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INDETERMINACIES BY POLANYI

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It is a basic and recurrent theme in Polanyi's texts that justification of knowledge essentially involves personal judgments. "[T]acit operations play a decisive role … in the very holding of scientific knowledge." (Polanyi 1969: 105) This presupposes that observable facts cannot justify or falsify our theories solely on the basis of the rules of rationality. The acceptability of theories – from the simplest to the most complex ones – is *not determined* by logic and observation. This claim is essential for Polanyi because it is to make room for personal judgment. It may also bring Quine's indeterminacy theses (Quine 1990a) in one's mind – and this association is not completely unfounded. As, for example, if the acceptability of a theory is not determined by the rules of rationality and observations, then there is probably more then one theory that is acceptable in the light of logic and observations. In this paper I will reconstruct and analyze Polanyi's indeterminacy theses and the arguments invoked to support them. Finally Polanyi's resolution for the indeterminacies will be considered.

1. Empirical underdetermination

Polanyi is not interested in the epistemological problem of underdetermination as such. Rather, he attacks the idea of the empirically-methodologically grounded science in order to reveal an essential gap in its foundations where tacit knowing comes in. "The avowed purpose of the exact sciences is to establish complete intellectual control over experience in terms of precise rules which can be formally set out and empirically tested." (Polanyi 1958: 18) "It is thought that in science facts alone count" (Polanyi 1947/97: 216) He has a twofold challenge against this orthodox picture. First he argues that observable data do not determine the theoretical relations accounting for them. The second argument derives from the truism that measured values never exactly match the calculated ones.

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1.1. Empirical data do not determine their theoretical relations

Polanyi proceeds from a mathematical model. Suppose we have two measurable parameters x, and y, and having measured them on various occasions the set of pairs of measured values ($a_1 b_1, a_2 b_2, \ldots a_n b_n$) was the result.

> Can we decide form a series of points … whether there is a function [$y= f(x)$] and if so what it is? Clearly we can do nothing of the kind. Any set of pairs of [x] and [y] variables is *compatible with an infinite number of functional relations* between which there is nothing to choose from the point of view of the underlying data. To choose any of the infinite possible functions and give it the distinction of a scientific proposition is so far without any justification." "[S]uccessfull prediction … only adds a number of observations, the predicted observations, to our series of measurements and cannot change the fact that any series of measurements is incapable of defining a function between measured variables. (Polanyi 1947/97: 216, italics added and the mathematical notations are slightly modified.)[2]

The thesis is that several functions can describe the same set of data and that underdetermination cannot be eliminated by increasing the amount of empirical data. Theoretical functions are underdetermined by all possible data. Polanyi's example intuitively supports this vividly. Imagine dots representing the measured values in a Cartesian coordinate system. It is always possible to draw more then one curves – actually infinitely many curves – connecting these points. A more solid theoretical support can be given this thesis by means of the mathematical theory approximation. Both the intuitive picture and the mathematical background entail that the different functions describing the same set of data are logically incompatible. (That is, they cannot be transformed into each other under sufficiently strong conditions for the transformation.) Thus Polanyi's thesis of the underdetermination of theories reads: infinitely many and logically mutually incompatible theoretical functions can describe the same set of observation data, these functions are underdetermined by all possible data.

This is exactly Quine's celebrated thesis of underdetermination (Quine 1975, 1990a, 1990b) – restricted to quantitative theories only. The more general statement of this thesis says that logically incompatible and mutually intranslatable[3] but equally

[2] A less detailed version of this argument can also be found in Polanyi 1946/64: 21. It is worth noting the date of the original publications and compare them to that of Quine's *Two Dogmas.* (Quine 1953/63) (The first version of the *Two Dogmas* was read at a conference in Toronto in December 1950. Quine 1953/63: 169)

[3] Such theories cannot be transformed into each other, that is, their difference is not merely terminological. (It is not true for any two theories that they are such that they have all and only common set-theoretical models.)

adequate comprehensive theories of the world are possible, or in other words there are empirically equivalent systems of the world. (Quine 1975)

Underdetermination emerges, as Quine points out, because theoretical claims go beyond observations.[4] They talk about entities and connections that are not empirically accessible. As a consequence of this the logical relationship is asymmetrical between theories and observations. Theories imply observation sentences[5] but not on the other way round. Observation sentences or empirical data do not imply the theoretical claims that establish their connections. Observation sentences and logic do not determine empirical theories, hence the underdetermination.

However an interesting qualification is needed here. Only theories with infinite observational consequences generate epistemologically interesting underdetermination. For if a theory applies to a finite set of observation sentences then the theory can be given a finite formulation in terms of the conjunct of these observation sentences. This theory formulation is uniquely determined by the observation sentences, leaving no slack for underdetermination.[6]

It should be noted that the thesis of underdetermination claims the existence of rival and incompatible theories and not that *any* theory can be devised for a class of observation sentences. Because one-way logical relationship must hold for an empirically adequate theory: the observation sentences should be its logical consequences.

Critics of this thesis accuse its proponents of being misled by the intuitive picture of different curves connecting the same set of dots. Sure, if only a *finite* number of dots are available to represent the observable values then infinitely many different curves can connect them. But the representation of all possible data would be the points representing the measurable value for all points of the *x*-axis. In this case, however, there is only one continuous curve connecting all the points and it is precisely the one constituted by the infinite set of points itself. Consider an *analogue* measuring instrument, for example, a thermograph registering the temperature

[4] The observable-theoretical distinction is highly problematic for a fundationalist project. But we are not engaged in such a project. So we may be content with some pragmatic demarcation: something is observable/measurable if scientists claim that they can observe/measure it. N.B.: Polanyi's (and Quine's) semantic holism even excludes the possibility of the theoretical separation of the two categories.

[5] In fact, no theory by itself can imply observation sentences. The one above is just shorthand for the correct statement that a theory together with observable initial conditions implies other observation sentences. Or as Quine (1990a, 1990b) elegantly points it: theories can imply very special kinds of observation sentences, namely observation categoricals that formulate empirical coincidences in the form: "When this, then that".

[6] Even those theories can be taken uniquely determined that have infinitely many observational consequences but that are simply the quantification of the observation categoricals. A theory of this kind consists of simple empirical generalizations. ($\forall x(Fx \supset Gx)$, where *Fa* and *Ga* are observation sentences.) (Quine 1975)

of its spatial vicinity in time. If this thermograph operated from the beginning of the Universe till the end of it, then it would produce a single "theory", that is, a single function of temperature in time without any underdetermination. Critics may be right at that. The thesis needs further qualification. Nevertheless the case in point cannot eliminate all instances of underdetermination. For most of the scientifically interesting theoretical connections – if not all of them – can be tested by the observation of discrete events, and, in such cases, there is no chance – in principle – to perform continuous measurement. (E.g. particle collisions.) The phenomena that can be studied in principle only by measuring discrete values is subject to underdetermination without further qualification. Or, in other words, underdetermination is not the result of shortage of data in such cases, but rather inherent in the logical relationship between theory and measurable data.

The thermograph example may be misleading also in a second sense. The problem to be illuminated by the example is *not* the problem of induction. The point is not that the thermograph can *produce* a "theory" connecting the measurable values of temperature and time. It is rather that no different alternative functions can be proposed to account for the measured data equally well because there is one and only one going through all the measured points, and it accounts for the measured data better then any other conceivable. This is true even if this curve probably does not succumb to symbolic formulation in terms of a *mathematical expression*, $y=f(x)$. Nonetheless the function as a mapping of the values (points) of x-axis into the values (points) of y-axis is defined by the curve drawn by the thermograph in a mathematically faultless way. However, it would be hard to use a function defined graphically by its diagram in science, and it would be absurd to consider it as part of a theory. A theory should admit linguistic (symbolic) formulation, (No matter how the notion of a theory is defined, a syntactic or a semantic view is adopted). So the curve on the paper should be described by a mathematical formula and this takes us back to the mathematical theory of approximation and the underdetermination reoccurs, because the curve drawn by the thermograph can be approximated equally well by different and logically incompatible functions (e.g. by Taylor or Fourier series.) Thus the amended thesis sounds like this: any theory admitting linguistic (in particular mathematical) formulation is underdetermined by all possible observable data.

1.2. No exact match between measured and calculated values

The relation between theories and observations are even more complicated than that. The underdetermination of theories by all possible empirical data is about the *logical relation between* theoretical and observation *sentences*, that is, between the values of the observable parameters and the possible theories including those observable parameters and supplying their values. Different and contradicting theories can include exactly the same set of observable parameters assuming exactly the same values in each of the theories. But how do we observe the values? How are the "formulae" applied to "the facts of experience"? (Polanyi 1958: 18-19) Instead of discussing the logical relations between theoretical and empirical *sentences*, Polanyi invites us to investigate the empirical input itself. The observable data are always supplied by measuring instruments in quantitative theories, and the theoretically calculated value of a measurable quantity never exactly matches the actual reading of the instrument. (Polanyi 1958: 19) Therefore observations can neither automatically falsify nor confirm a theory. There is no epistemic rule to tell us what sort of difference should be enough to repudiate the theory.[7] It is a personal judgment, says Polanyi, that decides whether the observations are to be interpreted as confirming or falsifying the theory. The difference between the observed and the calculated values are interpreted as measurement error in the first case and as systematic error in the second. The conclusion of Polanyi's argument is that logic and measurements (observations) can not judge the empirical adequacy of a theory. "In consequence of such random errors we can only proceed from the probable values of initial data to probable values of predicted magnitudes, and since no strict relationship exists between these two sets of figures, the process remains to this extent indeterminate." (Polanyi 1958: 19)

But how would this lead to underdetermination? My suggestion is this. If there is no exact match between the calculated and the measured values of observable quantities then different sets of calculated values of observable quantities can produce similarly acceptable approximations to a given set of measured data; where the different sets of calculated values are supplied by different and logically incompatible theories. In other words, *empirically different theories* can be devised to account for – that is, to approximate – the same set of observable data *equally well*. This thesis may be dubbed, for convenience, the underdetermination of theories by measured data.

According to what sort of standard would the empirically different theories perform "equally well"? Since there is no single universal rule for approximation, different conditions may be set for the rival theories. Reasonable conditions could be, for example, that the maximum or the average of the difference between the calculated and the measured values be less then a given value.

Let us have a closer look at this thesis because certain provisos need to be made here. This kind of underdetermination does not emerge if we have a qualitative theory predicting only qualitatively sufficiently different observations. Neither does it emerge, if a quantitative theory supplies observable quantities with discrete values separated by intervals that are out of the range of measuring error. For instance, precision is not a problem for IT (digital) measurement. If there is no signal then the

[7] Obviously he allows that the established practice of scientists would lead us to treat certain observations contradictory – but not the theory and the logic of falsification.

potentiometer says something around 0V, if there is, then it points to somewhere around 5 V mark. Even if the potentiometer never says exactly 0 or 5 V, there is no slack to be interpreted between the reading and the predicted value of the theory, because the theory predicts signal or the lack of it. Thus the underdetermination by measured data is less general then the former kind of underdetermination. It applies only if the observable quantities of a theory are finer grained then the range of the maximal measuring error.

However this is not a serious theoretical limitation because if our total theory of the world has at least one continuous (or fine grained) observable parameter then it is subject to the underdetermination of this kind.

We should clearly see the scale of the underdetermination if data is available in abundance. Suppose we have two continuous observable quantity, and the theory predicts that when $x = 3$, then $y = 2$. Testing the theory we set the value of the system's *x*-parameter to be 3 by using an *x*-gauge pointing at 3 as precisely as possible and then we read the y-gauge. The *y*-gauge will never say exactly 2. But as the measurement is repeated the values read on the *y*-gauge will scatter around a mean. (Provided that the variance is indeed due to measuring error.) The less the standard deviation is, the sharper the mean comes out. If the theory is good then one possible candidate of the mean will be 2. Of course the scattered values read on the *y*-gauge will never tell exactly around what theoretical mean value they are scattered around. (Because the same problem reoccurs with the definition of the probability function: no set of measured value will fit exactly one and only one Gauss function.) But the less the standard deviation (variance) is, the smaller the range is from which alternative theoretically predicted values for *y* can be chosen. For any such alternative theoretically predicted value should also be a possible candidate for the mean of the standard deviation. If it is possible to make repeated measurements for every measurable (a_i, b_i) pairs, then empirically only insignificantly different $y = f(\ldots x \ldots)$ functions will approximate the set of data equally well. Repeated measurements of the same observable quantities can thus substantially reduce the rationally acceptable alternative theories (functions) predicting different values for the same parameter. Mathematics, however, cannot fully eliminate the underdetermination but can only reduce it.[8]

However theories including functions with certain kind of instability or singularity around $x = 3$ will resist the narrowing-down of the range of the possible theoretically predicted values by repeated measurements. Say, for example, the function $y = 1/|x-3|$ will produce enormous scattering on the *y*-gauge while we try to set system in a way that the *x*-gauge points to 3. Therefore the scattering and the variance of the measured values of *y* will not assist the choice among the possible functions. But these considerations are relevant to our problem just the other way round. Obviously, we are not concerned with the problem of how to guess the appropriate

8 Polanyi is fully aware of the role of statistical analysis (See, e.g., Polanyi 1969a: 107-8).

function from measured values in a single point. (It is an absurd problem anyway.) Our problem is how to choose one from the functions supplying different *y*s for some *x*s. We have a hypothesis about the candidates in accordance with scattered measured values of *y* in *various x*s. If the measurements of *y* in $x = 3$ show enormous scattering compared to the measurement of y in other *x*s, then it will tell us that some of the functions (e.g., the function $y = 2/3x$) are much less likely candidates to account for the measured values.

The second kind of underdetermination is a matter of degree while the first is not. Being a logical relationship between observation sentences and a theory, the first kind of underdetermination either holds or not. In the case second kind, however, it may be dubious at times whether what we see is within the measuring error or not. But on other occasions it may be clear that the difference between what is observed and what was predicted by the theory is clearly significant, and the result calls for serious consideration.

Up until now we have supposed the abundance of measured data. What if we have only limited access to measured values because of the nature of the available experimental setup? If to keep all but one of the measurable values fixed is not possible because of the nature of the system measured, then no scattering around a particular theoretically calculated value can arise. This is the case in astronomical observations, one of Polanyi's favorite examples,. We cannot fix time in order to measure the spatial coordinates of a planet repeatedly at a particular time coordinate to narrow down the acceptable theoretical predictions for the position of the planet. Both time coordinate and the spatial location of the planet are changing. Only one measured spatial position is available for each value of *t*. (Obviously we can measure the position of a planet by different instruments at the same time. But in such cases the interpretation of the scattering gets even more difficult.) We know that the measured space-time coordinates approximate the calculated values within measuring errors, but having no exact rule as to *how* the calculated values are approximated by the measured ones. Therefore it is possible to approximate the measured values by empirically (and of course also theoretically) different functions.

The access to data is limited theoretically in another way, namely, there is no access to data within measuring error, and sometimes measuring error in principle cannot be reduced under certain values. As a result of this we have no chance to decide by measurement whether space-time is continuous or only dense. (That is whether the cardinality of space-time points is like that of real or only that of rational numbers.) Because our measuring instruments can produce only rational numbers as they are based on ratios. (Newton-Smith 1978) Despite the fact that measured values can in principle be represented already by a theory about a dense space-time, it is generally supposed that space-time is continuous.

To summarize, the underdetermination of theories by measured data springs from measurement errors, and claims that *empirically different theories* can be devised to approximate the same set of measured data *equally well*. This underdetermination plays a decisive role if we have limited access to data or if we theorize about quantities beyond measuring error. Qualitative theories are not vulnerable to this kind of underdetermination. If a quantity can be measured repeatedly on the same instrument while all the other parameters are kept fixed, then the underdetermination of this kind can be substantially reduced. As in this case the variance of the measured value can seriously limit the rationally acceptable theoretical functions.

2. The indeterminacy of the extensional semantic values

The discussion above supposed that the meaning of the terms and sentences applied in a scientific theory is well defined and raises no problem concerning the application of the theory. However, Polanyi thinks that the definition of the semantic values of the terms and sentences are undermined by mechanisms similar to the ones discussed above. "[T]he process of applying language to things is also necessarily unformalized: that it is inarticulate. Denotation, then, is an art, and whatever we say about things assumes our endorsement of our own skill in practising this art. This personal coefficient of all affirmations inherent in the use of language…" (Polanyi 1958: 81)

Three arguments can be reconstructed from Polanyi's texts to support the indeterminacy of *extensional* semantic values, that is the reference (denotation) of terms and the truth values of sentences.

The first one says that there are no explicit rules to determine how language refers to the objects of the external world. For it is impossible to state linguistically – that is explicitly – how to apply language to what it refers to, because either any such rule would presuppose itself or its application would require further explanation and so on ad infinitum. He refers to Kant (*Critique of Pure Reason*, A.133.) that "no system of rules can prescribe the procedure by which the rules themselves are to be applied". (Polanyi 1969: 103)

This argument, however widespread it is, fails to make his case. There is indeed a category difference between a rule and its application, but as Wittgenstein convincingly points out (e.g., Wittgenstein 1958 §§201-219.) there exists no gap between the two to be bridged by the rules of application. To understand a rule *is* to know how to apply it, to know what counts as following or violating it. For rules are our standards of correctness. Thus if we have rules, then we have their application and there remains no indeterminacy here: they do determine the correct use of language, including the correct application of predicates and sentences.

The second argument can be stated like this. Even if this infinite regress is set apart, there remains the problem of the variety of the reference of a predicate unmanageable by definitions and rules. Because "*in applying our conception of a class of things we keep identifying objects that are different form one another in every particular*" (Polanyi and Prosch 1975: 51) (Italics is original) Therefore Polanyi seems to conclude that "[t]here is an ultimate agency which, unfettered by any explicit rules, decides on the subsumption of a particular instance under any general rule or a general concept." (Polanyi 1969: 103) And "striving to eliminate the indeterminacy involved in subsuming a presumed instance under that class" seems to have been misguided. (Polanyi and Prosch 1975: 52)

According to this argument the lack of rules, explicit or implicit, springs from the vagueness generally inherent in language. Not necessarily all concepts are vague in a language, but most empirical concepts allow of borderline cases, and no rule can define the referential content of such concepts without vagueness, in terms of necessary and sufficient conditions. Borderline cases require a decision whether to be included in the reference class of the predicate or not. Certainly not *all* application of a vague predicate is like this. Even a predicate with vague borderline cases generally has also clear-cut cases of application. Thus the indeterminacy of referents is restricted only to the borderline cases. This implies that the truth value of some sentences may also vary according to whether the borderline cases are included in the reference class of the predicate or not. Such indeterminacy often emerges in recording the reading of an instrument. For example, if the pointer of an analogue a-gauge points close to 3, should we write "$a = 3$" in the report of the experiment or should we refuse to identify the value of a with 3, and try to determine more exactly the position of the pointer between 3 and 3.1?

Polanyi's third argument for the indeterminacy of extensional semantic values rests on his holism. He uses the analogy of a text to illuminate how linguistic units are interrelated and how they are related to what they refer to.

> Three things will have to be born in mind: the *text*, the *conception* suggested by it, and the *experience* on which they may bear. Our judgment operates by trying to adjust these three to each other." "Thus to speak a language is to commit ourselves to the double indeterminacy due to our reliance both on its formalism and on our own continued reconsideration of this formalism in its bearing on experience. (Polanyi 1958: 95)

Thus it is always possible to reinterpret our language in different ways in the light of new evidence. Polanyi ventures the bold claim that all observation is subject to this kind of reinterpretation.

> Since the world .... never exactly repeats any previous situation ... we can achieve

> consistency [of repeated use of terms and sentences] only by identifying manifestly different situations in respect to some particular feature.
> First, we must decide what variations of our experience are irrelevant to the identification of this recurrent feature, as forming no part of it, i.e. we must discriminate against its random background. Secondly, we must decide what variations should be accepted as normal changes in the appearance of this identifiable feature, or should be taken, on the contrary, to discredit this feature altogether as a recurrent element of experience. (Polanyi 1958: 79-80)

These two decisions may be easy to make in some cases of observation by virtue of the rules of the language-game called "observation" leaving little indeterminacy behind. But on other occasions the established practice does not assist us much, and leaves substantial indeterminacy behind, for instance when we are faced with unaccepted observations.

Semantic holism not only supports the indeterminacy of semantic values for Polanyi, but it also has an interesting bearing upon epistemic holism. He writes:

> Any contradiction between a particular scientific notion and the facts of experience will be explained by other scientific notions; there is a ready reserve of possible scientific hypotheses available to explain any conceivable event. Secured by its circularity and defended further by its epicyclical reserves, science may deny, or at least cast aside as of no scientific interest, whole ranges of experience which to the unscientific mind appear both massive and vital. (Polanyi 1958:292)

Thus Polanyi evidently combines semantic holism with Duhemian epistemological holism. (Duhem 1906/54, Ch. VI. and also Quine 1953/63) Epistemological holism is the tenet that scientific statements are not separately vulnerable to adverse observations, but only jointly as a theory. Again this is underlain by the logical relation between theoretical claims and observation sentences. No single hypothesis can imply observation sentences, but only a conjunction of them. Therefore if an observation sentence proves to be false, then at least one of the premises that entailed it, must be false. But it is not determined by logic and evidence which of them is to be blamed: the hypothesis tested or some other premises employed in the inference. Which one to take to be false is exactly the matter of how the truth values are distributed over the sentences, and thus it comes down to the indeterminacy of the extensional semantic value of sentences.

In sum, Polanyi thinks that the description of what is observed always involves indeterminacy. There is no single determinate way how to apply our words to what they refer and it is not determinate in the light of observations whether an observation sentence is true or false.

3. The relationship between indeterminacies

A three-storeyed system is unfolding from Polanyi's texts.

1. It is not determined without ambiguity how to put observations into words. Different and mutually inconsistent observation sentences may be held true about a particular observation or measurement. (indeterminacy of extensional semantic values)
2. Even if we have settled for some observation sentences as describing accurately what we have seen, these observation sentences will not exactly match the observation sentences derived from a theory – at least, as far as quantitative theories are concerned. Hence *empirically different* and mutually incompatible theories can approximate the same set of measured *data* equally well. The theoretically derived observation sentences are underdetermined by measured data. (underdetermination of theories by measured data)
3. Even if the observation sentences to be derived from a theory were specified still more, and logically incompatible theories could supply the same set of observation sentences. (underdetermination of theories by all possible observable data.) There are incompatible but *empirically equivalent* theories.

The underdetermination by all possible observable data (3.) supposes the one-way logical relationship that a theory has observation sentences as logical consequences *and thereby* a theory predicts certain *determinate empirical experiences*. That is what Polanyi's other two theses dispute. Because the relation between logically implied observation sentences and actual observations is "underdetermined" or rather is not determined. It is not determined whether what I see bears out the prediction of the theory or it does not. This dilemma springs from two separate factors. The first reason is that no measured value of a parameter is exactly equal to the theoretically predicted one (2.). The second factor is the indeterminacy of the meaning of observation sentences (1.). Therefore, as a consequence of these two factors, you always have to decide whether observed value bears out or challenges the theory. The first and the second thesis make it possible to accept *empirically different* theories that are equally adequate to the actual observations.

Each of the theses above generates a new slack in the decision procedure about the rationally acceptable theories by multiplying the reasonable theoretical options as we try to select a theory empirically adequate to the sense experience we have.

These three theses are logically independent of one another, each may arise without the other two. They also supplement each other. The underdetermination generated by one comes on the top of the underdetermination generated by the others. Eventually they cover the entire logical relationship between the sensory input and the theoretical output.

Arguing against empiricists and critical rationalists (and probably against all foundationalists) Polanyi tries to show what sort of gaps are in the flow of information from the sensory input, from a phenomenon to the theoretical account of the same phenomenon.

4. Resolving indeterminacies

Polanyi does not claim that the theoretical alternatives are decisive, or interesting in all cases. The point is not that we would have completely different picture of the world, if we accounted for the experience we actually have by some other palatable theory. Certainly, it may be the case too, but this is not Polanyi's problem. Rather his concern is the very existence of the gaps in the theory-choice that goes unnoticed in the practice of science because they are bridged by the personal coefficient of knowing:

> It is the principle that matters; and in fact the slight gap between theory and instrument readings turns out to be thin only in the way the edge of a wedge is thin – a wedge that will be thick enough at its base to completely separate 'knowledge' from 'detached objectivity'. Personal, tacit assessments and evaluations ... are required at every step in the acquisition of knowledge – even 'scientific' knowledge. (Polanyi and Prosch 1975: 31)

Personal contribution fixes what is left undetermined by logic and experience. In order to see how knowledge gets determined, let us have a closer look at Polanyi's conception of knowing.

Knowing is understood on the analogy of the pattern recognition of Gestalt psychology. A pattern that is to be recognized, acquired, known or understood – e.g. a face, some skill, regularity in nature, etc. – is more than the sum of its parts. The parts are integrated into a holistic form. The parts of a recognized whole possess meaning only in their contribution to the form, that is, they are subsidiary components of the whole. When focusing our attention on a whole, we are only subsidiarily aware of its parts. Of course, it is possible to switch the focus of our attention to a particular part, but this also changes its semantic and cognitive status. It is not attended as a subsidiary component of the former whole any longer, but as an independent whole. According to Polanyi, this structure characterizes all kinds of our cognitive efforts including both propositional and nonpropositional knowledge (knowing that and knowing how) (Polanyi 1958: 56) The selection of the relevant subsidiary components and their integration are the constituents of tacit knowledge and they determine all of our knowledge, including our theories as well. Polanyi mentions several subsidiaries influencing the theory-choice: our tacit knowledge of our body, the accepted scientific tradition, the research skills acquired in our apprenticeship etc. They are the main factors in general to determine the theory-choice.

But how they do their job? What exactly determines the theory on a particular moment of decision, and how it does? These questions cannot be answered because subsidiaries and their integration are *logically unspecifiable*. (Polanyi 1958: 56-57) Knowledge represented by the focal whole, is the result of two interrelated components: the subsidiaries and their integration. They are subsidiaries and integration *only* with respect to the focal whole. But the focal whole alone cannot determine these two interrelated components for there are many possible combinations of these two factors to construct the same focal whole. It is possible to counterbalance the modifications of the available subsidiaries by the appropriate modifications in the integration process and *vice versa*. Metaphorically, the same stimuli, information, data, situation etc. (the "same" subsidiaries) can be integrated into different focal wholes, and different stimuli, information, data, situations, etc. ("different" subsidiaries) can be integrated into the same focal whole. The famous ambiguous pictures (Rubin vase, Leeper's ambiguous lady, etc.) may serve as an example for the first case and the recognition of a face under different circumstances, for the second. As it was pointed out earlier it is possible to focus on a given particular that was formerly a subsidiary in a context, but in a focal position it is already a different cognitive object. As the structure and the function changes also the meaning of the particular changes. "Subsidiary awareness and focal awareness are mutually exclusive … Our attention can hold only one focus at a time and … it would hence be self-contradictory to be both subsidiarily and focally aware of the same particulars at the same time." (Polanyi 1958: 56-57)

Even granting all this to Polanyi we may conclude that the decisive factors and the mechanism of the decision for a particular theory are unspecifiable, that is we do not know them. But despite of our ignorance they are *ontologically* determinate and they are determined by our biological structure, experiences, upbringing, social circumstances etc. This assumption, however, runs contrary to Polanyi's ontology suggesting the hierarchical-holistic structure of reality. He thinks that logical unspecifiability is an ontological notion. It is not only a claim about what we can know, but also a claim about the structure of the world.

The hierarchical-holistic ontological structure applies to reality in general and, thus, to knowing man in particular. First, according to the emergent holism, a whole possesses properties and structures that are absent from the constituting parts. For instance, what a machine is, cannot be defined in terms of its parts, but only in terms of its structure functioning as a whole. Therefore a machine is ontologically different from, and not determined by its parts. Secondly, according to type emergence, a machine is not only a different entity, but it is also ontologically different in kind. While the properties of its material are governed and explained by the laws of physics and chemistry, the machine itself cannot be understood by virtue of these kinds of laws. We need a structural-functional description to define what a particular kind of machine is. In such cases, a new *type* of entity emerges. The emergent type of entity is not determined by the constituting entities neither by their laws.[9] But these lower level laws are satisfied by the emergent entity, they serve as boundary (necessary) conditions for it. (See, e.g., Polanyi 1958, Part IV, and Polanyi 1966, Ch 2.). The higher level laws determine the functioning of the emergent entity within the playground left open by lower level laws. Reality is regimented by a multi-layered type-hierarchy beside the part-whole hierarchy and this structure of emergence characterizes knowing as well. A knowing human being is itself an emergent type. As knowing persons, we are determined by the emergent structure of knowing that is governed by the (Polanyian) principles of personal knowing. The laws of physics, biology and the values of our culture stake out the boundary conditions for our functioning as knowing being, but they fail to determine our knowing. This is the ontological basis for the logical unspecifiability of the subsidiaries and their integration. [10] Our beliefs, skills and actions are not fully determined by the deterministic structure of the physical, biological or even social reality.

Well, then what determines our theories? They are clearly not indeterminate as they appear in science, therefore, something must determine them. It is the person as an integrated, *irreducible,* emergent whole who makes the theory-choice by accepting it. It is not a capricious decision however, because the person integrates not only her mind and body but also her professional and cultural tradition, and she is guided by her intellectual, social and cultural commitments. While neither the person herself nor her decision is determined by all these factors. This freedom saturates all acts of knowing with responsibility.

[9] Polanyi's notion of emergence is different from its contemporary use. For, if his key example is taken seriously, then a machine can be realized by various physical structures and a physical structure may embody various machines according to the means-end context in which they are used or functioning. For example, a screwdriver may function as a chisel and *vice versa*. Therefore it is not a kind of supervenience.

[10] It is clear that Polanyi's motivation is the other way round. "Our theory of knowledge is now seen to imply an ontology of the mind. To accept the indeterminacy of knowledge requires …that we accredit a person entitled to shape his knowing according to his own judgment, unspecifiably. … This ontology – which flows from my theory of knowledge – will be outlined further in Part Four." (Polanyi 1958: 264) But the direction of Polanyi's line of thought does not affect the claim that the unspecifiability is rooted in the ontology of reality rather than in our ignorance.

5. References

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