratory Life*, 17.
[9] Latour, Woolgar, and Salk, *Laboratory Life*, 18
[10] Latour, Woolgar, and Salk, *Laboratory Life*, 18.

Woolgar depict it, is most likely due to a focus on macroconcerns prevalent in such social studies of science rather than the novelty of the field.

In this context, Latour and Woolgar's work differentiated itself from the previous sociological studies of science as the two chose to focus on the actual content of fact production and "provide a reflexive understanding of the detailed activities of working scientists".[11] In order to do so, their strategy was to carefully examine the everyday experiences of a laboratory for two years. By looking at everyday behaviors and events rather than isolated controversial instances – such as cases of fraud or impressive success which other studies might favor, their sociological study turns its attention from macroconcerns to microprocesses and so depicts a more tangible and realistic image of science. Central to their anthropological analysis is their critique of the distinction between the social and the scientific, which will be examined next.

1.2 Social-Scientific Distinction

Not only do they try to promote this emphasis on detailed microprocesses rather than ample sociological phenomena for their own analysis, but also to make it clear to the members of the laboratory that this is what they aim to examine. Choosing to avoid the term 'sociology' when it comes to studying scientific activity stems from the effect that it has in such instances – scientists generally feel like sociologists are only interested in scandalous events. Sociological interest seems to be associated with a variety of behavioral phenomena which are separate from scientific interest. Furthermore, this stance views such social phenomena as surfacing from time to time and disrupting the otherwise unaltered scientific process. At the heart of this assumption rests the belief that the social realm is separate from the scientific world. Successful science, according to this perspective, proceeds in isolation of social factors.[12] Consequently, an observation or discovery is faulted by the presence of deleterious social circumstances. If a researcher makes a claim given the circumstances of being sleep deprived, that claim would not get the same treatment as one made by a researcher that seemed to avoid the interference of such factors. Most likely, however, one can never really 'avoid' these factors as they are not extraneous to the scientific process.[13] The notion that socio-political factors only occasionally interfere with the scientific process is erroneous, according to Latour and Woolgar. In addition to that, clarifying the way scientific knowledge is generated will render it more comprehensible for the public and replace the existing views of science as something mystical, that "generates too much hope and too much fear".[14]

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[11] Latour, Woolgar, and Salk, *Laboratory Life*, 18.
[12] Latour, Woolgar, and Salk, *Laboratory Life*, 21.
[13] Latour, Woolgar, and Salk, *Laboratory Life*, 22.
[14] Latour, Woolgar, and Salk, *Laboratory Life*, 13.

Before Latour and Woolgar, Mulkay tried to bring attention to this neglected technical aspect of science by adopting Kuhn's view of scientific activity as being paradigm-bound.[15] More exactly, Kuhn explains that within a paradigm, research is limited to a certain range of concepts and methods that are in line with the theories of that paradigm. Moreover, stumbling upon phenomena which cannot be explained by these paradigm specific conceptualizations leads to a crisis that starts what he called a paradigm shift. Extrapolating on this, Mulkay asserts that technical factors should be treated just like social factors since they are not as robust as one may think. Cognitive and social developments should be examined simultaneously rather than separately, as they are highly intertwined.

In this sense, the two philosophers break away from this view of the social and scientific realms. Rather than isolated, the two argue that "the scientific realm is merely the end result of many other operations that are in the social realm".[16] By saying this, the two authors assert that human affairs are always intertwined with fact production, and that conceptualizing the two as separate is not only inappropriate, but also disadvantageous for the scientists themselves and "the reports about science which they produce".[17] Choosing to only focus on what is considered as social aspects of science rather than technical narrows down the array of phenomena that can be studied, often to political and economic factors. If there is no obvious social interference, a sociological analysis of science is then considered obsolete.

Moreover, what we can often observe in science, however, is that once a fact is officially "established", the process of fact construction in which social factors have also played a role is forgotten.[18] All in all, the most prevalent view among sociologists of science is that the presence of social factors is limited to politically influenced or fraudulent events, and that ideally science would lack such deleterious determinants. There is too much emphasis put on these "social" aspects and too little on what was deemed as "technical".[19] In their book, *Laboratory Life*, Latour and Woolgar promote a view of science which highlights the way in which the social and technical realms are highly interconnected. The emphasis, however, is not placed on the technical aspects in order to compensate for the lack of studies examining it. Rather, the two argue that finding a right balance between studying social and technical factors is unnecessary since such factors are difficult to tell apart and would mean that they need to adopt the very presuppositions which they try to deconstruct.[20] Undermining this social-technical distinction is fundamental for their anthropological method, which will be discussed in the next section.

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[15] Latour, Woolgar, and Salk, *Laboratory Life*, 24.
[16] Latour, Woolgar, and Salk, *Laboratory Life*, 13.
[17] Latour, Woolgar, and Salk, *Laboratory Life*, 13-23.
[18] Latour, Woolgar, and Salk, *Laboratory Life*, 23.
[19] Latour, Woolgar, and Salk, *Laboratory Life*, 24.
[20] Latour, Woolgar, and Salk, *Laboratory Life*, 27.

1.3 Anthropological Method

Latour and Woolgar's approach of treating scientists and their production of science as tribes seems quite intriguing given the fact that most people associate tribes with exotic groups of people, often living in the wilderness. Their analogy concerning tribes, however, is neither satirical nor meant to compare scientists with such populations. Rather, it concerns the observers' attitudes and the methodology they chose to adopt, which is similar to the strategy an anthropologist would use when coming across a strange, unfamiliar community – the scientists. A commonly encountered problem with sociological studies of scientific activity is that scientists expect outsiders to seek "tales of scandal and intrigue, of behavior which fails the usual high standards of scientific enquiry, or which is unethical, of the exchange of great ideas over coffee, or of renewed acts of genius and various eureka experiences".[21] The famous case of psychology professor Stapel is a good example of such an intriguing and gossip worthy event. Consequently, the type of information that scientists choose to share with the sociologists is influenced by this expectation.[22] Latour and Woolgar's anthropological method, by contrast, tries to adopt an unbiased view towards the members of the laboratory. To do this, they specifically chose a laboratory which has no reputation of notable sociological events.[23] The focus of their study is simply the "routine work" of the laboratory and the sum of their observations constitute a "monograph of ethnographic investigation of one specific group of scientists".[24] As such, the two aim to encounter scientific activity aspects, which are often taken for granted, as foreign.

In addition to breaking free from their own biases regarding science, this anthropological investigative approach maintains a sort of independence from the scientists' statements and beliefs regarding the nature of their work.[25] The goal of their study is to dissect and understand the way scientific practices collectively led to the construction of facts. Studying the day-to-day activities of scientists, according to them, is one of the most appropriate strategies to reach this goal, since this way their observations will not be specifically focused on sociological, historical, psychological, or technical aspects of science in particular. In addition to this, their lack of competence within the field which the chosen laboratory specializes in contributes to the maintenance of this neutral observer stance. By "anthropological strangeness", Latour and Woolgar refer to this view of interpreting laboratory activities in the same manner as those of a remote culture.[26] This approach entails having "no a priori reasons for supposing that scientists' practice is any more rational than that of outsiders".[27] Hence, the adoption of an estranged attitude is meant to help the two observers abandon their prejudices about scientific activity.

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[21] Latour, Woolgar, and Salk, *Laboratory Life*, 19.
[22] Latour, Woolgar, and Salk, *Laboratory Life*, 20.
[23] Latour, Woolgar, and Salk, *Laboratory Life*, 31.
[24] Latour, Woolgar, and Salk, *Laboratory Life*, 27-28.
[25] Latour, Woolgar, and Salk, *Laboratory Life*, 28.
[26] Latour, Woolgar, and Salk, *Laboratory Life*, 29.
[27] Latour, Woolgar, and Salk, *Laboratory Life*, 29-30.

One last distinctive feature of their work is their aspiration to maintain a certain degree of reflexivity through their study, in the sense of being aware that their own observations are very similar to those of the scientists that they study. They show their awareness throughout the book and emphasize the methodological shortcomings which they stumble upon in their own attempt to construct and present their observations. Moreover, what is often difficult in such sociological studies of scientific activities is the fact that they are often accompanied by a connotation of lack of respect towards science and scientists. Consequently, members of the laboratory might be apprehensive and defensive towards the scrutiny of the observers. The two philosophers, by contrast, adopt an agnostic position and promote the belief that the nature of scientific activity is "widely misunderstood".[28] The premise of their entire work is that scientific activity is just like any other "social arena" where knowledge and facts are being constructed.[29] Instead of writing a sociological analysis regarding scientific norms and thus subscribing to the social versus technical mentality, the two are interested in the social construction of scientific knowledge and the processes which steer researchers to bring meaning to their observations.[30] This process by which facts are socially constructed will be detailed in the next chapter, followed by a discussion of how psychology can be interpreted from this perspective and what implications it brings to psychological research.

Chapter 2: Social Construction of Scientific Facts

2.1 The Transformation of Statement Types

In their pursuit to make sense of the alien activities of the members of the laboratory, the "observer"[31] – a fictional character which Latour and Woolgar use throughout their book in order to highlight the "fictional nature of the account-generating process",[32] depicts the scientists as a "tribe or readers and writers" spending most of their time deciphering large inscription devices.[33] The inscriptions which result from their craftwork are then useful for another important scientific activity – persuasion. This persuasion skill is crucial as they need to convince others that their work is important, valid, and worth the funds that they have received. Through this persuasion, they are able to give off the impression that they are merely adhering to the sole valid interpretation of the available information. This sort of approach is limited as it views scientific facts as truths out there to be discovered, and science as concerned with "hard facts" only.[34]

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[28] Latour, Woolgar, and Salk, *Laboratory Life*, 31.
[29] Latour, Woolgar, and Salk, *Laboratory Life*, 31-32.
[30] Latour, Woolgar, and Salk, *Laboratory Life*, 32.
[31] Latour, Woolgar, and Salk, *Laboratory Life*, 69.
[32] Latour, Woolgar, and Salk, *Laboratory Life*, 41.
[33] Latour, Woolgar, and Salk, *Laboratory Life*, 69.
[34] Latour, Woolgar, and Salk, *Laboratory Life*, 70.

Participants of scientific endeavors advance the idea that all their activities are straightforward, thus showing a lack of reflexivity in their thought processes. The process by which such claims reach this 'fact' status will be elaborated next.

The main goal of the scientific activity done by the members of the laboratory studied by Latour and Woolgar is the production of scientific papers which, in turn, has the objective of persuading the readers that their content is valid. Hence, in their attempt to gauge the importance of these papers, the two writers focused on the content of such papers as well as the steps which scientists go through in order to produce them. More exactly, they classified statements based on the extent to which some appear more "fact-like" than others.[35] A statement gains the status of a fact when the readers are certain that there is no ongoing debate concerning it or the process of discovering it, or when it becomes taken for granted. Statements which possess such a fact-like status can then be used in debates regarding the validity of other discussed statements. They constitute the fundamental framework of a certain theory and do not often occur in conversations. Unlike such statements – called type 5, type 4 statements are more often made explicit, especially in learning materials such as textbooks. They are, nonetheless, accepted as fundamental knowledge. Type 3 statements involve modalities – statements that refer to other statements. Instead of making a direct assertion, like "rates of depression are higher among women", type 3 statements use modalities – "rates of depression *were reported to be* higher among women". This formulation casts a completely different nuance on the discussed statement as they bring attention to the human agency involved in their production and thus make it less likely that the statement is viewed as an objective truth out there. These statements are much more present in scientific papers and discussions than type 4 and type 5 statements. Still, even more present are type 2 statements which may be more readily labeled as claims rather than facts. Similar to type 3 statements, type 2 statements contain modalities. These modalities, however, emphasize the circumstances surrounding the claim. They contain formulations such as "there is a large body of evidence that supports this claim" or "what is generally known about this claim".[36] They are frequently accompanied by tentative suggestions and solicitations for further investigations. Lastly, type 1 statements can be found most often towards the end of papers or in less formal discussions and involve speculations or hypothesized relationships.

As such, Latour and Woolgar depict the fact-like status of a statement using a continuum, with type 1 statements being the most speculative, and type 5 statements being the closest to the 'fact' label.[37] They also add that a statement goes through all these states before attaining the fact-like status of a type 5 statement, as it gains more empirical support and becomes accepted by more and more researchers. The status of a statement is not always easy to identify, however. The

[35] Latour, Woolgar, and Salk, *Laboratory Life*, 76.
[36] Latour, Woolgar, and Salk, *Laboratory Life*, 70.
[37] Latour, Woolgar, and Salk, *Laboratory Life*, 79.

inclusion of a reference can place a statement both under the type 3 and type 4 labels.[38] On one hand, by providing a reference, it makes evident the circumstances surrounding the statement and brings it closer to a type 3 statement. On the other hand, due to the addition of that reference the statement gains more credibility - provided that the source is credible. During the course of a conversation in the context of a laboratory specific activity, statements are often invoked and transformed. Consequently, through such discussions the claims change their ontological status, from mere presuppositions to unquestionable facts believed to reflect the external reality.

Seen through this lens, the objective of laboratory activities was creating type 4 statements and in doing so convince other researchers that they should also adopt these statements as "an established matter of fact".[39] To a certain extent, this reveals the way scientific facts possess a sort of normative dimension, as their legitimacy is, after all, dependent on the ones who accept it as valid. Others' recognition of a certain statement increases the feelings of concreteness regarding the objective existence of what the statement asserts – an object or condition, as if the statement seems "to relate to something outside of, or beyond, the reader's or author's subjectivity".[40] Likewise, the inclusion of a figure in the results section enhances the credibility of a statement as it tacitly assumes the idea that the findings have "an external reference and an independent existence".[41] However, these figures are themselves based on the measures and inscription devices that were created by the researchers, thus creating a sort of circularity when it comes to the interpretation of the figures. Taken together, statements seem to be dependent on the judgments of the members of a scientific community as they constitute the basis for which the statement's legitimacy is evaluated. The exact processes by which these value judgements are made will be further explored in the next section.

2.2 Microprocessing of Facts

As mentioned previously, Latour and Woolgar depict laboratory activity as a continuous generation and transformation of statements – also called literary inscription, with the end goal of persuasion. Whereas members of the laboratory insist on merely discovering facts, the anthropological perspective posits that their activities closely resemble those of "writers and readers" who are taking part "in the business of being convinced and convincing others".[42] In doing so, statements are discussed and analyzed, and their fact-like status is either enhanced or diminished. Certain historical cases, such as the discovery of the releasing factor TRF, make it evident how this process is influenced by contingent and contextual factors. The choice of chemical analysis methods specific to peptides, the resilience of two researchers – Guillemin and Schally, their choice of strategy, the funding they had available and standards they followed – all

[38] Latour, Woolgar, and Salk, *Laboratory Life*, 80.
[39] Latour, Woolgar, and Salk, *Laboratory Life*, 81.
[40] Latour, Woolgar, and Salk, *Laboratory Life*, 84.
[41] Latour, Woolgar, and Salk, *Laboratory Life*, 85.
[42] Latour, Woolgar, and Salk, *Laboratory Life*, 88.

of these factors made the 'discovery' of TRF's structure possible. In the spirit of Latour and Woolgar's anthropological approach, a more suitable way to describe it rather than mere discovery would be the construction of TRF's structure as the result of a collective social process. As such, the two authors dedicate a chapter to discuss these seemingly minor aspects of fact construction, or as they call them, "microprocesses".

Although the 'social' in social constructivism seems to allude to tales of scandal and ideological influences, Latour and Woolgar actually refer to the less evident aspects underlying science, such as the routine exchanges and subtle gestures shared between scientists. Ultimately, they wish to "demonstrate the idiosyncratic, local, heterogenous, contextual, multifaceted character of scientific practices".[43] The seemingly logical thought processes characteristic of science are accompanied by "local, tacit negotiations, constantly changing evaluations, and unconscious or institutionalized gestures".[44] During conversations, for instance, the microprocesses of fact construction often surface as statements are reinforced, modified, or negated. Within the context of a short discussion several "complex negotiations" take place in which the expertise, history, and status of a researcher will influence what arguments will be seen as appropriate and what arguments will be dismissed.[45] If a claim made by researcher X was subsequently proven false, that researcher's credibility will be affected and, consequently, future claims made by that researcher will be treated with reluctance. This further illustrates the normative dimension inherent in fact construction.

Conversely, some other claims are reinforced as researchers rest their arguments on them. In other words, type 5 and type 4 statements gain more and more legitimacy as they are used to explore less 'fact-like' type 3, type 2, and type 1 statements. Altogether, these small exchanges between scientists seem to be devoid of objective statements considering the fact that they are always influenced by the "negotiation between participants".[46] Moreover, conversations between scientists also reveal the plethora of evaluations underlying their choices regarding different courses of action. Standards and exigencies of practice, time constraints, controversial outcomes – these are among the factors and interests which preoccupy scientists' decision-making process in addition to theoretical and methodological aspects. Furthermore, bringing up human agency in the evaluation of a statement is also frequently encountered during such discussions. As mentioned earlier, the credibility of the claim is dependent on the credibility of the researcher who made that claim.[47] In the example of the researchers investigating TRF, Latour and Woolgar point out the different approaches that the two laboratories had when interpreting each other's findings. While one laboratory dismissed what the other was saying, the other laboratory made

[43] Latour, Woolgar, and Salk, *Laboratory Life*, 152.
[44] Latour, Woolgar, and Salk, *Laboratory Life*, 152.
[45] Latour, Woolgar, and Salk, *Laboratory Life*, 157.
[46] Latour, Woolgar, and Salk, *Laboratory Life*, 158.
[47] Latour, Woolgar, and Salk, *Laboratory Life*, 164.

use of the discoveries of the first and treated them as legitimate findings. This shows the advantages and disadvantages of a competitive environment. On one hand, competitiveness encourages innovation and facilitates motivation. On the other hand, it can lead to biases that impede innovation. Nonetheless, it shows how credibility influences decision-making in research and may negatively interfere with the validity evaluation of certain pieces of evidence.

Taken together, it seems like the construction of facts is dependent on these microprocesses happening between researchers. Different types of preoccupations and interests, of both technical and seemingly less technical nature, are enmeshed in conversational exchanges that take place in the laboratory. Eventually, when a statement reaches its final, fact-like status as a result of these microprocesses, "the retrospective characterization of scientific activity" ends up being replaced "with epistemological descriptions of thought processes and logical reasoning".[48] It is at this point that a statement becomes taken for granted. Statements are situated on a continuum that reflects the extent to which the inclusion of a reference regarding the conditions of its construction is necessary. When they reach the status of a type 4 statement, including such a reference becomes obsolete and may even be considered unusual.[49] More exactly, what often happens in this process is that the social, "localized, heterogeneous, and material" circumstances that lead to the construction of an idea or connection are overlooked.[50] Instead, tales of brilliant minds coming up with innovative ideas take their place.[51] Consequently, although the work done by researchers is portrayed as consisting in mere analogical reasoning most of the time, Latour and Woolgar argue that the processes underlying their work often employ logical leaps and are reliant on the contingency of a "complex set of local circumstances which temporarily makes possible a weak link" and translates into the intuition that a certain scientists had.[52] Thus, the two show how certain discussions and decision-making processes indirectly challenge the fact-like status of statements, as well as the way that the retrospective characterization of the construction statement is forgotten when it achieves such a fact-like status. It still remains, however, unclear how statements are produced in the first place. As such, the next section will look into the way scientists interpret observations to come up with initial claims.

2.3 Production of Order from Disorder

In understanding how statements come about initially and how researchers make sense of their observations, Latour and Woolgar put forward an example regarding Bell's discovery of pulsars. Previously, sociologists were interested in the norms or social circumstances of that time, and only a handful of them addressed the technical constraints which made Bell's discovery stand

[48] Latour, Woolgar, and Salk, *Laboratory Life*, 41.
[49] Latour, Woolgar, and Salk, *Laboratory Life*, 176.
[50] Latour, Woolgar, and Salk, *Laboratory Life*, 170.
[51] Latour, Woolgar, and Salk, *Laboratory Life*, 171.
[52] Latour, Woolgar, and Salk, *Laboratory Life*, 174.

out.[53] His reluctance to investigate the abnormal activity earlier than he did can be explained by invoking the way research is organized at Cambridge, or the influence of some past conflicts within the field. Latour and Woolgar argue that technical events are not limited to "mere psychological operations", but rather that such observations based on perception are shaped by "prevalent social forces".[54] Innovation and discovery, in this sense, are quite interesting to investigate given the fact that they entail making sense of some chaotic perceptions and turning them into well-established scientific facts. The two writers invoke different sociological features to explain the occurrence of such scientific processes which they call the "production of order" (p36) from chaos.[55] More concisely, they want to answer the questions "what social forces shape our observations and perceptions into scientific facts?" and "why is a certain explanation chosen over another?". By choosing a certain statement over others, an observer is always faced with the "philosophical problem of the constant availability of alternative descriptions and readings".[56] What often happens is that our observations are theory-laden in the sense that those taken for granted facts act as a lens through which we view novel phenomena.[57] In Latour and Woolgar's words, the adoption of diverse frameworks aids researchers in creating a coherent depiction of the studied phenomena. Training in a specific field as well as theory driven behavior will materialize into expectations that, in turn, will shape the way one perceives phenomena.

To explain how this happens, Kuhn proposed that we are subjected to perceptual theory loading – our conceptual resources influence the way we perceive phenomena.[58] He depicts the experiment of perceptual psychologists Bruner and Postman in order to illustrate this. The two researchers showed atypical playing cards to participants and asked them to report what cards they have just seen. The participants, although exposed to cards such as a black four of hearts, report seeing a red four of hearts – consistent with their existing schema about playing cards. Only after a few trials are they able to notice that the cards are atypical and start giving the correct descriptions. In addition to that, Kuhn asserts that we are also heavily influenced by the notions and concepts of a theory. He called this semantical theory loading and justified that the meaning of the words we use to describe our observations affects the way we conceptualize and explain our perceptions. Lastly, Kuhn adds that a theory makes certain phenomena and features stand out more, and that depending on the theory, some aspects are more salient. In the previously mentioned discovery of pulsars, Bell did not see the observation he made as salient and treated it as noise. It took him a while to turn his attention to the phenomenon and examine it, just like it took participants in Bruner and Postman's study some time to notice the atypical cards. On one hand, relying on a theory to "determine what one takes to be the epistemic bearing of observational evidence on

[53] Latour, Woolgar, and Salk, *Laboratory Life*, 33.
[54] Latour, Woolgar, and Salk, *Laboratory Life*, 33.
[55] Latour, Woolgar, and Salk, *Laboratory Life*, 36.
[56] Latour, Woolgar, and Salk, *Laboratory Life*, 36.
[57] Thomas S Kuhn and Ian Hacking, *The Structure of Scientific Revolution* (Chicago; London: University Of Chicago Press, 2012), 63.
[58] Kuhn and Hacking, *The Structure of Scientific Revolution*, 63.

that very theory" can be considered as inadequate as simply ignoring that piece of evidence in the first place.[59] On the other hand, we need to commit to a certain theory if we want to make sense of empirical evidence.[60]

Another good example of social construction can be observed in the way neuropsychology conceptualizes the various functions of the brain. The functions and connections of the brain are very complex and intricate. To aid our understanding, however, we chose to view these highly connected features as distinct separate layers. As such, we are using social constructions to make it easier to conceptualize these neuropsychological processes. These social constructions, in turn, heavily influence our subsequent observations and even perceptions, as was elaborated earlier.[61] Whereas for the natural sciences such observations and perceptions can be altered and subjected to different preconceptions and semantical meanings, sociological sciences are by their nature even more vulnerable to such factors as their main activity is often interpretation. Based on the process of fact construction elaborated in this chapter, the rest of the thesis is dedicated to analyzing trends in psychology from this anthropological perspective. Towards the end, implications based on this way of viewing knowledge will be considered.

Chapter 3: Psychology as Socially Constructed

3.1 Development of Psychology

Before delving into the development of psychology as a science over the past two hundred years, it can be insightful to review more recent perspectives regarding the direction it followed. Krugman identified a trend within the social sciences to aspire towards the rigor of natural sciences when it comes to methods and frameworks.[62] Despite the questionable assumption that the methodology of natural sciences is as rigorous as he portrays it, what feels inadequate about this tendency on a conceptual level is that disciplines centering around complex human behaviors are striving to have the mathematical certainty of fields such as physics. While in physics, frameworks such as Newtonianism or Relativism have dominated the discipline for long periods of time, the state of psychological research is significantly younger and less consistent. In the beginning, Wundt – the founding father of psychological research, had an approach that focused on investigating the conscious processes of the human psyche, consisting of emotional,

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[59] James Bogen, "Theory and Observation in Science," *Stanford Encyclopedia of Philosophy*, March 28, 2017, https://plato.stanford.edu/entries/science-theory-observation/.
[60] Kuhn and Hacking, *The Structure of Scientific Revolution*, 63.
[61] Bogen, "Theory and Observation in Science."
[62] Paul Krugman, "How Did Economists Get It So Wrong?," *The New York Times Magazine*, September 2, 2009, https://www-nytimes-com.eur.idm.oclc.org/2009/09/06/magazine/06Economic-t.html.

cognitive, and volitional functions among others.[63] In the context of the current discussion, Wundt had two important contributions. First of all, he viewed psychology as a hybrid between philosophy and science. On one hand, psychology belongs to philosophy because it shares the philosophical objective of "explaining rules of genesis, connection, and separation of those mental representations with an epistemic character".[64] On the other hand, it adopts a scientific methodology in the way that it synthesizes facts, thus proving to be similar to other sciences. Psychology, for him, was supposed to find the biological correlates of thought and behavior, and the mental laws which determine the way our consciousness operates.[65] Second of all, Wundt stresses that psychology should only entail the analysis of conscious phenomena and processes and rejects any attempt to account for subconscious processes. His main objection was that making assertions about unconscious phenomena would entail faulty methodological approaches. As such, Wundt limits psychology to the study of direct human experience. However, he also connects *völkerpsychologie* – cultural psychology, to it as the branch of psychology which explores the general mental development as seen through cultural artefacts, social customs and norms, myths, laws, and other cultural features, but only as they are situated in direct relation to the individual.

Conversely, psychologists that followed Wundt do not share the same reluctance when it comes to hypothesizing about unconscious processes. The branch of psychoanalysis, promoted by figures such as Freud or Lacan, is centered around what goes on in the unconscious.[66] Freudian psychoanalysis, for instance, depicts psychological troubles as often stemming from repressed traumatic memories. Despite the controversies which surround it, such an approach was thoroughly criticized from a methodological point of view. According to a Popperian perspective, psychoanalysis is based on faulty methodological practices: the concepts used are vague and difficult to measure, its theories lack thorough testing with representative samples, its effectiveness as a form of psychotherapy is limited, and most importantly, it lacks falsifiability. More exactly, the predictions made within a psychoanalytical framework cannot be proven wrong. Rather than following a hypothetico-deductive approach of testing assumptions, psychoanalysis relies on explaining events retrospectively. On the other hand, if psychoanalysis proves to be effective for some people, perhaps it should not be so readily dismissed. Although Popper's criticism is valid for hard sciences such as physics, its applicability to the humanities might be more limited. In line with Krugman's view, rather than prediction or falsifiability, other criteria should be evaluated for judging the value of these sciences, as the complexity of human behavior might not be adequately captured by prediction models.

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[63] Alan Kim, "Wilhelm Maximilian Wundt," *Stanford Encyclopedia of Philosophy*, September 10, 2016, https://plato.stanford.edu/entries/wilhelm-wundt/#OrdeKnow.
[64] Kim, "Wilhelm Maximilian Wundt."
[65] B R Hergenhahn and Tracy B Henley, *An Introduction to the History of Psychology* (Toronto, Ontario: Nelson Education, 2014), 248-278.
[66] Hergenhahn and Henley, *An Introduction to the History of Psychology*, 491-532.

As a countermovement to the questionable practices of psychoanalysis, behaviorism took a completely divergent approach to studying the human mind and limited psychology to the study of behavior, as it represents the only objective phenomena that we can reliably study.[67] Whereas Wundt's Leipzig school relied on introspection in order to study the basic elements of thought and on researchers themselves to act as subjects, behaviorism adopted a more rigorous approach. Watson, an important figure in behaviorism, had a major impact as he proposed to change the objective of psychology from description and explanation of mental states to the prediction and control of behavior. He thus brought psychology as a discipline in line with logical positivism and operationalism, as it presupposed the use of concepts that rely on verifiable and measurable phenomena. However, as with any school of thought or, in Kuhn's terms, paradigm of psychology, there were some things which behaviorism could not explain. The stimulus-response model was deemed as too simplistic and has been criticized as being unsuitable to be used for humans. Although loyal to the systematic scientific methodology and reliant on observable phenomena only, behaviorism was too reductionistic to explain complex higher order behaviors such as language and motivation.

All of these different theoretical frameworks contributed to the establishment of the cognitive-behavioral approach as the most accepted school of psychology nowadays. The focus on technological innovation and interaction between humans and machines taking place around the beginning of the Second World War facilitated the emergence of cognitive psychology.[68] The invention of the computer influenced the conceptualization of human cognition through three key ideas. First of all, the idea of information as something measurable found its way into psychological research through experiments studying processing speed and memory capacity. Second of all, the notion of feedback and feedback loops was reminiscent of biological homeostatic processes that sustain purposive activity. Lastly, programming was compared to the way human behavior can be understood in terms of programs of action that an organism follows in order to reach a specific objective. Together, these notions borrowed from computer science inspired psychologists such as Baddeley, Miller, and Boden to write about memory, language, and creativity from a cognitive perspective.

What makes this cognitive-behavioral approach stand out from the previous ones is that it adopts a more nuanced perspective when it comes to what phenomena can be operationalized and researched. It has both the strong empirical character of behaviorism, but also the necessary complexity to account for higher order cognitive processes. Moreover, it is a branch of psychology which strives to follow rigorous scientific standards, such as systematicity, clarity and objectivity.[69] It is systematic in the sense that the theories are coherent and applicable without exceptions, and objective as it relies on controllable and reliable observations. In

[67] Hergenhahn and Henley, *An Introduction to the History of Psychology*, 368-436.
[68] Hergenhahn and Henley, *An Introduction to the History of Psychology*, 585-608.
[69] Hergenhahn and Henley, *An Introduction to the History of Psychology*, 609-628.

addition to that, this type of psychological research also adheres to well-defined methods, avoids ambiguous phrasing and produces scientific knowledge which is open and revisable rather than definite. All these values are crucial for the process of producing valid and reliable scientific knowledge. Nonetheless, recent replication studies depict a pessimistic picture about psychology's ability to adhere to these standards.

3.2 Replication Crisis and Other Issues

Reproducibility is an important aspect of sound research practices as it indicates whether a certain study followed the values of systematicity and objectivity, and whether it adopted a well-defined methodology. The Reproducibility Project set forth to explore the results of 100 papers published in three well known psychology journals – the Journal of Personality and Social Psychology, Journal of Experimental Psychology: Learning, Memory, and Cognition, and Psychological Science.[70] Consequently, they performed 100 direct replications of these studies. The rationale behind a direct replication is attempting to recreate the exact conditions of a previous study with a different sample in order to test whether a different data set produces the same effect that was postulated in the original paper. A successful replication, where the data analysis is consistent with the results of the initial paper, can be considered an additional proof of the validity of that initial hypothesized effect, and hence of its systematicity and objectivity. The findings of the Reproducibility Project cast a negative light on the empirical soundness of the initial papers which were replicated. While 97% of the initial papers claimed to have significant results, only 36% of the replication studies proved to have reached statistical significance. In addition to that, only 47% of the initial papers' effect sizes were in the 95% confidence interval of the effect sizes of their replications.

To assess the quality of their replications, correlational analyses were carried out in order to determine which factors contributed to the success or failure of the replications.[71] Their findings suggest that it was the strength of the initial evidence rather than the characteristics of the replication teams which determined the success of a certain replication study. On this account, it seems like despite the use of original materials, reviews for methodological fidelity, and overall higher statistical power, these replications show that what was previously considered as evidence for certain effects is founded on unstable methodological grounds. Although fraud seems like a plausible explanation, in practice it seems like researchers rarely engage in such malignant behaviors. Perhaps because tales of scandal are more likely to capture our attention, as Latour and Woolgar mention in their book, it is common to have the impression that they occur very often.

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[70] Open Science Collaboration, "Estimating the Reproducibility of Psychological Science," *Science* 349, no. 6251 (August 2015): 4716, https://doi.org/10.1126/science.aac4716.
[71] Open Science Collaboration, "Estimating the Reproducibility of Psychological Science," 4716.

Researchers behind the Open Science Collaboration suggest that it is rather the potentially problematic practices which led to the current situation – a replication crisis. Selective reporting of findings and selective analysis of the data, as well as insufficient specification of the necessary or sufficient conditions required to obtain the postulated results are among the practices that they mention. While selective reporting and analysis are harmful for the statistical validity of a study, the latter two practices lead to generalizability issues as the omission of such details affects the applicability of their conclusions. In a satirical spirit inspired by Dante Alighieri's famous poem, "The Nine Circles of Scientific Hell" highlights the problematic research practices that are prevalent in psychological research.[72] Among the ones mentioned in the paper are post-hoc story telling – referring to researchers that do not commit to a hypothesis and then test it, but rather analyze their results first and then claim to have been expecting those findings all along. P-value fishing, which consists of trying all sorts of statistical tests until reaching a result significant at a desired p-value, was also criticized in the paper. Outlier manipulation is described as the preferential inclusion and exclusion of outliers. Nonpublication and partial publication of data have led to what is known as the file-drawer effect or publication bias, and it refers to the custom of selectively publishing positive or significant results while insignificant, negative, or neutral studies are forgotten in the file drawers of researchers.[73]

Publication bias has perhaps the most negative impact on scientific research. Because meta-analyses and systematic reviews often rely on a multitude of studies in order to establish the presence of a certain effect, lacking neutral or negative studies about it will lead to an erroneous judgment about the validity of the effect.[74] As Latour and Woolgar portrayed it, scientific activity is often about manipulating statements and determining their fact-like value.[75] This process entails persuasion to a certain degree, as one brings forward evidence and arguments in favor of a certain dimension, and this also reveals the normative dimension of scientific facts as it is through others' recognition that a certain statement gains legitimacy. Consequently, when scientists only choose to publish results which confirm their hypothesis and whose results are in line with a certain theoretical framework which they endorse, the entire persuasion and statement evaluation processes that constitute science are affected. Since the retrospective account of how a certain fact was constructed is forgotten, neglecting the problem of publication bias might lead to the acceptance of many invalid statements and theories as facts. Consequently, as they are being cited in further studies, these questionable statements will lead to

[72] Neuroskeptic, "The Nine Circles of Scientific Hell," *Perspectives on Psychological Science* 7, no. 6 (November 2012): 643–44, https://doi.org/10.1177/1745691612459519.
[73] Priscilla Joys Nagarajan et al., "The File Drawer Effect: A Call for Meticulous Methodology and Tolerance for Non-Significant Results," *Indian Journal of Anaesthesia* 61, no. 6 (2017): 516, https://doi.org/10.4103/ija.ija_280_17.
[74] Nagarajan et al., "The File Drawer Effect: A Call for Meticulous Methodology and Tolerance for Non-Significant Results," 516.
[75] Latour, Woolgar, and Salk, *Laboratory Life*, 84.

more and more erroneous claims and have deleterious effects for entire sub-branches of psychology.

These problematic research practices could indicate a deeper, more systemic issue within the psychological research community. It is often proposed that this issue could be explained by the incentives that steer scientists' behaviors as they seek funding in order to be able to carry out their research.[76] In order to receive funding, their claims must be judged as worthwhile and their studies reliable.[77] Consequently, as they strive to appeal to different publication journals, researchers are pressured to produce significant and positive findings, and to have their hypotheses confirmed in most instances. Perverse incentives may incite them to modify, falsify, or even fabricate results. Since individuals' decision-making cannot be seen as independent from their career objectives, it seems sensible to assume that these scientific processes will be affected by career pressures and other individual factors. These factors differ for researchers, as while some may prefer stability in a secure area of science, others strive for recognition and innovation. Hence, it is difficult to draw a clear conclusion regarding the objectives of all researchers. In addition to that, individuals' expertise and reputation – and generally speaking, career, are important criteria for the judgment of their claims.[78] Consequently, researchers must maintain a certain reputation in order to be able to perform their studies and have their claims considered by the research community. Taken together, individuals' careers and objectives are dependent on their studies and claims, and at the same time, their studies and claims depend on their careers and objectives.

3.3 A Potential Solution for Psychological Research

It is difficult to depart from the frame of thought that depicts facts as independent entities which exist objectively, and whose discovery entails "skillful revelation".[79] Latour and Woolgar call this perspective into question and emphasize the challenge that negative findings pose to the process of scientific discovery. In such situations, we are faced with several alternative explanations. Among these, there are two notable alternatives that should be highlighted. Either our methods are to blame and thus we were unsuccessful in showing the existence of an effect out there or, conversely, our methods are sound, but we must face the possibility that the effect we are trying to study cannot be replicated, for instance because the phenomena we are studying are too complicated or because the effects that were initially observed were artefacts of particular (irreproducible) circumstances. If we adopt the former alternative, and after further research we come across strong evidence for that certain effect, we will be inclined to believe that it corresponds to an objective phenomenon existent in the external world. Once the statement under

[76] Yong, "Psychology's Replication Crisis Is Running Out of Excuses."
[77] Latour, Woolgar, and Salk, *Laboratory Life*, 171.
[78] Latour, Woolgar, and Salk, *Laboratory Life*, 171.
[79] Latour, Woolgar, and Salk, *Laboratory Life*, 175.

study begins to gain credibility and advance towards a more fact-like status, it "becomes a split entity" according to Latour and Woolgar.[80] More exactly, by split entity the two argue that to a certain extent there is, on one hand, a collection of words which comprises the statement, and, on the other hand, there also is the object which the statement depicts. This object gradually earns independent existence from the initial statement. As the statement receives more support and credibility, the object eventually becomes the reason behind the statement rather than the other way around. Once this inversion takes place, it becomes difficult to see the process of discovering a fact as something other than merely following a logical sequence based on available evidence.

Nevertheless, adopting the perspective of science as socially constructed comes with certain challenges as well. Most importantly, accepting this view makes it difficult to distinguish between what should be considered viable knowledge and what should be treated as mere opinion. To a certain extent, it implies that all knowledge is socially constructed and thus speaking of this distinction would not make sense. On the other hand, we cannot deny that some forms of knowledge seem more reliable and better validated than others. While this demarcation problem was approached by previous philosophers of science such as Popper, the criteria proposed by him seems inadequate for sciences such as sociology or psychology. More recently, Krugman questioned the appropriateness of these physics-centered approaches for social sciences such as economics and suggested that human behavior is far too complex to abide by those types of standards.[81] At the same time, there should be a clear distinction between beliefs and legitimate knowledge. Marx suggested practice as the way through which "man ought to prove the truth, that is the reality and the power of the something beyond his thought".[82] Perhaps this approach is flexible enough to encompass both humanities and hard sciences since it does not necessarily impose one single objective for all of them. Practice in physics is radically different from practice in psychology. Hence, Marx's solution is able to provide a middle ground for this demarcation problem.

For psychology specifically, adopting this criterion of practicality might not be sufficient in order to help solve the replication crisis. Promoting incentives for reproducibility rather than novelty, however, might reduce the pressure that is put on scientists to come up with revolutionary positive findings.[83] Moreover, if publication journals would encourage publishing results which are not statistically significant, and which disconfirm hypotheses, this may reduce the file-drawer effect and provide the research community with a more complete picture regarding the studied phenomena. All things considered, having a more reflexive attitude concerning research in general, similar to the way Latour and Woolgar conducted their anthropological analysis, would

[80] Latour, Woolgar, and Salk, *Laboratory Life*, 177.
[81] Krugman, "How Did Economists Get It so Wrong?."
[82] Latour, Woolgar, and Salk, *Laboratory Life*, 179.
[83] Open Science Collaboration, "Estimating the Reproducibility of Psychological Science," 4716.

greatly benefit psychology in general. It also seems appropriate for psychology – the science centered around human behavior, to be analytical about psychological research – an instance of human behavior. As such, this reflexive stance would not only bring methodological improvements, but also reinforce the idea that scientific facts are constructed as researchers favor certain interpretations of their observations over others. In line with the view promoted in *Laboratory Life*, it is important to be aware that the theoretical frameworks which we use to interpret the outside world might not be faithful representations of what the external world really is like. Nonetheless, this does not render such frameworks useless. Rather than a goal in itself, it encourages us to view these representations as tools that help us navigate the external world. Just like with any tool, however, we must be aware of their sharpness and utility, as such characteristics drastically influence the way we choose to use them.

Conclusion

In their attempt to deconstruct the influential perspective of science as simply following logical steps in describing the external reality, Latour and Woolgar present the process by which facts come about as a social construction in which certain interpretations are chosen over others. Rather than describe the way norms, rewards and institutional elements intermingle and affect scientific processes, the two substantiate the inherently social character of scientific discovery in general. In their view, science is just like any other social enterprise. Claims are born as hypotheses and as the research community is faced with more and more arguments in their favor, they slowly gain the status of facts. Discussions between scientists reveal the multitude of microprocesses which underlie this transition from mere statement to well-established fact. Whereas previously, a claim had to rest its validity on other facts, as an established fact itself it becomes the reference on which other claims rest. Moreover, at that point in the development of the claim, the circumstances of its emergence are forgotten or deemed redundant. Since the fact is just meant to be a description of an objective and external phenomenon, describing the way it was discovered does not carry the same significance as it did before, when its existence was questioned.

The aim of this thesis was to determine whether we can prevent contingent and non-technical factors from interfering with the process of scientific discovery in psychology. By adopting the social constructivist view it becomes clear that such a thing would not be possible, considering the fact that the process of scientific discovery has a fundamental social character in itself. As such, this prevalent rational scientific method seems to be unable to provide a complete and elaborate understanding of how scientific facts emerge. Leaning towards a more sociological understanding of how facts come about can, however, improve our discernment of not only the emergence of knowledge, but also the implications and uses of this knowledge. As mentioned before, instead of seeing facts and scientific activity as either purely objective or purely subjective, we should see them for their practical value. As a future psychologist, judging the

utility of scientific knowledge is a requisite for my practice. This, in turn, requires adopting a critical attitude and reflexive mindset, as facts should be taken as useful tools rather than unmovable truths. By reflexivity, I specifically refer to paying attention to the interpretation process of the phenomena that are described and analyzed. After all, reflexivity is just "a way of reminding the reader that all texts are stories",[84] regardless of such texts being labeled as scientific or not.

[84] Latour, Woolgar, and Salk, *Laboratory Life*, 284.

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