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Getting the FCC auctions straight: a reply to Nik-Khah

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Eddie Nik-Khah and I have had several very interesting and constructive exchanges during the last couple of years, and my impression was that only relatively minor disagreements remained. Reading his paper in this journal (Nik-Khah 2006) largely confirmed this impression, but a couple of important issues are still worth discussing. They have to do with the so-called “performativity” approach in the (new) social studies of economics, and with the differences between this approach and more traditional studies like Nik-Khah’s.

Before I get to that, however, let me summarise briefly the state of art. In 2001 I published an article in the journal *Studies in History and Philosophy of Science*, focusing on the design of the auctions for spectrum licences for mobile phones and similar devices, run in the mid-nineties by the Federal Communication Commission (FCC) in the US. That paper was part of a larger project on the methodology of experimental economics, a booming research programme that is changing the landscape of mainstream economics. I found the auctions case fascinating mainly for two reasons: first, because the economists involved seemed to tackle effectively the most challenging problem faced by experimental economics, namely the “external validity” of their laboratory results. If markets in the real world are admittedly rather different from those that experimenters study in their laboratories, how can experimental results be extended to real-world circumstances? The obvious thing to do is to change the world so as to make it more similar to the lab conditions where our “toy” markets seem to work so well. I say “obvious” because this is what physicists, molecular biologists, etc. do all the time;¹ only social scientists are for some bizarre reason expected to predict what happens in extraordinarily complicated and uncontrolled circumstances.

The second fascinating feature follows quite naturally: experimental economics and its applications resemble rather closely the “sciences that work”. Despite some valiant attempts, the incursions of STS (Science and Technology Studies) scholars in economics always conveyed the distinct feeling that they were dealing with dodgy science.² This is easily explained: when the “materiality” of science (apparatus, materials, data) offer only minimal constraints – as they do in most economics – political interests, personal authority, rhetoric, and so forth are bound to play a larger role in the determination of scientific and technological outcomes. In contrast, when economists operate in the laboratory and enjoy some of the powers of manipulation, negotiation, etc. that are granted to natural scientists, they look much more like “good” scientists (surprise, surprise!).

These were the intuitions behind the 2001 article, which was constructed around a simple argumentative structure: philosophers take natural science seriously because the natural sciences “work”. In order to have a serious philosophy of social science, we must look at social science that “works”. So let’s look at one of the few applications of rational choice theory that are almost universally hailed as a big success. Now it turns out that achieving such a success was an incredibly messy process, and it is not even clear how rational choice theory could legitimately get all the praise for the final product (the FCC auctions). If the auctions “worked” it wasn’t certainly thanks to game theory only. Moreover, even if the rational choice approach broadly conceived (theory plus experiments) “works”, it does so in very special circumstances – those circumstances that have been artificially created so as to make it “work”.

This line of argument had many possible (and in my view quite obvious) targets: theory-based philosophy of social science (especially the philosophy of rational choice theory); attempts to hail the FCC auctions as a success for game theory, where in fact they were a typical example of a theoretically non-tractable problem; attempts to present the auctions as a demonstration of the power of the rational

choice approach, where in fact it demonstrated how narrow its domain of application is likely to be; but also instrumental interpretations of the theory (*à la* Chicago), because the FCC design process demonstrated the importance of interpreting realistically many important features of rational choice models.

I have put “works” between quotation marks because I want to be neutral about what it means for a scientific research programme to work. The main point is that in the FCC case economists engaged in something resembling proper science (or technoscience). The reader is free to interpret it as he/she likes: “scientificity” is a complex thing, and it does not necessarily mean “science as advertised by the scientists themselves” or “science as conceived by a logical positivist philosopher”. It may well mean “science as STS scholars have described it”, in all its richness and complexity. This does not mean that I subscribe to all the claims that STS scholars routinely make about science, either – I am probably more conservative on philosophical matters than many sociologists. The point is rather that my argument was predicated on minimal assumptions, and indeed could even be introduced by a big “if”: *if* this is the most successful application of game theory to date, well, look how limited game theory turns out to be.

Much of this was admittedly implicit in that paper, but I think that most readers got the message. In fact the paper attracted a remarkably wide audience, well beyond my expectations. The fact that sociologists like Michel Callon and Donald MacKenzie found it interesting speaks volumes, in my view, about the rather weak assumptions lying behind that article. The fairly vague gestures that I made towards performativity also helped of course. At the time of writing the article I had long conversations with my senior colleague Barry Barnes on the ontology of social reality, which explains the brief remarks on reflexivity and self-reference at the end of the article (Guala 2001, pp. 474-5). If I remember well, I had just completed a semi-final version of the paper when I had a chance to talk to Donald MacKenzie, who was then in the early stages of his project on the economics of finance. I had also read Michel Callon’s (ed. 1998) volume, and even participated in a workshop organised to launch the book, but at the time I spectacularly failed to appreciate the interesting aspects of his project. At any rate it was not totally by chance that those brief remarks appear in the article, even

though the article was not primarily intended as a contribution to the budding performativity literature.

This is only to highlight the serendipity of the whole story, *not* to say that I disown those remarks. Quite the contrary, I have since then become more involved in the debate on performativity, which I have found increasingly interesting and promising (indeed I hope to do more research in the ontology of social science in the future).

Apologies for the overly autobiographic style so far: I promise that it’s relevant for understanding the following remarks on Nik-Khah’s paper. Nik-Khah presents his paper as a critique of the “performativity account” of the FCC story, when in fact it’s at best a critique of my 2001 account. As I will show shortly, there isn’t a single element in his story that refutes or is incompatible with the thesis that performativity is an important phenomenon in economic engineering and market design. The only target hit by Nik-Khah is the “Research & Development” (R&D) narrative of the FCC auctions, which constitutes the main thread of most “official” accounts and is also implicit in much of my 2001 article. The R&D story begins with the government exogenously setting a set of goals, and proceeds with the FCC and academic economists working together to find a solution that will achieve these goals, given a series of (partly predictable, partly unexpected) obstacles and constraints. Nik-Khah does an excellent job at showing how this narrative overlooks an important part of what happened. In particular, it does not shed light on how the goals set by the government were modified, indeed almost entirely changed, by the FCC and the consultants so as to fit their agenda. The other aspect of the story that is left in the dark is the role played by the Telecom companies in directing the consultants’ work, and hence the format of the auctions.

Performativity has got nothing to do with this. Changing the goals into something that you think you can deliver is indeed a very sensible thing to do, from the point of view of a “performing” engineer. This is a confirmation, not a refutation of the performativity view. So when Nik-Khah writes that “the performativity narrative ... gives the impression that the goals for the auction were propounded independent of the process” (2006, p. 16), one should replace “performativity” with “R&D”. Ditto for a number of other similar claims.³

Highlighting the limitations of the R&D narrative is Nik-Khah's main novel contribution, and a genuine improvement on previous accounts of the FCC enterprise. Let me just mention that I very much welcome this sort of work: given the rather special target of my article, I never thought that it would provide a complete story. From what I have seen of his unpublished PhD dissertation, Nik-Khah is well-positioned now to write the definitive account of the FCC case and I'm looking forward to read it. (I assume that his paper in this journal is just an appetizer.) Having said that, I have a few complaints and a more general critique.

Complaints: too often in the paper Nik-Khah puts in my (and Callon's, and MacKenzie's) mouth some claims that none of us would ever subscribe to. Some examples: MacKenzie and I are accused of believing "that economists' game theoretic accounts of auction theory dictated the format of the auctions adopted, and therefore rendered the economists' theories 'true' by construction" (Nik-Khah 2006, p. 15). There are two claims here. The second one (economic theories becoming true by construction) is at the centre of the performativity thesis – which I will discuss below. The first claim (game theory dictating the auctions) is a crude version of technological determinism and sounds plainly ridiculous, if attributed to a sophisticated sociologist of science like MacKenzie. Even I am not so stupid to claim anything like that; indeed as already mentioned, a key thesis of my 2001 article is that game theory dictated absolutely nothing on its own, and other actors had to enter the scene to fix the auctions design.⁴ Even the economists who participated in the design enterprise recognised that existing theory was unable to give any precise advice! Yet, Nik-Khah is keen to present his own story against the background of an "official" deterministic hagiography. It's a pity that he's just bashing a straw man.⁵

Elsewhere one has the impression that we are guilty of "hoping to ground controversial public policy in uncontentious science" (p.16). But again, I cannot think of a science that is more contentious than economics, and both Callon and MacKenzie have been preaching for years the STS mantra that *all* science is contentious to some extent – despite scientists' continuous attempts not to make it look so.

I also disagree with Nik-Khah on a few matters of empirical detail. One is the role played by experimenters in the design of the auctions. According to Nik-Khah, experimental economists entered the scene rather late, and were mainly called in to solve a technical problem with the computerization of the auctions. Nik-Khah recognizes that they ended up playing a more substantial role than that, but still, like in the case of academic game theorists, he sees their contribution as largely overdetermined by the Telecoms' influence. In order to sustain this argument, Nik-Khah presents the two competing designs (the combinatorial vs. the continuous ascending auction design)⁶ as equally promising (or unpromising) in light of the available scientific evidence, with supporters lining up for one solution or the other entirely for political reasons (p.17). The real story, in my view, ought to be a bit more complicated. John Ledyard, Professor of economics at Caltech, had been a supporter of the combinatorial design right from the start. This is not surprising, given that combinatorial auctions were Ledyard's "own stuff", from well before his involvement in the FCC business: in other words, he was probably just lobbying for his own academic interests. The interesting part of the story is that at a conference held in January 1994 Ledyard recognized publicly that, in light of the experimental tests done at Caltech, the ascending auction design advocated by Paul Milgrom and Preston McAfee was easier to implement and could be reasonably expected to deliver all the goods of a combinatorial design (Kwerel 2004, p. xx).

This contrasts with Nik-Khah's story, where the struggle between the two parties is resolved "from above" by John McMillan, a game theorist working on behalf of the FCC. As a matter of fact, it seems that experimental and theoretical considerations did play a role in declaring one solution superior to its main competitor. There may have been of course some kind of political negotiation or gentleman's agreement behind the scenes, which would explain Ledyard's public acknowledgement. But unless we have some evidence of that, it's hard to say. Moreover, the "purely political" version of the story does not explain the sudden resurgence of theoretical and experimental work on combinatorial mechanisms right after the first round of auctions, as evidenced for example by the conferences organised by the FCC in the late nineties and in the first years of the new millennium. A plausible explanation in my view is that *some* R&D considerations did play a role in the design of the FCC auctions.

Almost everybody agreed that *in principle* it would have been desirable to have a combinatorial market; unfortunately, nobody had a precise idea of how to run it, and given the tight deadlines, there simply was no time to develop and test a reliable auction mechanism of that kind. It was therefore decided to go for the second best and implement a simpler design that was likely to do the job. For the future, however, the interesting task (academically, scientifically, and from a policy point of view) was to construct combinatorial auctions. Unless a radical change in market power has taken place since the mid-nineties, the subsequent rise in prominence of combinatorial bidding is pretty inexplicable from Nik-Khah's perspective.

Notice that this is not meant to suggest that Nik-Khah's story is entirely wrong; on the contrary, I believe it is largely *right*. My point is more general: it is unlikely that a complex story like the FCC's can be adequately captured from a single perspective, because it is unlikely that one single set of factors (political interests, the Telecoms' interests, theoretical and empirical considerations) can explain all the twists and turns in the story. I'm rehearsing a very traditional point of view here: it's a cornerstone of the Strong Programme in the sociology of science that every major scientific or technological event is always causally co-determined by a number of factors. "The strong programme says that the social component is always present and always constitutive of knowledge. It does not say that it is the only component, or that it is the component that must necessarily be located as the trigger of any and every change: it can be a background condition" (Bloor 1976/1991, p. 166). This is what philosophers, following John Leslie Mackie (1974) call an INUS view of causation: a cause being an insufficient element in a set of jointly sufficient conditions for the instantiation of an event (or effect). Just like the breaking of the window is caused by a set of factors that includes my kicking the ball, the blowing of the wind, the fragility of the glass and so on, a given scientific achievement is usually determined by a complex set of social, cognitive, economic, political (etc.) factors. Depending on the explanatory context, we may choose to highlight one factor rather than another for pragmatic reasons, or simply because that factor is actually more prominent in one particular instance. This is why I see stories like the one told by Nik-Khah as largely (although not entirely, of course) complementary with stories focusing on

cognitive, empirical, or theoretical considerations like the one that I told.

However, this is not how Nik-Khah sees it, as is even clearer from a forthcoming paper co-authored with Phil Mirowski (Mirowski and Nik-Khah, forthcoming). Even though, as I have argued above, the performativity approach is left untouched by Nik-Khah's story, it is singled out as the main target of criticism. Performativity is attacked not because it leads to a substantially incorrect account of some episodes in the history of economics, but because it may divert attention away from the traditional themes of STS, which Nik-Khah and other scholars find more interesting and useful for their own project – a project that uses STS methods to criticize neoclassical economics.

We are getting here at some core foundational issues in STS, which have been repeatedly discussed over the years. Instead of trying to resolve them (which would be ambitious to say the least), I'll just try to highlight where I think the disagreement lies. The key point of contention is the symmetry thesis (Bloor 1976/1991): the idea that sociological studies of scientific knowledge do not necessarily debunk science, for the very reason that sociological explanations apply to both good science and pseudo-science. Although not all STS scholars subscribe to the letter of the symmetry thesis, I do think that it's one of the foundational principles that should not be prematurely tossed in the bin of history. Unfortunately symmetry cannot be endorsed by scholars who have a stake in the scientific field, which is why, I think, it does not inform the work of STS scholars in economics.⁷

What's performativity got to do with this? Performativity suggests the dangerous idea that economics can be made to work (in some circumstances, some of the time), and that entities like those described by economic theory (efficient markets, *homi oeconomici*) can be made to exist. Performativity, to be sure, comes in degrees⁸: at the weakest level, it amounts to the proposition that economics matters. Few people, I believe, would disagree with this. Surely the FCC auctions would have taken a rather different shape, had the relevant economic theory been different (remember the INUS view of causation above). A stronger version claims that markets are reformed so as to instantiate the conditions (institutions) postulated by economic theories, models, or experiments. At the strongest level, finally, we find the claim

that economic theory shapes the actions of the agents in the market. The strongest version is also the most controversial, but I don't find it particularly shocking in some of the cases examined in the literature so far; game theorists after all advised Telecom companies during the bidding for FCC licences, while stock brokers used mini-versions of the Black-Scholes model to calculate the price of options (MacKenzie, forthcoming).

Despite all the provisos (in some circumstances, some of the time...), this is unacceptable for the critics of neoclassical economics. Economics must be denounced as a dodgy, ideological, dangerous, and ultimately failing enterprise, that for contingent reasons is achieving unprecedented and unjustified prominence in the socio-political arena. Not having a particular stake in the economics profession, I'm not terribly opinionated on such matters. Here I just want to mention that the failure of mainstream economics within the narrow conditions highlighted by the performativity studies has to be demonstrated independently. I cannot find in Nik-Khah's whole article a single argument in support of that conclusion; only a promise, in a footnote (note 16, p. 2), that the arguments are to be found elsewhere (in his unpublished work). This happens, I believe, for very good reasons: to demonstrate the real efficiency (or inefficiency) of the FCC auctions is a terribly complicated business. We do not know what the real underlying values (or even valuations) of the licences were, and we can only try to infer by means of complicated ex-post analysis of data.⁹ The point anyway is that we need a scientific argument, not an STS case study, to settle this issue (remember the symmetry principle).

I recognize that my own remarks in the 2001 article can be read as an enthusiastic endorsement of the efficiency of the auctions, contrary to what I've just said. In partial defence, let me just point out that a substantial portion of that paper was devoted to outlining the strategy used by Charles Plott and other experimental economists to argue that the mechanisms tested in the laboratory had been successfully exported in the real world. This is one of the few areas where my article (and later my 2005 book) was probing an uncharted territory. To date, there simply is no good story about external validity inferences, how they may fail or succeed. So I can't be sure that I (or Plott and his associates) got the inference right. More modestly, I say that this is

about the most convincing argument to be found in the scientific arena (in and out of economics) to support an inference from the lab to the real world. The proof that the auctions were approximately efficient *in the laboratory* is as strong as scientific evidence can be. If that isn't worth calling "knowledge", then nothing is.¹⁰ The inference from the lab to the real world is as strong as any other inference of that kind that I have come across. That's it. Of course some argument to the contrary can (and perhaps will) be put forward; but it will be a scientific argument, to be assessed according to scientific (as opposed to STS) standards.

I hope this clarifies some of the confusions and helps identifying what is really at stake. Unless we achieve more clarity, we will continue to read conclusions like the one that closes Nik-Khah's article: "the enthusiasm for the doctrine of performativity is fostering a situation where science studies will come to increasingly resemble neoclassical economics, if not serve as its cheerleader" (2006, p. 19). This is quite ridiculous and I hope it will be evident to everybody. But surely Callon, MacKenzie and the other proponents of the performativity approach within economic sociology don't need my help to defend themselves from these silly accusations.

Endnotes

1 This is a surprisingly neglected point in "standard" philosophy of science. One exception (and direct source of inspiration in my case) is Nancy Cartwright's work (e.g. Cartwright 1999). In *Science and Technology Studies*, this aspect of "technoscience" has been repeatedly highlighted by Bruno Latour (e.g. 1988) and others.

2 See for example Evans (1999).

3 "A performativity account might attribute the lack of a determinate recommendation to the essential inadequacy of abstract theory" (p. 17), "the performativity narrative informs us that the FCC sets the goals for the economists to attempt to achieve" (p. 19), and so forth. Always replace "performativity" with "R&D", and you will get these straight.

4 Of course Nik-Khah introduces *more* actors in the story, but that doesn't justify a blatant misrepresentation of the work of others.

5 The same straw man appears, less directly, at p. 19 ("Game theorists and experimenters were not ... seeking to bridge the inevitable gap between pure science and its applied context").

6 There was also a third “hybrid” design advocated by the FCC, which however was never a serious competitor.

7 This opens another set of tricky issues regarding the status of the history and sociology of economics, especially whether it should become a sub-field of STS, or continue to defend its (small and shrinking) stronghold inside economics departments.

8 For some attempts to distinguish different versions of the performativity thesis, cf. MacKenzie (forthcoming) and Guala (forthcoming).

9 See for example Cramton (1998) for one such attempt.

10 Notice that I’m still subscribing to the rich notion of “scientific knowledge” outlined above.

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