EPISTEMOLOGIES AND THE KNOWLEDGE SOCIETY

New and Old Challenges for 21st-Century Europe

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NEMESIS PUBLISHER

The research leading to these results has received funding
from the European Commission, Seventh Framework Programme,
under grant agreement No 217796

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1. Evidence, Objectivity, Social Policy

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This paper is intended to further the discussion on the use and dissemination of *expert knowledge* – in particular on the relation between the *plausibility* of scientific discourse and the *credibility* of the figure of the scientific expert in the context of a democratic use of science in the domain of policy making and social intervention.

An analysis of the concepts of plausibility and credibility cannot be effectively pursued in terms of the traditional discussion on what counts as strict criteria of demarcation between science and pseudo-science (criteria which are indeed notoriously difficult non only to formulate in principle, but also and even more so in practice).

Such an analysis should instead lead us to rethinking the form as well as the substance of scientific discourse. To make this possible, a *multidisciplinary,* broad-spectrum analysis appears necessary in view of combining, in a resourceful and effective way, diverse and sometimes heterogeneous methodologies and lines of questioning.

For example, as we will see, the use of scientific evidence in the formulation and implementation of social policies requires a skilled awareness of how to relate hard and softer methods for data collection, factual content and cultural, ethical, social judgement, etc.

However, as will be argued, such awareness does not necessarily detract from the *objectivity* and the *transparency* of the results we rightly expect of scientific discourse. It brings us instead to identify a more realistic model of scientific research, that is well in control of the conditions and limitations that the complex domains of socio-political decision-making impose over the use of such research.

I believe that Europe has the resources, both intellectual and practical, to play a fundamental and leading role in this direction.

1.1 INTRODUCTION

There is an emerging area of inquiry in the philosophy of science which, in Nancy Cartwright's description, goes under the name 'evidence for use'. It brings to the fore aspects of the concept of evidence which traditional philosophical theories of evidence do not specifically take on board. The latter mainly focus on issues related to the production of sound evidence (its truth value, its quality, its efficacy, etc.), which might not be sufficient, or even appropriate, to account for what a purported user of evidence might need to consider once confronted by the context(s) where some evidence is summoned, or required. *Production* and *use* are not two equivalent areas of analysis.

For example, suppose we have at our disposal a vast (infinite) amount of true facts. Philosophically we would argue that when a true fact is evidence for a hypothesis, the hypothesis is more probably true (the fact must be true if we want to increase the probability that the hypothesis is true). Let us suppose now that we want to argue in favor of a certain social policy rather than another. What facts do we bring to the table for policy deliberation? Among all the true facts, we want only those that are relevant to the policy. We must introduce *relevant facts* even before than true facts. A true fact is not, ipso facto, a relevant fact. Relevance is a non-negligible condition imposed by the context of use, and as such it requires a specific analysis.

The same goes for the *effectiveness* of the results of a certain methodology in domains of applications different from those which the methodology has been devised for (a typical case is that of randomized control trials, as we will see: their internal validity does not automatically translate into external applicability). The same also goes for the *certainty* of the facts produced to support a hypothesis or a decision. The purpose of producing evidence is to provide certainty (high probability) to the decision of accepting (or rejecting) a hypothesis. However, in practical contexts, we often find ourselves making such decisions before conclusive evidence is/can be provided (or even when conclusive evidence is an unreachable goal). Besides, there are contexts where only evidence produced in a less than rigorous way might be available. Still, in these contexts (there included policy contexts) non-conclusive and less than rigorous evidence might be (and often is) better than no evidence at all. This might

sound unacceptable from an epistemological point of view, and yet it might prove plausible in practice.

Paying attention to the features required by evidence users (relevance, effectiveness, uncertainty) is precisely where the issue of use imposes due re-adjustments on an analysis of the concept of evidence – a concept which finds itself not so much challenged by theoretical dilemmas, but rather by practical demands. In this paper I will not analyse the details of the latter (which are so clearly identified and discussed in Cartwright's work). I want instead to focus on what consequence this practical viewpoint on evidence has on, arguably, the main motivation for resorting to evidence in the first place.

In the age of science, evidence is considered to be the benchmark of objective knowledge. To know objectively is to prove that a hypothetical claim is true (or false), or to be able to form an undisputed, or at least well supported belief concerning the hypothesis. Evidence is the means to achieve this.

In practical contexts, such as the formulation or the implementation of a policy, this role of evidence extends to the realm of decision-making. If we can count on evidence to prove that, say, a certain intervention will be effective (or stands a good chance to be so) then basing our decision to implement such an intervention on the evidence will make our decision 'objective'. Evidence is the means to objective decisions (the so called 'evidence-based policy' perfectly exemplifies such a strategy).

We would indeed be tempted to think that the objectivity of our decisions is a direct consequence of the objectivity of the knowledge established via the evidence. The more objective the knowledge supported by the evidence (and the more objective the evidence), the more objective the decisions (assuming equal degrees of relevance).

There are then three crucial steps to consider in assessing the objectivity of our decisions based on evidence:

1) evidence is what makes scientific knowledge objective;
2) objectivity is what evidence itself needs to possess in order to play its role appropriately;
3) evidence-informed knowledge is what drives a decision, an action, a policy in the direction of objectivity.

At least two problems lurk in the background.

The first has to do with the very definition of 'evidence for use'. If the evidence appropriate to policy making is to be subject to the practical constraints of shifting contexts, background assumptions, uncertainties of facts, or viewpoints, how entitled are we to trust that it is the appropriate guide to the objectivity of our policy decisions?

The second has to do with the way we evaluate our decisions on the basis of evidence for use. If we were to try and formulate a rational model for policy-making (Blakemore 1998, p. 287) we would start from identifying an ideal scenario wherein, given a problem and a possible solution as our goal, we would proceed by

- collecting all relevant facts and all available information;
- analysing the available options;
- evaluating the pros and cons of all the options included (in the light of constraining standards);
- finally, selecting the alternative which we deem closer to achieving the chosen goal(s) – with the best balance of our constraining standards.

Unfortunately we encounter problems at each step. Constraints of various forms (political, social, legal, ethical, economic, etc.) all limit the scope of policy decisions. Do then all these constraints defeat from the very start the aim of pursuing an objective plan of action? Or instead, to what extent does a type of evidence which is sensitive to these constraints allow us to assess how objective our actions or decisions are, as pursued in the face of the complexities of the world we live in?

In other words: *if evidence is subject to the conditions imposed by use, does objectivity retain some realistic connotation in the contexts where we pursue social and political decisions?*

A question of this sort becomes particularly crucial when expert knowledge is invoked to adjudicate controversies and rank preferences among alternative courses of action outside the disciplinary fields of science. What is at stake is the legitimacy of making use of this type of knowledge, and the very possibility of assessing its reliability (in practice, this has considerable costs, financial and others, as the dilemmas of 'evidence-based policy' instructively demonstrates).

Of course, to answer this question the concept of objectivity itself is to be discussed and possibly revisited. A practical concept of evi-

dence calls for a concept of objectivity itself suitable to practical demands (for example, in shifting from theory to practice, it can be questioned whether the same concept of objectivity is at stake when we refer to a *decision* rather than a *belief* as being 'objective').

To answer our question above, I will start by looking at what connotations a concept of practical objectivity should include. I will then give a concrete illustration of what types of issues and consequences the interaction between evidence for use and practical objectivity would produce in the context of policy making.

By sketching out some possible directions for the evaluation of such issues and consequences we will be able at least to suggest what role expert knowledge might take in the domain of governance.

1.2 ASPECTS OF PRACTICAL OBJECTIVITY

Philosophical concern about objectivity frames the concept either in an ontological context (to be objective is to exist independently of any knowledge, perception or conception we may have about what exists), or in an epistemological one (objectivity is a property of the content of mental states and acts). What is then normally questioned is the possible relation between the two frames (the well known debate between various types of realists and idealists).

Traditionally, both frames entail the exclusion of some features which are deemed to defeat any meaningful definition of objectivity. Among the most commonly referred to are the following (see Martin 2006):

1) subjectivity;
2) value-ladenness;
3) conflict of opinions.

Interestingly, in the realm of practice none of these features seem to be eliminable from the contexts of both knowledge production and decision making.

The strategy normally adopted is trying to see whether objectivity can be preserved *despite* the existence of these features.

I will try to argue for a more radical view.

If objectivity is necessarily confronted by these 'interfering' fea-

tures, would it be possible to rethink their negative connotations in such a way that the features themselves can become instrumental (rather than being detrimental) to the identification of a concept of objectivity suitable to practical demands?

To make this arguable, there are at least three lines of investigation which, I believe, should be explored:[1]

1) Objectivity is not a feature of a well defined or pre-designed method, or the exclusive result of a specific method; it is rather an outcome of inquiry 'cashed out' when methods of different types are used in particular contexts, under particular circumstances, with the aim of asking particular questions and by following specific assumptions, etc. (the question 'by what means are we objective' goes hand in hand with the question 'what do we want to know').
2) Objectivity is not to be defined on a choice of virtuous and vicious features of inquiry fixed a priori (as for example the fact/value distinction in the empiricist view would lead us to believe); what is deemed to be a vice in principle (eg. a value-laden claim) might well become a virtue in the pursuit of achieving objectivity in practice.
3) Objectivity in practice, just like evidence, is to be assessed 'on balance'; it pays attention to different sources of information, and issues a judgment which takes as far as possible disagreement and dissenting views into account.

These lines might appear very abstract, too philosophical to be of any real, practical use. Indeed a substantial part of an argument in favor of a concept of practical objectivity must show how they work when applied to a context of use. Only in this way we can figure out on one side, how the purported features of exclusion listed above might become a relevant part of what ascribing objectivity to a claim entails in the context of use; and on the other, how evidence for use relevantly contributes to bringing the objectivity of decisions forward as a practicable as well as commendable pursuit.

To appreciate the practical potential of this argument I will 'test' one of the lines of investigation listed above (i.e., the first one) by means of a possible context of application.

[1] For some of these themes see Montuschi 2003 and 2006.

1.3 Objectivity and method

Can some method be objective in principle – that is, because of the way it is designed?

Also, should a policy which aims at effective results only choose those methods which appear to possess intrinsically objective features (normally couched in quantitative/operationalised/probabilistic language)?

A method of analysis, to be considered objective, should normally entail two features:

1) It is to be *internally valid*: on the basis of agreed upon premises certain conclusions must consistently follow. The agreed upon standards are usually identified by a number of formal requirements, which are normally taken to secure the consistency of results (the reasons why they obtain and why they are considered to be valid).
2) It is to be *externally valid*: it must not only prove its efficacy, but also a repeatability of its results in different contexts (not necessarily experimental contexts).

The possession of both features is what predisposes a method to secure objective results.

Are there methods which possess both features, namely inbuilt assumptions to shift from the former to the latter? In other words, if the former feature is strongly achieved by the design of a method, can the latter be simply inferred from the former?

An example will help in addressing these questions.

What do we know about HIV/AIDS in Africa? Are the methods we rely on to acquire our knowledge in this context an objective means to assess the problem and devise appropriate action?

I will follow how H. Seckinelgin addresses the issue in his recent book (2008).

In 2003 a Randomised Control Trial was conducted in order to assess the therapeutic effects of an antibiotics called co-trimoxazole on HIV-infected African children. The researchers involved already knew that the daily use of this drug had beneficial effects on dealing with opportunistic diseases, both in children and adults, but their evidence

only concerned low-bacterial-resistant areas. They wanted to find out whether the same results apply to high-bacterial-resistant areas.

The RCT in question was set up on a sample of HIV-infected children aged 6 months-5 years (then extended to 15 years) at University Hospital, Lusaka, Zambia. The trial followed the typical protocols of RCTs (strict selection of eligible individuals, randomization strategies, regular checks, etc.). The trial was stopped prematurely because it was claimed that substantial results were achieved fairly quickly (by October 2003): 43% reduction in mortality and hospital admission rates down by 23% compared with match placebo.

The conclusion reached by the researchers was that the 'results can be generalised to a policy that could be applied universally to children with clinical features of HIV infection in Africa'.[2]

This was taken as 'the evidence' to work on (and from) in agreeing on the policy to set out, and which was subsequently endorsed by the *2006 Report on the Global Aids Epidemic*, presented to the UN General Assembly meeting on AIDS in May 2006: 80% of African children should be treated with co-trimoxazole by 2010.

There are at least two series of issues emerging from this research and from its policy implications, which prove particularly interesting vis a vis the framework for objectivity claims I am trying to sketch out. One concerns specifically the methods by which the facts were established; the second, more generally, the knowledge claims on the basis of which decisions of policy gain reassurance as to their predictive effectiveness and reliability.

Starting from the former. A randomised control trial is able to establish how warranted a hypothetical link set out between two co-occurring facts is (in our case, co-trimoxazole prophylaxis and decrease in child mortality rate in high-bacterial-resistant areas). It has been pointed out that an RCT 'can only be as certain as its premises'. In the case of the Zambian trial 'what the RCT tells us about is the causal link between the treatment and the outcome within a particular sub-population under the particular circumstances of the research' (Seckinelgin 2008, quot., p.88).

Normally, though, a 'generalising move' follows. What an RCT has established within a target population is taken to be indicative

[2] As reported in Chintu et al. 2004.

of what would/will happen outside the so designed trial: in the case under scrutiny, it is assumed that we can generalise from a sample of 500 children to millions. What justifies this move? Does the way RCTs are designed include some assumptions which warrant the step from internal to external validity?

Clearly the researchers involved in the trial *assume* external validity: what works for the biology of one child must work for the biology of all children. Still, arguably, individual biologies have their own histories, if not 'biographies'.

It is interesting to point out that, even if we do not have reasons to dispute that a randomized control trial is well designed and well executed, we are nonetheless led to doubt the generalising power of its outcomes. What the method has objectively established work for some (i.e., we have well warranted beliefs that it does work, as well as a valid and reliable procedure which demonstrates those beliefs) might not work for others and/or for all. Unjustified generalisations lead to a travesty of objectivity claims.

Why was the trial then endorsed by the *2006 Report on the Global Aids Epidemic*, and its external validity assumed without dispute? This indeed brings us to consider a more general issue which impinges directly on the reason why we are inclined to expect objective outcomes more from the use of some methods rather than others (the second emerging issue mentioned above).

The Zambian trial tries to answer a particular question: is co-trimoxazole effective in decreasing mortality rate in children living in high-bacterial-resistant zones? The way the question is answered is by means of the methodologies and the empirical resources that we rely upon in reaching a trustworthy reply. In this case, experimental science and medicine offer the knowledge and procedures we trust will answer the question the way it should be. To put it as it has been put, 'we know what works'[3] – that is, we know what works in terms of best technology, best methods, best science. However, it could legitimately be asked: *how do we know that what we know is what needs to be known in context?*[4]

[3] Repeatedly said at the African Union Summit, World Forum on Health and Development, 10 July 2003 (Seckinelgin 2008, quot., p. 96).

[4] Paraphrasing Seckinelgin 2008, *ibid.*

Adopting the 'we know what works' strategy has led to a medicalisation of the HIV/AIDS problem in Africa and a consequent preference for undertaking policy actions which endorse this strategy. Medicalisation is an overarching process which goes as far as defining and treating non-medical problems as if they were medical problems. For example, HIV/AIDS sufferers are normally referred to, and dealt with, as 'patients'. No doubt HIV/AIDS is a medical condition, but being such is only part of the story. An HIV/AIDS sufferer is also a mother, a son, a husband, a member of a community regulated by certain mechanisms which might prevent from predicting the way in which these 'patients' will/would behave once their health condition becomes known.

This is indeed one of the major problems in dealing with the disease in Africa, claims Seckinelgin: most of the models of behaviour assumed by the policies are abstract models, and are often based on behavioural expectations which have very little if not nothing to do with real behaviour (or else they are extrapolated from typically Western courses of conduct). Of course such models have very little impact on dealing with, and changing what people do.

This does not mean to deny validity to scientific progress in medicine. However, claiming objectivity for results that we obtain only on the basis of what we consider to be established knowledge, while neglecting the issue of where this knowledge is actually applied, or whether other potential sources of knowledge might also be relevant, will translate into courses of action which might prove inefficient, or even worse detrimental, to the treatment of the disease.

In the specific case here discussed, there are indeed other sources of potentially useful knowledge, brought forward by specific contexts – for example, the so-called 'anecdotal evidence', made up of individual stories and biographies of affected people. These sources, however, are often disregarded for lack of reliable methods of appropriate use (or at best, they are treated as 'obstacles' to effective analysis).

Should this type of 'local' knowledge, difficult to generalise, or systematise, simply be left behind? Conversely, if we were to try to make use of it, how can we devise a 'mixed' methodology (local and general at the same time) which can retain an objective stance, in its assumptions as well as in its outcomes?

1.4 Objectivity 'on balance'

The discussion of the case study of HIV/AIDS in Africa has led us to question what concept of objectivity we are dealing with in contexts where we use well established methods and scientific knowledge in view of informing our decisions and choices, and of protecting the latter from lack of plausibility and transparency.

How objective can we hope to be? To answer this question we must first of all reassess the frame of our expectations. On one side, we must accept the fact that the evidence at hand might not be conclusive, and yet even an appeal to a type of evidence which is sensitive to the requests of context might still give us a better chance to reach a well grounded decision than no evidence at all.

On the other side, we must question whether the use of knowledge which is foreign to our best methods and techniques (e.g. judgment calls, local knowledge, individual experience) might nonetheless contribute to a correct description of problem situations (as the HIV/AIDS case might suggest), and rather than interfering with the task of objectivity, they might offer a more adequate and realistic chance for an objective assessment of the real problems we confront.

Let us go back to our initial question: *if evidence is subject to the conditions imposed by use, does objectivity retain some realistic connotation in the contexts where we pursue social and political decisions?*

We can only here sketch a possible answer (being aware that more empirical, as well as philosophical work is needed).

A user-friendly type of evidence can still be the benchmark of objective knowledge, provided that a suitable concept of objectivity is put in place. Its suitability is to include the following assumptions:

1) we should be well aware of how facts are identified, described, classified, etc. and turned into relevant empirical tools of inquiry;
2) we should also consider the ways and contexts whereby methods are put to work, under what conditions, by means of what auxiliary factors, etc. and how they can prove the objectivity of their results while shifting from the ideal (an experimental setting) to the applied (a real life situation);
3) we should learn how to re-draw the borderlines of objectivity claims in the context of the demands posed by the problem situations we put under investigation, and in such a way that what

looks like a non-objective decision, if compared with ideal features of objectivity, might well become objective (or useful to an objective goal), when confronted by the needs of practice.

In all these assumptions, practical objectivity appears to be a 'balancing act' – that is, not less of a real aim, only a more arduous to achieve.

One more thought to conclude. If we want objectivity to be, more than an abstract ideal, an actual guide for our choices and decisions, we need the appropriate type of expert knowledge to pursue this aim. Together with scientists and policy makers, I envisage an active role for the philosopher-methodologist – a figure of expert which is willing and well versed to reflect on the real conditions which make a good use of scientific knowledge possible, whatever the needs practice might impose on the strictures of theory.

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2. Entrepreneurship and Frontier Theory of Inquiry

Thomas Nickles

2.1 Introduction: beyond lisbon 2000

The title of the MIRRORS final conference, Beyond Lisbon 2000, can mean several things. It can mean that the Lisbon goals have been met and that it is now time to formulate new goals. It can imply that that the Lisbon goals have not been met, that the program was a failure, and that we need to turn our attention to other matters. More positively, it may mean that more time is needed to meet the goals. More radically, it can mean that some of the goals themselves should be rejected; or at least that they need to be reformulated, since they flow from faulty assumptions. In my presentation I shall take a fairly optimistic line. Part of the optimism follows from some positive developments within capitalist economies, part from challenging the basic assumptions that generate the sense of crisis in the Lisbon Agenda, and part from criticizing some pessimism-inducing features of *Taking European Knowledge Society Seriously* (Wynne and Felt 2007, TEKSS hereafter), the report to the European Commission by the Expert Group on Science and Governance, a select group of science studies experts.

Here is an extended quotation from the "Presidency Conclusions" that summarize the Lisbon Agenda of March 2000 for the European Union (EU).[1] The document opens with a statement of the challenge facing Europe and a summary conclusion of the way forward (emphasis in original):

[1] Presidency Conclusions, Lisbon European Council 23 and 24 March 2000.