Models, Science, and Intersubjectivity

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Among the many philosophical vices Husserl is accused of having committed, the charge of solipsism looms particularly large. Of course, Husserl is not said to hold the absurd view that nothing except one solitary mind exists. The point is rather methodological: Although phenomenology's self-declared goal is to "*explicate the sense this world has for us, prior to any philosophizing*" (Husserl 1960, 151), the methodological operations to be performed by the phenomenologist seem to restrict the field of analysis to her own consciousness and phenomena. But if this is the case, an all-encompassing description of all kinds of experience is beyond the reach of what phenomenology can deliver. Besides foreign subjects and all sorts of social occurrences, phenomenologists seem to be unable to account for the way reality is given to us pre-philosophically. To experience things as objectively existing means to experience them as "there for everyone". But how should phenomenology account for this "thereness-for-everyone" if phenomenological descriptions are limited to a purely egological basis?

If this line of criticism is justified, then phenomenology is also unlikely to provide a promising framework for the analysis of the positive sciences. One reason is that, as a growing number of philosophers stress (Longino 1990; Solomon 2001; Kusch 2002), science is a collective endeavor. Consider, for instance, one of the most fundamental methodological principles in modern experimental science, the principle that "non-reproducible single occurrences are of no significance to science" (Popper 2002, 66). It is clear that "reproducibility" here means *intersubjective reproducibility*: In order for experimental data to be relevant, it must be reproducible by different subjects at different times and at different places. If phenomenology was really limited to a purely egological basis, then it would be hard to see how this principle could be accounted for in phenomenological terms. What is more, scientific theories aspire to correctly describe things, properties and processes that exist objectively, i.e. that are experienced as being "there for everyone". But, again, how should phenomenologists account for this kind of transcendence if their methodological approach restricts them to the sphere of immanence?

The charge of methodological solipsism is still part of the anti-Husserlian folklore, despite arguments to the contrary. Since it would lead me too far afield to rehearse these arguments in detail, I will restrict myself to just one point. Critics accusing Husserl of being a methodological solipsist typically read him in a decidedly Cartesian vein (cf., e.g., Blackburn 1996, 222-223). On this reading, the motive behind the introduction of the reduction is the demand for absolute justification. Guided by a distinction between the absolute being of the immanent and the merely presumptive being of the transcendent, the epoché is understood as a radical turning away from the latter and thus from everything worldly. Once the sphere of immanence is identified as the realm of apodictic indubitability, it is on this foundation that all kinds of presumptive knowledge must be secured in a philosophically rigorous manner.

Although, admittedly, this Cartesian reading is not entirely without textual support (cf., e.g., Hua II), it must still be regarded as a caricature of Husserl's true intentions. To begin with, the point of the reduction is not to withdraw from the realm of transcendence, but rather to open up a new attitude through which the "how" of the givenness of all kinds of objectivities can be studied. Instead of naively taking the objects of our intentional directedness as a given, the reduction unveils the subjective accomplishments that go into the constitution of these objects. On this understanding, the aim the reduction is not to exclude the transcendent from the field of phenomenological analysis. The reduction rather works like a contrast medium: its purpose is to highlight the structuring activity of consciousness that is always already operative in every cognitive involvement with the world. Seen from this perspective, then, "[t]he point [of phenomenology] is not to secure objectivity, but to understand it" (Husserl 1970, 189). What this means can be illustrated by taking a closer look at the constitution of physical things.

In the usual course of events, perceiving a thing is to be intentionally directed towards a three-dimensional object in space. However, what is sensuously given at each moment in time is merely a two-dimensional appearance that presents one profile of the intended thing. Hence, there is a describable difference between what is meant through an act of perception ("The thing over there") and what is sensuously given ("The thing's facing side with its currently visible features"). Another way to put this is that, although only a profile is given "in person" in perception, there is always already a consciousness of the complete thing towards which we are intentionally directed. In order to account for this, the mature Husserl sought to describe the nature of intentional directedness in terms of its *horizonal structure*. In perception we are not only conscious of what is actually given at each moment in time. To perceive a thing also means to co-intend a horizon of aspects that are absent in the currently present perception, but that could be actualized in the course of a continued perceptual encounter with the thing. Hence, as phenomenological analyses reveal, intending is always and necessarily an "*intending-beyond-itself*" (Husserl 1960, 46). On this view, then, perception consists in the projection of ever-changing appearances against the background of an open horizon of possible further experiences through which momentarily absent aspects would be actualized.

It is no exaggeration to say that the notion of horizon is one of the most productive concepts in Husserl's mature philosophy. However, as compelling as it may be, it calls for further clarification. One question concerns the absent but co-given aspects that make up the horizon into which the sensuously given is always embedded. In particular, what is the constitutive status of these co-intended aspects (cf., for the following, Zahavi 2001, chapter II.4)? Going through Husserl's writings, two answers seem to prevail: Husserl sometimes claims that absent but co-intended aspects are constituted as aspects of the thing that were or could be actualized through past or future experiences. On other occasions, Husserl writes as if co-intended aspects were constituted as actually perceivable possibilities. Yet, as Zahavi has argued, both answers are phenomenologically suspect.[i] Zahavi thus points to a third line of interpretation that indeed seems to capture the meaning these co-intended aspects have for us in the course of normal perceptual episodes: On this interpretation, it is still correct that absent but co-intended aspects are the correlates of possible experiences. The crucial point is, however, that these co-intended aspects are not constituted as the contents of possible experiences *I* could have, but as the contents of possible experiences *every member of an open community of foreign subjects* could have. Or, to put it in Husserl's own words:

> Every appearance that I have is from the very beginning a member of an open, endless, but not actualized range of possible appearances of the same, *and the subjectivity of these appearances is the open intersubjectivity.*" (Hua XIV, 289; my translation)

Hence, rather than pushing intersubjectivity beyond the limits of phenomenological analysis, the reduction makes us realize that intersubjectivity is the very condition of the possibility of perceptual objectivation: In every perceptual encounter with things there is always already an implicit relation to an open community of subjectivities without which the thing could not be constituted as objectively existing.

Zahavi's reading of Husserl fits nicely with other parts of Husserl's philosophy, especially with those relevant for a phenomenological interpretation of the sciences. Consider, for instance, part four of *Ideas 1* in which Husserl develops his phenomenological conception of rationality. Quite generally, Husserl's aim is to describe how rationality manifests itself in consciousness. The question that needs to be answered is thus "what the 'claim' of consciousness actually to 'relate' to something objective, to be 'well-founded', properly signifies" (Husserl 1983, 308) and, consequently, "of what *rational consciousness* consists" (Husserl 1983, 326). In line with his general meta-philosophical approach, Husserl tackles this question by first considering a case in which the rationality of an assertion would be beyond dispute. Phenomenologically construed, such an ideal situation would occur when the intended object is given exactly as it is intended and when the congruence between the object-as-intended and the object-as-given is registered by the experiencing subject. Since, in this situation, the rightness of the intention is licensed by the experience of *fulfillment*, it would be countersensical to not regard the beliefs about the object as rationally justified. However, the experience of fulfillment is not only the natural stopping point of all of our epistemic endeavors. According to Husserl, fulfillment is also the *sine qua non* of all rational positing and hence the core of his phenomenological notion of rationality. "*The posited characteristic has*", Husserl writes, "*a specific rational character* [...] *if and only if* it is a position on the basis of a fulfilled, originarily presentive sense and not merely on the basis of just any sense" (Husserl 1983, 327). Let us call this the *phenomenological condition of rationality*, or *RC* for short.

How is RC to be interpreted? If RC ought to be a general condition of rationality, it cannot be read as asserting that a subject, S, is rationally justified to believe that p if and only if S's intention towards p *is* intuitively fulfilled. If this was the case, S would be rationally justified to believe just one single proposition at a time, namely the one for which the corresponding state of affairs is being intuitively fulfilled for S (Hardy 2013, 112). Hence, since we should be hesitant about principles that render almost everything we think we know defective, RC must be liberalized by introducing a modal qualification. A more cogent formulation might thus take the following form: S is rationally justified to believe that p if and only if *it is possible* that the intention towards p is intuitively fulfilled. But this formulation raises the obvious question of how to understand the notion of possibility that is being invoked here. One option would be to explicate it on a purely egological basis, for instance by tying it to S's subjective capacities. But this interpretation is hardly any better than the previous one. Suppose that I believe that the neighbors' dog vandalized our front lawn yesterday while I was in the gym. Although I might have all kinds of reasons for holding this belief (the lawn is in shambles and there are paw prints all over it; my wife has witnessed the act of vandalism), it is still not rational for me to believe the proposition in question because it is not in my power to bring the corresponding state of affairs to intuitive fulfillment. This result—which is clearly at odds with our epistemic practices—shows that RC is too restrictive if its modal qualification is tied to the capacities of a solitary subject. As far as I can see, the only solution is to further modify RC by adding an intersubjective dimension. A more sensible formulation might thus take the following form: S is rationally justified to believe that p if and only if it is possible *for any member of S's epistemic community* that the intention towards p is being intuitively fulfilled. On closer inspection, however, this formulation is unconvincing too. Suppose I believe that there is a rock lying on the far side of Kepler-452b. Although I might have all kinds of reasons to hold this belief (like Earth, Kepler-452b orbits a G2V-type star within the conservative habitable zone; all available data suggests that Kepler-425b is a rocky planet), it is still not rational to believe the proposition in question because it is impossible for me or any actual member of my epistemic community to travel distances of roughly 1,400 light-years. Assuming that my belief about Kepler-425b is nevertheless rational, the lesson to be learned is that RC is still too restrictive if its modal qualification is understood in terms of *real* possibilities that depend on the capacities of *actual* members of my epistemic community. Instead, it appears more promising to tie the possibility of intuitive fulfillment to *an open community of possible subjectivities*. Accordingly, the final formulation reads as follows: S is rationally justified in believing that p if and only if it is possible *for any member of an open community of possible subjectivities* that the intention towards p is being intuitively fulfilled. I take it that this is a defensible formulation of RC that also does justice to the reality of our epistemic practices.

The point of the previous discussion was to show that intersubjectivity—far from being the death knell of phenomenology—occupies a central position in many key areas of Husserl's thinking. This is not only true of his philosophy of perception, where one of the main insights is "that even what is straightforwardly perceptual is communalized" (Husserl 1970, 163). Reference to an open community of possible subjectivities is also always already presupposed when a claim of consciousness to relate to something objective is rightly deemed rational. Let me now, with these remarks as a backdrop, come to my actual topic, to Husserl's treatment of the positive sciences.

Even though Husserl nowhere offers a systematic exposition of his philosophical views about science, the remarks scattered throughout his oeuvre give a good idea of how a phenomenological philosophy of science might look like and how Husserl's views about science evolved over the course of his career. Judged by today's standards, the early Husserl seems to advocate a rather conservative construal of scientific methodology. Quite generally, the empirical sciences—unlike phenomenology and other eidetic sciences—are said to rely on "indirect methods" (Husserl 1965, 147), which, as Husserl remarks in the *Logical Investigations*, have "deduction, verification and [...] repeated modification" (Husserl 2001, 160) as their components. Furthermore, the early Husserl strongly emphasizes the role of demonstrative reasoning by claiming that "every explanatory interconnection is deductive" (Husserl 2001, 147) and that every scientific explanation depends on "the explanatory ground of a law, from which a class of necessary truths follow" (Husserl 2001, 146).

Those familiar with the history of philosophy of science will not fail to notice the similarities between these remarks and the model of scientific method that was widely discussed until the 1960ies under the label of *hypothetico-deductivism*, or *HD* for short (Hempel 1966, chapters 2 and 3). In its simplest form, the idea behind HD is that a theory is confirmed (or falsified) by its true (or false) observable consequences. Consider, to use an example given by Popper (2002, 38), the general hypothesis according to which pieces of thread will break whenever they are loaded with weights exceeding the threads' tensile strength. This general hypothesis entails the singular-predictive statement that a thread with the tensile strength of 1 kilogram will break if it is loaded with a weight of 2 kilograms. If experimental data proves the singular-predictive statement to be true, the general hypothesis is thereby confirmed (or, on Popper's view, corroborated). If, on the other hand, experimental data proves the singular-predictive statement to be false, the hypothesis must be rejected or at least modified. Note also that HD is isomorphic to one of the classical accounts of scientific explanation, the so-called *deductive-nomological model* (Hempel 1965, 335-376; Popper 2002, 38-40). In line with Husserl's aforementioned remarks about explanation, the point of this model is that an empirical occurrence is explained if it can be deduced from a set of premises that includes at least one law that is necessary to the deduction. On this view, then, the fact that a piece of thread is broken is explained by deducing the singular statement describing this occurrence from a general, law-like statement ("All pieces of thread will break whenever they are loaded with weights exceeding the threads' tensile strength") and certain singular statements specifying the initial conditions ("The tensile strength of the broken thread was 1 kilogram"; "The weight that was put on the broken thread was 2 kilograms").

Given his early remarks on the matter, it comes as no surprise that some commentators see "Husserl [as subscribing] to something like the hypothetical-deductive model" (Hardy 2013, 29). And, coming back to our earlier discussion about intersubjectivity, this seems to make the communal character of science easily accountable for in phenomenological terms. In order to see why, consider first the level of methodology. According to HD, theories can only be indirectly confirmed or disconfirmed by, first, deducing observable consequences from these theories and by, second, comparing these consequences with corresponding experimental data. However, as I have noted at the beginning of this paper, underlying this procedure is the basic methodological demand that the experimental data must be *intersubjectively reproducible*. Now, phenomenologically construed, this demand is neither a matter of stipulation or social convention, nor is it something that is specific to science itself. Rather, the demand for intersubjective reproducibility is but one concrete instantiation of RC, which, as we have seen, ties the rationality of assertions to an open community of subjectivities for whom it must be possible to bring the objects or state of affairs about which something is asserted to intuitive fulfillment. Since only the observable consequences of theories (and not the unobservable mechanisms they stipulate) can be brought to intuitive fulfillment, it seems natural for phenomenology to anchor scientific rationality in the ways in which theories manifest themselves "within the sphere of what is actually experienced and experienceable in the lifeworld" (Husserl 1970, 52).

According to what has just been said, the kind of rationality that is embodied in science is but a special case of RC. And since RC necessarily involves reference to an open community of possible subjectivities, the fact that intersubjectivity is an essential ingredient of scientific methodology is grounded in a more general phenomenological description of the nature of rational positing. This, however, is not all. As we have seen, HD puts special emphasis on the observable consequences of theories because only these consequences, and not the theories themselves, can be compared with what is observed in experiments. Yet, given our earlier discussion about perceptual objectivation, it is apparent that this recognition of experimental reports as the final arbiters of a theory's supposed truth or falsity opens up another avenue for intersubjectivity to enter into the very fabric of science. If our earlier remarks about Husserl's philosophy of perception are correct, every perceptual encounter with things necessarily involves an implicit relation to an open community of subjectivities without which the thing could not be constituted as objectively existing. And this, of course, is no less true of the things and processes that are observed in experiments. As a consequence, intersubjectivity not only enters into scientific research through the methodological demand for intersubjective reproducibility. From a phenomenological point of view, the communal character of science also stems from the fact "that even what is straightforwardly perceptual is communalized" (Husserl 1970, 163).

Let us pause for a moment and take stock. Contrary to a widespread prejudice, a closer look at his writings shows that intersubjectivity occupies a central position in Husserl's philosophy. As Zahavi has argued, the reference to an open community of subjectivities underlies every act of perceptual objectivation. And, as the discussion about RC has indicated, the same can be said about acts of rational positing. Combined with Husserl's early remarks about scientific method, this seems to suggest that the communal nature of science is easily accounted for within the framework of phenomenology: According to HD, empirical confirmation is achieved by comparing singular-predictive statements that are deductively entailed by a theory with the experimental reports that correspond to these predictive-singular statements. On this view, then, intersubjectivity is woven into the very fabric of science, first, because of the nature of perceptual objectivation and, second, because of the methodological demand for the reproducibility of experimental data. But note also that phenomenology accounts for the intersubjective nature of science on a much more fundamental level than most other approaches: On a phenomenological view, science is not only *contingently* collective, for instance, because of its complexity and the resulting need for division of cognitive labor. Rather, the intersubjective character of science is grounded in the very architecture of subjectivity, which, as Husserl came to realize, "is what it is [...] only within *intersubjectivity*" (Husserl 1970, 172; my emphasis). For phenomenologists, then, a thorough analysis of consciousness shows that the idea of a purely egological science is nothing but a *contradiction in terms*.

Although the results obtained so far are promising, there is, I think, a problem with this line of argument. As I have pointed out, HD was one of the dominant approaches in philosophy of science up until the 1960ies. Yet, a review of the current literature shows that since then, HD has fallen more and more out of favor. In part, this has to do with some of its immanent shortcomings, such as its over-permissiveness (Lipton 2004, 21-29). More important, however, was the shift of focus that occurred in the aftermath of Kuhn's *Structure of Scientific Revolutions* and that led to a more serious appreciation of *scientific practice* as the basis for evaluating philosophical theories about science. Many philosophers who looked at scientific practice more closely came to the conclusion that real science is too complex to be amenable to a small set of rules comprising only deduction and observation. Before I explain why this is relevant for the phenomenological acknowledgment of the communal nature of science, let me briefly discuss a case study that makes the limitations of HD more apparent (cf., for the following, Bahcall & Davis 1982; Pinch 1986).

A question that occupied the astrophysical community for almost half of the twentieth century concerned the processes that occur in the interior of the sun. What made this question challenging is that, first, the assumed processes are unobservable (they happen in the sun's core and involve subatomic particles), and that, second, the observation techniques at the time were based on the detection of electromagnetic radiation (which is produced at the surface of stars). Building on knowledge in nuclear physics, cosmology and related fields, scientists started out with proposing a tentative version of the *standard solar model*, a mathematical representation according to which the sun performs a special form of nuclear fusion (the p-p chain) in which protons are converted into alpha particles, positrons, neutrinos and energy. Of special interest for the confirmation of this model were the neutrinos, elementary particles that only interact via weak subatomic force and whose rest mass was originally thought to be zero. Since neutrinos do not participate in strong interaction, they typically do not interact with matter. This, of course, makes them ideal as a probe. But there is a downside as well: Because they normally do not interact with matter, neutrinos are exceptionally hard to detect.

The next important step was made when the physicist Raymond Davis proposed another mathematical model, this time specifying the possible experimental conditions under which solar neutrinos could be detected after all. Building on the available knowledge in particle physics and radiochemistry, the basic stipulation behind this model was that solar neutrinos should occasionally interact with ^{37}Cl, an isotope of chlorine, thus transmuting into ^{37}Ar, a stable radioactive isotope of argon. But before this theoretical idea could be put to test, two main challenges had to be faced. First, even under the assumption that solar neutrinos would in fact trigger the $^{37}Cl \rightarrow ^{37}Ar$ reaction, it was clear that the reaction would happen so infrequently that only a large target could generate a sufficient amount of data. It is for this reason that Davis used a 100,000-gallon tank of C_2Cl_4, which contains ^{37}Cl and with which the neutrinos could interact. The second challenge was even greater: Davis suspected that not only solar neutrinos could trigger the $^{37}Cl \rightarrow ^{37}Ar$ reaction, but also cosmic-ray muons. Hence, in order to make sure that only neutrino-induced $^{37}Cl \rightarrow ^{37}Ar$ reactions would be registered, the tank had to be shielded off from incoming cosmic rays. This was achieved by locating the tank 1,480 meters underground in an abandoned gold mine.

After the experiment had been set up, Davis started to gather data in the late 1960ies. Building on the basic idea to use ^{37}Ar as a surrogate for solar neutrinos, the tank was swept once a month with helium gas in order to collect the ^{37}Ar isotopes with a super-cooled charcoal trap. The ^{37}Ar would then de-excite itself via electron capture, thereby emitting Auger electrons of characteristic energy. It was these electrons that were finally detected with a Geiger counter. However, it would still be premature to think of the counter clicks as the relevant experimental data. Since some of the clicks could be produced by other sources (such as background radiation interfering with the counter), sophisticated methods of data analysis had to be employed over several experimental runs in order to distinguish data from noise on purely statistical grounds. Figure 1 shows the data collected in one such run. Counts within the window are considered data, counts outside are considered noise.

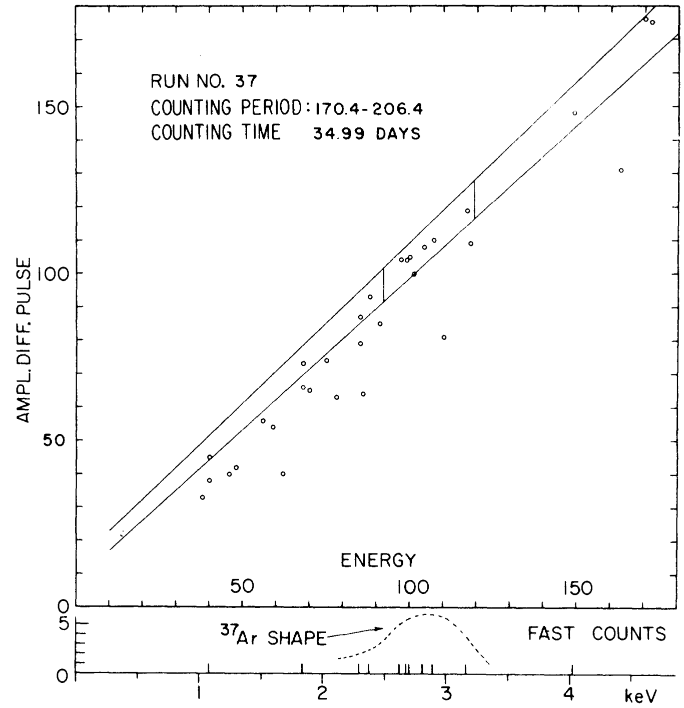

Figure 1 - Plot of Geiger counter events detected in one 35-day run (taken from Pinch 1986, 45).

Let us now, with this brief presentation of the solar neutrino case as a backdrop, return to our discussion of HD. The first thing to note is that, of course, HD is not entirely unsuccessful as a general model of scientific method. It does capture some of the essential features of science such as the fact that scientific inquiry always proceeds by proposing a hypothesis that, if true, would explain the phenomena under consideration. Furthermore, it is also correct that an equally important second step is to identify consequences of the hypothesis that could in principle be confirmed or disconfirmed by experimental data. However, beyond these rather general points, HD faces several problems for which the solar neutrino case is an excellent illustration. Take, for instance, the idea that experimental reports are the final and unambiguous arbiters of a theory's supposed truth or falsity. When a sufficient amount of data had been gathered after a number of runs, the results of the solar neutrino experiment did not turn out to be what Davis had hoped for. The problem was that, although ^{37}Ar isotopes were detected, their number was much lower than expected (between one half and two thirds). For supporters of HD, this would have been a clear disconfirmation of the initial hypothesis from which the singular-predictive statement about the expected number of ^{37}Ar isotopes was derived. In reality, however, the situation was much more complicated. The reason is, as the case study has shown, that the singular-predictive statement about the expected number of ^{37}Ar isotopes was not derived from one single theory, but rather from a conjunction of several different theories, including, among others, theories about the chemistry of chlorine, physical theories about muons or auxiliary theories about Geiger counters. Now, if an experimental report contradicts the singular-predictive statement that has been derived from the conjunction of several theories, all this shows is that at least one part of the conjunction is wrong, but not which one(s). In fact, it took the scientific community four decades to reach an agreement on which part of the conjunction had to be modified in order to reconcile the prediction with the number of detected ^{37}Ar isotopes. This illustrates one of the most profound insights in philosophy of science, namely “that the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses” (Duhem 1954, 187).

The second problem I wish to address concerns the alleged deductive nature of scientific method. According to HD, the relation between theories and predictions is one of deductive entailment. Traditionally, this assumption has been associated with a particular interpretation of the nature of scientific theories, the so-called *syntactic view*. On this view, theories are conceived of as linguistic entities or, to be more precise, as axiomatized systems of sentences, analyzable in terms of predicate logic. This fits well with HD: Roughly put, the idea is that the axioms of the system—the underived laws fundamental to the theory—allow for the deduction of general hypotheses. From these general hypotheses, singular-predictive statements are derived. And, finally, these singular-predictive statements are compared with corresponding experimental reports.

Although this basic scheme may work well when applied to toy examples like Popper’s pieces of thread, it is questionable whether it can also account for more realistic episodes such as the solar neutrino case. As I have pointed out, the first step in modern solar neutrino research was the prediction of the estimated flux of neutrinos that are emitted as a by-product of the nuclear reactions in the sun’s core. However, it is highly implausible to interpret this prediction as the result of drawing deductive inferences from general statements that are in turn entailed by a set of fundamental laws (Pinch 1985). Besides the fact that the prediction “depends on a whole morass of theories [that] are not even drawn from one single area of science” (Pinch 1985, 175), the main problem is this: A quantitative estimation of the neutrino flux at any moment can only be given on the basis of a mathematical account of the sun’s evolution over its entire lifetime. In practice, this is done by constructing a primeval model that is in accordance with what is accepted as the fundamental principles of the sun’s structure. After several input parameters have been fed into this primeval model, it is possible to calculate a sequence of several successive models that are supposed to represent the different stages of the sun’s evolution up to any given point in time. While some of the input parameters (like photon luminosity or the sun’s radius) are actually measured, others (such as the mixing-length parameter or the helium abundance) are free parameters that must be “juggled” (Pinch’s expression) in order to produce a mathematical model that matches the known data. It is also worth noting that the only way to make the mathematical computations manageable is to introduce several deliberate “distortions”, such as the idealizing assumption that the sun is made up of spherically symmetrical shells (which, of course, is known to be empirically false). The sequence of models resulting from all this is what scientists call the standard solar model. And it is from this mathematical model—and not from a statement of the “all ravens are black” variety—that the prediction of the estimated solar neutrino flux can finally be obtained (cf., for further technical details, Bahcall & Cribier 1989).

As the example of the standard solar model shows, the generation of testable predictions is no deductive exercise in the HD sense of the term. First, although fundamental theories certainly do impose restrictions on models, model construction is a much more unrestrained business, depending, for instance, on the “juggling” of free parameters. Second, virtually every mathematical model that has been used since the times of Galileo and Newton involves idealizations that are known to be empirically false. This is hard to accommodate within HD: If models in virtually all areas of science involve falsehoods and if these falsehoods are deductive consequences of the respective fundamental laws, then this implies that one or more fundamental law in every field of science is also false.

The last two problems point to what I take to be the greatest weakness of HD. In its attempt to force science into a logico-linguistic straightjacket, HD's sole focus is on theories and their observable consequences, thus effectively ignoring (or, at least, downplaying[ii]) the role of mathematical models. How grossly incomplete the resulting image is becomes apparent when looking at what the observable consequences actually were in the solar neutrino case. As a first stab, one might want to argue that the relevant consequence concerned the neutrino flux from the sun's core. However, while I do not question the importance of this consequence, it is most certainly wrong to consider it *observational* in any straightforward sense of the term. The next option would be to say that the relevant consequence concerned the number of ^{37}Ar isotopes that were treated as a substitute for solar neutrinos. But, again, since it is hard to imagine how ^{37}Ar isotopes could be brought to perceptual fulfillment, this consequence can hardly be considered *observational* either. Finally, one might want to argue that the relevant consequence concerned the number of emitted Auger electrons that were taken as a substitute for the ^{37}Ar isotopes. But since Auger electrons are no more observable than ^{37}Ar isotopes or neutrinos, this interpretation is questionable too. In conclusion, then, the best candidate for being considered the relevant *observable* consequence in the solar neutrino case is the number of statistically relevant "splodges" (Pinch's expression) on the graph in figure 1. If anything, it is these straightforwardly perceivable splodges that ultimately sealed the fate of the solar neutrino experiment.

How should we think about these results? As far as the theoretical side of their work was concerned, what kept researchers on their toes for several decades was the need for countless refinements in a long chain of mathematical models in which a series of theoretical entities were treated as surrogates for even more fundamental theoretical entities. Now, while I agree that the final prediction concerned the estimated number of splodges on a computer printout, paying exclusive attention to this observable endpoint is like reviewing a book just by looking at its cover. This, then, is what I consider to be the main problem with HD: Even if it is correct that the observable consequences are the ultimate arbiters of a theory's supposed truth or falsity, HD effectively ignores a crucial part of scientific inquiry, the extensive use of mathematical models. This is problematic because it gives rise to an inadequate image of science. However, even more important in the context of this paper, this restriction also impoverishes phenomenology in its attempt to account for the intersubjective character of scientific inquiry. It is this second aspect to which I shall now turn.

If phenomenology is interpreted along the lines of HD, the focus is on the observable consequences of theories and the experimental reports with which these consequences are compared. Hence, as has been pointed out earlier, intersubjectivity is woven into the very fabric of science, first, because reference to an open community of subjectivities is implicit in every act of perceptual objectivation and, second, because the methodological demand for the reproducibility of experimental data is but a concrete instantiation of RC. Now, while I do not deny the importance of these points, it is noteworthy that, on this view, intersubjectivity enters into scientific research only at its outermost margins, i.e. at the points where the complex compound of fundamental laws and models manifests itself observationally. Admittedly, there is nothing straightforwardly incoherent about this view. The problem is just that the restriction to the observational level leads to an unnecessary limitation to the phenomenological attempt to prove intersubjectivity as a non-contingent element of science. Husserl's mature philosophy offers the resources to appreciate the communal nature of scientific research in a much more fundamental way, and it does so by unraveling the conditions under which *abstract objects* are constituted as objectively existing. Since mathematical models such as the standard solar model are complex abstract objects, and since Husserl's analyses show that the constitution of such abstract objects necessarily involves an intersubjective dimension, this opens up yet another avenue for intersubjectivity to enter into the very fabric of science.

A common thread running through Husserl's oeuvre is the idea that phenomenology is devoted to a "clarification" of the givenness all kinds of objectivities towards which we are directed in our scientific and extra-scientific endeavors. From 1907 onwards, Husserl held the view that in order to achieve this task in a sufficiently radical fashion, phenomenological clarification must be preceded by the performance of the epoché. Only the temporary suspension of the existential beliefs underlying the natural attitude puts us in a position to recognize the subjective accomplishments that go into the constitution of objects. The analysis required to unearth these subjective accomplishments is both descriptive and regressive: One starts with a provisional characterization of the way in which objects are given to us pre-philosophically. One then seeks to describe the conditions necessary for these objects to be thus given. An intriguing example is Husserl's account of perceptual objectivation: As we have seen earlier, it is through regressive analyses that we uncover the horizonal structure of intentionality and become aware of the role intersubjectivity plays in the constitution of physical things.

Although this account is in many ways paradigmatic for his overall project, Husserl did not restrict his constitutional analyses to the realm of physical thinghood. Given his background in mathematics, it should come as no surprise that Husserl paid special attention to the constitution of abstract (or "ideal") objects such as numbers, sets, point masses or frictionless planes. Following the methodological prescriptions outlined above, the clarification of the conditions necessary to the constitution of these objects proceeds from a tentative characterization of the way in which they are given to us pre-philosophically. In particular, a promising strategy is to compare their givenness with the way physical things appear to us under normal circumstances.

A comparison between abstract objects and physical things discloses several differences: Physical things, as we normally experience them, exist in space, stand in causal connections to each other and are subject to change over time. None of these determinations are true of abstract objects: Once discovered, abstract objects remain self-identically the same, regardless of specific spatial situations, causal influences or historical contexts. This self-identical sameness leads us to another property of abstract objects, namely their exactness. While, say, the sphericity of physical things always comes in degrees, the abstract object "sphere" sets the ideal limit against which concrete instantiations of more or less spherical things can be ordered. Finally, there is also a difference in cognitive availability: Although we must, on pain of psychologism, eschew the idea that abstract objects are reducible to private cognitions, it would also be wrong to say that frictionless planes or point masses are "simply there" in the lifeworld of pre-scientific experience. Indeed, as further constitutional analyses reveal, abstract objects such as geometrical objects are quite sophisticated intellectual accomplishments whose original constitution depends on "abstraction, idealization, reflection, formalization, and other 'higher-order' cognitive activities" (Tieszen 2005, 185; Duke & Woelert 2016). What this means can be illustrated by means of a concrete example.

Although they are ubiquitous in post-Galilean physics, frictionless planes do not belong to the inventory of the empirical world. They only come into existence through a special mental operation whose point it is to generate a limiting case against which actual instances of real planes can be projected. But how does this mental operation come about? Following Husserl's argument in the *Crisis*, there are two basic preconditions for the original constitution of a frictionless plane: first, the acquaintance with real surfaces of different degrees of flatness; and, second, the acquaintance with tools that give us the "technical capacity of perfecting, e.g., the capacity to make [...] the flat flatter" (Husserl 1970, 25). Looking at a series of real surfaces with increasingly flat surfaces, one can either ponder over practical ways to push the limits of technological perfection. Or one can decide to stop bothering with questions of technological realizability and instead focus on the ideal limiting pole "towards which the particular series of perfectings tend" (Husserl 1970, 26), namely the abstract, empirical unrealizable conception of a perfectly flat plane. However, what is needed to grasp this ideal limiting case in a distinct and self-conscious manner is a "peculiar sort of mental accomplishment: idealization" (Husserl 1970, 348). Idealization, as Husserl understands the term, is the process through which the vague, imprecise and morphological concepts with which we describe real things are replaced by exact, precise and mathematical concepts. Hence, it is through a progression of similarities between concrete things and an additional act of idealization that abstract objects such as frictionless planes initially come into existence.

What the late Husserl of the *Crisis* offers is a genetic account of how abstract objects and mathematical thought originally emerged. The take-home message is that their origin cannot be understood without acknowledging, first, the role of the lifeworld, "which constantly functions as subsoil [...] for theoretical truths" (Husserl 1970, 124), and, second, the role of higher-order acts of idealization. However, this cannot be all there is to it. In order to see why, let us return to the realities of scientific practice. It seems safe to assume that every physics student who has completed her first mechanics class not only knows what frictionless planes are, but also how they are used to solve traditional textbook exercises involving rolling trucks and sliding skiers. But, at the same time, it is highly unlikely that even a single one of these students has ever reflected on tools and progressions of increasingly flat surfaces, let alone on higher-order acts of idealization that, as Husserl claims, are necessary to the constitution of frictionless planes. As blunt as this argument may appear, it nevertheless seems to cast phenomenology in a negative light: How can lifeworld experiences and acts of idealization be necessary for the constitution of abstract objects, if these conditions are plainly irrelevant to the subjects who successfully use frictionless planes as part of their daily trade?

In order to solve this apparent problem, it is important to realize that the original constitution of abstract objects, which indeed depends on lifeworld experiences and acts of idealization, is not yet sufficient for the "ideal objectivity" (Husserl 1970, 356) that we normally attribute to abstract objects. Following Husserl's analysis, this kind of objectivity is only attained if the meaning of abstract objects is consolidated and stabilized by detaching it from the intellectual accomplishments of singular subjects. Husserl calls the process through which such a consolidation is achieved *sedimentation*. Essential to this process is the externalization of original, intuitive thought by means of formal notations: Once abstract objects have been constituted in intuitive acts of idealization, these objects can be "liberated from all intuited actuality" (Husserl 1970, 44) through further acts of formalizing abstraction. One of the historical examples Husserl gives for this process is the algebraization of geometry (Husserl 1970, 43-48). Considering, for instance, the proportional geometry that operates at the heart of Galilean mechanics, it is clear that the concepts used by Galileo retain their reference to the material contexts that originally gave meaning to them. This is particularly obvious in the case of Galileo's graphical representations of levers, weights or planes: Although the referents of these representations are without doubt abstract objects, the symbols used by Galileo are easily recognizable as idealizations of sensible shapes that can be found in the lifeworld of pre-mathematical experience (Wiltsche 2016). It is exactly this intuitive connection between geometric symbols and the underlying sensible shapes that is undermined when the materially determined concepts of proportional geometry are replaced with purely formal algebraic expressions. Innovations such as the Cartesian coordinate system allow for the direct translation of complex geometrical properties into the formal language of algebra. As a consequence, complex geometrical problems can be solved by means of materially undetermined algebraic equations.

The processes of sedimentation and formalization are important for two reasons. First, once a field such as geometry is formalized, it can become a "calculating technique" in which strings of symbols are manipulated "according to technical rules" (Husserl 1970, 46) and without regard for the content to which these symbols correspond. This means not only that it becomes possible to solve geometrical problems without making the effort to repeat the intuitive acts that were necessary for the original constitution of geometrical objects. It also means that one can solve equations in an almost game-like fashion, i.e. without even asking for what the purely formal symbols stand for or how they were bestowed wit meaning in the first place. For the development of modern mathematized science, this "technization of formal-mathematical thinking" (Husserl 1970, 48) is a blessing as well as a curse. Returning to the earlier example of physics students, it is a blessing because science would be practically impossible if novices were under the constant pressure to think everything from anew. However, as Husserl repeatedly stresses in the *Crisis*, formalization is also a curse because it harbors the danger of a dangerous forgetfulness with regard to science's roots in the lifeworld of pre-theoretical experience.

Second, and even more importantly in the context of this paper, the processes of sedimentation and formalization are crucial for the full constitution of abstract objects. Although, as we have seen, the original constitution of abstract objects necessarily depends on lifeworld experiences and acts of idealization, these conditions are not yet sufficient for the "ideal objectivity" that we normally attribute to these objects. The reason is simple: As long as they are tied to the intellectual accomplishments of singular subjects, abstract objects remain "within the personal sphere of consciousness" (Husserl 1970, 356) and therefore lack the property of "being there for everyone". Hence, in order to constitute abstract objects as objective in the fullest sense of the term, they must be externalized in a way that makes possible their "sensible embodiment" (Husserl 1970, 26) through a collectively shared system of signs. It is this linguistically mediated, essentially communal process of meaning consolidation that finally allows abstract objects to have "*persistent existence* [...] even during periods in which the inventor and his fellows are no longer wakefully so related or even are no longer alive" (Husserl 1970, 360).

I now come to my concluding remarks. The aim of this paper was twofold: First, I have argued that intersubjectivity—far from being the death knell of phenomenology—plays an important role in many areas of Husserl's thinking. This is true in particular of the phenomenological interpretation of the positive sciences where intersubjectivity turns out to be a necessary condition of scientific inquiry on several levels. Following the argument developed above, science is not only contingently communal, for instance, because of its complexity and the resulting need for division of cognitive labor. Rather, the intersubjective character of science is grounded in the very architecture of subjectivity, which, as Husserl realized, "is what it is [...] only within *intersubjectivity*" (Husserl 1970, 172, my emphasis).

The second aim of this paper was to critically discuss some of the specifics of Husserl's philosophy of science. Taking a cue from his early remarks about scientific method, I have considered the view according to which Husserl was as an early supporter of HD. On this reading, intersubjectivity enters into scientific inquiry at the observational level, namely through the methodological demand for intersubjective reproducibility of experimental data and through the intersubjective nature of perceptual objectivation. However, as the discussion of the solar neutrino example has shown, one of the main problems of HD is that it ignores a crucial aspect of scientific activity, the immense effort that goes into the development of mathematical models. This is problematic not only because it yields a grossly incomplete image of science, but also because it impoverishes phenomenology in its attempt to account for the intersubjective character of scientific inquiry. Husserl's late philosophy offers the resources to appreciate intersubjectivity as a necessary condition of the constitution of abstract objects. And since mathematical objects are complex abstract objects, this shows that the intersubjective saturation of science is not restricted to the observational level, but also extends to the level of model construction. Hence, the final take home message is that, phenomenologically construed, science is intersubjective all the way down, and necessarily so.

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[i] In a nutshell, the problem is that both these interpretations are *descriptively* wrong. If I perceive a physical thing, its currently concealed sides are neither co-intended as something that has been experienced in the past or as something that will be experienced in the future, nor as something that is just experienceable in principle. Rather, the absent aspects are experienced as something that is no less actual than the aspects that are currently in my visual field. Of course, further experiences can always override the anticipations I might have concerning currently concealed sides of the perceived thing. But this does not change the descriptive fact they are co-intended as belonging to the perceived thing in its full actuality.

[ii] Of course, proponents of HD were aware of the fact that models are an integral part of scientific practice. However, a common strategy was to downplay their epistemic significance by claiming that "a model has no more than an aesthetic or didactic or at best a heuristic value" (Carnap 1969, 210).