Traction in the world: economics and narrative interviews

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Abstract

This article critically examines the methods used in economics by contrasting them with the tools used in experimental physics. I argue that economics is superficially empiricist in that it confines itself to the comparison of data and models, whereas experimental physics is realist in that it fully takes into account how the data is produced from and is ultimately traceable back to underlying physical quantities. The thorough treatment of the relationship between the data and the underlying physical world it makes tangible is where physics gets its traction in the world. Data plays an intermediary role between what is out there and knowledge about it. Before data can be used to compare higher-level theories to what happens in the world, a lower level understanding of how the data relates to that world is necessary.

Economics should not be done like physics, biology or any other natural science. Rather I use the comparison with physics to argue that economics must be done as a social science with a set of methods tuned to the subject matter of economic activity as it occurs in human societies. The heart of the argument is that because of the nature of the data used, mainstream economics has little traction in the world. Instead, borrowing from realist social theory, I describe how narratives can have traction in the social world to the extent that they convey the internal conversations of key actors operational at the time under study as a real causal mechanism driving economic and social change. In other words, if, as Margaret Archer suggests, the internal conversation acts as a real causal mechanism mediating between structures and agency in the social world, then we need data that makes that internal conversation tangible. Once we have access to what is actually happening in the social world, we can perhaps compile knowledge from the data thus obtained or compare that data to various theories.

Introduction

The main argument I make in this paper is that empirical economics as it is generally practiced today differs from experimental physics in the way that the data available for use in the social sciences differs from that used to study the physical world. The problem is unbridgeable and necessitates different methods to be used in economics. A comparison with physics is however useful for suggesting a good direction to take in empirical economics. We can use a helpful metaphor to clarify the distinction between empirical economics and experimental physics. The metaphor is that methodologies are like vehicles for exploring the world. The methods used in contemporary economics are like a sleek race car spinning its wheels in the surface data of the world, whereas the methodology of experimental particle physics is much more like a tractor, with its tyres well-suited for maintaining traction in the world. Data is thus the surface of the road between the vehicle and the underlying world. The way that the tractor maintains its traction is through the optimal use of frictional effects. Indeed, as I will show, experimental particle physics looks for clever ways of harnessing frictional effects (energy loss by the passage of particles through matter and fields) in order to get its traction in the world.

This paper thus concentrates on the critical layer represented by data between that which exists in the world and our models or knowledge of it. Accordingly, the first part of the paper presents a diagram of how data fits in between these two spheres. The diagram is used to compare research approaches in particle physics and contemporary economics. I will also include a discussion of what I consider to be a common misconception in the social sciences of the role of friction in physics.

In the second part of the paper I consider a particular narrative approach to economics that I advocate as a way of doing research in economics that has traction in the world. This requires some prior understanding of how the social world operates, and for this I use the realist social theory of Margaret Archer (1995; 2003; 2007) as a basic starting point. I will explain the necessity of using some kind of lower order social theory, which is also tested and improved at the same time that higher-level phenomena, such as economic activity, are studied. This is perfectly compatible with the way that the understanding of phenomena associated with the passage of particle through matter and fields is used for exploring new phenomena in physics. Using the comparison, I suggest how narrative interviews can be used to produce the required layer of data that allows economic research to have good traction in the world. I end with a discussion of a research project that explores the use of this narrative technique.

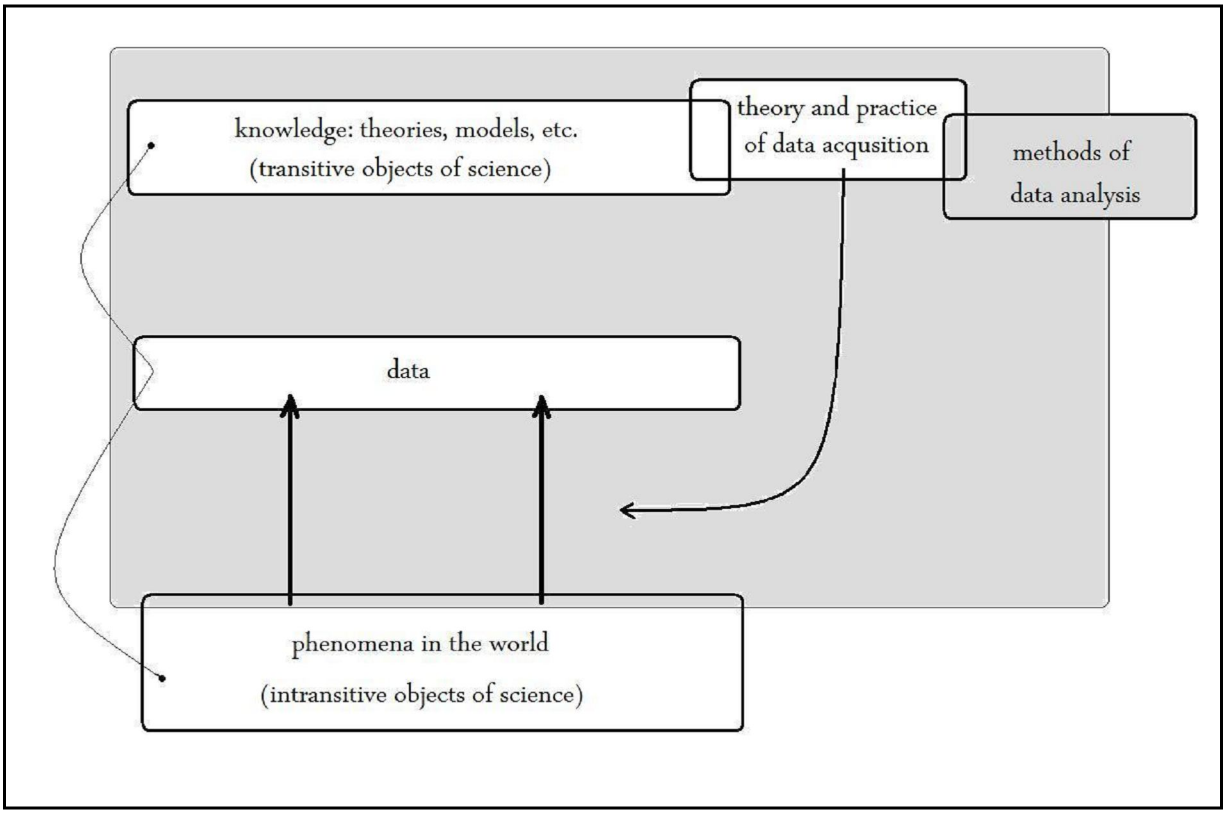

Figure 1   Science: knowledge, data and the world.

1   Data, particle physics and economics

In this part I present a model of how data fits between the phenomena in the world that we wish to study and the knowledge we have of those phenomena. I demonstrate how this works in experimental particle physics and discuss why the methods used there are inappropriate for economics. Part of the reason is that economics as a social science does not use frictional effects the same way that particle physics does, as will be explained. I also discuss the treatment of systematic or type B uncertainty in physics and economics, explaining how the differential treatment derives from the different methods of data acquisition.

Figure 1 is the basic picture I will use for this analysis. For science to make any sense, there have to be things in the world independent of our understanding of them. These are the intransitive objects of science, to use Bhaskar’s (2008 [1975]) terminology. It is then the purpose of science to improve our understanding of those intransitive objects by creating and improving the theories and models that constitute our knowledge of those objects. These theories and models constitute the transitive objects of science. In order for science to make any sense, there must be ways of checking how well the transitive objects are consistent with the intransitive ones and improving upon that consistency. This is generally done through the two separate steps of the collection of data and the analysis of that data with reference to some set of theories/models. Both the theory and the practice of the collection and of the analysis of data are transitive features of the sciences in that they can be improved upon through better understanding of the intransitive objects they are used to study.

1.1 Data acquisition

What is the relationship of data to the phenomena we wish to study? As our knowledge of what the world is like improves, our data acquisition techniques improve as well. Improved techniques of data acquisition allow the data to have better and better traction in the world – meaning they are better connected to the real phenomena we wish to study. Data is thus very much theory dependent: it depends on our prior understanding of the nature of the phenomena it is used to probe; and it also depends on our understanding of the techniques of data acquisition and thus on our previous knowledge of the relationship between the data produced and the phenomena they are used to probe. Techniques of data acquisition are of critical importance in the experimental physical sciences, such as in particle physics, so this will be covered in more depth in Section 1.4 as background for our discussion of data acquisition in economics.

1.2 Data analysis: the big picture

The purpose of data analysis is to check and improve how well our theories/models fit with real phenomena. We are accordingly not particularly interested in explaining data, but in explaining the phenomena that the data are used to probe. Data analysis is therefore incomplete if it does not include a thorough analysis of effects and uncertainties from the data acquisition process as an integral part of comparing models to real phenomena. To exclude this analysis is symptomatic of the superficial empiricism normally found and accepted in economics. Data analysis must instead use the data to compare transitive to intransitive objects, and thus needs to consider how the data come from and relate to the intransitive objects in addition to how well models fit the data.

1.3 Measurement

In order to facilitate the comparison of empirical techniques in the physical and social sciences I follow the terminology used in *Measurement in Economics: A Handbook* (Boumans 2007). I use Figure 2 to define statistical and systematic uncertainties in measurement. In the figure, the crosshairs represent the true values, whereas the dots represent the outcomes of individual measurements of the quantity of interest. A larger random error is apparent for the measurements in Case ii than in Case i, whereas there is a clear systematic error in Case iii evidenced by the measured values clustering away from the true value.

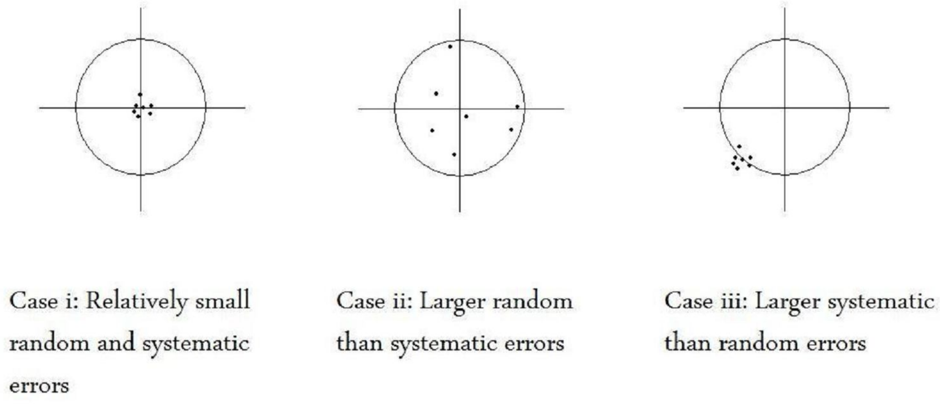

Figure 2: Random and systematic errors (adapted from Mari 2007: 63)

The problem in actual measurement is that the crosshairs are not there. However, we would still like to estimate how well we have measured the true value of the estimated quantity. This is defined as the *uncertainty* of the measurement and is broken down into two components – statistical and systematic. Since the total uncertainty is the quadratic sum of the two components, it tends to be dominated by the larger of the two. In the case of random errors, the statistical uncertainty is estimated from the spread of the individual measurements. The statistical uncertainty thus corresponds to the expected random error. Because there are statistical means for estimating the statistical uncertainty of a measurement using only the data and the given model of interest in a similar manner in both the physical and social sciences, it is relatively unproblematic for our discussion.

The big problem (and a great area of difference between particle physics and economics) comes in the estimation and treatment of systematic uncertainty, which I discuss in the next few sections. However before moving on we note that when the systematic uncertainty of a measurement cannot be or for some reason is not estimated, then the total uncertainty of a measurement is not defined, making the measurement meaningless. This amounts to superficial empiricism and is what I refer to as spinning wheels in the data without any traction in the world.

1.4 Measurement in particle physics

For the first part of this section I will rely on a good general overview of the current state of particle detection techniques, *The Physics of Particle Detectors* (Green 2000). The problem in particle physics addressed here is how to acquire data to probe real physical processes happening in the world. Note that it helps to have a laboratory setting to produce the events we wish to study in relative isolation, but the problem we focus on here is how to get data from the events so produced. As Green puts it in his first two sentences:

> The subject of particle detectors covers those devices by which the existence and attributes of particles in a detecting medium is [*sic.*] made manifest to us. The full and complete understanding of these devices requires a good understanding of basic physics. (Green 2000: 1)

Green broadly divides the techniques for particle detection into two general types: destructive and non-destructive measurements. In both cases, a detector medium is designed in order to induce particles to deposit energy in traversing it, which can be picked up and used for producing data. In the non-destructive types, the particles gradually lose energy in the detector medium so that ideally the particle is minimally affected by the energy loss. In the destructive types, the particle is ideally fully absorbed in the detector medium so that its total energy is captured for measurement. In either case, these are essentially frictional effects that need 'full and complete understanding' through 'a good understanding of basic physics' in order to be used. A quick reading of Green's book shows the variety of different frictional effects that can be harnessed in order to produce signals that can be used in measuring the various particle properties. Cerenkov radiation in particular is a very subtle frictional effect that is exploited in ingenious ways (although well beyond the scope of this analysis). The important point is that friction in physics is a collection of well-understood and optimally exploited phenomena that are used for the acquisition of data. This will be contrasted with a common misconception of friction in economics and other social sciences, as will be discussed in Section 1.5.

Continuing our discussion on systematic uncertainty from Section 1.3, we now consider how we use our knowledge of the data acquisition process to estimate our systematic uncertainty of measurement, when that cannot be done simply through the statistical analysis of data in comparison to some theoretical model we might wish to test or improve upon. All of the systems of a particle detector are rigorously tested and modelled in order to estimate all possible biases on the measurement made through the measurement process. This requires a thorough understanding of the entire data production and acquisition process, as well as possible effects of any dependencies of the measurement on input parameters that must be assumed, before a final measurement can be made. This is always done as part of a measurement in experimental particle physics: it is how it gets its traction in the world.

1.5 Friction and measurement in economics

In this section I first try to dispel a common misconception about the role of friction in physics. First, let us note that the use of the analogy of friction in physics to describe processes secondary to the main processes under consideration has a long history in economics. Indeed John Stuart Mill used the words '[l]ike *friction* in mechanics, to which they have been often compared' (Mill 1967: 330; quoted with emphasis by Blaug 1980: 64) as early as 1844 to describe how 'disturbing causes' modify the more general laws of interest. (Note that this quote from Mill was touted as an important contribution to economic methodology by an influential economic methodologist.) This discourse is still prevalent today and implies that there is no need to account for all of the effects involved in a given process, just the main ones, with the less important effects left aside analogously to how physicists are presumed to treat friction, which has little in common with how friction is actually exploited in experimental particle physics, as discussed in the previous section.

However another difference is even more disturbing. It is the idea that we can forgo any notion of true values of things in the world, or even any consideration of ontology in making measurements. In order to make this project work, Mari, for example, advocates shifting from ontological considerations to 'the *structural characteristics* of the measurement process' (Mari 2007: p. 48; emphasis in the original):

> it is precisely the fact that measurement can be characterized in a purely structural way, therefore not considering any requirement on the usage of physical devices, that leaves the issue of measurability open to both physical and non-physical properties. Accordingly, measurement is ontologically-agnostic: in particular, it does not require measurands to have a "true value", however this concept is defined, although it does not prevent and is usually compatible with this hypothesis. (Mari 2007: p. 48)

Thus if we can remove any need to measure with reference to true values, we can measure things without them, putting economics on an equal footing with experimental physics. In other words, things very well may have true values in physics, but they are totally unnecessary and thus not required for economics. Mari continues:

> Because of the mentioned shift from ontology to epistemology, Measurement Science emphasizes now *certainty* instead of truth. Accordingly, the quality of measurement is more and more conceptualised in terms of uncertainty, i.e., lack of complete certainty on the value that should be assigned to describe the object under measurement relatively to the measurand, thus acknowledging that measurement is a knowledge-based process. (Mari 2007: p. 64)

These ideas are further developed in a footnote on the same page:

> My opinion is that Measurement Science is currently living a transition phase, in which the historically dominant truth-based view is being more and more criticized and the model-based view is getting more and more support by the younger researchers. On the other hand, the truth-based view is a paradigm that benefits from a long tradition: the scientists and the technicians who spent their whole live [sic.] thinking and talking in terms of true values and errors are fiercely opposing the change. (Mari 2007: p.64 – footnote 4)

Mari implies that there is a radical change in conceptualisation underway in measurement, even in the physical sciences, whereby reference to true values is being abandoned – signalled by the change from the use of the word 'error' to the word 'uncertainty'. For a reality check we can look at the standard reference for particle physicists, the *Review of Particle Physics*, which is made available both online and in a condensed booklet format by the Particle Data Group (Yao et al. 2006). Immediately on the first page of the section on 'Statistics' under 'Mathematical Tools' in the part on 'Reviews, Tables, Plots 2007' we find the clarification: 'Following common usage in physics, the word "error" is often used in this chapter to mean "uncertainty"'(http://pdg.lbl.gov/2007/reviews/statrpp.pdf). Indeed, the terms 'error' and 'uncertainty' have been used interchangeably in particle physics for decades now without any loss of commitment to the concept of 'true values', which remain the point of reference throughout the above review and in particle physics in general. There is no indication that the concept of 'true value' is in any sense dispensable in particle physics.

While there have been recent books dealing with current problems in physics (Smolin 2006; Woit 2007), these problems have come about entirely because the programme of particle physics has performed so well within the range of experimental reach that any new physics is beyond it. Despite this, I do not believe that physics has given up on trying to understand the ontology of the physical world. So whoever Mari's 'young researchers' are that are at the vanguard of change in the standard conceptualisation of measurement, surely they do not come from experimental particle physics. Measurement in particle physics ain't broke and there is no need to fix it. Economics, however, *is* broke. According to Tony Lawson and like-minded realists, economics as a discipline is in serious trouble. Not surprisingly, his assessment of the problem with economics is precisely the flight from ontology that Mari espouses (Lawson 1997; 2003; 2007).

Let us take another example from the *Handbook* on *Measurement in Economics* to demonstrate how economics starts with physics and then loses its traction. Fixler states very clearly: 'In the natural sciences, the true value is well defined and the assessment of accuracy usually involves the construction of measuring instruments to compare an estimate with the true value' (Fixler 2007: p. 416). Thus (perhaps contradicting Mari) in the case of natural science, true values are well-defined and therefore not subjective. From there his wheels begin to slip: ‘In the case of a measure of economic activity, the notion of the true value is more subjective and therefore more difficult to define and quantify’ (Fixler 2007: 416). To say that true values are subjective would be obvious nonsense; so instead it is just the *notion* of true values that is *more* subjective. (Being *more* subjective than a non-subjective quantity perhaps makes the ‘more’ unnecessary.)

Fixler explains:

> The notion of measuring economic activity requires the use of economic theories to form the conceptual foundation for the measurement concept and the production boundary for the economy. Both provide the context for defining the “true” value of economic activity that serves as the basis for gauging accuracy. The production boundary limits the set of activities that are deemed admissible to the measure. For example, though household production is clearly an important economic activity, it is treated as outside the boundary of aggregate economic activity measures such as Gross Domestic Product (GDP). Such exclusions apply to a host of non-market transactions (Fixler 2007: p. 417).

Here it is economic theories that form the basis of what true values are instead of true values providing a basis for the evaluation of economic theories. Evaluators of the theory can thus ‘deem’ what fits within the scope of economic activity and leave out anything that does not conform to an arbitrarily defined market transaction.

Fixler continues:

> In addition, national economic accounts are designed to provide a system yielding a measure of aggregate production or aggregate economic activity, as well as ways of measuring the component parts. The acceptance of these measures, which is based on a perception of their accuracy, relies in turn on the acceptance of the system. If decision makers had no confidence in the conceptual foundations of the system upon which the estimates are based then it would be meaningless to talk of accuracy – regardless of the statistical properties of the estimates. Manuals such as the one for the United Nations System of National Accounts and other standardizations of techniques provide imprimaturs of general acceptance that in turn provide the aura of objectivity necessary to perceptions of accuracy and confidence in the estimates. (Fixler 2007: p. 417)

Measurement in economics is thus all about ‘perceptions’ and ‘auras of objectivity’ based on statistical conservatism and ‘acceptance of the system’. Just to be clear, to say that measurement is theory dependent is not controversial. But when you begin assuming that the things out there to be measured depend on your theory, that’s when the wheels begin to slip. Fixler is well aware that physics does not have this problem. Social science does. This is precisely the problem of social ontology and it cannot be avoided. A possible way to deal with the problem is now discussed in Part 2.

2 A narrative approach

In this part of the paper I discuss a research programme in economics designed to have traction in the social world. Since much of this analysis has been published in greater detail elsewhere (Turk 2007) I will only briefly review some essential parts. Let us return for reference to Figure 1 where we note that experimental particle physics has a well-developed theory and practice of data acquisition that is taken into account in the methods of data analysis; and that this is done in order to enable an accurate estimate of the systematic uncertainty of a measurement, giving it traction in the world. On the other hand, contemporary approaches to measurement in economics tend to downplay ontology, focusing only on epistemological considerations in measurement. The methods are therefore limited to a focus only on data and models, largely ignoring the nature of the social world, how the data comes from it and how well the measurement goes back through the data to really existing quantities. The approach I will propose here is thus focused on accepting the nature of the social world and considering ways of acquiring data with traction there. In order for economics to have any traction in the real world, we need to develop ways of using interview material to allow our theories to come into contact not just with data, but with real world phenomena made tangible through data; thus to have any practical use they must do more than merely explain data – they must explain some real phenomenon in the world. We must also expect that since the social world is qualitatively different than the physical world, the methods we invent to study it are also likely to be different.

2.1 Realist social theory

In order to get anywhere we have to begin with a basic social theory that can be improved as our analyses and research methods improve. A good place to start is Margaret Archer's (1995; 2003; 2007) *Realist Social Theory*, which works well within Bhaskar's (2008 [1975]) *Realist Theory of Science*. However, regardless of what we take as our basic social theory, the social world is what it is. It is the theory we wish to improve in order to better capture social reality. If this sounds circular it is certainly no different than in experimental particle physics, where 'a good understanding of basic physics' is required for the 'full and complete understanding' of the devices needed to produce the data to do physics (see Green 2000: 1).

The beauty of Archer's theory is that it provides a natural way of acquiring data with real traction in the social world, as well as apparently otherwise being quite compatible with the way in which the social world we know works. We therefore begin with a brief recounting of the basics of Archer's realist social theory, where she stresses the necessity of fully accounting for the *deliberated* actions of agents within their social context:

> Fundamentally, we cannot account for any outcome unless we understand the agent's project in relation to her social context. And we cannot understand her project without entering into her reflexive deliberations about her personal concerns in conjunction with the objective social context that she confronts.
>
> Indeed, it is what agents seek to do, the precise projects that they pursue, which are responsible for the activation of the causal powers of constraint and enablement otherwise, structural and cultural properties which are constitutive of situations remain real, but their causal powers are unexercised. Yet once an agential project has activated a constraint or an enablement, there is no single answer about what is to be done, and therefore no one predictable outcome. Conditional influences may be agentially evaded, endorsed, repudiated or contravened. Which will be the case and what will be the outcome only become intelligible by reference to the agent's own reflexive and therefore internal deliberations (Archer 2003: 131).

With this in mind, she then provides us with a succinct summary of the dynamics of realist social theory:

> … the process of mediation between structure and agency must be considered as entailing three stages, which capture the interplay between objectivity and subjectivity, as follows:

(i) Structural and cultural properties *objectively* shape the situations which agents confront involuntarily, and possess generative powers of constraint and enablement in relation to

(ii) Agents' own configurations of concerns, as *subjectively* defined in relation to the three orders of natural reality – nature, practice and society.

(iii) Courses of action are produced through the reflexive deliberations of agents who *subjectively* determine their practical projects in relation to their objective circumstances.

> Taken together, these three propositions seek to capture the interplay between the objective and subjective components of the mediatory process, whereby structural and cultural influences condition agential doings. Obviously, the last thing that such an account attempts to do is to transcend the difference between objectivism and subjectivism, precisely because it respects the independent causal powers possessed by both structures and agents, and usually exercised by each to some degree. In interplay with one another they determine the practical courses of action adopted by agents […], whose own interaction is ultimately responsible for the reproduction or transformation of society – or a sector of it (Archer 2003: 135).

Archer thus proposes the *internal conversation* as the real causal mechanism mediating between structures and agency. The reason this works well for us is that the internal conversation can be brought out for study. Indeed she does this in her books *Structure Agency and the Internal Conversation* (2003) and in more depth in *Making Our Way through the*

World (2007). In the next section we discuss using narrative interviews as a way of accessing the internal conversation, thus producing data directly associated with the deliberative processes responsible for actions taken by agents.

2.2 Narrative interviewing as data acquisition

Once we have a workable realists social theory as outlined in Section 2.1, we need an adequate theory and practice of data acquisition that is compatible with our basic understanding of social reality, we would like to use our data to improve upon. Of course, the social world is much different than the world of particle physics. Most interactions between people are mediated through verbal exchanges in a common acquired socio-cultural context as opposed to the exchange of the mediating gauge bosons of particle physics (the carriers of the known forces in nature). Whatever information remains from our human interactions is mostly carried in the memories of the direct participants to the social phenomenon in question. Our data acquisition technique must thus be optimally geared to accessing the memories of the participants to social interactions in a way that best preserves the way the relevant data is stored in human memory.

In this regard, biographic-narrative interviewing is a rapidly growing area of research (for an overview of biographical research see Chamberlayne, Bournat & Wengraf 2000). A good approach to take here is the interview technique of the Biographic-Narrative Interpretive Method (BNIM). The method requires strict adherence to the principle of uninterrupted narrative and non-interference by the interviewer. Wengraf (2001) explains:

> As for the interviewing part, … its characteristic is that the interviewee's primary response is determined by a single question (asking for a narrative) which is not followed-up, developed, or specified in any way during that subsession. In this first subsession, after the posing of the initial narrative-seeking question, interventions by the interviewer are effectively limited to facilitative noises and non-verbal support. Any other type of intervention effectively terminates the session with extreme prejudice to the research purpose of the BNIM interview. …
>
> This makes it rather distinctive. One way of understanding the philosophy behind a minimalist-passive reception of interview narrative is that of the *Gestalt* principle, … which requires the spontaneous pattern of the speaker to complete itself fully and so be fully exposed for analysis. (Wengraf 2001: 113)

The recorded material from such an interview are as free as possible (although never completely) from the influence of the interviewer, allowing a data set as free as possible from bias. The interviewee is encouraged to produce a narrative that in principle reflects the working of their own internal conversation in action as it mediates between subjectively perceived objective structures and the personal projects of the particular agent. As this area of research is developed, we can expect the theory and practice of data acquisition from the social world to have better and better traction in that world, just as a better understanding of basic physics leads in turn to better data acquisition techniques in particle physics.

2.3 BNIM analysis for using data to compare models to social phenomena

The final piece to a narrative form of social science is a developed theory and practice of using narrative interview data to improve theories, models and general knowledge about what really happens in the world. The essence of the method we propose here is to gain access to Archer's internal conversation. Ideally one would like to listen in on that internal conversation and witness how agents use it in manoeuvring within and reproducing or restructuring their social context. Since this is difficult to do in practice, what we settle for is a reproduction of that conversation in as pure a form as possible, comparing the narrative as it is told to what can be learned as objectively as possible about the lived life of the narrator; and then examining the influence of the told narrative – as presumably a reproduction of the operative internal conversation – on both the lived life of the narrator and the objective changes in the social context. The BNIM approach thus focuses on separate analyses of the twin tracks of (subjective) told story and (objective as possible) lived life, examining the interplay at a later stage of analysis. This approach dovetails well with Archer's concern not to transcend the difference between the objective and subjective, but to capture their interplay, respecting the independent causal powers of agents and social structures.

Unfortunately, the past is always recalled from the present perspective, which necessarily complicates analysis of the operation of the past internal conversation in context. Wengraf (2001: 285) addresses the problem with the assumption 'that the perspective on the past that I have now (a) is not the same as I had in the past, but (b) that it has emerged from the past in an intelligible way that I am attempting to reconstruct'. Furthermore:

> One task of the researcher into the life history is to attempt to reconstruct what may be several phases in which the retrospective perspective of the individual changed, in order to understand through what history of lived experience the present retrospective perspective came to be formed. A narrative constructed by the researcher about that evolution is called 'the (or 'a') BNIM case-history'. (Wengraf, 2001: 285)

A very complex analysis is thus involved in using present narratives and historical sources to try to untangle the past interplay between agents and the evolving structures they lived within and helped mould.

And once the analysis of individual cases has been completed and a good understanding of the interplay between internal conversations and external structures has been attained for these individual cases, the question remains: What have we added to social science? Given that this is an endemic problem in qualitative research, it must be addressed. Although the methods we propose here cannot yield a mathematical description of the underlying laws of economics, we do not pretend that those laws are there to find. Realist social science fully embraces the messiness and difficulty of social reality. What we propose is something akin to the constant comparative method (Glaser & Strauss, 1967) discussed by Wengraf (2002) as a way of bringing added value to social science from the use of BNIM research. This entails a drawn-out process of comparing theory to cases, cases to cases and cases to theory, constantly and iteratively refining each in light of the other. Theory will remain historically contingent, as it must. Indeed, as Archer (1995: 344; italics in the original) argues:

> Practical social theorising cannot avoid the work of producing … a narrative each and every time the aim is to explain why things structural, cultural or agential are so and not otherwise, at a given moment in a given society. These analytical histories of emergence are explanatory, retrodictive and corrigible accounts. Therefore analytical narratives cannot be 'grand' since the need to narrate arises *because* contingency affects the story and its outcome; they can never be unanalytical because what is narrated is the interplay between necessity and contingency; and they cannot be purely rhetorical because they are avowedly corrigible, dependent upon the present transitive state of knowledge and revisable in the light of new scholarship.

This is unfortunately all we expect from realist economics as well.

2.4 A practical example from Slovenia

Before concluding we should note that the above realist approach to economics does have real world applications. It is now being used in a research project to understand the remarkable development of Slovenia during the socialist period and during its aftermath (see Turk 2008). This analysis uses the narrative interviews of numerous Slovene directors of major companies under socialism to help understand how Slovenia could develop so well, and managed to avoid the pitfalls of the Shock Doctrine (Klein 2007) in the transition period.

In our analysis we will made use of Easterly's (2006) distinction between 'Planners' and 'Searchers':

> Planners apply global blueprints; Searchers adapt to local conditions. Planners at the top lack knowledge of the bottom; Searchers find out what the reality is at the bottom. … A Planner believes outsiders know enough to impose solutions. A Searcher believes only insiders have enough knowledge to find solutions. (Easterly 2006: 6)

Thus in addressing the failure of Western Planners to impose functioning markets according to some standardised blueprint, Easterly considered:

> how introducing free markets from the top down is not so simple. It overlooks the long sequence of choices, institutions, and innovations that have allowed free markets to develop in the rich Western economies. It also overlooks the bottom-up perspective on how markets often *don't* function well in the low-income societies of Africa, Latin America, Asia, and the former Communist bloc. Markets everywhere

> emerge in an unplanned, spontaneous way, adapting to local traditions and circumstances, and not through reforms designed by outsiders. The free market depends on the bottom-up emergence of complex institutions and social norms that are difficult for outsiders to understand, much less change.
> Paradoxically, the West tried to *plan* how to achieve a *market*. Even after evidence accumulated that these outsider-imposed free markets were not working, unfortunately, the interests of the poor did not have enough weight to force a change in Western policy. Planners underestimated how difficult it is to get markets working in a socially beneficial way. People everywhere have to explore with piecemeal, experimental steps how to move toward free markets. (Easterly 2006: 60-61; emphasis in the original)

Accordingly we consider directors in socialist Slovenia as people forced into the role of Searchers in a complex and evolving socialist system, where the market played an important role. Socialist Slovenia was indeed not without its own share of republican and federal bureaucratic (Party) Planners. However, following Easterly, we were *not* interested in whether socialism or capitalism is a preferable conceptual approach to development. We were *not* interested in capitalism versus socialism per se, but rather the distinction between imposing abstract plans from above and searching for small-scale solutions that work in a given context at the operational level. The difference is that whereas Easterly focuses on the failure of *capitalist Planners*, our focus was on the relative success of *socialist Searchers/entrepreneurs*. We wanted to know how the directors tasked with working within that bureaucratic framework functioned and what freedoms they had in searching for ways to develop their own parts of that larger system. We therefore let the directors tell their own life stories. The result is a complicated story of cooperation and competition, solidarity and illegality with a relatively favourable outcome so far.

Conclusions: Is this science?

What I have argued is that following Bhaskar, in order for science to make any sense at all, there have to be intransitive objects which are what science studies, as well as the transitive objects that are created by scientists in order to further understanding of those intransitive things. Economics has serious problems in furthering our understanding of the intransitive objects because of its current aversion to ontological commitments. Instead it is currently spinning its wheels in slippery data because it does not fully deal with how its data relate back to the social world. I have discussed an alternative narrative approach to economics that is yielding results in helping to understand one particular case of development in a former socialist country. The very serious problem of this approach is that it lacks the trappings of what we take to be hard science: mathematically precise models. On the other hand, mathematically precise models are arguably inappropriate for economics and other social sciences. A more important concern is for the knowledge, models and theories we develop to have real purchase in actually existing human social systems, and this requires acquiring data with traction there. I believe that this is a fundamental issue of science that requires further development.

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