Robustness, Reality and The Transcendental
: Open Letters to Kerstin Knight

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1. Introduction

On Monday August 3 2009, I gave a talk 'When Did Electron Come into Existence?' at the University of Melbourne. It was about my stance of scientific realism; *activity realism*, and a modified version of my paper written in Japanese (Deguchi 2009a). Among audience was Kerstin Knight. Next day she sent questions to me via my Melbourne host, Graham Priest, and I replied to them. That was the beginning of our exchange of philosophical letters, culminating in, on her side, her paper in this volume.

My replies are basically relied on my previous papers (Deguchi 2008, 2009a, 2009b). But, in responding to her, I occasionally got on to new implications of my old ideas, and tried to elaborate them. Her questions and criticisms were interesting indeed. And it is also interesting, not only for us but also for the general reader, I guess, to see directly how they provoked my thoughts. Thus I decided to publish my replies with minimum amendments and corrections as a sort of open letters rather than to draft an entirely new paper. Although what follows is intended to be self-contained text, it can also be read as discussion paper for my previous works on this topic.

2. The First Reply

I composed the following first letter in response to Knight's questions and comments that had reached out to me on 4 August 2009.

2.1 A double membership check

Each way of measurements together with its values and theoretical backgrounds must undergo a double check for its membership in the measurement networking; one is check in advance, and the other is check within the network. If a particular way of measurements of, say the charge of electron, performed poorly; i.e., the standard deviation of its result was quite large, then it may be not allowed to be a member of the network. Because the poor quality of each member damages more or less the quality of the whole network; i.e. an overall index of standard deviations of adjusted values for all the constants. This is the check in advance.

The measurement networking is a quite dynamic activity. Results from each method of measurements have been constantly revised and renewed due to, say, advancements of relevant technologies. So we have a series of values obtained from a method of measurements, say *A*. Suppose the following situation. An earlier result of this method *A* was of so good quality that *A* was given its membership of the network in which we had already had other measurements methods, say *B* and *C*, for the same object of *A*. After the several renewals of those three methods, we find that *A*'s values becomes more and more different from values from *B* and *C* that remain, in contrast, close with each other. Now *A* becomes a strange minority among those three, so to say. Any considerable difference between *A* on the one hand and *B* & *C* on the other is also damaging for the overall quality of the network. So scientists have to make their decision, at a stage, that *A* should be ruled out from the network. This is an example of the check within the network.

Furthermore let us suppose the following situation. Scientists invented new methods of measurements for the same object of *A*; say, *D, E, F*. It turns out that results from *D, E, F* are much closer to those of *A* rather than *B*'s and *C*'s. Now the power balance has shifted. *B* and *C* become strange minorities while *A* is a part of the majority. Then *B* and *C* might be expelled from the network, and *A* might regain its membership. In such a way, the membership of the network changes dynamically. This is just a simplified schematic example. But we can find similar phenomena in the history of precision measurement.

2.2 Possible and actual inconsistencies in science

There are many possible or even actual inconsistencies in scientific activities. Take some samples.

Theoretical scientists introduced many laws, principles, and formulas. Some of those were derived boldly from a limited number of phenomena like the principle of the constancy of light velocity, and others from thought experiments, say the principle of equivalence. Some of them were put together to form a theory, say the theory of relativity, without any proof or assurance of consistency among them. This is also the case with thermodynamics, and statistical mechanics.

Theoretical scientists sometime commit contradiction more explicitly. In statistical mechanics, scientists still use the classical model in which atoms or elementary particles are thought to obey classical rather than quantum mechanics. And at the same time they use conjointly in their explanations quantum mechanical ideas that are inconsistent with classical ideas.

Things go even worse when we look at the experimental science. The inconsistency in the measurement networking is just among many examples where experimental scientists run into contradiction. As an another instance, some scientists regularly assume the general relativity effects when they try to explore or examine the quantum field theory through experiments, while noticing that there is inconsistency between those two theories. Thus many possible and actual inconsistencies abound in scientific activities.

Why? Why don't scientists in both theoretical and experimental sides care about possible or actual contradictions? My answer is given in the light of instrumentalism with theories and realism with entities. Generally speaking, scientists take their theories just as useful instruments or tools for explaining and predicting phenomena and for planning and conducting experimentation, or at best a tentative approximation to the reality that is, they believe, contradiction-free. Science was and still is in so imperfect condition that they are better off in using more or less inconsistent views of the world than using nothing. This is a sort of instrumentalist's picture that I take.

On the other hand, scientists' attitudes towards the inconsistencies are deeply immersed in realism. What they are pursuing with the help of more or less incomplete theories which are even inconsistent with each other is the reality that is independent of or neutral to either of those theories. Only in this realistic picture the following research strategy becomes sensitive, I claim. First, construct as many as possible and as different as possible theories or methods of measurements with no regard to consistency among them. Then approach independently to the reality through each of those theories or methods. Among many results from those approaches, take the one that remains robust with the change of theories or methods. If one result obtained within a theoretical framework disappears within a different one, this result is not robust with the change of these theoretical schemata. The more results from many different measurements divers one another, the larger the overall standard deviation of the measurement, being other conditions equal. So construct the network in such a way that it leads to the smallest possible overall standard deviation. In short, in the light of this realism, practical eclecticism is a sound scientific strategy.

2.3 Super-rational decision on the rationality of science

Several versions of realism including my own that was mentioned in my Monday talk claimed that as far as you want to take scientific activities rational or making sense, you should commit the existence of electron. So their claims are conditional in that the required commitment presupposes the rationality and making-sense-ness of science. Then is science rational or does it make sense? This is another big issue.

Science can be rational or make sense even if it runs into inconsistencies as far as they can be explained away in the light of instrumentalism and realism, I think. What matters about the rationality of science is the rationality of its method or methodology. Among many methods and methodologies in science, what is the most important for and the most peculiar to science is statistics. Then is statistics rational?

In my talk that was made three years ago in Melbourne, I pointed out that the most basic assumption of classical statistical testing is a non-trivial assumption about a matter of fact that cannot be proved or disproved by statistics any more. (The assumption is that any measurement or sampling is a probabilistic event in the sense that long run relative frequencies of its outcomes should converge to a particular value.) In this sense, it is transcendental. Here we face a choice whether to accept this transcendental assumption or not. This is a choice both of whose options are equally rational, I claim. In making a choice between equally rational alternatives, we should appeal to some criterion or whatever that is beyond rationality. In this sense, this choice or decision is super-rational rather than rational, irrational, or non-rational. Scientific rationality is based on our super-rational decision. If we accept the basic assumption by our super-rational decision, any statistical technique or ideas including the overall standard deviation in the measurement network, and therefore any scientific activities including the measurement networking that utilize statistic become rational and making sense. If otherwise, all those things and activities become irrational. This means that the precondition of the versions of realism fails to be met. My talk three years ago tried to open this super-rational decision to the public. My talk on Monday is based on one possible option of this decision making.

3 The Second Reply

In response to my first reply, Knight wrote to me her letter ‘More (and/or More Diverse) is not Always Better’ dated 9 August 2009. It contained her though experiment ‘finding Atlantis project’ whose shortened and modified version we can see in her paper (Knight, 2009, 70-71). My second reply is largely framed in the context of this thought experiment.

3.1 Robustness in the Two-valued Case

In my talk, robustness is exemplified by measurements of the charge of electrons that takes continuous values represented as real numbers. But it is possible to talk about robustness for the case of properties that take discrete values, say only two values. Suppose that an alternative whether Atlantis is in Denmark or in Sweden is a matter of dispute. In this case Atlantis is given two possible properties or values; 'being in Denmark' or *D* and 'being in Sweden' or *S*. Suppose also that we have ten different cognitive approaches each of which can tell us whether Atlantis have either the property *D* or the property *S*. Then there are at total possible eleven cases for overall results of ten measurements (see below).

	Denmark or *D*	Sweden or *S*
Case 1	0	10
Case2	1	9
:		
Case6	5	5
:		
Case 10	9	1
Case 11	10	0

For instance, in case 1 all the ten approaches or methods unanimously show that Atlantis is in Denmark, while in the case 2 only one method tells us that Atlantis has the property *D* and nine others show that it has the property *S*. The bigger is the difference in numbers between methods that show *D* and those that show *S*, the more robust is the result of the majority of those methods. For example, we can take cases 1, 2, 10, and 11 are the cases where a robust result is obtained. On the other, obviously no robust result is obtained in cases 5, 6, and 7. But it is more or less arbitrary to decide in which case we have a robust result.

In the 'finding Atlantis' project, we got the following result.

Devices	1	2	3	4	5	6	7
	D	D	D	D	D		D
	S	S	S	S	S	S	

The result shows that six devices indicate that 'Atlantis is in Denmark' whereas six devices 'it is in Sweden'. Since in this case the difference in numbers between devices that show *D* and the ones that show *S* is zero, no robust result is obtained.

Under this situation, what to do? Though, as shown later, there can be many ways to do at least *in* *theory* rather than *in practice*, the following is the ‘standard’ option. (I will explain later why it is the standard.) Suspend any judgment, and invent new devices that are as different as possible and as independent as possible. Then make decision only when a clear robust result, say $D : F = 6 : 24$ (or $2 : 8$), is obtained.

3.2 Underdetermination

Let me give some examples of ‘the framework of networking’ in scientific settings: one or another idea of causality and supposition of its existence, the belief in the external world as independent reality, the principle of the uniformity of nature, and etc. Such fundamental scientific theories as Newtonian mechanism, Maxwell’s equation for electromagnetic field, quantum mechanism, and relativity theory are among a patchwork of theories that supports one or another measurement in the network, so they are part or patch of the theory patchwork. Since I cannot find any more fundamental scientific theory than those, the framework should consist of metaphysical ideas rather than scientific ones.

It is true, as you pointed out, that, like any other scientific activities, the measurement networking and therefore any robust result obtained in the network presuppose those or other metaphysical claims. Those metaphysical ideas are also, as you suggested, empirical in the sense that, if the networking goes wrong so often and so seriously, it may be possible, at least theoretically, for scientists to revise some of them.

Also each theory as a part or patch of the theory patchwork, such as Newtonian theory, Maxwell’s equation, is empirical. Theoretically speaking again, it can be revised or revoked if the networking goes wrong seriously.

These possibilities lead to underdetermination in Quinenian sense with regard to what to do in the face with no robust result. Besides the standard option above mentioned, many other options open *theoretically*. Revision of the framework and/or the theory patchwork is among those. Even in the standard option, scientists constantly face an option; either to kick out some methods from the network to obtain a robust result instantly or to wait for the invention of other methods, in other words, an option between instant robustness and possible robustness in future. Thus a (series of) failure of obtaining robustness doesn’t uniquely determine what to do next. In other words, as you pointed out, any robust result is obtained, at least partly, through decisions that are not fully determined by measurement results, therefore by the reality. A robust result doesn’t *necessarily* mean the objective reality.

3.3 Mitigated Underdetermination in Practice

The underdetermination is largely mitigated in practice. In the networking, any part or patch of the patchwork has never been put in doubt even when the network went wrong. It has been just taken for grounded as background. Researchers in precision measurements never paid their serious attentions to its empirical adequacy. Take an example. In 1930s, there was a significant disagreement between values of the charge of electron, or *e*, that were obtained from different methods and background theories. It was known to specialists as 'e-discrepancy'(Hopper and Lavy, 1941, DuMond and Cohen, 1948, 82) Though facing with e-discrepancy, however, no researcher casted their doubts on one or another of the background theories. Instead they doubted one or another values of other physical constants that were used for deriving the values of *e*. Needless to say, any of the metaphysical ideas has never been put in doubt whenever the networking went wrong.

There still remains an option between instant robustness and robustness in future. But scientists' options *in practice* in their networking are more limited than those *in theory*.

3.4 Why limited?

Then why practical options are so limited? One possible answer is context-dependence of scientific activities. In my view, there are several different contexts within which various scientific activities are carried out. The context of theory formation and of theory confirmation are among those. The measurement networking is done in another context. Scientists' attitudes to theory and entity are remarkably different from one context to another.

In the context of theory formation and confirmation, scientists don't take existing theories for granted. They are very sensitive to those empirical adequacies. Even a few mismatching between a present theory and observed phenomena can make revolutionary minded researchers suspicious to it and seek its alternatives.

While scientists' prime concern is theory in the theoretical contexts, it is the objects of measurements; i.e., quantities of one or another object, in the networking. In the context of the networking, researchers simply don't care much about empirical confirmation or disconfirmation of scientific theories. Admittedly, their objects, say electron, were first introduced by one or another scientific theory. But scientific activities in this context are far more deeply committed to the existence of electron than those in the theoretical context. Researchers are now confident of the existence of electron without taking as literally true any theory that originally posited it. For their eyes any theory that introduced electron looks like Wittgensteinian ladder: it was once useful for the discovery of electrons, but once we got them, it can be thrown away. For them electrons have become just a familiar part of the reality, like stones or trees, so they don't need any theoretical assurance for its existence any longer.

Activity realism takes into account those differences across the contexts. It is one of its main claims that scientists' commitment to the existence of electron in the networking context is so strong that we cannot explain their activities as rational or making sense without taking into account it. Activity realism also points out that electron was originally introduced by scientific theories, but later it was also posited by such scientific activities as the networking. Like Hacking's remark that some theoretical entities become 'experimental entities', activity realism claims that some theoretical entities becomes 'activity entity'.

Then why do the contexts of scientific activities differ one another? The difference in the contexts is mainly due to a scientific convention, that is to say, the labor division among scientists. Some researchers were trained as theoretical scientists, and they do their works mainly in the theoretical context. On the other hand, others were educated as experimental scientists and goes to the context of networking.

3.5 Social and Subjective determinants of robustness

The difference in contexts is certainly among factors that determine, at least partly, any robust results obtained through the networking. If it were not a part of the scientific convention, non-robust results might cause some substantial changes to the theory patchwork and/or the metaphysical ideas. The networking and its robust results should be influenced by those changes. The decision between instant and future robustness is also another determinant of robust results.

Those determinants of robust results are more or less independent of or irrelevant to the reality. The labor division and therefore the difference in contexts are parts of social convention among scientists. The deep commitment to unobservable entities is scientists' attitudes toward the reality, not the property of the reality. Any decision between instant and future robustness is a matter of judgment, therefore doesn't correspond directly to the reality. Shortly they are social or subjective.

3.6 Criterion of What?

A robust result is determined by those social or subjective conditions to some extent. And no one knows to what extent it depends on social and subjective conditions and which robust result is more or less independent on those conditions and therefore more or less objective. Thus we cannot say that an robust result in the network is objective or real in the sense that it indicates what the reality is. And the robustness criterion or the criterion of the success of the networking is not the criterion of existence in this sense; i.e., when measurement results meet this criterion, it can be said to be real, or if a value of the electric charge becomes robust, the existence of electron is assured.

Then in what sense the success of the networking can be seen as a criterion of existence? When any robust result of the electric charge is obtained, we cannot explain the scientific activity for obtaining the robust result as rational or making sense without committing the independent reality of electron. The commitment of the existence of electron as independent reality is incorporated so deeply into the networking that, if scientists don't commit its existence, their activity should become irrational or it makes no sense at all. So the criterion is of the commitment for the existence of electron that is indispensable for the rationality or making-sense-ness of scientific activity. In other words, it is a criterion of indispensability of the commitment to the existence of electron for making sense and rationalizing scientific activity.

Activity realist is satisfied with the criterion of indispensability of existential commitment rather than that of existence. He gave up rejecting any form of skepticism about independent reality; e.g., that of the external world, and that of unobservable entity. Instead he intends to repudiate a sort of anti-realistic claim that scientific activity can be made sense and rationalize without committing the independent existence of electron, or that scientific rationality is free from existential commitment to unobservable entities.

3.7 Realistic Explanation

How can existential commitment explain the measurement networking? Realistic explanation of the standard strategy of the networking can be schematized as diagram 1. In this diagram, [A] → [B] means that *A* explain *B* as rational and making sense, that *B* entails or implies *A*, and that *B* presupposes *A*. Also [A] ↔ [B] means that *A* and *B* are equivalent.

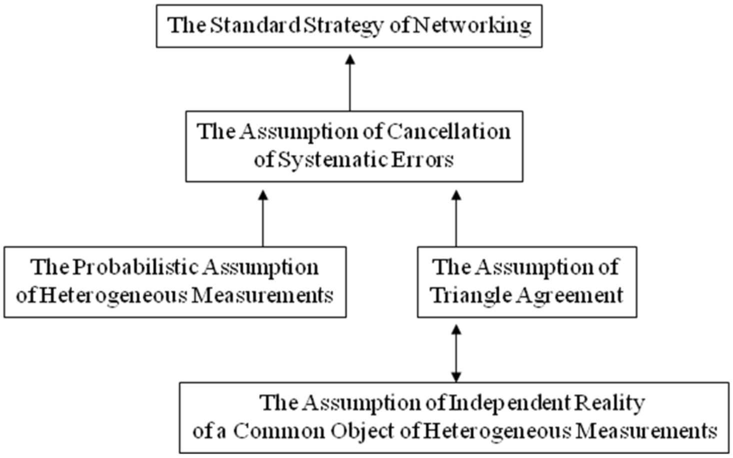

Diagram 1

Probabilistic Error and Systematic Error

There are two sorts of errors; i.e., systematic and probabilistic errors that are believed to be involved in any measurement. Probabilistic error is supposed to vary from one application to another of homogeneous measurement, that is, measurement with the same method, device or background theories. This supposition is based on a *transcendental* assumption that homogeneous measurements are stochastic trials. Also another important assumption; i.e., probabilistic assumption of homogeneous measurements, is made with regard to probabilistic error. It claims that probabilistic errors tend to be distributed, in the long run, symmetrically with the true value as the center of distribution. This assumption renders rational and natural the basic strategy in statistics and precision measurement: take an average from as many as possible measurement results and you can nearly cancel out probabilistic errors.

On the other hand, systematic error is supposed to be constant with homogeneous measurements. But it is thought to vary from one measurement method to another. As far as we use only one method, we cannot know whether any systematic error exists and how large it is.

Assumption of Cancellation of Systematic Errors

With this systematic error, the assumption of their cancellation is made: if heterogeneous measurements; i.e., measurements with different methods, devices, and theories, are independent with each other, and the number of them is large enough, then the systematic errors tend to be distributed symmetrically with a true value as the centre of their distribution. This assumption renders the strategy of the networking as making sense and rational: take an average from results of as independent as possible and as many as possible measurement methods, and you can nearly cancel out systematic errors.

The assumption of cancellation of systematic error is as old as the precision measurement. We can find one of its earliest expressions in W.S. Jevons's *The Principles of Science* (1874). Moreover, precision measurements as a scientific disciple itself was established on, among others, this very assumption in 1870's in Germany and 1880's in US. So it is constitutive for precision measurements. In this sense, the strategy based on it is the 'standard' for that discipline.

Probabilistic Assumption of Heterogeneous Measurements

The assumption of cancellation of systematic errors is based on two further assumptions: the probabilistic assumption of heterogeneous measurements and the assumption of triangle agreement. The former is that heterogeneous measurements are stochastic trial. This is another transcendental proposition. It is a claim about a fact; i.e., about heterogeneous measurements. So it is synthetic rather than analytic. It has neither evidence nor counter-evidence, therefore cannot be justified empirically. So it is a priori. Empirical determination of physical or other scientific quantities is based on this assumption. In other words, it makes possible scientific experience in this sense. So it is transcendental. Also we cannot claim that either acceptance or rejection of this assumption is rational. Whether to accept this transcendental assumption is a super-rational decision.

Independent Reality and Assumption of Triangle Agreement

Another basis of the assumption of cancellation of systematic errors is the idea that objects of heterogeneous measurements is the same and they exit independently of measurement activities. This idea is equivalent to the assumption of triangle agreement, that, if each result of two or more heterogeneous measurements agrees or corresponds exactly to the reality, their result must agree with each other. This assumption is so old that it has even a Latin phrase: '*consentienti a uni tertio consentiunt inter se* (what agrees with the third agrees with each other)'. (Kant referred to it in his first Critique (B848).)

The reality implied in this assumption is not constructed by, nor dependent, on any of heterogeneous measurement methods (or their background theories). Rather it is neutral to, and independent of any of them. If the object of each measurement is taken as an independent reality, then it is natural and rational to suppose two measurement results that correspond to it also agree with each other. On the other hand, it becomes neither natural nor rational to suppose the agreement among measurement results if the object of measurement is not independent but rather constructed by each measurement. So this assumption can be made rational and natural only with the idea of independent reality of the common object of measurements. So roughly, the assumption of triangle agreement is equivalent to the supposition of independent reality.

On this assumption of triangle agreement, we can say that the 'true' values for heterogeneous measurements must coincide with each other, and therefore those values can be represented as a single value; i.e. the center value of distribution of systematic errors. So a core idea of the probabilistic assumption of systematic errors can be made plausible under the assumption of triangle agreement or equivalently the idea of independent reality of a common object of heterogeneous measurements.

The assumption of triangle agreement is also presupposed when researchers guess the existence of systematic error from apparent disagreement among the mean values obtained through heterogeneous measurements. According to the assumption, a disagreement among heterogeneous measurements indicates that something is wrong with any or every of those heterogeneous measurements.

Independent Reality as a Transcendental Idea

It is possible to think that this world is just a life-long dream, while conducting everyday life. From the exactly the same evidence for independent reality, one can conclude equally well both the existence of independent reality and its non-existence. Therefore we cannot reject any skepticism about independent reality, e.g., that of the external world, and that of electrons. In other words, the idea of independent reality of electron is neither empirically proved nor empirically disproved. It is not empirical but a priori claim. The idea is also a factual claim, a claim of a fact of matter; i.e., the mode of existence of electron. So it is synthetic rather than analytic. It is also a basis of the standard strategy of precision measurement. It makes possible empirical determination of fundamental physical constants, so it is transcendental.

Also it is rational to accept the independent reality of electron whereas it is also rational to be skeptical about it. Thus we face another super-rational decision between independent existence and non-existence of electron.

Summary

The standard strategy of the networking is based on two transcendental assumptions: the probabilistic assumption of systematic errors and the idea of independent reality of a common object of heterogeneous measurement (or its equivalence; i.e. the assumption of triangle agreement). Putting another: the former can be explained as making sense and rational in the light of the latter. With the help of the probabilistic assumption, the realistic idea can explain the networking.

Those two transcendental assumptions are to be adopted by super-rational decisions if they are to be adopted at all. The networking or the entire practice of precision measurements is based on those super-rational decisions.

3.8 Anti-realism' s failure of explanation

On the other hand, anti-realism cannot explain the networking, I claim. Here let us see how anti-realistic pragmatism cannot explain it. It claims that the networking can be explained in terms of pragmatism without appealing to realism. It is *useful* for science, technology and industry to have a standard value of good quality for electric charge. That's why scientists try to determine its standard value through the networking. Also scientists are inclined to choose the 'cheapest' way to attain their goal, it claims. The cheapest way is to adjust only the network; e.g., to abandon one of 'problematic' measurement results. On the other hand, it costs much for them to change any basic parts of scientific knowledge. That's why they cast no doubt on any background theory or metaphysical idea even if the networking goes wrong. I am happy with these pragmatic explanations of the networking. So far there is no need to appeal to realism. But anti-realistic pragmatism cannot explain, among others, the following point, I claim.

There are available several measurement results of electric charge before the networking. Some of them can attain relatively high qualities, and others not. The quality of the value that is adjusted through the networking is a function of the qualities of those pre-networking values. So the quality of the adjusted value is almost certainly lower than that of the best pre-networking value, because the best pre-networking quality is deteriorated by the lower quality of other pre-networking values. So from the pure pragmatic point of view, it is not sensible to do networking. The networking requires tedious calculations and its result is worse than the best pre-networking value. Why not simply adopt the best pre-networking value as the standard value? It is certainly a cheaper way to attain a better goal. This means that anti-realistic pragmatics cannot fully explain the networking.

To obtain the standard value for the convenience of scientific practice is just one of reasons of the networking. In addition to it, scientists aspire for approximately true value of the independent reality by canceling out systematic errors. Realism is indispensable for a full explanation of the networking.

4. The Third Reply

My third and, at the moment, final reply is directed to Knight's letter 'A Response to Yasuo Deguchi's Second Reply' dated 10 September 2009. The letter made several points, but I take only the final one because it eventually becomes an import point for her paper. In her third letter, she wrote;

> **Finally**, you posed the challenge that the anti-realist cannot explain why one would go through the trouble of networking. Networking, you claim, only makes sense when viewed in reference to independent reality. Mere 'usefulness', you say, is not enough to make networking rational, for it seems useless rather than useful, because triangulation dilutes accuracy and is cumbersome.
> Here I must disagree. I think you entertain too narrow a view of usefulness. I think the benefit, which is achieved by networking lies in the gain of greater context.
>
> My point is that by triangulation you are able to include more contexts and although you lose in accuracy, you gain application opportunities of the idea or measurement (or whatever is the object of concern) and you gain contexts in which the object of concern makes sense. Broadening of context is what allows a physicist to talk with a biologist, chemist, meteorologist, zoologist, artist etc. This is a significant gain, which derives its sense purely from pragmatic criteria; no reference to external reality needed.

Defects or 'uselessness' of triangulation, the loss of accuracy and computational cumbersomeness etc., can be handsomely compensated by the pragmatic gain of the network; i.e., the broadening of the context. (We can add here another important pragmatic value; the standardization of physical constant.) Knight seems to appeal to this sort of idea of 'pragmatic compensation'. But this line of pragmatic explanation cannot make sense the particular ways in which the network is constructed. In other words, it cannot explain why scientists take the trouble to use those apparently 'costly' ways rather than 'cheaper' ones. So it cannot give a full explanation of the networking.

One can construct the network in an easer or pragmatically better manner simply by choosing the best single value as standard rather than by taking a weighted mean over many, better or worse, results. A cheaper alternative is also available to the least square adjustment. Suppose that there are available three measurement results, *x, y, z* of physical constants, *X*, *Y*, and *Z*, and the quality of *x* is considerably lower than that of *y* and *z*. In such a case one can forget about *x* and compute another value *x'* for *X* from *y* and *z* rather than making the least square adjustment between *x, y* and *z*. This alternative way requires much less cumbersome computation than the least square adjustment does. Moreover the resulting values can have better qualities than adjusted values. By using those cheaper ways, however, one can achieve exactly the same pragmatic benefit; i.e., the broadening of the context and the standardization of physical constants, without the loss of accuracy and the awkward computation. From a purely pragmatic viewpoint, there is no need to make any compensation. We had better apply the 'cheaper' ways from the beginning.

Then why do scientists take the trouble to average over many results and to make the least square adjustment in their construction of the network? Or why do scientific activities involve the 'cost' that is compensated, if it is to be compensated, by pragmatic benefits? This is my question and the question that a pragmatist cannot answer any longer, I claim. Why cannot? Those costly ways are intended to 'cancel out' one or another systematic error which no single measurements, whatever quality it has, can escape from. And the idea of systematic error entails the independent reality of what to be measured. So the reason for the costly methods has to appeal to scientists' commitment to the independent reality of objects of their networking. In other words, the wanted explanation should be a realistic rather than pragmatic one.

Let me add one more comment. Scientists did indeed use the cheaper ways *occasionally*.(1) So in this respect too, the construction of the measurement network is guided, to a considerable degree, by pragmatism. But it is also fair to say, I believe, that the cheaper ways are merely of occasional use rather than indispensable parts of 'regular' or 'standard' practice in the network building. In other words, the scientific activity in question is occasionally pragmatic but largely realistic. It may be better to say that, largely or not, it requires realistic account, at least, for its non-trivial and crucial aspects.

5. A Final Remark

I don't contend that activity realism is the best or an unassailable position. Nor is it completed. It is substantially under construction yet. But good criticism can nourish good philosophy. Activity realism has so far been well benefited from Knight's criticisms.

Note

(1) For their 1963 least square adjustment, Cohen and DuMond computed a weighted average over previous eight results of measurements of the speed of light. But their final choice was not the weighted mean value but a single value to be averaged that was quite close to the mean and happened to be identical to a standard value at that time (Cohen and DuMond, 1965, 549-551). Similar move can be seen in another least square adjustment in 1969 (Taylor et al. 1969, 385-387).

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