The Achievement of Newtonian Science,
As Seen From Its Peak

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INTRODUCTION

The purpose of this essay is to answer the following two questions: how did those writing in the 19th century understand the achievements of Newtonian science, and how do their accounts measure up against what Newton actually did in the *Principia* and what actually happened in the post-Newton research period? In particular, I will focus on those writing at the peak of Newtonian science before it came to be threatened in the latter part of the century by discoveries such as the 43 arc second discrepancy in the motion of the perihelion of Mercury, which was resolved in the transition to Einstein's theory of relativity. The accounts that I will consider are those of Mary Somerville, John Herschel, and John Stuart Mill, who each took notice of the unprecedented achievements that were being made in science and decided to form their own accounts of how it was ever possible.

CHAPTER ONE: MARY SOMERVILLE—*ON THE CONNECTION OF THE PHYSICAL SCIENCES*

About Mary Somerville

Mary Fairfax Somerville is typically characterized as a popularizer of science. She wrote extensively about different fields of science in a style that was comprehensible to lay readers, though her writing was read by scientists and laypeople alike. Others such as Adam Chapman have insisted on characterizing her as an "interpreter" of science, in contrast to a science "popularizer".[1] Chapman's label is quite appropriate; it is also a better characterization of what she set out to do in *Connection of the Physical Sciences.* The focus of her work was on the success of mathematical physical laws and their application across different areas of physics.

It is in this regard that we can infer from her writing an account of how Newtonian science had achieved so much success by the first half of the 19th century. Most generally, we can read her as providing a demonstration of the how the laws (and the quantities that constitute them) in physics and chemistry are related to each other. She describes how theory has been developed, tested, and refined; on occasion she also offers a picture of what future research will look like and how the sciences will be developed further based on the open problems she identifies. She produced new editions as time went on in order to accommodate the frequent scientific discoveries being made during the period she was working on *Connection*, which was from 1834 until 1849. Born in 1780, Somerville was fifty-five when the first edition was published. The fifth edition, which is the focus of this chapter, was published in 1840.

It would be unfortunate to introduce her writing without a sketch of her personality, for she was known to have an admirable character along with her intellectual spirit. One can become acquainted

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1. See Chapman (2004).

with Somerville through a collection of letters and journals assembled by her daughter Martha, titled *Personal Recollections of Mary Somerville.* Martha described how her mother held onto her youthfulness into her old age, in large part due to her open-mindedness to new ideas. She had a kind and humble disposition, and only let herself get angry in the face of injustice, especially when people questioned the capacity of women or were cruel to animals.[2]

For the most part, Somerville's access to the scientific community was made possible by her social connections and her independent studies. As a woman she did not have access to formal education, though she and Caroline Herschel were the first two women to be accepted into the Royal Astronomical Society in 1835. Much of her knowledge of physics and mathematics was achieved by painstakingly going through texts all on her own, though she sought clarification to be sure she was not misunderstanding the material. She and her husband were very close to the Herschel family; Somerville was the godmother to Rosa, the Herschel's daughter. She wrote in her journals about her warm experiences in the Herschel's home, where together they discussed the highest sciences. She was quite a socialite and enjoyed going to balls and the theater, though her scientific aspirations were always her priority. What is clear from the biography is that she was invested in learning her whole life, always seeking to clarify her own understandings. Her biography is, by the way, well worth reading.

Somerville's Interpretation of Newtonian Astronomy

The State of Astronomy in 1840

For Somerville, the primary signs of success in orbital astronomy were the impressive level of accuracy in the predictions of latitude and longitude of the celestial bodies (including planets, satellites,

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[2] She was haunted for quite a long time by the death of her pet goldfinch, which had died from neglect while she was away. She also absolutely detested vivisection on live animals, which some of her contemporaries were doing.

and comets) and the pattern of successful refinements in the development of the field up until then. These predictions were laid out in the astronomical tables. At the time she was writing, the predictions for Jupiter and Saturn were nearly within observational limits. The tables for Uranus, meanwhile, were noticeably wanting. The observational data for Uranus was scarce—the planet had only been discovered in 1781 and so the data on it were limited. Somerville also states that the tables for Mars, Venus, Mercury, and the sun required improvements as well.[3]

Organization of Scientific Practice in Astronomy

In her discussion of how astronomers generate the astronomical tables, which describe the locations of planets in their orbits at given times, Somerville states that the basic structure of Newtonian astronomy is threefold:

> Astronomy is divided into three distinct departments, of theory, of observation, and computation. Since the problem of three bodies can only be solved by approximation, the analytical astronomer determines the position of a planet in space, by a series of corrections...this process is continued till the corrections become less than the errors of observation.[4]

The threefold distinction between theory, observation, and computation is accompanied by a method to refine the astronomical tables to achieve better predictions and a truer account of the celestial motions. This picture is one of successive approximations, beginning first with a simple idealized case upon which further adjustments can be made to handle more complex cases, either computationally or physically speaking. This continues until prediction is within the level of observational error. She argues that some degree of observational error is unavoidable and can only be remedied by averaging across thousands of observed values, with the assumption that the errors are just as likely to fall in either direction.[5] Though I will go into more detail about what these corrections amount to, I will

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[3] See Somerville (1840, p. 74).
[4] Ibid., pp. 70-71.
[5] Ibid., p. 72.

simply state here that they generally involve amendments to theory to accommodate more details of the physical world, such as the number of bodies in the system, the shape of those bodies, and the more complicated interactions between the orbits. This does not, of course, preclude amendments to observational techniques or computational strategies; in fact, she discusses adjustments made to all three departments. I will now describe how each of the departments make their refinements.

Computational Corrections

The orbits of the celestial bodies are to be computed in a series of steps. Since the actual interactions are not mathematically tractable, approximations are required. The method of computing the positions of the celestial bodies comes with a particular order, moving from simpler cases to the more complex ones. For example, the computer (that is, the person doing computations) may start with a perfectly circular orbit, then correct the orbit with the equation of the center to get the body's position along the ellipse, then apply the principal periodic inequalities, and then continue to correct for other configurations of three body arrangements. This stage is done after the stage of observation, which obtains the orbital values needed to do the computations.

Observational Corrections

One of the roles of observation in orbital astronomy is to obtain measurements for the orbital elements. In orbital theory, values for each planet must be obtained for the length of the major axis of the orbit, the eccentricity, the inclination of the orbit, the longitudes of the perihelion and ascending node at a given time, the orbital period, and the longitudinal values for any arbitrary times. Observations also provide approximate values of secular and periodic inequalities, which are to be corrected “till theory and observation agree.”[6] Now, since the predictions of the locations of the celestial bodies requires each of these values, there are multiple possible sources of error when it comes time to determine the source of a discrepancy between the positions of the planets as predicted by the orbital theory and the actual observed longitudes and latitudes:

> However, the values of the elements determined separately can only be regarded as approximate, because they are so connected, will induce errors in others. The excentricity depends upon the longitude of the perihelion, the mean motion depends on the major axis, the longitude of the node upon the inclination of the orbit and *vice versa*. Consequently, the place of a planet computed with the approximate data will differ from its observed place. Then the difficulty is to ascertain what elements are most in fault, since the difference in question is the error of all.[7]

The goal of the observational department, then, is to refine these measurements of the orbital values, so that the theory itself (and not just the measurements it is equipped with) can be more directly tested against observations. There are limits to the refinements of the measurements, however, that have to do with observational error. As mentioned in the introduction to this section, the goal is to achieve predictions within observational limits, because some degree of observational error is inevitable. Averages are used to compensate, but she emphasizes that the predicted positions obtained are always approximate and thus will always differ from the true positions because of imperfect measurements going into the theory and the need for mathematical approximation.

Corrections to Theory

Modifications to theory are made after comparing prediction with observation. If the predictions are not within the limits of observational error, then astronomers have the options of either improving the computational techniques or seeking better values for the orbital elements (and both are often necessary). A third strategy is to make corrections to the orbital theory itself, which is recommended

6. Ibid., p. 73.
7. Ibid., p. 72.

whenever the attempts to obtain better predictions are not particularly successful for long periods of time. It might also be required to improve the theories in other areas in physics that are necessary for astronomical predictions, including geophysics or optics. Better representations of the figure of the earth were necessary to improve the tables for the lunar orbit, for instance. But in terms of astronomical theory, the astronomer is required to refine the theoretical determination of the masses of the planets and their shapes according to what can be inferred from perturbations. Other amendments to theory include the need to include newly discovered celestial bodies, such as satellites, which can be found with telescopic observations. Perturbational analysis can also be used to discover the existence of other planets exerting forces on the others. She conjectures that this might be required in the case of Uranus:

> The tables of Jupiter and Saturn agree almost perfectly with modern observation; those of Uranus, however, are already defective, probably because the discovery of that planet in 1781 is too recent to admit of much precision in the determinations of its motions, or that possibly it may be subject to disturbances from some unseen planet revolving about the sun beyond the present boundaries of our system. If, after a lapse of years, the tables formed from a combination of numerous observations should be still inadequate to represent the motions of Uranus, the discrepancies may reveal the existence, nay even the mass and orbit of a body placed for ever beyond the sphere of vision.[8]

Her discussion here in some respects describes the discovery of Neptune made a few years later in 1846, which was achieved through perturbational analysis.[9] Somerville's recommendation demonstrates her awareness that astronomical theory can be used to interpret failures of prediction as indicative of some other perturbing force, either from a known body or one yet to be discovered. She discusses this idea very early on in the text in Section II:

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8. Ibid., p. 74.

9. Somerville wrote in her journal that John Couch Adams had been inspired by this passage to compute the theoretical trajectory of a body that would perturb Uranus in such a way: "Mr. Adams told [William] Somerville that the following sentence in the sixth edition of the 'Connexion of the Physical Sciences,' published in the year 1842, put it into his head to calculate the orbit of Neptune" (Somerville, 1874, p. 290). In 1845, the trajectory of Neptune was computed independently by Adams and LeVerrier. When the planet was discovered a year later a dispute emerged over who ought to be given the credit. The Royal Society credited LeVerrier for the discovery, though Adams has some claim to it as well.

> Were the planets attracted by the sun only, they would always move in ellipses, invariable in form and position; and because his action is proportional to his mass, which is much larger than that of all the planets put together, the elliptical is the nearest approximation to their true motions. The true motions of the planets are extremely complicated, in consequence of their mutual attraction; so that they do not move in any known or symmetrical curve, but in paths now approaching to, now receding from, the elliptical form; and their radii vectors do not describe areas or spaces exactly proportional to the time, *so that the areas become a test of disturbing forces.* To determine the motion of each body, when disturbed by all the rest, is beyond the power of analysis. It is therefore necessary to estimate the disturbing action of one planet at a time...a problem equally applicable to planets, satellites, and comets.[10]

There are two important points to be made about this passage. The first is her acknowledgment that in certain idealized situations, the orbiting bodies would move in some definite orbit, her example being that if there were only one planet orbiting the sun with no other outside forces acting on it, its trajectory would be an ellipse. Second, since the exact motions of the planets under gravitational interactions exceed computational limits, orbital theory should instead start with some idealized motions—e.g., trajectories that follow the area rule—and then discover disturbing forces from violations of the area rule. Theory, on her account, is an indispensable part of research into the celestial motions, since the true motions cannot feasibly be computed from the interactions of all the planets directly. Somerville does not have a word to describe these kind of discrepancies between theory and observations, but there is one available in the 19th century coming from Herschel: “residual phenomena”. Mill will choose to use this term as well after reading *Preliminary Discourse*.

Somerville is certainly aware that theory has an important role in ongoing research and that it can act as a tool for discovery. What is unclear, however, is whether she does in fact recognize that a long and successful pursuit of residual phenomena could be more powerful evidence than mere prediction alone—more persuasive evidence than, say, if the predictions had been within the levels of observational error in the first place and no additional disturbing forces were identified. Her treatment

[10] Ibid., pp. 14-15. Emphasis is mine.

of hypotheses and emphasis on verification in later sections suggest that she perhaps did not fully appreciate the potential evidence that can come from pursuing residual phenomena. She may perhaps be aware of this to *some* extent—it is likely that she is—but since she does not say so explicitly, I am not inclined to attribute full awareness to her, especially since in later sections she emphasizes alternate forms of evidence (which I discuss later in this chapter). The matter is complicated. It gets clearer, in my opinion, when reading her account alongside Herschel and Mill.

On one hand, her picture of continued astronomical research is one of fine-tuning; small tweaks to the tables to achieve better predictions. If small adjustments are not enough, however, the theory may need to be amended in more dramatic ways. She suspects, after all, that another planet might be perturbing Uranus, and she is aware that any effects of very distant bodies may never make their way into theory anytime soon given how difficult it would be to detect them. Still, she writes, “In the present state of astronomy, the masses and elements of the orbits are pretty well known, so that the tables only require to be corrected from time to time as observations become more accurate.”[11] She anticipates that the predictions will only continue to get better as the orbital values are better measured, until the predictions agree with observations. Her vision of future research in orbital astronomy might include adding other planets and more complicated interactions to theory; there is no way she could have anticipated more dramatic revisions to theory, including those that would be required in the shift to Einsteinian relativity. She offers no insights on what it would mean to come across a discrepancy that could not eventually be resolved within Newtonian theory.

She does recount the moon's apparently “unaccountable anomaly” (referring to the acceleration of its mean motion) that was attributed by some to an ethereal medium and by others to a successive

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11. Ibid., p. 73.

transmission of gravitating force.[12] But, she says, LaPlace showed that neither possible cause would have successfully accounted for the anomaly, since they would not affect the motion of its perigee or the nodes. The cause was identified as the secular variation in the eccentricity of the terrestrial orbit, which was a lesson learned from the satellites of Jupiter. Thus, she does recognize that it is possible to have seemingly unaccountable anomalies with no obvious source, meaning that future developments may not be so straightforward as “fine-tuning”. To avoid *ad hoc* claims that could be made to cover up those discrepancies, she recommends that theoretical analysis, involving sensitivity checks using laws from other areas of physics, be used to safeguard against that. As she saw it, the discrepancy was resolved with a correction to the theory. But again: did she see the discovery and resolution of the discrepancy as further evidence for theory? In this case, it is unclear.

Why This Method Appropriate For The Historical As Well As Future Development of Astronomy

Somerville offers several reasons, both explicitly and implicitly, for why it was necessary for astronomy to proceed in the way she describes. The first reason is that the complexity of gravitational interactions between three or more bodies necessitates first breaking down the problem into simpler idealizations. These idealizations are formulated in terms of counterfactual reasoning, which is a feature of how she chose to guide her reader through the gravitational interactions at work in the solar system. Counterfactual knowledge of this sort is therefore a part of astronomical theory and is an aim of scientific knowledge in gravitation research.

For example, in Section V she discusses how complicated the motion of the moon is, since not only do the planets perturb the moon's orbit directly, but their effect on the earth also perturbs the moon

12. Ibid., p. 45.

indirectly. The indirect effects on the moon from the planets are actually more considerable than the direct ones.[13] Thus, to compute the motions of the moon (and the other planets) astronomers could accommodate the interactions mathematically by building up from simpler interactions to more complicated but truer ones. She describes how astronomers generally begin with idealized orbits (such as a circular orbit) and then correct them, according to the method described earlier.

A second reason is that theory can compensate for observational limitations. Orbital inequalities caused by perturbations could be identified by theory when they occur at either too large or too small timescales to be practically sensible. She describes how the factors that determine the Mars orbit necessitate the use of theory to identify otherwise unobservable celestial phenomena:

> For example, the masses of all the planets revolving within the orbit of any one, such as Mars, by adding to the interior mass, increase the attracting force of the sun, which therefore, must contract the dimensions of the orbit of that planet, and diminish its periodic time; whilst the planets exterior to Mars's orbit must have the contrary effect. But the mass of the whole of the planets and satellites taken together is so small, when compared with that of the sun, that these effects are quite insensible, and *could only have been discovered by theory.*[14]

Once theory has identified these inequalities, the predictions of the amended theory can be compared with observation, since then astronomers would know where to direct attention to. Agreement with observation is then needed for verify the inequalities. She writes, “All the periodic and secular inequalities deduced from the law of gravitation, are so perfectly confirmed by observation, that analysis has become one of the most certain means of discovering the planetary irregularities, either when they are too small, or too long in their periods to be detected by other methods.”[15]

Despite the complexity of the perturbations, the strategy she describes is a reasonable one, since the influence of the sun on the planets is so much greater than their actions on each other: “It is an

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13. Ibid., p. 46.
14. Ibid., p. 26, emphasis is mine.
15. Ibid., p. 30.

extremely difficult [problem], and would be infinitely more so, if the disturbing action were not very small when compared with the central force; that is, if the action of the planets on one another were not very small when compared with that of the sun."[16] Furthermore, theory can be used to assess the potential impact of certain interactions, to first see whether they would have a significant effect: "The revolutions of the satellites about Jupiter are precisely similar to those of the planets about the sun: it is true they are disturbed by the sun, but his distance is so great, that their motions are nearly the same as if they were not under his influence."[17] Strategies such as this can be used to determine the order in which the corrections for the inequalities are made, helping to remedy the problem of complexity.

Still, Somerville sees accurate prediction as the ultimate mark of successful science; failures to accurately predict are undesirable and for her would undermine the authority of the theory. She writes, "Notwithstanding the permanency of our system, the secular variations in the planetary orbits would have been extremely embarrassing to astronomers when it became necessary to compare observations separated by long periods."[18]

The pursuit of inequalities is important for better prediction, but it is also a way to get knowledge of the ultimate causes that operate in the physical world: "After Newton's discovery of the mechanical laws of the elliptical orbits of the planets, La Grange's discovery of their periodical inequalities is, without doubt, the noblest truth in physical astronomy; and, in respect of the doctrine of final causes, it may be regarded as the greatest of all."[19] This is in contrast to merely seeing the work on the inequalities as finishing the biggest remaining gaps in the theory that needed to be bridged in order to obtain accurate predictions. Still, as we will see, her emphasis in other parts of the text is on a different kind of evidence: prediction in the context of highly connected physical laws and quantities.

16. Ibid., p. 15.
17. Ibid., p. 33.
18. Ibid., p. 28.
19. Ibid., p. 28.

Somerville On Other Forces Besides Gravitation: The Search Into "Ultimate" Principles

In section XIV, Somerville discusses forces other than Newtonian gravitation: the attractive and repulsive forces between individual atoms or molecules. She introduces the topic by the work of Ottaviano-Fabrizio Mossotti, who offered a somewhat conjectural account about these forces and the electric fluid (electricity) and how these might relate to the law of gravitation. Mossotti's theory (to the extent that Somerville describes it) gives a qualitative description of what happens between particles at various distances. In the first case, at sensible distances, the attraction between particles follows the law of gravitation, in which the attractive force is proportional to the inverse square of the distance and is directly proportional to mass. In the second case, at extremely close insensible distances, a repulsive force exists between the particles. Mossotti then proposes a third scenario: there may be a distance at which the forces are balanced in a state of equilibrium, which he argues is what is responsible for cohesion. He proposes that electricity is both what binds particles and also what fills all of the space between them.

Somerville then discusses Mossotti's connecting hypothesis to build a link between these scenarios, in which the net effect of attractive and repulsive forces at different levels results in gravitation at sensible distances:

> Thus on the hypothesis that the mutual repulsion between the electric atoms is a little more powerful than the mutual repulsion between the particles of matter, the ether and the matter attract each other with unequal intensities, which leaves an excess of attractive force constituting gravitation. As the gravitating force is in operation wherever there is matter, the ethereal electric fluid must encompass all the bodies in the universe; and as it is utterly incomprehensible that the celestial bodies should exert a reciprocal attraction through a void, this important investigation of Professor Mossotti

> furnishes an additional presumption in favor of a universal ether, already all but proved by the motion of comets and the theory of light.[20]

That there are spaces between particles is argued from the phenomenon of the compressibility, which implies that rather than being in contact, the particles have indefinitely small distances between them as a result of repulsive forces. Part of the evidence she offers for the universality of the ether invokes metaphysical considerations, treating the action of forces across a void as inadmissible:

> ...yet as it is inconceivable that the particles of matter should act upon one another without some means of communication, there is every reason to presume that the interstices of material substances contain a portion of that subtle ethereal and elastic fluid with which the regions of space are replete...It has long been a hypothesis among philosophers that electricity is the agent which binds the particles of matter together. We are totally ignorant of the nature of electricity, but it is generally supposed to be an ethereal fluid in the highest state of elasticity surrounding every particle of matter; and as the earth and atmosphere are replete with it in a latent state, there is every reason to believe that it is unbounded, filling the regions of space.[21]

Meanwhile, evidence of the attraction between particles comes in the form of agreement with known physical phenomena, including one in plate-glass manufacturing. It was well known that mirrors having been polished and placed together would come to be bound together after a period of time, as if they had been fused. The plates would even tear off sections of the other when forcibly separated.

Somerville understands Mossotti's theory as a response to the problem of building a lawlike connection between Newtonian gravitation and other types of forces:

> Still these philosophers were unable to reconcile the attraction of molecules of matter inversely as the squares of the distance as proved by Newton, with their mutual repulsion according to the same law. But Professor Mossotti has recently shown...that there are strong grounds for believing that not only the molecular forces which unite the particles of material bodies depend on the electric fluid, but that even gravitation itself, which binds world to world and sun to sun, can no longer be regarded as an ultimate principle, but the residual portion of a far more powerful force generated by that energetic agent which pervades creation.[22]

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20. Ibid., pp. 122-123.
21. Ibid., p. 120.
22. Ibid., p. 121.

Somerville's mention here of "ultimate" principles will again return as we come to Mill's account of progress in science. The aim here seems to be to achieve a sort of unification, to bring together different phenomena under a common law. Her suspicion that there may be a more ultimate principle regarding types of forces is quite reasonable viewed retrospectively, as we have now come to unify electromagnetic forces, strong interactions, and weak interactions into a single conception of force (though gravity is not yet part of this unification).

Somerville is aware that this venture into more "ultimate" principles is only predicated on a hypothesis. Scientists in her time did not have access to events taking place at the molecular and atomic level, and electricity was not well understood. Still, she sees hypotheses as an initial aid when there are limits to observation: "It is true that this connexion between molecular forces and gravitation depends on a hypothesis; but in the greater number of physical investigations, some hypothesis is requisite in the first instance to aid the imperfection of our senses."[23] Thus, one stance she takes on hypotheses is that they can help to get new areas of research going. She continues to say that if hypotheses agree with observations, then they can be established: "Yet, when the phenomena of nature accord with the [hypothesis], we are justified in believing it to be a general law."[24] Achievement in physics for her is partly based on successful predictions across a wide range of phenomena, taking advantage of the sharing of quantities across different laws. What she is dealing with here involves a weaker form of evidence here than the sort that was involved in orbital astronomy where residual discrepancies could be pursued. This is a reason to think that she perhaps did not see the full potential for getting high quality evidence from resolving residual phenomena. This leaves her emphasizing agreement with observations as the crucial evidential requirement; the primary purpose of residual phenomena,

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23. Ibid., p. 121.
24. Ibid., p. 121.

meanwhile, is to aid in discovery and manage the complexity of the physical world so that agreement can ultimately be achieved.

How the "Connection" of the Physical Sciences Generates Stronger Evidence

Somerville is certainly aware that gathering evidence for theories is by no means straightforward. In general, she argues that the physical sciences are connected by the mathematical laws and the quantities in them. These laws, she argues, have been extensively tested by their application in many areas of physics and have led to many important discoveries. Since these laws relate to such a wide range of phenomena and have decent agreement in many cases, one can have greater confidence that the laws are true. Evidence is also made stronger when parts of a theory lead to a large number of consequences, as the various theories can then corroborate each other:

> Great discoveries generally lead to a variety of conclusions: the aberration of light affords a direct proof of the motion of the earth in its orbit; and its rotation is proved by the theory of falling bodies, since the centrifugal force it induces retards the oscillations of the pendulum in going from the pole to the equator. Thus a high degree of scientific knowledge has been requisite to dispel the errors of the senses.[25]

The intersections of different parts of physics also help to build stronger evidence, as it enables cross-checks. Though she does not use that word, she is clearly aware of how cross-checks generate evidence and corroborate measurements. She describes, for instance, how the speed of light can be determined by alternate methods as a way of getting a more secure measurement: "The velocity of light deduced from the observed aberration of the fixed stars, perfectly corresponds with that given by the eclipses of the first satellite. The same result, obtained from sources so different, leaves not a doubt of its truth."[26] In this case, not only is there perhaps more security in particular measurements of the speed of light

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25. Ibid., pp. 39-40.
26. Ibid., p. 40.

obtained in this fashion—the theories behind each derivation are themselves supported by greater evidence. A similar motivation is behind her comment on the laws of refraction that she believes connect astronomy, fluid motion, and the forces mentioned in the previous section: “The oscillations of the atmosphere and its action upon rays of light coming from the heavenly bodies, connect the science of astronomy with the equilibrium and movements of fluids, and the laws of molecular attraction.”[27] Connections of this sort, as she understands it, are a way to build stronger evidence when single predictions are not sufficient to draw deep conclusions about the truth of theories. Her vision of the success of physics is tied to the connection between the laws; future developments to extend that success ought to focus, therefore, on building more connections between the laws and forming more general principles.

[27] Ibid., p. 119.

CHAPTER TWO: JOHN HERSCHEL—*PRELIMINARY DISCOURSE ON THE STUDY OF NATURAL PHILOSOPHY*

About John Herschel

John Herschel was a member of a family of scientists. He is the son of the famous astronomer William Herschel, who was behind the discovery of Uranus, and the nephew of Caroline Herschel, who had worked alongside William. Mary Somerville was a family friend of the Herschel's. *Preliminary Discourse on the Study of Natural Philosophy* was first written in 1830 and had several notable readers: John Stuart Mill, Charles Darwin, and William Whewell. If there is anything I ought to emphasize here, it is that there was a great deal of interaction between Somerville, Herschel, and Mill. Reading their works in the context of the others' is quite useful to really understand how those in the 19th century thought about science. Mill cannot properly be understood, it seems, without considering Herschel's work. Mill's writing introduces a lot of organization and clarity to what Herschel says as well. In some ways, Mill simply borrows Herschel's insights and makes them more systematic, while filling in gaps along the way.

Herschel's other works included *Outlines of Astronomy* (1849)*,* which became a standard textbook, and *General Catalogue of Nebulae and Clusters*. Herschel was educated at St. John's College in Cambridge and was one of the founders of the Royal Astronomical Society four years later.

Induction & Deduction In Cooperation for Superior Progress in Science

Herschel sees three ways to go about discovering natural laws. These are, in effect, three candidates for scientific method. They are: 1) the inductive method, 2) a hypothetico-deductive method, and 3) a deliberate combination of the first two that avoids their individual shortcomings.

Herschel characterizes inductive reasoning as a process of "examining all the cases in which we know [the actions of primary agents] to be exercised, inferring, as well as circumstances will permit, its amount or intensity in each particular case, and then piecing together, as it were, these *disjecta membra*, generalizing from them, and so arriving at the laws desired."[28] The second option is a hypothetico-deductive approach, which involves "forming at once a bold hypothesis, particularizing the law, and trying the truth of it by following out its consequences and comparing them with facts."[29] The third option combines elements of the first two: induction and deduction. The laws, which are preferably the products of induction (though under certain conditions they may be hypothesized), are assumed for the purposes of deducing testable consequences from them. When those consequences are then compared to observations, we can learn about the true scope of those laws and whether the laws need to be modified. This process continues until agreement is achieved:

> By assuming indeed the laws we would discover, but so generally expressed, that they shall include an unlimited variety of particular laws; —following out the consequences of this assumption, by the application of such general principles as the case admits;— comparing them in succession with all the particular cases within our knowledge; and, lastly, *on this comparison*, so modifying and restricting the general enunciation of our laws as to *make the results agree.*[30]

This combined method of induction and deduction is Herschel's prefered method, and it is the method that he attributes to Newtonian science. He writes about these advantages, and Mill will later quote what Herschel says here:

> In such cases the inductive and deductive methods of enquiry may be said to go hand in hand, the one verifying the conclusions deduced by the other; and the combination of experiment and theory, which may thus be brought to bear in such cases, forms an engine of discovery infinitely more powerful than either taken separately. This state of any department of science is perhaps of all others the most interesting, and that which promises the most to research.[31]

28. See Herschel (1845, p. 198).
29. Ibid., pp. 198-199.
30. Ibid., p. 199.
31. Ibid., p. 181.

The basic picture is to carry out an induction from the phenomena and then deduce particular consequences from them. Those deductions are the basis for verification, which is in the form of agreement with observations (with an emphasis on observations in new circumstances, such as those produced experimentally). But this brief characterization is worth nothing on its own, so I will now go through each part of the process in detail. We will begin with the inductive part.

From Phenomena to Candidates for Laws, Causes—The Inductive Step

The products of scientific induction are either 1) a "real cause" and knowledge of its manner of acting, which enables a "full explanation" of the facts, or 2) an abstract law of nature.[32] This discussion of the inductive step includes several parts. I first consider what the relevant phenomena are for the inductions. Then I describe how the causes of phenomena are ascertained, which is followed by an account of how the laws of the causes are first obtained. I then give Herschel's account of the ultimate aims of induction, which has to do with working towards "ultimate principles". The last part is an alternative to induction: the formation of hypotheses.

What Does Induction Begin With?

The relevant phenomena worth considering during induction are whatever are currently taken to be "ultimate phenomena,"[33] which cannot be further analyzed or broken down into separate components. Future considerations may and often do cause scientists to reconsider what those ultimate phenomena are.

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32. Ibid., p. 158.
33. If this is unclear, it will become clearer during the chapter on Mill, as he goes into more detail on what it means to pursue "ultimate" phenomena.

It is possible in scientific practice to instead begin by pursuing empirical laws, but they should be treated as unverified inductions, for there is no guarantee that they will hold for any circumstances outside those from which the data were collected for their formation. It is also not guaranteed that any discrepancies between the empirical laws and observations can justifiably be attributed to observational error.[34] Sometimes following up with an empirical law can be quite successful in the development of theories, as long as the law is extensively tested. He cites Kepler's laws as an example of success in building off of empirical laws, since they play a role in verifying Newtonian theory: "The finest instances of this kind are the great laws of the planetary motions deduced by Kepler, entirely from a comparison of observations with each other, with no assistance from theory...[the laws] affording...the most conclusive and unanswerable proofs of the Newtonian system."[35] Herschel then gives another example to show the worst-case scenario of pursuing empirical laws, which is when they lead to a dead-end and fail to hold beyond the data that initially defined the empirical laws. He describes how the empirical formulas generated from data for the elasticity of steam and resistance of fluids led to such a dead-end when trying to construct a theory off of them.[36]

Discovering the Causes of Phenomena

Given some phenomenon, the first step is to decide whether it can be attributed to some known law or cause.[37] For Herschel, knowing the law or cause behind a phenomenon amounts to knowing its explanation. The aims of induction include identifying the true causes (*vera causae)* of phenomena and the laws that describe how they take effect. Herschel borrows the term *vera causae* from Newton,

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34. Ibid., pp. 178-179.
35. Ibid., p. 178.
36. Ibid., p. 179.
37. Ibid., p. 144.

contrasting it to “mere hypotheses or figments of the mind.”[38] When the explanation is not known, we will be in one of two possible scenarios.

The first case is when the cause is known neither for the phenomenon in question nor for any analogous cases. He says that if the immediate producing cause of the phenomenon is not known, the next step is to generalize the phenomenon by grouping it with analogous cases and forming some law for the group. The hope is that at some point later on, when the science has advanced, a “proximate cause” might be discovered, which serves a provisional stand-in for the ultimate cause. If instead the cause is unknown but an analogous phenomenon has a known cause, then it is likely that the cause is involved in both. He cites as an example that the motion of the moon in orbit around the earth is analogous to the motion of an object in a sling moving about a center. This analogy motivates further research to verify that a centripetal force directed towards earth is actually the cause of the moon’s motion.[39] Analogical reasoning is, in Herschel’s account, the core strategy for the inductive process. The grounds for pursuing these analogies in further research lies in their potential to expose more general laws and explanations; they serve as a tool for scientific discovery.[40]

When it comes to assuring that the true cause of a phenomenon has been identified during induction, the following requirements should be met:

> 1. They must not be arbitrarily assumed.
> 2. They require good inductive grounds.
> 3. They must appear to act in a set of analogous cases.
> 4. They need to be “demonstrated by unequivocal signs”.
> 5. They should be shown to exist, to act, and have laws describing their action that can be obtained independently through direct induction (which often involves experiment); otherwise further verification is needed.[41]

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38. Ibid., p. 144.
39. Ibid., p. 149.
40. Ibid., p. 209.
41. Ibid., p. 197.

Unfortunately, Herschel does not explain in detail what exactly is wanted for each requirement, but Mill will later take up the task of clarifying some of these points, including what it means for two phenomena to be analogous.

Since it is possible to have competing alternative accounts, a method must be used to eliminate alternatives. Another way to make the case for a "true cause" is by eliminating any plausible known alternatives through the use of "crucial instances", an idea that Herschel borrows from Bacon. Herschel argues that experiments designed around crucial instances are "the readiest and securest means of eliminating extraneous causes, and deciding between rival hypotheses."[42] The role of crucial instances is to distinguish between two theories which both account for a similar set of empirical data by finding a way to expose a difference empirically, and this is often experimentally done: "When two theories run parallel to each other, and each explains a great many facts in common with the other, any experiment which affords a crucial instance to decide between them, or by which one or other must fall, is of great importance."[43] A failure to rule out alternatives threatens the validity of an induction; crucial instances are Herschel's solution to this problem.

Developing Natural Laws

On Herschel's account, natural laws come in two different forms. The first sort of law accounts for the effects of causes under various sets of conditions that are specified. Herschel defines this sort of law as a "general proposition, announcing, in abstract terms, a whole group of particular facts relating to the behavior of natural agents in proposed circumstances."[44] The second set of laws are those that have to do with the assignment of properties, or as he puts it, "a proposition announcing that a whole class of individuals agreeing in one character also agree in another."[45]

[42] Ibid., p. 186.
[43] Ibid., p. 206.
[44] Ibid., p. 100.
[45] Ibid., p. 100.

The laws typically contain quantities that must be empirically measured. Herschel worries about circularity when it comes to supporting the values of the measurements, and so establishing those values is going to depend more on verification than on initial induction. These measurements cannot be obtained without the aid of existing theory, and the logic of how the values themselves will come to be tested is more complicated; I discuss this later on in the section on verification.

Herschel does, however, offer some advice on how to arrive at good measurements of the values which constitute laws in the first place:

> To arrive inductively at laws of this kind, where one quantity depends on or varies with another, all that is required is a series of careful and exact measures in every different state of the datum and quaesitum. Here, however, the mathematical form of the law being the highest importance, the greatest attention must be given to the extreme cases as well as well as to all those points where the one quantity changes rapidly with a small change of the other.[46]

His point here is that it is helpful to look at both extreme cases and also sensitive points, which can assist is getting more precise and accurate determinations of values that constitute laws.

The Ultimate Aims Of Induction and The Connection of Sciences

The aim of natural science, in Herschel's mind, is to achieve ever more general laws that capture more elementary features of the physical world. By doing so, the different areas of a science can become connected, by sharing laws and by referring to the same elementary kinds. He writes:

> So, in natural philosophy, we must account every phenomenon an elementary or simple one till we can analyze it, and show that it is the result of others, which in their turn become elementary. Thus, in a modified and relative sense, we may still continue to speak of causes, not intending thereby those ultimate principles of action on whose exertion the whole frame of nature depends, but of those proximate links which connect phenomena

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[46] Ibid., p. 176.

> with others of a simpler, higher, and more general or elementary kind....It is thus that sciences increase, and acquire a mutual relation and dependency.[47]

This process does not invalidate earlier inductions and the causes identified by them. It only means that they should be understood as "proximal links" rather than as "ultimate causes": "we may continue to speak of causes, not intending thereby those ultimate principles of action on whose action the whole frame of nature depends, but of those proximal links which connect phenomena with others of a simpler, higher, and more general or elementary kind."[48] The previously held claims about the action of those causes now taken to be proximal links are still intact, it is just that they are shown to follow from a more general rule that they are compatible with.

This process is made possible by exploring analogies, hence their role in induction is a crucial one. Analogies enable induction and generalization by identifying "general heads or points of agreement", thereby generating a systematic classification of natural phenomena. The result are laws that hold more generally, for a wide range of phenomena not previously understood to be connected:

> When we have amassed a great store of such *general facts*, they become the objects of another and higher species of classification, and are themselves included in laws which, as they dispose of groups, not individuals, have a far superior degree of generality, till at length, by continuing the process, we arrive at *axioms* of the highest degree of generality of which science is capable...This process is what we mean by induction.[49]

The cases in question come to be seen as two instances of a general kind that are invariably connected. This inductive process results in the discovery of higher order processes, and it seems to on without end.[50] But the classifications that we are capable of discovering are limited to those that have effects on the material parts of the world, for otherwise they would not be exposed empirically, and we would have no way to discover and analyze them:

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47. Ibid., pp. 92-94.
48. Ibid., p. 92.
49. Ibid., p. 102.
50. Ibid., p. 158.

> The reader will be at no loss to perceive that we know nothing of the objects themselves which compose the universe, except through the medium of the impressions they excite on us, which impressions are the results of certain actions and processes in which sensible objects and the material parts of ourselves are directly concerned. Thus, our observation of external nature is limited to the mutual action of material objects on one another; and to facts, that is, the associations of phenomena or appearances.[51]

The Role of Hypotheses

Some of what Herschel says, at first glance, seems to be compatible with hypothetico-deductive process. Yet he contrasts "true causes" to mere hypotheses or "figments of the mind".[52] So what exactly is the role of hypothesizing, and under what conditions is it legitimate? Herschel raises the standard worry about hypotheses, namely that many hypotheses could lead to similar conclusions, thus creating the problem of discriminating between them. This does not rule out their utility during the early stages of theory development, however:

> Now, nothing is more common in physics than to find two, or even many, *theories* maintained as to the origin of a natural phenomenon…Now, are we to be deterred from framing hypotheses and constructing theories, because we meet with such dilemmas, and find ourselves frequently beyond our depth? Undoubtedly not. *Est quodam prodire tenus si non datur ultra*. Hypotheses, with respect to theories, are what presumed proximate causes are with respect to particular inductions: they afford us motives for searching into analogies; grounds of citation to bring before us all the cases which seem to bear upon them, for examination.[53]

Part of their role, then, is to assist in analogical reasoning. Hypotheses may be used to connect multiple laws and form a more general expression of them: "A well imagined hypothesis, if it have [sic.] been suggested by a fair inductive consideration of general laws, can hardly fail at least of enabling us to generalize a step farther, and group together several laws under a more universal expression."[54] And if

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51. Ibid., p. 121.
52. Ibid., p. 144.
53. Ibid., pp. 195-196.
54. Ibid., p. 196.

hypotheses acquire a great deal of support, it can sometimes be concluded that either it, or something very similar to it, must be true in the actual physical world. This kind of knowledge can then be used to direct experiments or pursue other research to further explore the hypothesis. And, if the deductions from the hypothesis are verified, those conclusions can contribute to theory:

> … it may happen (and it has happened in the case of the undulatory doctrine of light) that such a weight of analogy and probability may become accumulated on the side of an hypothesis, that we are compelled to admit one of two things; either that it is an actual statement of what really passes in nature, or that the reality, whatever it may be, must run so close a parallel with it, as to admit of some mode of expression common to both, at least in so far as the phenomena actually known are concerned. Now, this is a very great step, not only for its own sake, as leading us to a high point in philosophical speculation, but for its applications; because whatever conclusions we deduce from an hypothesis so supported must have at least a strong presumption in their favor: and we may be thus led to the trial of many curious experiments, and to the imagining of many useful and important contrivances, which we should never otherwise have thought of, and which, at all events, *if* verified in practice, are real additions to our stock of knowledge.[55]

To make the case for the use of hypotheses in research, he says that the pursuit of certain hypotheses has actually paid off. Herschel here cites Coulomb's and Poisson's theories of electricity and magnetism, which "are referred to the actions of attractive and repulsive forces, following a law similar in its expression to the law of gravitation."[56] He continues to say, however, that since so much effort is required to carry out a proper verification, only the most promising ones ought to be pursued:

> But the difficulty and labor, which, in the greater theories, always attends the pursuit of a fundamental law into its remote consequences, effectually precludes this method from being commonly resorted to as a means of discovery, unless we have some good reason, from analogy or otherwise, for believing that the attempt will prove successful, or have been first led by partial inductions to particular laws which naturally point it out for trial.[57]

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55. Ibid., pp. 196-197.
56. Ibid., p. 200.
57. Ibid., p. 200.

Hypotheses are not worth pursuing in further research unless there are very good reasons to think it will pay off. He reiterates that hypotheses are only useful to an extent: as far as they help to develop general laws and design experiments. After that point, their utility diminishes:

> … to lay any great stress on hypotheses of the kind, except in as much as they serve as a scaffold for the erection of general laws, is to 'quite mistake the scaffold for the pile'. Regarded in this light, hypotheses have often an eminent use: and a facility in laying them aside when they have served their turn, is one of the most valuable qualities a philosopher can possess.[58]

As I mentioned above, the aim of the inductive step is not to speculate about the underlying mechanisms when there is no empirical expression of them. The same goes for hypothesizing. This precludes, for example, hypotheses aimed at a mechanical explanation of gravitational forces: "we conclude that there *is* a force, and a mode of connection, between the moon and the earth; though, what that mode can be, we have no conception, nor can imagine *how* such a force can be exerted at a distance, and with empty space, or at most an invisible fluid, between".[59] Whatever the mechanism of gravity may be, as long as it has no empirical expression, it is not a priority of science to search for it. It is rare to actually obtain a mechanical account, and in any case, Herschel adds, it is not as important to have a mechanical account than it is to have a complete one. Completeness is more valuable to a theory when it comes to verification and developing strong evidence:

> In estimating, however, the value of a theory, we are not to look, *in the first instance*, to the question, whether it establishes satisfactorily or not, a particular process or mechanism; for of this, after all, we can never obtain more than indirect evidence which consists in its leading to the same results. What, in the actual state of science, is far more important for us to know, is whether our theory truly represent *all* the facts, and include *all* the laws, which observation and induction lead.[60]

Herschel seems to be conceding that representation is, at least in some cases, a more appropriate aim than mechanistic explanation. Herschel's idea of explanation in science has more to do with accounting

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58. Ibid., p. 204.
59. Ibid., p. 193.
60. Ibid., p. 204.

for all known facts with laws, and the pursuit of causes is restricted to those that are accompanied by clear empirical expressions.

Verifying Laws and Causes—The Deductive Step

The Purpose of the Deductive Step

The aim of the deductive step is to verify laws by extensively testing the particular conclusions that can be deduced from them. The evidence from the verification of particular claims flows upwards, to the most general laws. Herschel writes, "Theories are best arrived at by the consideration of general laws; but most securely verified by comparing them with particular facts, because this serves as a verification of the whole train of induction, from the lowest term to the highest."[61] Provided that the requirements of verification are met, there will be sufficient grounds to accept theories and the laws that constitute them as secure knowledge. Part of the requirement is to verify that the laws hold for their assigned scope: "if they only lead us, by legitimate reasonings, to conclusions in exact accordance with numerous observations purposely made under such a variety of circumstances as fairly to embrace the whole range of the phenomena which the theory is intended to account for, we cannot refuse to admit them."[62] The laws are more amenable to this kind of testing when they are more general, since then they include "an unlimited variety of particular laws", thereby generating many opportunities for testing under different circumstances.[63]

The deductive step is not intended to establish the existence of a cause initially, since that was supposed to be established inductively. Rather, the purpose of this step is to test the particular quantitative or qualitative expressions of a cause or to see if a cause, whose operation has already been

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[61] Ibid., p. 208.
[62] Ibid., pp. 208-209.
[63] Ibid., p. 199.

established inductively from some set of phenomena, also acts in other circumstances. The evidence developed in this step is for both the laws that describe the operation of causes and for the validity of the inductions themselves.

The inductive step does not, on its own, result in general knowledge. It is therefore a responsibility of the deductive step (or, the verification process) to assess whether (and if so, how) the effects of the causes are altered under different circumstances. Herschel describes how the reality of the law of gravity becomes apparent when trying to verify particular orbits, since the gravitational interactions modify the trajectories:

> It is the verification of such inductions which constitutes theory in its largest sense, and which embraces an estimation of the influence of all such circumstances as may modify the effect of the cause whose laws of action we have arrived at and would verify. To return to our example: particular inductions drawn from the motions of the several planets about the sun, and of the satellites round their primaries, &c. having led us to the general conception of an attractive force exerted by every particle of matter in the universe on every other according to the law to which we attach the name of gravitation; when we would verify this induction, we must set out with assuming [the law of gravity], considering the whole system as subjected to its influence and implicitly obeying it, and nothing interfering with its action; **we then, for the first time, perceive a train of modifying circumstances which had not occurred to us when reasoning upwards from particulars to obtain the fundamental law; we perceive that *all the planets* must attract *each other*, must therefore draw each other out of the orbits which they would have if acted on only by the sun; and as this was never contemplated in the inductive process, its validity becomes a question, which can only be determined by ascertaining precisely how great a deviation this new class of mutual actions will produce. To do this is no easy task, or rather, it is the most difficult task which the genius of man has ever yet accomplished**…[64]

Thus it is in the verification step that we are brought to think about how the attractions between all of the planets are going to interact; those new interactions are mathematically estimated and then need be verified as well. In Herschel's picture, those more complicated interactions were not realized during

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64. Ibid., p. 201, emphasis mine.

the inductive step, rather they become a priority when theory is compared to observations and the discrepancies suggest that there may be an interaction occurring.

The Characteristics of Laws That Make Them Testable

It is preferred that laws have certain properties that allow for useful deductions from them. Herschel emphasizes that the deductions should have a mathematical, exact, and context-sensitive character whenever possible; quantitative laws are therefore preferable to merely qualitative ones. That the law of gravitation has such a mathematically precise character is a mark of (and perhaps a reason for) its success:

> Thus, the law of gravitation, the most universal truth at which human reason has yet arrived, expresses not merely the general fact of the mutual attraction of all matter; not merely the vague statement that its influence decreases as the distance increases, but the exact numerical rate at which that decrease takes place; so that when its amount is known at any one distance it may be calculated exactly for any other.[65]

The same goes for crystallography:

> Thus, too, the laws of crystallography, which limit the forms assumed by natural substances, when left to their own inherent powers of aggregation, to precise geometrical figures, with fixed angles and proportions, have the same essential character of strict mathematical expression, without which no exact particular conclusions could ever be drawn from them.[66]

The laws must say what exactly will happen in specific arbitrary circumstances, since that is needed to verify the law in a variety of circumstances, including circumstances that have the potential to be very informative, such as crucial instances. Merely qualitative laws do not submit to exhaustive testing in the same way that quantitative ones do. It is also necessary for a complete verification to have accurate values in the theory that must be obtained empirically, such as the masses of the planets and the distances between them. Herschel writes:

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65. Ibid., p. 123.
66. Ibid., p. 123.

> The importance of obtaining exact physical data can scarcely be too much insisted on, for without them the most elaborate theories are little better than mere inapplicable forms of words. It would be of little consequence to be informed, abstractly, that the sun and planets attract each other, with forces proportional to their masses, and inversely as the squares of their distances: but, as soon as we know the data of our system, as soon as we have an accurate statement (no matter how obtained) of the distances, masses, and actual motions of the several bodies which compose it, we need no more to enable us to predict all the movements of its several parts…[67]

It is also preferable that the laws are universally stated, as this is conducive to discovering more general laws:

> But a law of nature has not that degree of generality which fits it for a stepping-stone to greater inductions, unless it be *universal* in its application. We cannot rely on its enabling us to extend our views beyond the circle of instances from which it was obtained, unless we have already had experience of its power to do so; unless it actually *has* enabled us before trial to say what will take place in cases analogous to those originally contemplated.[68]

Strong verification is made possible when the theories come with rich mathematics and interconnected laws that share quantities. Not only do these qualities lead to a large number of testable deductions, they also support in particular the design of crucial experiments, which allow us to eliminate alternatives: “In thus verifying theories, since they are grounded on general laws, we may appeal, not merely to particular cases, but to whole classes of facts; and we therefore have a great range among the individuals of these for the selection of some particular effect which ought to take place oppositely in the event of one of the two suppositions at issue being right and the other wrong.”[69]

The Verification Process

[67] Ibid., p. 212.
[68] Ibid., p. 167.
[69] Ibid., p. 206.

Again, induction is used to expose the operation of a cause in particular cases and form general statements of their action; the purpose of the deductive component, on the other hand, is to explore whether the causes operate under new circumstances and whether their effects are modified in them.

If a cause is shown to act in new circumstances, then evidence is generated for the original induction by assuring that the original phenomena used in the induction were not systematically misleading: "The surest and best characteristic of a well-founded and extensive induction, however, is when verifications of it spring up, as it were, spontaneously, into notice, from quarters where they might be least expected, or even among instances of that very kind which were at first considered hostile to them."[70] This is especially true when the new circumstance is "of a widely different nature from those which gave rise to the inductions themselves."[71] The emphasis is on discovering new applications of a law in other domains or in 'extreme cases' that were not the original sources of the induction. This kind of evidence is a safeguard against the possibility that the initial phenomena used to formulate the laws were somehow accidental or contingent on certain circumstances that are not representative of the other parts of the physical world.

An example of a test of a law in new circumstances is the moon test, in which the goal was to show that gravitational forces held the moon in an orbit around earth just as the earth was held around the sun. Here the contrapositive of the law of inertia is used to infer the presence of a gravitational force guiding the moon in a circle around the earth:[72]

> For example, in the theory of gravitation we suppose an agent,—viz. force, or mechanical power,—to act on any material body which is placed in the presence of any other, and to urge the two mutually towards each other. This is a vera causa...Now, that which opposes and neutralizes force is force...Moreover, since it is a fact that the moon does circulate the earth by a force, it must be drawn towards the earth by a force; for if there were no force

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[70] Ibid., p. 170.
[71] Ibid., p. 171.
[72] The actual history is, of course, way more complicated than what Herschel suggests here.

> acting upon it, it would go on in a straight line without turning aside to circulate in an orbit...This force, then, which we call the force of gravity, is a real cause.[73]

Another way to test under new circumstances is through direct experimentation, which Herschel defines as “studiously varying the circumstances under which our causes act, with a view to ascertain whether their effect is general; and in pushing the application of our laws to extreme cases.”[74] Herschel lists five rules to guide experimental practice, to test for the existence of a cause:[75]

1. Invariable connection and invariable antecedence of the cause and consequence of the effect.
2. Invariable negation of the effect with the absence of cause.
3. Increase or decrease of the effect, whenever the cause is increased or diminished in intensity, whenever applicable (i.e., whenever the quantities can be graded in this way).
4. Proportionality of the effect to its cause in every case of "direct unimpeded action".
5. Reversal of the effect with that of the cause.[76]

These rules can be used both in the initial inductions to demonstrate the existence of a cause and in the verification step, in which experiments can be used to see whether a cause continues operates under new circumstances.

Experiments and observations are to be repeated many times, with high precision, to make any deviation sensible, even if they are really small and hard to detect. He discusses this using the law of gravitation as example:

> In the verification of a law whose expression is quantitative, not only must its generality be established by the trial of it in as various circumstances as possible, but every such trial must be one of precise measurement. And in such cases the means taken for subjecting it to trial ought to be so devised as to repeat and multiply a great number of times any deviation (if any exist); so that, let it be ever so small, it shall at last become sensible...For instance, let the law to be verified be, that the gravity of every material body is in the direct proportion of its mass, which is only another mode of expressing Galileo’s law above mentioned. The

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[73] Ibid., pp. 197-198.

[74] Ibid., p. 167.

[75] Mill borrows these from Herschel, and they become known as Mill’s Rules for Induction. We will see some of these again in as Mill’s inductive strategies in the following chapter.

[76] Ibid., p. 151.

> time of falling from any moderate height cannot be measured with precision enough for our purpose: but if it can be repeated a very great multitude of time without any loss or gain in the intervals, and the whole amount of the times of fall so repeated measured by a clock; and if at the same time the resistance of the air can be rendered exactly alike for all the bodies tried, we have here Galileo's trial in a much more refined state; and it is evident that almost unlimited exactness may be obtained…Thus any difference, however inconsiderable, that might exist in the time of one such fall and rise would be multiplied and accumulated till they became sensible.[77]

Repeating observations to expose small deviations can lead to what Herschel calls 'residual phenomena', which I discuss in the following section.

Not only do the causes and the laws that describe them need to be verified, but so too do the particular values of the quantities that enter the laws. Herschel is aware that laws often contain quantities that must be empirically measured, meaning that theory and observation necessarily interact at different stages during theory development. He responds to a worry about this interaction, namely that the logic of the testing is in some way circular, since observations are used to both compose and test the laws. He first articulates this concern: "Now, these can be learned only from observation; and it may seem to be arguing in a vicious cycle to have recourse to observation for any part of those theoretical conclusions, by whose comparison with fact the theory itself is to be tried."[78] He then borrows an example from the laws of definite proportions (or the laws of composition) in chemistry to discuss how laws make use of measurements and how those measures can enter into the logic of testing in a reasonable and non-self-defeating way:

> It is clear, that when particularized by restricting its expression to sulphur and lead, the law should state what are those particular fixed proportions in which these bodies can combine. That is to say, there must be certain data or numbers, by which these are distinguished from all other bodies in nature, and which require to be known before we apply the law to the particular case. To determine such data, observation must be consulted; and if we were to have recourse to that of the combination of the two

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77. Ibid., pp. 168-169.
78. Ibid., p. 209.

> substances in question with each other, no doubt there would be ground for the logical objection of a vicious circle: but this is not done; the determination of these numerical data is derived from experiments purposely made on a great variety of different combinations, among which that under consideration does not of necessity occur, and all these being found, independently of each other, to agree in giving the same results, they are therefore safely assumed as part of the system. Thus, the law of definite proportions, when applied to the actual state of nature, requires two separate statements, the one of announcing the general law of combination, the other particularizing the numbers appropriate to the several elements of which natural bodies consist, or the data of nature.[79]

Herschel's solution to the circularity concern, then, is to develop alternate ways to take the measurements that presuppose a different set of independent theoretical assumptions. He does not explicitly state that obtaining the same value through alternate means is in itself a form of evidence, though he does state that it prevents a vicious cycle. In the testing of the laws, both the form of the law and the particular values are subject to experimental verification.

Laws and the theories that they constitute are not taken to be immutable and permanent, for this cannot be known with absolute certainty. What can be learned scientifically, however, is whether the laws do change, and if they do, in what manner they change. Part of the scientific process is to find a way to show that any apparent counter examples are not actually cause for concern; that is, to show that they are consequences of theory, not exceptions to it. He writes, "No theorist regards such changes as alterations in the fundamental principles of nature; he only endeavors to reconcile them, and show how they result from laws already known, and judges the correctness of his theory by their ultimate agreement."[80] This is a parallel line of thought to what is going on with resolving residual phenomena, which I describe in the section below.

Residual Phenomena

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[79] Ibid., pp. 210-211.

[80] Ibid., p. 42.

Residual phenomena are what remain in the form of discrepancies after comparing the predictions deduced from exact law with observations. When pursued, they are assumed to be the effects of causes not yet taken into account. It is desirable to have laws or theories that are supposed to represent the phenomena completely, so that whatever remains unexplained in comparing predictions to observation becomes important. If the initial induction is valid, then such discrepancies amount to new phenomena to investigate. When a residual phenomenon is discovered, it becomes a relevant phenomenon for induction. The next step is to use inductive reasoning once more to identify a law or cause that accounts for it, and this cause may be a new sort or one already known:

> Now, this is precisely the sort of process in which residual phenomena...may be expected to occur. If our induction be really a valid and comprehensive one, whatever remains unexplained in the comparison of its conclusion with the particular cases, under all their circumstances, is such a phenomenon, and comes in its turn to be a subject of inductive reasoning to discover its cause or laws.[81]

The investigation of residual phenomena is a necessary feature of scientific practice, since in most cases in the physical world, there are multiple causes at work simultaneously. As such, residual phenomena are opportunities to discover other physical agents whose effects are not easily noticed. Once a residual phenomenon is successfully attributed to a cause or law, it ceases to be one—that is, it is no longer considered a residual phenomenon. Not only is this a powerful strategy for scientific discovery, it is in fact one of the primary reasons that physics had reached a state of considerable maturity. Herschel writes,

> Complicated phenomena, in which several causes concurring, opposing, or quite independent of each other, operate at once, so as to produce a compound effect, may be simplified by subducting the effect of all the known causes, as well as the nature of the case permits, either by deductive reasoning or by appeal to experience, and thus leaving, as it were, a residual phenomenon to be explained. *It is by this process, in fact, that science, in its present advanced state, is chiefly promoted. Most of the phenomena which nature presents are very complicated; and when the effects of all known causes are*

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[81] Ibid., p. 166.

> *estimated with exactness, and subducted, the residual facts are constantly appearing in the form of phenomena altogether new, and leading to the most important conclusions*.[82]

If the residual phenomenon is attributed to a new cause or law, then the existence of it must be verified independently. If it is attributed to a known cause or law, then some extra work (presumably, computational work) is required to show that it was actually a consequence of that cause or law all along, so that the verification may eventually be finished:

> In the conduct of this verification, we are to consider whether the cause or law to which we are conducted be one already known and recognized as a more general one, whose nature is well understood, and of which the phenomenon in question is but one more case of the in addition to those already known, or whether it be one less general, less known, or altogether new. In the latter case, our verification will suffice, if it merely shows that all the cases considered are plainly instances in point. But in the former, the process of verification is of a much more severe and definite kind. We must trace the action of our cause with distinctness and precision, as modified by all the circumstances of each case; we must estimate its effects, and show that nothing unexplained remains behind; at least, in so far as the presence of unknown modifying causes is not concerned.[83]

The best example of this process is in gravitation research, where residual phenomena appear as discrepancies between observations and the predicted orbits. Herschel offers an example in astronomy that reflects how the interactions between the planets were investigated:

> Physical astronomy affords numerous and splendid examples of [residual phenomena]. The law [of gravity], for example, which asserts that the planets are retained in their orbits about the sun, and satellites about their primaries, by an attractive force, decreasing as the square of the distances increases, comes to be verified in each particular case by deducing from it the exact motions which, under the circumstances, ought to take place, and comparing them with fact. This comparison, while it verifies in general the existence of the law of gravitation as supposed, and its adequacy to explain all the principal motions of every body in the system, yet leaves some small deviations in those of the planets, and some very considerable ones in that of the moon and other satellites, still unaccounted for; residual phenomena, which still remain to be traced up to causes. By further examining these, their causes have at length been ascertained, and found to consist in the mutual actions of the planets on each other, and the disturbing influence of the sun on the motions of the satellites.[84]

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[82] Ibid., p. 156, emphasis mine.
[83] Ibid., pp. 165-166.
[84] Ibid., pp. 166-167.

There is much more to say about residual phenomena and their role in the ongoing testing of theories and of Newton's theory in particular. I will not say more about it here, but that is only because it will be the focus of the final chapter.

Why It Is Necessary That Science Proceed in This Fashion

Herschel emphasizes that science ought to proceed by pursuing the laws and causes ascertained from induction in an ever wider range of conditions. It must also pursue any discrepancies that emerge along the way, for otherwise the laws would never be verified, and the validity of the inductions would not be assured. By looking into a wider range of phenomena, many discoveries are made and the laws become more and more established as general. This sort of generality is the goal of laws in science. Furthermore, by testing in a wide range, one gains more confidence that the inductions were correct, and that the data were not misleading or otherwise limited in some way.

For Herschel it was quite necessary in the development of Newtonian science to pursue any discrepancies between theory and observations. Since in the physical world many factors are acting at once and often interacting, the pursuit of residual discrepancies becomes essential to make sense of the discrepancies between theory and observations. If they could not be resolved, those discrepancies would be a major threat to the validity of the theory, since then no complete verification could be done:

> ...all those observed deviations in the motions of our system which stood out as exceptions...or were noticed as residual phenomena and reserved for further enquiry...in that imperfect view of the subject which we got in the subordinate process by which we rose to our general conclusion, prove to be the immediate consequences of the above-mentioned mutual actions. As such, they are neither exceptions nor residual facts, but fulfilments of general rules, and essential features in the statement of the case, *without* which our induction would be invalid, and the law of gravitation positively untrue.[85]

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[85] Ibid., p. 202.

Verification is, for Herschel, an essential part of the evidence for any induction. Much can of course be learned from uncovering the sources of residual phenomena, but at the end of the day, the reason they *must* be pursued is because they stand in the way of verification. In his example, when the deviations in the motions of celestial bodies were resolved, they ceased to be exceptions; they are instead "fulfillments of general rules"—presumably in the sense that they were actually the results of mutual attraction and as such they were just consequences of the law of gravity. The validity of the induction is therefore intact following what appeared to be a predictive failure.

Herschel's Account of Why Newtonian Science So Successful

Herschel believes that the success of astronomy depended crucially on its becoming an experimental science, which in his mind occurred when it became a branch of mechanics. Once this was so, he says, the progress "acquired a tenfold acceleration".[86] The development of the different fields, to him, depends on how well the phenomena in question may be experimentally manipulated: "Accordingly it has been found invariably, that in those departments of physics where the phenomena are beyond our control, or into which experimental enquiry, from other causes, has not been carried, the progress of knowledge has been slow, uncertain, and irregular; while in such as admit of experiment, and in which mankind have agreed to its adoption, it has been rapid, sure, and steady."[87] He does not seem to take into account that gravitation research did not rely on any artificial manipulations for a long period of its development (though measurements on earth did play an important role).

Another reason Herschel offers for the success of Newtonian science is that a single cause was identified that accounted for so much of the phenomena of motion, namely forces, which may be an

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86. Ibid., p. 78.
87. Ibid., p. 77.

instance of an even more general category of natural forces. He writes, “we are actually able to trace up a very large portion of the phenomena of the universe to this one *cause,* viz. the exertion of mechanical *force*; indeed, so large a portion, that is has been made a matter of speculation whether this is not the only one that is capable of acting on material bodies.”[88] Astronomy had the advantage that the dominant forces played such a large role in determining the overall phenomena, which might explain why it made so much more progress than other sciences:

> The great phenomena of astronomy, indeed, may be considered exceptions; but this is merely because their scale is so vast that one only of the most widely extending forces of nature takes the lead, and all those agents whose sphere of action is limited to narrower bounds, and which determine the production of phenomena nearer at hand, are thrown into the back ground, and become merged and lost in comparative insignificance. But in the more intimate phenomena which surround us it is far otherwise.[89]

Astronomical phenomena are distinguished here from phenomena on earth that are susceptible to many aspects of nature. Thus, it was because of the dominance of gravitational forces—and in particular, the dominating force of the sun—that apparent exceptions in astronomy could ever be so manageable and instrumental to discovery.

That dynamics submitted so well to mathematical representation also played a role in bringing physics to maturity:

> By far the most general phenomenon with which we are acquainted, and that which occurs most constantly, in every enquiry into which we enter, is motion, and its communication. Dynamics, then, or the science of force and motion, is thus placed at the head of all the sciences; and, happily for human knowledge, it is one in which the highest certainty is attainable, a certainty no way inferior to mathematical demonstration. As its axioms are few, simple, and in the highest degree distinct and definite, so they have at the same time an immediate relation to geometrical quantity, space, time, and direction, and thus accommodate themselves with remarkable facility to geometrical reasoning. Accordingly, their consequences may be pursued, by arguments purely mathematical, to any extent, insomuch that the limit of our knowledge of dynamics is determined only by that of pure mathematics, which is the case in no other branch of physical science.[90]

[88] Ibid., p. 88.
[89] Ibid., p. 174.
[90] Ibid., p. 96.

There are several reasons offered here for why Newtonian science achieved so much success. One is that it was equipped with a powerful system of mathematical laws, which was very capable of representing the relevant parameters of the phenomena of motion: space, time, and direction. Another reason for its success is that the phenomena of motion are so common and fundamental to many sciences. Yet the induction to the laws of motion and force was relatively direct, a historical feature that Herschel considers "remarkable and happy".[91]

Herschel says also that the laws achieved in Newtonian science were "simple, precise, and general"[92], which anticipates Duhem's 'simple, complete, and exact' criterion in *Aim and Structure*.[93] This made the inductions much easier to verify; the biggest difficulties in comparing the laws to particular phenomena were primarily the limitations of mathematics.[94] What Newton's theory did, in effect, was to account for so much using so few laws or axioms. This is rare, since in the physical world there are often so many factors at work that many sciences are needed at once: "It can hardly be pressed forcibly enough on the attention of the student of nature, that there is scarcely any natural phenomenon which can be fully and completely explained in all its circumstances, without a union of several, perhaps of all, the sciences."[95] Newtonian science was special because it did so much of the work only by appealing to the influence of forces.

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91. Ibid., p. 179.
92. Ibid., p. 179.
93. See Duhem (1991).
94. Herschel (1845, p. 179).
95. Ibid., p. 174.

CHAPTER THREE: JOHN STUART MILL—*BOOK III OF SYSTEM OF LOGIC:* "ON INDUCTION"

About John Stuart Mill

John Stuart Mill published *System of Logic: Ratiocinative and Inductive, Being a Connected View of the Principles of Evidence, and the Methods of Scientific Investigation* in 1843, following both Herschel's *Preliminary Discourse* and William Whewell's *Philosophy of the Inductive Sciences* (1840). Mill and Whewell were both writing in response to Herschel, though Mill quite deliberately went in a different direction than Whewell. In the *Cambridge Companion to Mill*, John Skorupski writes that although Mill was very likely envious of Whewell's knowledge of science history, he largely just used Whewell's knowledge as a resource for his own work: "Mill (never a man to be intellectually overawed) coolly drew for his own book on Whewell's encyclopaedic knowledge of the sciences, while rejecting almost entirely the older writer's philosophy."[96] Mill objected to Whewell's *a priori* position on science (Whewell was a follower of Kant).

To see "On Induction" primarily as Mill's response to Whewell would give an incomplete understanding of what Mill is doing in the text.[97] Mill was stalled on the project of writing but resumed after reading *Preliminary Discourse*.[98] The influence that this had on Mill is made very obvious from a reading of both works—so much so, that the chapter I present here on Mill will largely parallel the structure of the Herschel chapter, and we may use this overlap to further clarify the issues raised by Herschel.[99]

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96. See Skorupski (1998, p. 115).
97. Unfortunately, it seems as though the discussion of "On Induction" in the Cambridge Companion may be guilty of this; Herschel is only very briefly mentioned.
98. Ibid., p. 115.
99. I do not know whether Mill read Somerville's *Connection of the Physical Sciences*, though he surely would have had access to it. If she had influenced him, it is not nearly as apparent in the text as Herschel's influence.

What Kinds of Claims Does Science Make? For Mill, A Focus on Causation & Explanation

The Aims of Science

Mill considers the possible goals of science to be three: to describe facts, to explain them, and to predict similar facts. He decides that science should only pursue the latter two, namely: explanation and prediction. As we will see, explanation — in particular, deductive explanation — will play a crucial role for how Mill sees science being organized and how high quality evidence is achieved. A key part of scientific progress, for Mill, has to do with resolving laws into more general and secure ones. Under certain conditions, this process of resolution generates an explanation, and this is the basic picture of how Mill sees science progressing.

Mere description, on the other hand, should not be a core aim or feature of science, according to Mill. The reason he gives is that many descriptions can hold at once for a given set of observations. Alternate explanations, meanwhile, cannot both be true at once. Several portions of the text involve Mill's response to Whewell, and his discussion of the aim of science is one of those cases. He insists that Whewell would not distinguish between the wave and particle theories of light, since he emphasizes the descriptive function of science and ruling between those theories means taking a step beyond description into something more explanatory. Mill's view stands in contrast to other philosophies of science which put priority on description and representation, including Duhem's position in *Aim and Structure of Physical Theory,*[100] which argues that the best science can hope for is a representation of the physical world that is as simple, complete, and exact as possible, which may after a long period of refinement come to achieve natural categorizations that resemble the underlying physical reality.

The Law of Causation

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[100] See Duhem (1991).

Investigation into causation is at the core of Mill’s scientific methodology. He bases his inductive methods on what he refers to as the “Law of Causation”. The Law of Causation is the assertion that whatever happens once in a particular set of circumstances, will happen again under those same exact circumstances. This means that science ought to proceed by finding the correct way to describe those circumstances and then identifying which feature or features of the circumstances are the true causes of subsequent events. Put another way: for every event E, there exists some set of stable circumstances or a combination of events that is always followed by E. Usually, this is the sum of several antecedents. These circumstances are also defined by negative antecedents, which specify the absence of counteracting causes. Mill says that these negative antecedents need not be explicit in the statement of laws; they can simply be summarized.

The notion of causality as an invariable relation is the foundation of Mill’s whole account of induction, which aims to discover invariable relations between causes and the effects. Causal claims produce the highest sort of evidence when a candidate cause has been singled out and all alternatives have been eliminated: “The strongest assurance we can obtain of any theory respecting the cause of a given phenomenon, is that the phenomenon has either that cause or none.”[101] The real difficulty is in ruling out all of those alternatives.

If the Law of Causation were not universal, Mill says, then it would not be possible to express the inductive methods in rules, which is the principal aim of Book III. Recognizing this, he takes a moment to respond to the worry that discovering causes in accordance with his methods depends on the assumption that there is regular causation in the first place, which may not have any independent evidence or justification: “The assertion, that our inductive processes assume the law of causation, while the law of causation is itself a case of induction, is a paradox, only on the old theory of

[101] Mill (1973, p. 573).

reasoning, which supposes the universal truth, or major premise, in a ratiocination, to be the real proof of the particular truths which are ostensibly inferred from it.”[102] He continues to say that when the Inductive Method is used, evidence will be gathered for both the Law of Causality and for the individual laws of causation.[103] He does, however, admit that he is hesitant to extend the Law of Causation into very distant parts of the universe.[104]

The Types of Scientific Claims

There are four types of claims made in science, according to Mill: existence, order in place, order in time, and resemblance.[105] Existence claims amount to the promise of being able to observe something whenever the right observational or experimental conditions are available. If, however, those conditions are inaccessible, then the existence claims instead hinge on being able to demonstrate a connection “by succession or coexistence with some known thing”.[106] That is, they must have an empirical expression. When things are not accessible to observation, they must be accessed indirectly, and for that purpose generalizations and ratiocination are employed to reason from the laws of nature to observable uniformities.

Claims about order in place are about simultaneous phenomena, or “uniformities of coexistence”.[107] These have to do with claims about natural kinds and are discussed further in a section below. These claims do not have to do with succession (and he notes also that the same is true for the laws of number). Claims about order in time (that is, claims about succession) are treated basically as claims about causality. For Mill, causation is not understood to be fundamentally different from

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[102] Ibid., p. 572.
[103] Mill’s account includes the Law of Causation, which refers to his general principle, and also the laws of causation, which are just individual causal claims. When I am referring to the general principle, it will be in capital letters.
[104] Ibid., p. 575.
[105] Ibid., p. 604.
[106] Ibid., p. 605.
[107] Ibid., p. 579.

ordering in time. Claims of this sort are the most important to Mill's scientific method. We will see more about this throughout the chapter, so there is no need to go into length about them here. The fourth sort of claim, claims about resemblance, refers to the sharing of properties between individuals. These claims are also discussed further in the section on kinds and properties.

The Three Types of Laws: Empirical, Derivative, and Ultimate

Mill defines three types of laws: empirical laws, derivative laws, and ultimate laws. In the sections below, I define each of these and specify their role in organizing scientific knowledge. In particular, I will describe their scopes—that is, the extent to which we have grounds to project the laws beyond the observations that were used to acquire the laws in the first place. The laws can be projected beyond the available data in three ways: temporally, spatially, or circumstantially.

Empirical Laws

Empirical laws are defined as observed uniformities in nature.[108] There are two kinds of empirical laws: those that are the laws of causation, which means that they will eventually come to be resolved into simpler laws, and those that are not laws of causation.[109] Whether or not observed uniformities count as laws of causation depends on their being able to be explained deductively, which is how they are resolved into simpler laws.[110] Empirical laws are described as lacking causal explanation on their own and therefore being less secure as a result: "uniformities which observation or experiment has shown to exist, but on which [scientists] hesitate to rely in cases varying much from those which have been actually observed, for want of seeing any reason *why* such a law should

[108] Ibid., p. 516.
[109] Ibid., p. 524.
[110] Ibid., p. 525.

exist."[111] As we will see later, empirical laws can come to earn greater epistemic status when they have been resolved into other more general laws and explained deductively through those more general laws. When empirical laws are established as laws of causation and are resolved into more general laws, they can become candidates for "derivative laws".

In relation to derivative laws, empirical laws are derivative laws that have not yet been resolved. Prior to resolution, it is not yet known what cause or causes are at work, nor whether the apparent uniformities depend only on laws or if instead they rely on a combination of laws and collocations, where collocations are contingent arrangements in space of physical things and powers. If collocations are involved, then the projection of the law is going to be limited to wherever the collocation holds[112]—but see my comments on projection below.

Empirical laws can play a role in the verification of other laws. One way this can occur is when laws are found to agree with empirical laws.[113] Another way a law can be verified is by deducing an empirical law from it in a manner that allows the empirical law to be deductively explained. Both kinds of empirical laws (that is, both those that are laws of causation and those that are not) are used to verify inductions and are explained by deduction. The empirical laws themselves do not straightforwardly constitute theory when they have been deduced, however; instead, they come to constitute theory only when there are grounds to accept them as either derivative laws or ultimate laws. Only the empirical laws that are laws of causation can ever come to constitute theory.

Neither type of empirical law (those that are laws of causation and those that are not) has a high degree of certainty until it has been explained and connected with the ultimate laws (see below). Still, the empirical laws that are laws of causation are in better standing than those that are not:

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111. Ibid., p. 516.
112. Ibid., p. 519.
113. Ibid., p. 517.

> It has been shown on a former occasion that laws of causation which are derivative, and compounded of simpler laws, are not only…less general, but even less certain, than the simpler laws from which they result; not in the same degree to be relied on as universally true. The inferiority of evidence, however, which attaches to this class of laws, is trifling, compared with that which is inherent in uniformities not known to be laws of causation at all.[114]

When an empirical law is connected to the laws of causation via a derivation, it can change status; it becomes a "derivative law". An example of this is the area rule, which was an empirical law until its derivation from the laws of causation was shown.[115] It is also possible that an empirical law could come to be taken as a law of the cause as well. Mill uses as example of the law of refraction, which he says is a case where the laws of the phenomenon are inseparable from the laws that describe the production of the phenomenon.[116] Empirical laws can "rise" in generality and thus also in certainty. In such cases they can be more strongly relied on, until they cannot be distinguished from natural laws.[117] Let us now turn to the reliance on these laws and how they can reasonably be projected beyond the data that was used to formulate them.

The projection[118] of empirical laws usually depends on having more secure laws that explain them deductively; without this, we should be very hesitant to project an empirical law, since we do not know its boundaries and limits: "To state the explanation, the *why*, of the empirical law, would be to state the laws from which it is derived; the ultimate causes on which it is contingent. And if we knew these, we should also know what are its limits; under what conditions it would cease to be fulfilled."[119] When lacking a deductive explanation, empirical laws are only to be projected within the limits of the time and place where the observations were initially collected.[120]

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114. Ibid., p. 524.
115. Ibid., p. 608.
116. Ibid., p. 608.
117. Ibid., p. 586.
118. Mill's word for this is "extension".
119. Ibid., p. 516.
120. Ibid., p. 519.

There is also reason to limit the projection of empirical laws circumstantially; that is, to the same sorts of *conditions* that the observations were made under. This is because there is a possibility that some unknown cause could counteract the action of the law.[121] Unlike derivative laws, empirical laws cannot be extended to spatially adjacent cases, as there is no guarantee that the collocations will be uniform. Empirical laws should only be extended beyond observations when there is good reason to think the same agents will be at work. For example, if a new planet is discovered in our solar system, it is reasonable to conclude that it probably revolves on its axis, since the same mechanisms at work on the known planets are likely to act also on any other planets that may be in the solar system.[122] If an empirical law is the result of causation, then it can be projected more generally and more justifiably relied on. This is because when the causes are known it is easier to discern the likelihood of their being counteracted.

Empirical laws can be further projected, however, when there is reason to take them as laws of causation or when they are deduced from other known laws of causation: "Until [a] uniformity can…be taken out of the class of empirical laws, and brought either into that of laws of causation or the other of the demonstrated results of laws of causation, it cannot with any assurance be pronounced true beyond the local and other limits within which it has been found so by actual observation."[123]

Derivative Laws

Derivative laws are defined in the following way: "Derivative laws are such as are deducible from, and may...be resolved into, other and more general [laws]."[124] As we saw earlier, derivative laws are empirical laws that have been resolved. Ideally, they would be resolved into ultimate laws, which

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121. Ibid., pp. 548-549.
122. Ibid., pp. 552-553.
123. Ibid., p. 525.
124. Ibid., p. 485.

are the simplest and most general laws that describe the production of a certain set of phenomena (see below for more on ultimate laws). Mill offers an example: “All true propositions, therefore, which can be made concerning gravity, are derivative laws; the ultimate law into which they are all resolvable being, that every particle of matter attracts every other.”[125] Mill also says of derivative laws that they often “do not depend solely on the ultimate laws into which they are resolvable: they mostly depend on those ultimate laws, and an ultimate fact; namely, the mode of coexistence of some of the component elements of the universe.”[126] This is due to the fact that there are usually multiple causes at work that determine a phenomenon, a feature of the physical world that Mill calls the “plurality of causes”. The derivative laws that depend on multiple causes also depend on the causes coming together at the same time.

There are some signs that a law of causation might be a derivative law, in which case it is recommended to seek the more general law. One sign is a hint that there may be a link (or links) between the antecedent and the consequent. The reason this might indicate the law’s being derivative, on Mill’s view, is that less temporally fine-grained events are more liable to counteraction and contingency. Another sign is when the antecedent is an extremely complicated phenomenon, because in this case the effects are probably compounded from several elements of antecedent.[127] We will see more about derivative laws when we discuss resolution, so now we can turn to their projection.

The projection of derivative laws is wider than for empirical laws, though derivative laws are also restricted in their projection. Unlike empirical laws, derivative laws can be projected to “adjacent” times and locations.[128] It is not usually recommended, however, to project them to non-adjacent cases, such as to distant futures. This is because it is possible that if there are other agents at work, with

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125. Ibid., p. 522.
126. Ibid., p. 518.
127. Ibid., p. 522.
128. Ibid., p. 522.

effects that are hard to distinguish, they will be more likely to have taken effect by some later point in time. He considers, for instance, that there may be other celestial bodies in the solar system that have yet to be noticed.

Mill says that derivative laws are both less general and less certain than ultimate laws. This means that their propositions are less secure and also that they have a weaker projection. The reason is that they may depend on a particular collocation, in which case even if the laws hold, there will be an entirely different set of derivative uniformities in other circumstances.[129] On top of this Mill adds, "even when the derivative uniformity is a law of causation (resulting from the combination of several causes), it is not altogether independent of collocations."[130] This is because they are more liable to be counteracted than the ultimate laws: "While...each ultimate law is only liable to frustration from one set of counteracting causes, the derivative law is liable to it from several."[131]

If the derivative laws involve the conjoined effects of known causes, then their projection may be widened on the grounds that if we are acquainted with the causes that determine the phenomena, then we may be able to determine the likelihood that counteracting causes will interfere in a given circumstance. This means that these kinds of derivative laws (those with known causes) can be extended not just to local or adjacent locations but also to the extreme boundaries of the phenomena (assuming those boundaries are known). The degree to which derivative laws can be projected to the extremes is in proportion to the extent that the causes of the phenomena are known.[132] It is possible that when the causes are known, that it can be ascertained whether it is possible that there is something that could counteract the laws. Another virtue of knowing the relevant causes is we can know that there has been no change in the causes themselves for the period of time that the phenomena have been observed.

129. Ibid., p. 548.
130. Ibid., p. 548.
131. Ibid., p. 549.
132. Ibid., p. 553.

Adjacent cases are near-certain for derivative laws, but the certainty drops as more distant futures are considered.[133]

If, however, the phenomena depend on many causes or on certain collocations that may not exist everywhere, then the projection must be more limited: "But when the derivative law results not from different effects of one cause, but from effects of several causes, we cannot be certain that it will be true under any variation in the mode of coexistence."[134]

Ultimate Laws (The Laws of Nature)

According to Mill, progress in science is evident when empirical and derivative laws come to be deductively related to more general laws. The laws that have reached a point where they can no longer be resolved into more general laws are referred to as "ultimate laws".[135] They are known to exist, but it is unclear whether any have yet been identified—that is, it is not certain that any known laws are *actually* "ultimate". Each time that a derivative law is resolved into a more general one, we come closer to achieving an ultimate law, which links all phenomena with the same "mode of production".[136] Mill specifies that the lower bound on the number of ultimate laws is the number of senses or "other feelings of our nature" (e.g., heat and color)—that is, the types that are distinguishable in quality and not just quantity.[137] This limit is based on the need for qualitatively different phenomena to come with their own explanations (phenomena of different types may, however, share an "immediate antecedent").[138] Ultimate laws are, by definition, supposed to describe the production of all phenomena of a certain type; the statement of an ultimate law ought to summarize all of the causes that are liable to interfere or

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133. Ibid., p. 552.
134. Ibid., p. 519.
135. Ibid., p. 484.
136. Ibid., p. 486.
137. Ibid., p. 485.
138. Ibid., p. 487.

counteract the production of the phenomena. If the law were found to be countered by another set of causes, it would be deemed derivative, rather than ultimate.

On Kinds and Properties

Mill considers kinds and properties together, defining a kind as the set of properties that uniquely picks it out: "Since we know nothing of Kinds but their properties, the Kind, to us, *is* the set of properties by which it is identified, and which must of course be sufficient to distinguish it from every other kind."[139] Just as there are two sorts of empirical laws, there are likewise two types of assertions about the coexistence of properties (often referred to by him as "uniformities of coexistence"): those that depend on causes, and those that do not.[140]

Mill also distinguishes between "derivative" and "ultimate" kinds and properties. Ultimate properties do not (and cannot) depend on causal claims. Ultimate properties, which constitute ultimate kinds, are "the causes of all phenomena, but are not themselves caused by any phenomenon, and a cause for which could only be sought by ascending to the origin of all things."[141] Unfortunately, there are no methods for the induction of non-causal coexistences in the way that there are for causal laws (e.g., the Method of Agreement or the Method of Difference). There is no 'axiom' for kinds like there is for identifying causes (i.e., the Law of Causation). The discovery of ultimate kinds is more complicated, and there are fewer candidates for them than there are for ultimate laws. Derivative kinds, meanwhile, are the uniformities of coexistence that depend in some way on causation.

The Evidential Status of Kinds and Properties

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[139] Ibid., p. 579.
[140] Ibid., p. 581.
[141] Ibid., p. 579.

Since the methodology available for the identification of kinds is lacking when they are not dependent on causation, the evidential status for them is relatively weak. Candidates for ultimate kinds have a weak evidential status as a result, and they tend to be taken only provisionally. In terms of their rank, they have a similar status to empirical laws:

> It appears, then, that the uniformities which obtain in the coexistence of phenomena,—those which we have reason to consider as ultimate, no less than those which arise from the laws of causes yet undetected—are entitled to reception only as empirical laws; are not to be presumed true except within the limits of time, place, circumstance, in which which the observations were made, or except in cases strictly adjacent.[142]

The same is true if the uniformity of coexistence is a derivative one (i.e., dependent on causes):

> A generalization respecting coexistence, or in other words respecting the properties of Kinds, may be an ultimate truth, but it may, also, be merely a derivative one; and since, if so, it is one of those derivative laws which are neither laws of causation, nor have been resolved into the laws of causation on which they depend, it can possess no higher degree of evidence than belongs to an empirical law.[143]

This is because they may be dependent on certain arrangements of conditions that are liable to be interfered or counteracted in other circumstances, meaning that they cannot reasonably be projected beyond what is allowed for empirical laws. The evidential status of uniformities of coexistence is, as a general rule for both derivative and ultimate kinds, on par with empirical laws. Furthermore, it can be difficult to tell whether a kind is ultimate or merely derivative.[144] As such, there are few candidates for ultimate kinds, and these come primarily from chemistry. Other sciences, such as physiology, are less likely to involve ultimate kinds at all, since there are so many causes at work at once.

Given the difficulty of acquiring strong evidence for ultimate kinds and properties, the candidates for them are rare. For Mill, the only real candidates for ultimate kinds come from chemistry,

[142] Ibid., p. 586.
[143] Ibid., pp. 581-582.
[144] Ibid., p. 589.

and he has in mind specifically the chemical elements, whose properties were continually being discovered through the resolution of the uniformities of chemical combination. He writes:

> The Kinds therefore which are called in chemistry simple substances, or elementary natural agents, are the only ones, any of whose properties can with certainty be considered ultimate; and of these ultimate properties are probably much more numerous that we at present recognise, since every successful instance of the resolution of the properties of their compounds into simpler laws, generally leads to the recognition of properties in the elements distinct from any previously known.[145]

In parallel, the candidates for ultimate properties are constrained to chemistry. Mill's examples of ultimate properties are the following:

1. Atomic weights, which were being ascertained by resolving the uniformities in the proportions of how substances combine;
2. Polarity, supposed to be an inherent property of particles, which Mill says is suggested by the laws of crystallization, chemical composition, electricity, and magnetism;
3. Mutual attraction as inherent in all bodies, which had been ascertained by resolving the laws of the celestial motions.[146]

He also suggests here that many more "ultimate" properties may be discovered in chemistry. In the case of most things, however, he believes there is reason to think that they do not have ultimate properties but only derivatives ones, which result from causation. For instance, Mill argues that it is unlikely to discover ultimate properties in biological beings, as there is often a lot of variability from one individual to the next, suggesting that their properties are dependent on causes:

> But organized beings (from the extreme complications of the laws by which they are regulated) being more eminently modifiable, that is, liable to be influenced by a greater number and variety of causes, than any other phenomena whatever; having also themselves had a beginning, and therefore a cause; there is reason to believe that none of their properties are ultimate, but all of them derivative, and produced by causation. And the presumption is confirmed, by the fact that the properties which vary from one individual to another, also generally vary more or less at different times in the same

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145. Ibid., p. 580.
146. Ibid., p. 580.

> individual; which variation, like any other event, supposes a cause, and implies, consequently, that the properties are not independent of causation.[147]

Mill's Scientific Methods

In Book III, Mill offers three distinct scientific methods: the Deductive Method, the Hypothetical Method, and also a method of probabilistic reasoning.[148] Mill's purpose, generally, is to spell out exactly what is meant by empirical induction, and to lay out the whole process in explicit rules, as it had not been done satisfactorily before him. Influenced by Herschel, Mill sees the combination of induction and deduction as the most powerful way of securing high quality evidence in science.[149] Mill's Deductive Method runs in parallel to Herschel's method, though it contains three steps instead of two: induction, ratiocination, and verification. Induction is used to ascertain the causes and the laws that state their operation. Ratiocination is then used to deduce predictions from the laws, which are then brought to bear against observations in the verification step. The Hypothetical Method, meanwhile, bypasses the inductive step and proceeds directly to ratiocination and verification. The complete Deductive Method is more secure in Mill's mind than the Hypothetical Method. We will see how the Hypothetical Method is equipped with additional constraints in order to meet the standards set by inductive reasoning and avoid the downfalls associated with hypothesizing causes. But for now, let us turn to the Deductive Method.

The Deductive Method

The Deductive Method is Mill's preferred scientific method. It consists of three parts: induction, ratiocination, and verification, which are to be carried out in that order as needed. The inductive step is

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147. Ibid., p. 585.
148. This is more of a tool than a stand-alone method in his picture of science; since it does not enter into my final analysis, I will not be treating it further in the chapter, though I mention it here.
149. Here he quotes Herschel directly in the passage I quoted earlier. See Herschel, p. 181.

further broken into a set of five strategies: 1) the Method of Agreement, 2) the Method of Difference, 3) the Joint Method of Agreement and Difference, 4) the Method of Residues, and 5) the Method of Concomitant Variables. The ratiocinative step is the stage at which predictions are deduced from the laws achieved during induction. In the verification step, those predictions are brought to bear against observations. As we will see, there are also ways to achieve verification through the deduction of other, previously established laws (either derivative or empirical). We can now go through each of the three steps of the Deductive Method, and see what they involve.

The Inductive Step

Mill defines induction as "the operation of discovering and proving general propositions".[150] When he refers to inductive reasoning, he excludes cases where nothing new is learned—where the result is a mere description, such as Kepler's ellipse. He wants to avoid merely "representing the sum of the observed facts."[151] The point is that to do a proper induction, you have to be doing something more than just finding additional ways to describe the data; it has to reach beyond the data in some way.

The aim of the inductive step, going beyond mere description, is to ascertain causes and the laws that describe their manner of acting. The procedures used during the inductive step will depend on what kind of opportunities there are for observation and experimentation. There are five possible strategies to follow: the Method of Agreement, the Method of Difference, the Method of Joint Agreement and Difference, the Method of Residues, and the Method of Concomitant Variation. These fives methods correspond to Herschel's rules for experiment, but Mill goes into greater detail to describe how they ought to work. We will now go over each method and identify the following: i)

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[150] Ibid., p. 284.
[151] Ibid., p. 295.

under what circumstances that method should be taken ii) how the method works, and iii) how the method ranks against the others in terms of getting quality evidence and in terms of the kind of laws it can generate.

The Method of Agreement: The First Canon

The First Canon reads: "*If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree, is the cause (or effect) of the given phenomenon.*"[152] The logic behind it is that having ruled out every other candidate cause for a phenomenon, we would be left with the true cause, which must come from the set of antecedent circumstances. The thought is to see what some phenomenon *a* is always accompanied with, say *A*. If the circumstances of the instances are varied enough, there is reason to think that *A* and *a* have a causal relationship. Whatever can be eliminated, meanwhile, is not taken to be connected with the phenomenon by any law of causation.

The Method of Agreement is intended to be used in case artificial experimentation is impossible or impractical. Unfortunately it cannot be assured from this method alone that the only possible cause has been singled out. It can be helpful to have many instances to support the claim, but that is only a slight advantage against the plurality of causes—otherwise number of instances does not make an evidential difference. The failure to rule out other connections to *a* does not, however, invalidate the results of this method (such a failure would, however, be critical for the Method of Difference). Used on its own, this method cannot establish causation, only ascertain empirical laws, since the results may rely on collocations. The characteristic imperfection of the Method of Agreement is the risk involved in doing inductions from a limited set of data, which is that there may be other causes that can produce the

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152. Ibid., p. 390.

phenomenon. Mill says that the method is not radically discredited in this case, though it is a source of inferiority and is reason enough to never rest content with it in the results obtained by it. Thus, this method on its own does not produce any kind of high quality evidence. It should, therefore, be followed up by using the Method of Difference or by finding a way to connect the law deductively with some law already ascertained by the Method of Difference.

The Method of Difference: The Second Canon

The Second Canon reads: "*If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.*"[153] If, for example, we wish to understand the effects of some agent A, and A is present in some set of circumstances ABC, then we can see the effects of ABC and compare them to the effects of BC. If *a* is produced in the former situation (ABC) and not the latter (BC), then we know the cause of *a* must be A in some respect, either as the sole cause or in conjunction some other part of the circumstances present. As in the Method of Agreement, whatever cannot be eliminated is understood to be connected with the phenomenon by law. Mill characterizes both the Method of Agreement and the Method of Difference as "methods of elimination", meaning that they work by ruling out alternative causal explanations. The Method of Difference is, however, evidentially superior to the Method of Agreement:

> It thus appears to be by the Method of Difference alone that we can ever, in the way of direct experience, arrive with certainty at causes. The Method of Agreement leads only to laws of phenomena...that is, to uniformities, which either are not laws of causation, or in which the question of causation must for the present remain undecided. The Method of Agreement is chiefly to be resorted to, as a means of suggesting applications of the Method of Difference …

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153. Ibid., p. 391.

> or as an inferior resource, in case the Method of Difference is impracticable; which … generally arises from the impossibility of artificially producing the phenomena.[154]

Thus, the Method of Difference is more powerful than the Method of Agreement, as it is said to ascertain causes as well as empirical laws.

The Joint Method of Agreement and Difference: The Third Canon

The Third Cannon states: "*If two or more instances in which the phenomena occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance; the circumstance in which alone the two sets of instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.*"[155] This method, which is a combination of the first two methods, has the advantage of not being affected by the characteristic imperfection of the Method of Agreement, but is also able to be used when little or no aid from artificial experiment is available.

This method is the most powerful option, he says, for cases when the area of science is depending on mostly non-intervening observations. This method is recommended for cases when the effect in question cannot be produced at will, or when it is impossible to create a set of conditions just like another save for a single property (e.g., to bring about a set of circumstances that are identical to the Iceland spar except in one respect in order to investigate double refraction). This method is called the Joint Method of Agreement and Difference. Still, this is ultimately just an improvement and extension of the Method of Agreement, and it is not as powerful in generating evidence in the Method of Difference.

The Method of Residues: The Fourth Canon

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[154] Ibid., p. 394.
[155] Ibid., p. 396.

The Fourth Canon is: "*Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents.*"[156] The thought is this: if any parts of a phenomenon can be summarized as effects of known causes ascertained from previous inductions, then those effects may be subducted from the phenomenon; the remainder of the phenomenon must then be the effects of the remaining antecedents.

Whatever remains can then be informative in two ways: either to draw attention to a previously overlooked or unrecognized antecedent, or to figure out the (previously unknown) quantity of the effect of an already known cause. If, for example, it is known that ABC leads invariably to abc, and it is also known that A causes *a* and B causes *b*, then it can be learned that C is causally related to *c*, where *c* is a "residual phenomenon".[157] The Method of Residues is crucial as an instrument of discovery to handle the plurality of causes, since otherwise *c* may be hard to observe given the presence and "intermixture" of *a* and *b*.

Mill understands this method as a special adaptation of the Method of Difference. It is not guaranteed that any particular C is the only remaining possible cause of the residual phenomenon. So, the evidence from the Method of Residues is still not complete without either showing the action of C experimentally, by testing it separately and demonstrating its role, or by showing that it can be explained and proved deductively from known laws.[158]

In addition to the Method of Residues, Mill also describes another method to expose residuals, which works by removing parts of the phenomenon that are due to chance. It is difficult to develop the law of a cause when there is a plurality of causes, and this is especially true when the effect of a cause

[156] Ibid., p. 398.
[157] Mill is borrowing this term from Herschel.
[158] Ibid., p. 398.

is generally subtle so that it gets lost among the other causes that may vary from one instance to the next, creating what we could call noise. He writes,

> If the effect of the constant cause is always accompanied and disguised by effects of variable causes, it is impossible to ascertain the law of the constant cause in an ordinary manner, by separating it from all other causes and observing it apart"... "When the action of a cause A is liable to be interfered with, not steadily by the same cause or causes, but by different causes at different times, and when these are so frequent, or so indeterminate, that we cannot possibly exclude all of them from any experiment, though we may vary them; our resource is, to endeavor to ascertain what is the effect of all the variable causes taken together.[159]

This method can be used to discover residual phenomena that would otherwise be very hard to detect through subduction: "This may be called *the discovery of a residual phenomenon by eliminating the effects of chance.*"[160] After finding that a connection is not due to chance, it is reasonable to conclude that there is some kind of causal relationship involved, though it is only taken as an empirical law.[161]

The Method of Concomitant Variations: The Fifth Canon

The Fifth Canon states: "*Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation.*"[162] The logic here is that if the modification of something is invariably connected with the modification of something else, then the two are causally connected in some manner or other. It is not required to stipulate that any cause that is modified will have a modified effect, but when there is a variation in one that is connected to a variation in another, then a causal relationship is being suggested.

The nature of the relationship is ambiguous when the candidate cause cannot be artificially varied: it cannot be concluded exactly whether some *x* is a cause of some *y*, or instead the effect of *y*, or

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[159] Ibid., p. 530.
[160] Ibid., p. 532.
[161] Ibid., p. 533.
[162] Ibid., p. 401.

if they are causally connected in some other way (say, through some other *z*). Here Mill mentions another limitation on this method: the possibility that the cases in which the phenomena have been observed fall within a range that is a small or otherwise limited part of the total possible variation of the phenomena—that is, that the observations were not representative of the whole phenomena.

Mill borrows as example Herschel's discussion of the formulas that were generated from empirical laws for the elasticity of steam and their failure to project outside the limits of the observations that were used to obtain the formulas. Even with this uncertainty, the Method of Concomitant Variation can nevertheless give evidence that there is some connection between A and a, which is supposed to make us confident that it will at least hold within the limits of the initial observations.

Mill recommends that this method be used in cases where the relevant variations of the cause are quantitative variations. It applies especially when the cause is permanent or otherwise difficult to eliminate. One may, for instance, record the motions of a pendulum in cases where friction is more or less present, to ascertain what would occur if there were no resistance forces present; he mentions how Borda was able to demonstrate how a swinging weight would swing longer when friction is removed or limited.

The Ratiocinative Step

The purpose of the ratiocinative step is to determine the consequences that follow from the laws ascertained through induction or from a hypothesis (the Hypothetical Method is described below). This step refers to the calculation of those consequences, in particular the numerical calculation of them. It involves answering the following questions: "Given a certain combination of causes, what effect will follow?", and "What combination of causes, if it existed, would produce a given effect?"[163] The process of ratiocination can be used to help differentiate between alternatives. If laws can be connected by ratiocination, they tend to share evidence. If instead they lead deductively to incompatible consequences, they can act as tests of each other. Ratiocination is used during the verification step, but it can also be used during induction, as in the method of residues.

The Obstacles Faced During Ratiocination

One of the problems faced during ratiocination is when the mathematics available are insufficient to produce exact deductions from theory. He mentions, as did Somerville and Herschel, the failure to have a solution for the three body problem (as the only solutions available are approximations), and also the difficulty in plotting projectiles when there is air resistance. Another difficulty is what he calls the "plurality of causes", which exacerbates the task of identifying which parameters are relevant. He considers the objection that inadequate causal accounts threaten the validity of the inductive step:

> But (it may here be asked) are not the same arguments by which the methods of direct observation and experiment were set aside as illusory when applied to the laws of complex phenomena, applicable with equal force against the Method of Deduction? When in every single instance a multitude, often an unknown multitude, of agencies, are clashing and combining, what security have we that in our computation *a priori* we have taken all of these into our reckoning? How many must we not generally be ignorant of? Among those which we know, how probable that some have been overlooked; and, even were all included, how vain the pretence of summing up the effects of many causes, unless we know accurately the numerical law of each,--a condition in most cases not to be fulfilled; and even when it is fulfilled, to make the calculation transcends, in any but very simple cases, the utmost power of mathematical science with all its most modern improvements.[164]

Mill responds by saying that this is the whole reason for the verification step, which I will now describe.

[163] Ibid., p. 460.
[164] Ibid., p. 460.

The Verification Step

The Necessity of Verification

Mill says that the Deductive Method comes with a procedure to avoid such errors that arise in the face of the plurality of causation: namely, the verification step. Without it, he says, all of the generalizations that result from the Deductive Method must be taken as mere conjectures that cannot be relied on until the verification step is complete. In the verification step, the consequences that had been computed during ratiocination are compared with observations and the empirical laws.

What Constitutes a Verification

One way to achieve a verification with very strong evidence for a candidate law of nature is to deduce known empirical laws from it:

> If direct observation and collation of instances have furnished us with any empirical laws of the effect (whether true in all observed cases, or only true for the most part), the most effectual verification of which the theory could be susceptible would be, that it led deductively to those empirical laws; that the uniformities, whether complete or incomplete, which were observed to exist among the phenomena, were accounted for by the law of the causes—were such as could not but exist if those be really the causes by which the phenomena are produced.[165]

Mill offers as example the claim that Kepler's laws can be deduced from Newtonian theory. That this can be done means that Newtonian theory has, in Mill's mind, achieved high quality evidence for Newtonian science. Mill saw that any candidate celestial theory had to do so to be taken seriously: "Thus it was very reasonably deemed an essential requisite of any true theory of the causes of the celestial motions, that it should lead by deduction to Kepler's laws: which, accordingly, the Newtonian theory did."[166]

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165. Ibid., p. 461.
166. Ibid., p. 461.

Verification can also be achieved by demonstrating a certain level of agreement with the observed phenomena. For this to work, the phenomena must be properly described—that is, "in the most comprehensive as well as accurate manner as possible."[167] What Mill has in mind here is something like the developments in the mathematical descriptions of the shapes of the orbits, which were once "expressed by a circle, then by a system of epicycles, and subsequently by an ellipse."[168] It is also required that the scope of the verification includes circumstances that are "of at least equal complexity with any other cases in which its application could be called for."[169] Using complex instances is not particularly helpful when it comes to the initial discovery of laws. It is, however, useful for their verification. Complex cases become a "new experiment" on the law under different conditions, where it may be possible for the cause to be counteracted or otherwise interfered with by other factors. He then qualifies this by remarking that when a law is successfully tried in cases where it was not previously known to apply to, the evidence gained is something to which "scientific inquirers [habitually] attach rather too much value than too little."[170]

It is likely that apparent counterexamples emerge in the course of verification, given the complexity of the actual physical world and the plurality and interaction of causes. These cannot simply be dismissed or explained away: it is required to resolve apparent counterexamples to uphold the status of the laws of causality. It is permissible for these laws of causation to be counteracted, for such counteractions do not threaten the reality of his general causal principle (the Law of Causation):

> To the law of causation, on the contrary, we not only do not know of any exception, but the exceptions which limit or apparently invalidate the special laws, are so far from contradicting the universal one, that they confirm it; since in all cases which are sufficiently open to our

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[167] Ibid., p. 461.
[168] Ibid., p. 461.
[169] Ibid., p. 461.
[170] Ibid., p. 462.

> observation, we are able to trace the difference of result, either to the absence of a cause which had been present in ordinary cases, or to the presence of one which had been absent.[171]

Given the earlier discussion of the types of laws, it seems that a similar remark can be made for the universal laws that is here being made for the Law of Causation: the *actual* ultimate laws will not be counteracted; but when a candidate for a universal law is shown to be counteracted or interfered with, it is taken to be merely derivative, rather than universal. Derivative laws are liable to be threatened from many sources even if they are laws of causation, since there may be multiple causes at work to determine it, which can each be counteracted.[172] By tracing the circumstantial factor that led to the law's failing to hold, a more general law can be developed that in turn takes that factor into account.

A failure to resolve disagreement between theory and observation threatens the status of the theory: "But if our deductions have led to the conclusion that from a particular combination of causes a given effect would result, then in all known cases where that combination can be shown to have existed, and where the effect has not followed, we must be able to show (or at least to make a probable surmise) what frustrated it: if we cannot, the theory is imperfect, and not yet to be relied upon."[173] He offers an example related to wave propagation: "the difference between the observed and the calculated velocity of sound was ascertained to result from the heat [developed] by the condensation which takes place in each [sound] vibration. This was a trial, in new circumstances, of the law of the development of heat by compression, and it added materially to the proof of the universality of that law."[174] Depending on just what he means here, this might indicate his awareness that resolving a discrepancy in this way can contribute additional evidence for the existence of the cause initially associated with the law; of course, it is also possible (and, I think, more plausible) that he intends to say that the

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171. Ibid., p. 571.
172. Ibid., p. 549.
173. Ibid., p. 461.
174. Ibid., p. 462.

verification of a law threatened by apparent counterexample can only be attained if the counterexample is worked out. The discovery of the interfering cause served as proof inasmuch as it enabled the law to overcome an impediment to verification. Mill probably saw the resolution of the disagreement as contributing additional evidence in virtue of its being a test in "new circumstances" that can offer information about the actual scope of the law and the grounds to project it.

The Hypothetical Method

A second method, in addition to the Deductive Method, is the Hypothetical Method. The Hypothetical Method is construed as a method that bypasses the inductive step of the Deductive Method, but which still employs ratiocination and verification. Mill defines a hypothesis as "any supposition which we make (either without actual evidence, or on evidence avowedly insufficient) in order to endeavor to deduce from it conclusions in accordance with facts which are known to be real; under the idea that if the conclusions to which the hypothesis leads are known truths, the hypothesis itself either must be, or at least is likely to be, true."[175] Most hypotheses, he says, are formed with the intent to explain some set of facts by leading deductively to them; this is the case when the hypothesis involves a claim about the cause or mode of production of some phenomenon. Hypotheses are often made when no known laws lead deductively to the phenomenon in question:

> Since explaining, in the scientific sense, means resolving [a] uniformity which is not a law of causation from which it results, or a complex law of causation into simpler and more general ones from which it is capable of being deductively inferred; if there do not exist any known laws which fulfill this requirement, we may feign or imagine some which would fulfill it.[176]

There are, however, constraints on what kinds of hypotheses should be pursued or that count as being scientific. It is not permitted to merely conjecture a cause in *ad hoc* fashion. To avoid mere

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[175] Ibid., p. 490.
[176] Ibid., p. 490.

conjecturing, Mill recommends that hypotheses ought to “ally themselves by analogy with known laws of nature.”[177] This is achieved when either the cause is known to be real but its manner of acting is hypothesized or when the cause is hypothesized but its manner of acting resembles other established laws for a different set of phenomena.[178]

The Legitimacy of the Hypothetical Method

Since the use of hypotheses can lead to problems such as dead-ends, Mill outlines the constraints on hypothesizing that make the use of the Hypothetical Method more legitimate. He does so by laying out the risks of hypotheses and offering some precautions to avoid them. One of these risks is that mere agreement between a hypothesis and the phenomena does not guarantee the truth of the hypothesis. He points out that there are often plausible alternative hypotheses that agree just as well: “since [agreement with the phenomena] is a condition sometimes fulfilled tolerably well by two conflicting hypotheses; while there are probably a thousand more which are equally possible, but which, for want of anything analogous in our experience, our minds are unfitted to conceive.”[179] He then considers whether a hypothesis can obtain a more secure status if it is capable of yielding new predictions that are then shown to hold. But, he says, correct predictions do not completely verify a hypothesis. At most, predictive success can lead us to conclude that the hypothesized law and the true one are just “partially identical”.[180] He uses as example the debate between the wave and particle theories of light to explore the limitations of agreement to pick out a single hypothesis as the only true one:

> No one supposes the agreement with the phenomena of light with the theory of undulations to be merely fortuitous. It must arise from the actual identity of some of the laws of undulations

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[177] Ibid., p. 490.
[178] Ibid., p. 490.
[179] Ibid., p. 500.
[180] Ibid., p. 501.

> with some of those of light: and if there be that identity, it is reasonable to suppose that its consequences would not end with the phenomena which first suggested the identification, nor be even confined to such phenomena as were known at the time. But it does not follow, because some of the laws agree with those of undulations, that there are any actual undulations...Even the undulatory hypothesis does not account for all the phenomena of light. The natural colors of objects, the compound nature of the solar ray, the absorption of light, and its chemical and vital action, the hypothesis leaves as mysterious as it found them; and some of these facts are, at least apparently, more reconcilable with the emission theory than with that of Young and Fresnel. Who knows but that some third hypothesis, including all these phenomena, may in time leave all the undulatory theory as far behind …?[181]

At best, Mill argues, if a hypothesis explains the facts and has predicted others that have since been verified, then it can be concluded at least that the true laws of the phenomena "must bear a great similarity to those of the class of the phenomena that to which the hypothesis assimilates it."[182] There may be grounds, in some cases, to pursue hypotheses in further research. Still, hypotheses should not be "mistaken for scientific truth", or else they can become an impediment to progress if other possibilities are ignored.[183] On the matter of agreement, Mill is contrasting his own position to Whewell's:

> [Whewell] recognizes absolutely no mode of induction except that of trying hypothesis after hypothesis until one is found which fits the phenomena; which one, when found, is to be assumed as true, with no other reservation than that if on re-examination it should appear to assume more than is needful for explaining the phenomenon, the superfluous part of the assumption should be cut off.[184]

Mill is therefore going to require something stronger than prediction and agreement alone in the verification step of the Hypothetical Method. Hypotheses are permitted only if during their verification they satisfy the requirements of a complete induction, which means that it must be shown that there are

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181. Ibid., p. 502.
182. Ibid., p. 560.
183. Ibid., p. 560.
184. Ibid., p. 503.

no alternatives to the hypothesized law (which is similar to how, in induction, causes should be picked out as the only possible candidate from the antecedent conditions):

> This process may evidently be legitimate on one supposition, namely, if the nature of the case be such that the final step, the verification, shall amount to and fulfill the conditions of, a complete induction. We want to be assured that the law we have hypothetically assumed is a true one; and its leading deductively to true results will afford this assurance, provided the case be such that a false law cannot lead to a true result; provided no law, except the very one which we have assumed, can lead deductively to the same conclusions which that leads to. And this proviso is often realized.[185]

But given that many hypotheses can have the same level of agreement with the phenomena, how is a hypothesis to be singled out as the only possibility? It is not sufficient to just have no *apparent* alternatives to account for the facts, since there may still be alternatives unrecognized.[186] Crucial experiments can sometimes be used to put the facts in order and pave the way for future scientists and rule between two opposing hypotheses when they both hold fairly well with observations, but this does not rule out other alternatives.[187] In response to these issues, Mill offers two ways to make the Hypothetical Method more feasible and more legitimate: namely, analogical reasoning and reference to known causes.

Analogy

So far, Mill has not given a full account of how hypotheses ought to be used in science. He wants to be able to rule out the mere conjecturing of causes and also the long process of trying hypothesis after hypothesis that may just be met with dead-ends. To manage these problems, he recommends two techniques: the extension of known causes and the use of analogies. Let us start with the role of analogy.

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185. Ibid., p. 492.
186. Ibid., p. 503.
187. Ibid., p. 560.

An analogy is distinguished from a strict induction in the following way. In both strict induction and analogy, it is concluded that since A resembles B in one or more properties, it probably also resembles it in a certain other property. In strict induction, it is shown that there is an invariable conjunction between the former property (or properties) and the latter one. With analogies, no such conjunction is shown to exist.[188] In analogical reasoning, if some fact *m* holds of *A*, then *m* is more likely to be true of *B* if *B* resembles *A* with respect to some of its properties than if *B* did not resemble anything in any way that had attribute *m*. This is supposed to be the case even if there is not an established connection between *m* and those properties.

Of course, it is possible that the differences in properties can counter the sharing of other properties. If no evidence exists for a connection between *m* and the resembling properties, then the power of the analogy depends on the extent of the resemblance (greater resemblance means greater likelihood that fact *m* will hold). Analogies are by no means a substitute for proper induction, but they can act as a guidepost and point to where more rigorous investigation should be done in the future. Even if the resemblance in the analogy is low, analogy can at least be used to direct experiment or observations when it is not possible to do a strict induction.[189]

Analogies do not themselves constitute scientific knowledge on their own, as there are severe limits on projection of analogies. Analogies can only be projected to adjacent cases. In this case, adjacency is meant not in spatial or temporal terms, but in terms of similar circumstances (*i.e.* where similar other causes or factors are at work).[190] But while the analogies do not themselves amount to secure knowledge, they can be used to direct research where strict induction is not yet possible and lead to more secure knowledge down the road, at which point a strict induction may become possible.

188. Ibid., p. 555.
189. Ibid., pp. 559-560.
190. Ibid., p. 559.

Known & Unknown Causes

Another way to make the pursuit of hypotheses more practicable is by extending known causes into new circumstances. Hypotheses are less worthy of pursuit when they make use of an unknown cause rather than a known one, since the use of unknown causes tends to be more conjectural:

> Now it appears to me that this assurance cannot be obtained, when the cause assumed in the hypothesis is an unknown cause, imagined solely to account for *a*. When we are only seeking to determine the precise law of a cause already ascertained, or to distinguish the particular agent which is in fact the cause, among several agents of the same kind, one or other of which it is already known to be, we may then obtain the negative instance. An inquiry, which of the bodies of the solar system causes by its attraction some particular irregularity in the orbit or periodic time of some satellite or comet, would be a case of the second description. Newton's was a case of the first. If it had not been previously known that the planets were hindered from moving in straight lines by some force tending towards the interior of their orbit, though the exact direction was doubtful; or if it had not been known that the force increased in some proportion or other as the distance diminished, and diminished as it increased; Newton's argument would not have proved his conclusion.[191]

He then says that it was just a matter of figuring out the direction of the force and the various possible numerical relations between distance and the attractive force. Agreement in conjunction with the extension of a known cause points to a more appropriate use of a hypothesis. In this case, the hypothesis is about the particular manner of acting of a cause, the "law of correspondence":

> It appears, then, to be a condition of a genuinely scientific hypothesis, that it be not destined always to remain an hypothesis, but be of such a nature as to be either proved or disproved by comparison with observed facts. This condition is fulfilled when the effect is already known to depend on the very cause supposed, and the hypothesis relates only to the precise mode of the dependence; the law of variation of the effect according to the variations in the quantity or in the relations of the cause. With these may be classed the hypotheses which do not make any supposition with regard to causation, but only with regard to the law of correspondence between facts with accompany each other in their variations.[192]

Only in cases of known causes does agreement offer evidential force. In any other case, "it is no evidence of the truth of the hypothesis that we are able to deduce the real phenomena from it."[193] If a

191. Ibid., p. 493.
192. Ibid., p. 494.
193. Ibid., p. 495.

cause is hypothesized that has not already been shown to exist in other cases, then it needs to amass independent evidence to qualify as a *vera causa*.

Necessity of the Hypothetical Method in Immature Science

Mill does make note of Newton's '*hypotheses non fingo*' passage, in which Newton asserts that hypotheses have no place in science.[194] But he then remarks that he cannot fathom a way for sciences to get to an advanced stage without some kind of initial hypotheses: "Nearly everything which is now theory was once hypothesis."[195] He offers several circumstances that seem to necessitate some form of hypothesizing. First, hypotheses have a role to play in deciding what experiments ought to be done. Second, sometimes temporary assistance from hypotheses is needed to "convert" experimental into deductive truths, by seeing how suppositions (via their consequences) differ from the real phenomena and identifying what corrections to make in the assumptions: "In this way, which has been justly compared to the Methods of Approximation of mathematicians, we arrive, by means of hypotheses, at conclusions not hypothetical."[196] Finally, although mere agreement is not enough when the cause is not already known to be operating, there is still a need to identify the cause in the first place. Hypotheses about the cause of a phenomenon (when they have independent evidence and meet the criteria of a '*vera causa*') are permitted to give science a starting-point. He says: "It is perfectly consistent with the spirit of the method, to assume in this provisional manner not only an hypothesis respecting the law of what we already know to be the cause, but an hypothesis respecting the cause itself". For this purpose, the use of hypotheses is "allowable, useful, and often even necessary."[197]

Organizing Science: Resolution and Scientific Explanation

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[194] Or in Newton's own terms, "experimental philosophy".
[195] Ibid., p. 496.
[196] Ibid., p. 497.
[197] Ibid., p. 498.

To Mill, the characteristic feature of progress in science is the resolution of laws into more general ones—finding a way to deduce laws from other, more general ones in a way that produces an explanation of the law. Resolving laws is the best way to achieve their explanation. Mill considers Newtonian mechanics to be the exemplar for this process:

> In what cases, accordingly, has science been most successful in explaining phenomena, by resolving their complex laws into laws of greater simplicity and generality? Hitherto chiefly in cases of the propagation of various phenomena through space: and, first and principally, the most extensive and important of all facts of that description, the fact of motion.[198]

The goal of resolution is to achieve the simplest expression of the uniformities of nature; this is what is meant by a law of nature. Whatever can be deduced from the laws of nature cannot count as an additional law of nature. Likewise, if a set of laws can be resolved into more general laws, then those laws cannot be considered laws of nature. For example, Kepler's 'laws' ceased to be called "laws of nature" once more general laws of motion developed, since Kepler's laws are a special case of the laws of motion applying to the planets. There are three ways to go about resolving laws, or giving an explanation, which I discuss below.

Three Ways to Resolve Laws

For Mill, something is given an explanation by "stating the law or laws of causation of which its production is an instance."[199] When something is deductively explained, this can mean either discovering a law of the cause or explaining a previously discovered law with reference to other more general laws. There are three ways that a law of causation can be explained from other laws or resolved into other laws that are more general, which I describe below.

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[198] Ibid., p. 486.
[199] Ibid., p. 464.

In the first case, a law is explained “by resolving the law of a complex effect into the laws of the concurrent causes and the fact of their coexistence.”[200] That is, the law is resolved into simpler laws of the separate causes that are at work as well as the fact that all of those causes are present. Complex effects, which are the result of many causes, are broken down into the separate laws that describe the manner of acting of each of the causes. Laws that are so explained are thus dependent on collocations (or the existence of certain agents or powers in certain places at certain times). This dependency is a reason why the initial law has a smaller scope of projection, and why it is more liable to be counteracted. The more general laws that describe each individual cause are not subject to these limitations.

In the second case, the explanation is achieved “by the detection of an intermediate link in the sequence.”[201] The thought is fairly straightforward: “When, between what seemed the cause and what was supposed to be its effect, further observation detects an intermediate link; a fact caused by the antecedent, and in its turn causing the consequent.”[202] The smaller causal links are individually less likely to be counteracted, Mill argues. Again, the result is laws that are more general.

The third case involves the “subsumption” of less general laws under a more general one.[203] The less general laws, in cases like these, are found to be instances of a more general process. The process that he is describing is in effect the process of unification. Mill’s examples include the subsumption of terrestrial gravity and the force towards the sun under the general law of gravitation, and also the subsumption of the laws of magnetism under the laws of electricity. Resolution of laws in this way is a method to discover more general laws, and it is perhaps the only way to ever arrive at the most general laws: “It is thus that the most general laws of nature are usually arrived at: we mount to them by

200. Ibid., p. 464.
201. Ibid., p. 465.
202. Ibid., p. 465.
203. Ibid., p. 469.

successive steps."[204] The reason Mill gives for this is that observations are all made under sets of particular conditions. As the observations are put into laws and combined, the differences that the particular conditions make can gradually be worked out; each of the statements produced from this strategy will be more and more general. With this sort of resolution, any exceptions to the initial laws will likewise be exceptions to the general law that describes a wider range of phenomena. This is in contrast to the first type, where the derivative laws might be counteracted while the more general law is still intact, since the cause that counteracted or interfered with the derivative law can be pointed out and made part of the more general law.

Why These Methods Appropriate For Natural Science

The Plurality of Causes

One of the difficulties faced in natural science, which Mill's scientific methods are required to manage, is the plurality of causes. The characteristic imperfection of the Method of Agreement, as I mentioned above, is the plurality of causes—the possibility that some unrecognized or unappreciated factor plays a causal role in determining the phenomenon in question. Mill offers the Method of Residues to solve this problem in particular, since it lets us discover the influence of other causes not yet noticed.

The Tendency of Causes to be Counteracted & The Intermixture of Effects

A set of challenges more severe than the plurality of causes lies in what Mill calls the Intermixture of Effects and the interference between causes. He refers to them as the "principal part of the complication and difficulty of the study of nature."[205] It is difficult to make progress when the

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[204] Ibid., pp. 465-466.
[205] Ibid., p. 439.

tendencies that would be described by laws are counteracted or otherwise interfered with by other causes. To make progress, it is necessary to be able to parse out these causes.

> It is obvious that we cannot expect to find the law of a tendency, by an induction from cases in which the tendency is counteracted. The laws of motion could never have been brought to light from the observation of bodies kept at rest by the equilibrium of opposing forces. Even where the tendency is not, in the ordinary sense of the word, counteracted, but only modified, by having its effects compounded with the effects arising from some other tendency or tendencies, we are still in an unfavorable position for tracing, by means of such cases, the law of the tendency itself. It would have been scarcely possible to discover the law that every body in motion tends to continue moving in a straight line, by an induction from instances in which the motion is deflected into a curve, by being compounded with the effect of an accelerating force...Accordingly, in the cases...in which the causes do not suffer themselves to be separated and observed apart, there is much difficulty in laying down with due certainty the inductive foundation necessary to support the deductive method.[206]

Mill concedes that his methods of direct induction do little to handle this difficulty. Instead, deduction is the only adequate tool to detangle the complexities.[207] The challenge is even greater, he thinks, in other sciences such as physiology, since it is hard to know whether a cause of some physiological phenomena is local or not:

> This difficulty is most of all conspicuous in the case of physiological phenomena; it being seldom possible to separate the different agencies which collectively compose an organized body… and for this reason I am inclined to the opinion, that physiology (greatly and rapidly progressive as it now is) is embarrassed by greater natural difficulties, and is probably susceptible of a less degree of ultimate perfection, than even the social science.[208]

As a result, he says that those laws that can be counteracted should be described as making claims about mere tendencies rather than as about actual results.

The Necessity of Working in Steps Towards More General and Secure Laws

The resolution of laws into more general ones is for Mill an appropriate response to the plurality and interaction of causes. By building up to laws more general successively, through steps, scientific

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[206] Ibid., p. 455.
[207] Ibid., p. 439.
[208] Ibid., pp. 455-456.

research is able to tease through the complexity of the physical world and work towards ultimate laws. It would be impractical and downright impossible to seek the ultimate laws more directly. The Method of Residues characterizes this strategy; for it is the knowledge of one cause and its manner of acting that allows other causes to be discovered. Trying the laws in new circumstances is a way to ascertain their limits and also the reasons for their shortcomings, which informs what the more general law ought to be, since it must reconcile those shortcomings.

Why Physics Was So Successful

I have just given the reasons why Mill's scientific methods are appropriate for natural science given the evidence problems that are faced—namely, the plurality of causes, the tendency of causes to be counteracted, and the intermixture of effects. There are also reasons why Mill believes that the methods he describes have been used successfully to bring natural science to a state of maturity with high quality evidence. There are also reasons to think that there are particular features of physics and chemistry that were necessary for the method to be successful, meaning that the methods may not be as helpful to other sciences, such as physiology.

Mill remarks, for instance, that the continuity between terrestrial physics and celestial physics played a crucial (and maybe even necessary) role in the development of physics. This is because experiments could be done on earth that would shed light on celestial phenomena. Another virtue of physics is that there is a lesser degree of the intermixture of causes than in other sciences, including physiology. The sharing of underlying causes in physics makes the science very amenable to general laws. That motion is captured so broadly by the notion of forces means that the process of extending and generalizing laws is made easier. The process of resolving laws has been incredibly successful and probably constitutes the greatest achievements in physics:

> There is, therefore...no absurdity in supposing that all motion may be produced in one and the same way; by the same kind of cause. Accordingly, the greatest achievements in physical science have consisted in resolving one observed law of the production of motion into the laws of other known modes of production, or the laws of several such modes into one more general mode...when the motions said to be produced by magnetism were shown to be produced by electricity; when the motions of fluids in a lateral direction, or even contrary to the direction of gravity, were shown to be produced by gravity.[209]

So great were the successes earned by this method that Mill recommends that physics ought to proceed as far as it can by seeking to identify more and more general laws, with the end goal being a sort of unification. And Mill sees many open ends related to motion where there is a hint of a promise of successful unification: "There is an abundance of distinct causes of motion still unresolved into one another; gravitation, heat, electricity, chemical action, nervous action, and so forth; but whether the efforts of the present generation of savans to resolve all these different modes of production into one, are ultimately successful or not, the attempt so to resolve them is perfectly legitimate."[210] Even if it does not pan out, he says, the historical success of resolution is enough to motivate the effort.

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209. Ibid., pp. 486-487.
210. Ibid., p. 487.

CHAPTER FOUR: 19th CENTURY PERSPECTIVES VERSUS NEWTON

Theory Development and the Logic of Ongoing Research and Testing

This final chapter is organized into two parts, one consisting of remarks on the early stages of theory development, and the other with remarks on the role of theory in ongoing research and testing. In the first part, I compare the accounts of Somerville, Herschel, and Mill to what Newton actually did in the *Principia*. Recall that, at least for Mill and Herschel, scientific method includes an inductive step and a verification step, with an assistive computation step to make predictions that can then be compared with phenomena. It is helpful to think of the inductive step as the stage of initial theory building, and the verification step as the basis for continued research and testing. In my comparison, I will be emphasizing five key features of the *Principia* that were crucial for Newton's strategy for theory-building. These are: Newton's second-edition comment on hypotheses, his use of "inference tickets,"[211] his *quam proxime* reasoning, Newtonian idealization, and the evidential relationship between Newton's theory and Kepler's "laws".

In the second part, I compare the 19th century accounts of the maturation of Newtonian science to the post-Newton research period as accounted for by George Smith in "Closing the Loop".[212] The purpose of this section is to focus on the structure of ongoing research predicated on existing theory (namely, Newton's). In particular, I will focus on the role in evidential reasoning and ongoing research that residual phenomena play. I am, in effect, asking two questions. First, to what extent did Somerville, Herschel, and Mill appreciate the evidential value of pursuing residual phenomena? Second, what is the evidence generated from successfully accounting for residual phenomena *for?*

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[211] 'Inference ticket' is a term that I am borrowing from George Smith, who himself acquired the term from Arthur Prior.
[212] See Smith (2014).

PART ONE:
19th Century Perspectives Versus the Principia

Hypotheses and the *Principia*

In the General Scholium of the second edition of the *Principia*, Newton made the following remark, probably in response to a challenge that his account of gravity was merely an alternative to Cartesian vortices:

> I do not feign hypotheses. For whatever is not deduced from phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction. The impenetrability, mobility, and impetus of bodies, and the laws of motion and the law of gravity have been found by this method.[213]

Newton says that the law of gravity did not originate as a hypothesis; rather, he says, it was "deduced from the phenomena". In their writing, both Mill and Herschel expressed a disdain for the unrestricted use of hypotheses.[214] Yet they will both discuss the ways that the use of hypotheses can be legitimated, and they try to lay out in detail the conditions under which the use of hypotheses is permitted. Mill argues, for instance, that hypotheses are often necessary in the initial development of a science, especially in case we know neither the cause of a phenomenon nor the cause of anything analogous to it.

A feature of these inducto-deductive accounts is that the causes of phenomena are separate from the laws of the cause during theory development. The cause can, it seems, be identified prior to formulating the law of the cause, for the law of the cause can be modified in the face of new observations during the verification step. Let us first consider the discovery of the cause itself, which in

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[213] See Cohen (1999, p. 943).

[214] Somerville is cautious as well, given what I have said about her, but she does not elaborate as much on the ways that use of hypotheses should be restricted.

the case of Newton’s theory refers to gravitational forces. For Herschel, it is always better to proceed by extending causes that are known to operate in analogous cases. Hypotheses can assist in gathering relevant phenomena together so that analogies can be identified: “Hypotheses, with respect to theories, are what presumed proximate causes are with respect to particular inductions: they afford us motives for searching into analogies; grounds of citation to bring before us all the cases which seem to bear upon them, for examination.”[215] Herschel says that this move is just what legitimated the extension of forces from the terrestrial sphere to the celestial one, as there was an opportunity to pursue an even wider range of phenomena that may be under the influence of a similar cause:

> If the analogy of two phenomena be very close and striking, while, at the same time, the cause of one is very obvious, it becomes scarcely possible to refuse to admit the action of an analogous cause in the other … For instance, when we see a stone whirled round in a sling, describing a circular orbit round the hand, keeping the string stretched, and flying away the moment it breaks, we never hesitate to regard it as retained in its orbit by the tension of the string, that is by *a force* directed to the centre; for we feel that we do really exert such a force. We have here *the direct perception* of the cause. When, therefore, we see a great body like the moon circulating round the earth and not flying off, we cannot help believing it to be prevented from so doing, not indeed by a material tie, but by that which operates in the other case through the intermedium of the strong,—a *force* directed constantly to the centre. It is thus that we are constantly acquiring a knowledge of the existence of causes acting under circumstances of such concealment as effectually to prevent their direct discovery.[216]

So, this is what justifies the extension of forces into the celestial realm. But what about the mathematical law of gravitational force? Herschel states that in research, the laws are assumed and then compared with all phenomena in its assigned scope. It is on this comparison that the expressions of the laws are modified or restricted, so that agreement with the phenomena is achieved.[217] This statement from Herschel suggests that the way that laws are given their mathematical expressions has a hypothetico-deductive character to it, insofar as it is deliberately formulated with the criterion of

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215. Herschel (1845, pp. 195-196).
216. Ibid., p. 142.
217. Ibid., p. 199.

agreement in mind. This seems to be the case with Mill as well, who offers a picture in which the actions of causes are investigated under new circumstances so that we may ascertain their scope and how their effects are expressed.

It is perfectly legitimate and even recommended, per what Mill and Herschel have said, to pursue hypotheses whenever they assist in forming more general laws from other existing laws that have been arrived at inductively. Hershel says that Kepler's ellipse was found to be "more than empirical rule" when it was found to hold also for the satellites of Jupiter, which for Herschel amounts to a successful induction.[218] He then says that Newton improved upon Kepler's work by "ascending" through a "series of close-compacted inductive arguments to the highest axioms of dynamical science" until all important astronomical phenomena were given complete explanations.[219] Herschel's account of what Newton did in the *Principia* is that Newton managed to bring a wide range of phenomena together under a single law:

> Newton shows all the celestial motions known in his time to be consequences of the simple law, that every particle of matter attracts every other particle in the universe with a force proportional to the product of their masses directly, and the square of their mutual distance inversely, and is itself attracted with an equal force. Setting out from this, he explains how an attraction arises between the great spherical masses of which our system consists, regulated by a law precisely similar in its expression; how the elliptic motions of planets about the sun and of satellites about their primaries, according to the exact rules inductively arrived at by Kepler, result as necessary consequences from the same general law of force; and how the orbits of comets themselves are only particular cases of planetary movements. Thence proceeding to applications of greater difficulty, he explains how the perplexing inequalities of the moon's motion result from the sun's disturbing action; how tides arise from the unequal attraction of the sun as well as of the moon on the earth, and the ocean which surrounds it; and lastly, how the precession of the equinoxes is a necessary consequence of the very same law.[220]

Herschel continues to say that Newton's work in the *Principia* was subsequently verified by his immediate successors, who extended and improved the mathematical side of the theory.

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[218] Ibid., p. 269.
[219] Ibid., p. 271.
[220] Ibid., pp. 272-273.

What Herschel describes here is an instance of the what they all see as a quite legitimate way to employ hypotheses; namely, to formulate connecting hypotheses that either link together a group of laws or put them into a more general expression. The same justification is given by Somerville when she considers Mossotti's theory, which linked the different kinds of forces (gravitational and cohesive) together. We will, however, see how the actual evidential relationship between Newton's theory and Kepler's is not compatible with how Herschel and Mill portray it.

In any case, Newton says that he did not just hypothesize the law of gravity such that it agreed with the phenomena; he instead claims to have "deduced" it. We can now turn to the *Principia* to illustrate what such a deduction from phenomena could mean.

Inference Tickets & *Quam Proxime* Reasoning

Newton claims to have arrived at the laws of motion and the law of gravity by deducing them from phenomena. George Smith, in "Closing the Loop", describes how Newton managed to make strong inferences from phenomena by fleshing out a generic theory of centripetal forces and subsequently using it to infer the force law from phenomena, rather than merely hypothesize it. Newton does so by means of "inference tickets", which are licenses to make conclusions about forces (such as the force law or the direction of a particular force) from the motions of bodies.

For example, Newton argues from his generic theory of centripetal forces that the inverse-square law holds for a circular orbit if and only if the 3/2 power rule also holds.[221] Newton further identifies a second way of figuring out whether the inverse-square law holds, which he can use when

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[221] The inverse-square law says that the quantity of force between two bodies is inversely proportional to the square of the distance between them, as long as there are no outside forces. The 3/2 power rule says that the square of the period of an orbiting body varies as the cube of the semi-major axis of the orbit.

the orbits are not circular: the exponent of the force law is exactly -2 when the orbits are stationary. That is, the force law is inverse-square if and only if the aphelia do not precess:

> The forces by which the primary planets…are maintained in their orbits are…inversely as the square of their distances from its center…is proved with the greatest exactness from the fact that the aphelia are at rest. For the slightest departure from the ratio of the square would (by book 1, prop. 45, corol. 1) necessarily result in a noticeable motion of the apsides in a single revolution and an immense such motion in many revolutions.[222]

Newton's generic theory of centripetal forces also gives him a way to measure the direction of a force acting on a body. Newton does not need to hypothesize the direction of the force on a body; rather, he uses violations of the area rule to infer the direction of the force. For instance, if the areas swept out per unit of time with respect to the center of the orbit are increasing or decreasing, then the force is directed to a point slightly off of that center.

Newton realizes that the true motions of the celestial bodies do not match up exactly with the descriptions of motions that are used to license inferences about forces. One reason for this is that the true motions of planets under gravitational interaction are extremely complex, so there is no plausibly attainable description that accounts for the true motions, only approximations. Another reason is that our observations are always imprecise. Newton finds a way to solve this issue, however, by using what can be referred to as "*quam proxime"* reasoning, where "*quam proxime*" can be roughly translated as "nearly exactly". To be able to make conclusions about the force law from the motions of the planets when those motions may not be exactly described, Newton formulates *quam proxime* versions of the inference rules: the inverse-square law holds *nearly exactly* if, and only if, the orbits are *nearly exactly* stationary.[223] It is likewise for the area rule: if a planet's motion very closely approximates the area rule around some point, then the force-center is nearly exactly at that point.

222. Cohen (1999, p. 802).
223. Recall that Mill required that descriptions be as precisely and completely stated as possible. It seems clear that he could not have seen a way to work around inexactness in the manner that Newton achieved. I do not think he saw how necessary it was in the development of the *Principia*, either.

This sort of reasoning allows Newton to reach conclusions about the forces acting on the celestial bodies that hold to a certain level of approximation without actually having exact descriptions of the motions of those bodies. Since his generic theory of centripetal motion allows him to make these inferences, he is able, with the assistance of theory, to infer conclusions about forces from phenomena rather than hypothesize them and look for agreement. None of the 19th accounts that I have presented take this kind of reasoning into account. Without it, it seems that they would be forced to acknowledge a hypothetical character in the initial formation of the law of gravity, with the hope that the statement of the law could, after formulation, achieve inductive support from experiments or observations under new circumstances and ultimately satisfy the requirements of verification.

The Evidential Relationship Between Newtonian Theory and Kepler's "Laws"

The view held by Somerville, Herschel, and Mill about the evidential relationship between Newton's theory and Kepler's laws is the following: since Newton's theory leads deductively to Kepler's laws, evidence is generated for Newtonian theory. Consider once more this statement from Mill, which says that it was the requirement of *any* candidate theory to have Kepler's laws as consequence: "Thus it was very reasonably deemed an essential requisite of any true theory of the causes of the celestial motions, that it should lead by deduction to Kepler's laws: which, accordingly, the Newtonian theory did."[224] To these writers, this success amounted to being able to show not only that Kepler's laws are a necessary consequence of Newton's theory, but also that Newton's theory holds for an even wider range of phenomena, or as Herschel put it, "applications of greater difficulty".[225] Those apparent violations were actually just the consequences of the law of gravity, and since Newton's theory accounts for phenomena that Kepler's laws cannot, Newton's theory has demonstrated that it is

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224. Mill (1973, p. 461).
225. Herschel (1845, p. 273).

capable of explaining even more. Under this description, evidence has mostly been generated for Newton's theory, because it is capable of explaining Kepler's laws and more.

Herschel and Mill would also say that Kepler's laws have some new evidence for them as well, though not in the way I will later emphasize. As Mill would phrase it, Kepler's laws have new evidence because they have been "deductively explained" and are shown to be consequences of more general laws that hold for an even wider range of phenomena. Herschel's way of putting it is that Newton showed that Kepler's laws were not mere empirical laws, since they are derived from laws more general. The reality, however, is that Kepler's laws are not straightforward consequences of Newtonian theory. Consider the following passage from Somerville to see why:

> Were the planets attracted by the sun only, they would always move in ellipses, invariable in form and position; and because his action is proportional to his mass, which is much larger than that of all the planets put together, the elliptical is the nearest approximation to their true motions. The true motions of the planets are extremely complicated, in consequence of their mutual attraction; so that they do not move in any known or symmetrical curve, but in paths now approaching to, now receding from, the elliptical form; and their radii vectors do not describes areas or spaces exactly proportional to the time, so that the areas become a test of disturbing forces.[226]

According to what Somerville says here, it is a consequence of the law of gravity in our solar system that the ellipse *never* holds exactly, nor the area rule, because the planets are not only attracted to the sun but also to every other planet, so that the planets always deviate from a perfectly elliptical orbit. Yet, Herschel and Mill's earlier remarks suggested that Newton's theory leading deductively to Kepler's laws was a crucial evidential development.

I will argue now, and will continue to argue for the rest of the chapter, that what these three writers did not see was that any violation of Kepler's laws that is found to be the result of the influence of some other force is not only evidence for Newton's theory, but also simultaneously for Kepler's

[226] Somerville (1840, p. 14-15).

laws. The reason is that if all of the known deviations from Kepler's laws can be attributed to some particular gravitational interaction, we can take those resolved deviations to be evidence that Kepler's laws *would* hold exactly were it not for those gravitational interactions.

One reason why it seems that Mill and Herschel may not fully appreciate this is that their picture of evidence in Newtonian science does not emphasize that it is a consequence of Newton's theory that Kepler's laws do *not* ever hold exactly.[227] A second reason is that they did not seem to realize the historical fact that Kepler's laws were not established in the scientific community before Newton, for there were comparably accurate competing methods of generating the astronomical tables, including methods that did not use each of Kepler's laws.[228] Furthermore, Newton never required in the first place that the ellipse be a consequence of his theory and his law of gravitation. Again: Newton infers that the inverse-square law holds, *quam proxime*, from the 3/2 power rule and from the motion of the apsides. The true orbits of the planets are never exact ellipses, as we have seen.

If the planets did orbit in an exact ellipse, then the force rule that moves them in their orbits would have to be inverse-square, which Somerville tells us: "It has already been shown, that the force producing perfectly elliptical motion varies inversely as the square of the distance, and that a force following any other law, would cause the body to move in a curve of a very different kind."[229] If, however, the orbits are only nearly exactly ellipses, then it cannot be inferred that the exponent of the force law is nearly exactly -2. It is possible, in fact, to approximate the ellipse without the exponent being even close to -2. There are more reasons to think that they did not see how deviations from Keplerian theory, successfully resolved, could actually be evidence *for* Keplerian theory, and not just

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[227] Though Somerville discussed the differences between Newtonian celestial theory and Kepler's laws, she did not consider the evidential relationship between them in as much detail.

[228] See Smith (2014, p. 268) for the alternatives.

[229] Somerville (1840, p. 20).

for Newtonian theory. But allow me to first say more about Newtonian idealizations and the tradition of research predicated on Newtonian theory.

Newtonian Idealizations and the Complexity of the Physical World

For Newton, the aim of theory was never to yield exact predictions, for the true motions of the planets are too complicated to ever represent exactly.[230] Newton's approach in the *Principia* was therefore to begin with an idealization of the solar system that, as Smith emphasizes, would hold exactly (and not merely approximately) under certain physical circumstances. As such, Newton takes care in the *Principia* to establish what would occur under those idealized physical circumstances.[231] By doing so, Newton creates a baseline from which any observed deviations should be recognized as physically meaningful, rather than as mere artifacts leftover from a mathematical approximation. They can tell you, for instance, whether there is an additional force acting on the bodies that has not yet been taken into account. Put another way: the residual *discrepancies* between theory and observations can become residual *phenomena*. We will later see that residual phenomena exposed by pursuing Newtonian theory can give information about the physical world beyond the existence of other forces at work, though probably none of the three writers realize this.

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230. The only tractable solutions for the three-body problem are approximate ones.
231. For example, Kepler's laws would hold exactly if the only gravitational force was directed towards the sun.

PART TWO:
19th Century Perspectives Versus History of Research Predicated on Newtonian Theory

Although only Herschel and Mill use the term “residual phenomena” explicitly, Somerville demonstrates that she is familiar with the concept when she describes how violations of the area rule can be used as a test for disturbing forces. The pursuit of residual phenomena plays an important role in all three accounts of how Newtonian science came to be so successful. They saw fairly clearly how their sources could be uncovered and lead to new discoveries and more comprehensive theories. Since these three writers were confident that Newtonian theory was correct, they did not stop to consider what exactly it was about Newton’s theory that made it possible to expose residual phenomena in the first place. Given what we know now about events later in the century, we are posed to ask the following question more so than they were: if Newtonian theory turned out to be wrong, how was it so instrumental in making these discoveries? Asking how it is that residual phenomena can be exposed is, in a way, the same as asking what exactly residual phenomena are evidence for, which we will turn to later.

What is Required to Expose Residual Phenomena?

The 19th Century View

As Somerville, Herschel, and Mill are all aware, the activity of some physical agents can be hard to detect, since they may have effects that are difficult to observe directly. Somerville describes how theory can be practically necessary to identify the effects of certain gravitational interactions that would otherwise be insensible, typically because the phenomenon is dominated by some other cause, such as a dominating gravitational force: “But the mass of the whole of the planets and satellites taken together is so small, when compared with that of the sun, that [the gravitational interactions of the planets] are quite insensible, and could only have been discovered by theory."[232] Another reason the action and existence of physical agents may go unnoticed is that in the physical world, many causes are often "intermixing" and counteracting each other. This was Mill's contribution to the discussion of residual phenomena. If a cause and its law are correctly known, then its effects may be subtracted out from the phenomena, so that the effects of other causes may be exposed.

As Herschel points out, it is necessary not only to have correctly ascertained the cause and the law of that cause, but also to be able to deduce its actions as precisely as possible. Otherwise, residual discrepancies may be due to mathematical shortcomings during the deduction of the consequences. Mathematical approximations in the deductions could potentially hide any parts of the phenomena that are due to other agents not yet noticed. Somerville, Herschel, and Mill all recognize this, for they each emphasize the need to improve mathematics until it is capable of representing the true law of the cause, so that the law can ultimately be completely verified.

As I have come to understand it, the primary functions of the pursuit of residual phenomena, on these accounts of scientific method, are to facilitate scientific discovery and also to bring the laws closer to verification, by completing the theory and accounting for an ever-wider range of phenomena. Herschel, prioritizing completeness, says that the process must continue until nothing is left unaccounted for:

> In the conduct of this verification, we are to consider whether the cause or law to which we are conducted be one already known and recognized as a more general one, whose nature is well understood, and of which the phenomenon in question is but one more case of the in addition to those already known, or whether it be one less general, less known, or altogether new. In the latter case, our verification will suffice, if it merely shows that all the cases considered are plainly instances in point. *But in the former, the process of verification is of a much more severe and definite kind. We must trace the action of our cause with distinctness and precision, as modified by all the circumstances of each case; we must estimate its effects, and show that nothing unexplained remains*

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232. Somerville (1840, p. 26).

> *behind; at least, in so far as the presence of unknown modifying causes is not concerned.*[233]

The ultimate goal, in Herschel's mind, is to account for all known residual phenomena by showing that whatever was left unexplained was either actually just the consequence of a general law, and not actually a counter example, or instead the consequence of some other cause that has since been established inductively. In other words, it must be assured that nothing stands in the way of verification, which is needed to assure that the inductions were valid:

> As such, [the residual discrepancies whose sources are worked out] are neither exceptions nor residual facts, but fulfilments of general rules, and essential features in the statement of the case, *without* which our induction would be invalid, and the law of gravitation positively untrue.[234]

Verification is, on all three accounts, the highest quality evidence that can be obtained for physical theories. The only way to obtain a complete verification, however, is to assume that in scientific practice, a phenomenon can be broken down completely into the effects of various causes. This is the final reason why I am left to believe that none of the three were able to fully appreciate the potential evidence that can be generated by a long period of success resolving residual phenomena.

George Smith's View

Another term closely related to the notion of residual phenomena is "second-order phenomena", which is the term that Smith uses to emphasize that they can only be exposed after existing theory is compared with observations. It is not merely that these phenomena are hard to detect (say, by making a particularly small difference on their own). Rather, they are entirely unobservable without the assistance of theory, since they are what is left when observations are subtracted from theory.

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[233] Herschel (1845, pp. 165-166, emphasis is mine).

[234] Ibid., p. 202.

Considered this way, residual phenomena (or second-order phenomena) are capable of generating very high quality evidence when they are successfully attributed to some physical feature of the world, for those discoveries could not have been made without the assistance of theory. This suggests that evidence can be generated each time such a residual phenomenon is resolved, even if the theory has not yet come into perfect agreement with phenomena. But what exactly is the evidence *for?*

Evidence for What?

According to Mill, an induction is valid when it has singled out a theory as the only possible explanation for the phenomena. He writes, "The strongest assurance we can obtain of any theory respecting the cause of a given phenomenon, is that the phenomenon has either that cause or none."[235] Yet, Newtonian theory was not the only theory to fit all of the evidence that was available at the time Mill and the others were writing; Einstein's fit all of that evidence, too. Newton's theory, on this account, cannot claim the high quality evidence that Mill imagines for it, for it was not the only theory that could fit the available evidence.

The long period of success in resolving residual phenomena did not give conclusive evidence that Newton's theory was the only theory that could account for the phenomena. Yet many of the discoveries made from pursuing residual phenomena were predicated on Newton's theory, and would not have been exposed were it not for its assistance, again because residual phenomena take the form of observations subtracted from theory and as such are not directly observable.

As mentioned above, Newton took special effort to show that there is an ideal physical circumstance in which his theory *would* hold exactly. Smith points out that this is crucial for the evidential reasoning. Any discrepancies between the predictions made from ideal theory and

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[235] Mill (1973, p. 573).

observations (if they are not due to observational error, inexact mathematics, or imprecise measurements in theory) must be due to a deviation from the physical idealizations or assumptions.

If the theory holds any weight, these discrepancies, if they are quantifiable and well-defined (meaning that they appear to be systematic), can also give information about the physical world and the shortcomings of theory. A perturbation on the orbit of Uranus revealed that another planet was influencing its own orbit; by investigating this discrepancy, Neptune was discovered (though by no means straightforwardly). The existence of Neptune was further corroborated by tracing its effects on Jupiter and Saturn, whose orbits were made more accurate when taking into account the effects of Neptune. Smith calls this the robustness requirement: if a source is identified for a second-order phenomenon, then it must be traced into other parts of theory. This requirement prevents *ad hoc* attributions for the source of discrepancies.

What Smith came to realize is that this process can be recursive and that when it is recursive, a special form of evidence is developed. Once the source of some second-order phenomenon is taken into the theory, new second-order phenomena can be exposed. As this process continues, a series of successive approximations[236] are developed, with new discoveries about physical features of the world made along the way. Historical examples include the non-sphericity of the Earth on the motion of the moon and the higher-level interactions between the planets. Most importantly, this creates high-quality evidence for the all of the theoretical elements that were used to make the calculations that exposed the residual phenomena in the first place.

This means that even if the theory does not come to predict the phenomena exactly (and given the complexity of the celestial phenomena and the intractability of the three-body problem, exact

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[236] In the development of Newtonian theory, each of these iterations involves a new idealized physical model that would hold exactly if no other forces were at work.

prediction was always out of the question anyway!), there is still high quality evidence for the elements of the theory, including the measurements of theoretical quantities and, in the case of Newtonian theory, the perturbational influences of known gravitational interactions.

When in the second half of the 19th century a residual discrepancy was discovered that could not be resolved in a manner compatible with Newtonian theory—namely, the motion of the perihelion of Mercury—there had already been a great deal of evidence generated for the elements going into the computations that exposed second-order phenomena with robust physical sources, and this evidence did not simply go away in the transition from Newton's theory to Einstein's.

Evidence Both For A Theory and Against It

The motion of the perihelion of Mercury and the development of Einstein's theory revealed several things about the relationship between Newtonian theory and the world. For one, it showed that some of the physical assumptions behind Newtonian theory were false. Einstein showed that the gravity field equations in the Newtonian theory were wrong and that the field equations need to include nonlinear terms. Second, Newtonian theory did not take into account the fact of the finite speed of light. Third, Newtonian theory could not express the curvature of space.

The 43 arc second discrepancy was a crucial piece of evidence for Einstein's theory. Yet, the discrepancy was simultaneously evidence for Einstein's theory as it was for Newton's, though in the latter case it is only evidence that Newton's theory will hold to a very high level of approximation in a restricted domain: in the static, weak-field limit. Einstein's theory accounted for the motion of the perihelion, but that motion (namely, the 43 arc second discrepancy in the motion) was only observable with the assistance of Newton's theory. This motion was not observable without the assistance of theory, again because it was the result of subtracting observations from predictions made with Newtonian theory. Einstein took care to show that Newtonian theory holds in the asymptotic limit of his own theory: namely, in the static, weak-field limit. By doing so, he showed that the discrepancy derives from a violation of a Newtonian assumptions. Such a violation tells us about the physical world and *why* Newton's theory falls short of describing it. Einstein's theory also accounts for why Newtonian theory held as well as it did given the particular arrangement of our solar system and gives us reason to continue to project it to a very high level of approximation into situations in the static, weak-field limit.

The relationship between Einstein's theory and Newton's is similar in some respects to the evidential relationship between Newton's theory and Kepler's laws, wherein Newton's theory offers the resources necessary to interpret violations of Kepler's laws as indications of other forces at work. That residual phenomena could offer both evidence against a theory, while simultaneously offering evidence for its theoretical components, is what these 19th century writers did not realize. Perhaps they might have, if only they were instead writing some years later.

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