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Comment on "Excessive Ambitions" (by Jon Elster)

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Jon Elster's analysis of the current state of social sciences, and particularly of economics, is welcome in many respects. Elster's situation, as an 'informed outsider', allows him to offer an extremely interesting perspective - an exhaustive and intelligent overview free of the vested interests that tend to bias insiders' views. Moreover, Elster's text reminds us of some basic but crucial (and somewhat overlooked) methodological rules and requirements; his approach is built on sound and robust epistemological foundations. Lastly, he provides the profession with an excellent opportunity to open a necessary and long overdue reconsideration of some of its basic principles. As such, it is a useful and important contribution.

While I agree with most of Elster's arguments, I will, somewhat provocatively, submit that they may actually support the opposite conclusion. That is, a serious problem with current economic practice is probably more a *lack* of ambition than an excess of it. I will argue that in many respects, we have lost track of some basic methodological requirements of our discipline, and we have settled for the easy way out instead of looking for the hard but correct one. I will first suggest a reinterpretation of some of Elster's (well taken) examples along these lines, focusing on three aspects. I will then briefly discuss the issue of bounded rationality.

1. Insufficient ambition

Robustness

In principle, robustness should be a key concern of economic theory. One of the main advantages of the mathematical approach widely adopted by the profession is precisely that it allows (and actually requires) a very precise statement of the assumptions on which an argument is based. Most of the time, as Elster rightly remarks, these assumptions are quite strong: production functions are linear or at best CRS, utilities are quasilinear, probability distributions are uniform, etc. There is no problem with such simplifications; considering an elementary, 'bare bone' setting is a natural first step, and most fundamental ideas were initially introduced in an easy framework of this kind. But the next question is, or should be, whether the intuitions that have been elaborated in a simple model survive in more realistic settings. Such generalizations typically require very detailed investigations, and may often be more technically demanding than the initial contribution. In this respect, the profession's record varies across (sub)fields. To take only one example, the basic models of asymmetric information à la Spence, Akerlof, Rothschild-Stiglitz and others have been extended in several directions; it is fair to say that we now have a good vision of the robustness (and the limits)

of their basic insights.¹ That said, the picture is not uniformly rosy. Too often we see ambitious and sometimes extreme policy recommendations put forth with considerable strength and the alleged authority of economic theory, while they are based on simplistic models, the robustness of which is dubious at best. This propensity to take for granted the conclusions of highly simplified model without wondering too much about their exact domain of validity is probably a serious problem for the profession, and Elster is certainly correct to denounce it; but if anything, it reflects a lack of rigor, not an excess of ambition. From an epistemological perspective, there is not much new here; auxiliary assumptions have been known to play a crucial role in scientific practice for ages, and the dangers of an excessive layer of ‘protective belt’ have been analyzed in detail. What is more surprising is the profession’s reluctance to take the robustness issue seriously, at least in some fields. To come back to Elster’s idea of a ‘bad equilibrium’, it is probably true that while the introduction of a new idea, even based on an oversimplified framework, is (rightly) praised, the less flashy task of evaluating the robustness of the results and extending them to more general settings is often academically less rewarded.

Methodological individualism

Microeconomics is based, at least in theory, on methodological individualism. This does not mean that it cannot analyze complex structures or organizations consisting of several individuals. But there is a requirement – namely, the ‘preferences’ (or ‘objectives’, or ‘goals’) of these organizations cannot be imputed *a priori*; they have to be *derived* from those of the individuals within the organization. For instance, in a Modigliani-Miller world, a firm can be assumed to (collectively) maximize profit because the decision power belongs to the shareholders, who as individuals are unanimous regarding this goal, and there is no restriction to their ability to implement their preferred policy. Conversely, a household *cannot* be assumed to maximize a unique, ‘family’ utility function unless the latter can be derived from the aggregation of the individual preferences of the members. Theory shows that, except for very special situations (e.g., transferable utility or Becker’s ‘rotten kid’ framework), such an aggregation is not

¹ Incidentally, Elster’s judgment on the lack of empirical confirmation of economic models, especially those who warranted a Nobel to their originators, is probably too harsh. Having spent several years testing the empirical predictions of contract theory, I got convinced that to a large extent the basic insights of, say, Rothschild and Stiglitz’s model of competition under asymmetric information were remarkably well supported by the data.

possible, and consequently household demand does not satisfy the same testable restrictions as individual demand.²

While these ideas are by now well accepted, nowhere are they more difficult to implement than in the notion of equilibrium – and particularly, as emphasized by Elster, when the equilibrium concept involves a coordination of individual beliefs. It has been recognized for some time that, in general, reaching an equilibrium requires more than individual rationality from all agents, and actually more than common knowledge of individual rationality among all agents. For instance, when several equilibria coexist, and agents must coordinate on one of them, the ability to coordinate cannot be a simple consequence of individual rationality. Even when the equilibrium is unique but involves mixed strategies, deriving its implementation exclusively from individual rationality of the players is problematic, because the equilibrium requires the players to each choose a particular probability distribution while they are actually indifferent between a continuum of possible choices. As argued by Elster, the equilibrium concept often ‘lacks micro foundations’. From an epistemologic perspective, the economist imputes to the collectivity of players a joint ability (i.e., the ability to costlessly coordinate) that goes beyond, and cannot be derived from, the rationality assumptions made for each individual player, without specifying the mechanisms by which such coordination may be achieved.

This problem, however, has been identified for quite some time, and a considerable amount of (theoretical) work has been proposed to address it. While an exhaustive overview would obviously exceed the scope of this article (as well as my own competence), let me mention two examples. In the case of mixed strategies, Harsanyi has proposed as early as 1973 the notion of *purification*, whereby mixed strategy equilibria are derived as the limit of pure strategy equilibria for disturbed games of incomplete information, when the disturbance vanishes.³ A second and largely debated example is the notion of rational expectation equilibrium in macroeconomics. Roger Guesnerie (2005) has

² Interestingly, empirical tests strongly support this distinction. Most attempts at testing Slutsky symmetry have in the past lead to strong rejections. However, recent works have suggested that rejection may be due to the nature of the sample, which includes both single- and multi-person households. When tested on a subsample of singles (as individualism would require), Slutsky symmetry is usually not rejected. For couples, tests of a generalization of Slutsky conditions valid for couples typically fails to reject, whereas standard symmetry is strongly rejected. See for instance Browning and Chiappori (1998).

³ Pure strategy equilibria are immune to Elster’s critique, because players each play their unique best response. Of course, Harsanyi’s solution has its own weaknesses; for instance, the perturbations must be independent across players (see Reny and Robson [2004] for a recent analysis). Interestingly, mixed strategies, when tested ‘in the wild’ (for instance in tennis or soccer), are well supported by empirical evidence; see for instance Walker and Wooders (2001) and Chiappori, Levitt and Groseclose (2002)

carefully analyzed the theoretical foundations of the concept; his main concern is whether (and how) it can be derived from the sole assumption of common knowledge of individual rationality. In some contexts, such an ‘eductive’ derivation is possible, because the rational expectation equilibrium is the only ‘rationalizable’ strategy profile of the underlying game. Then the concept does have micro foundations (whether it is realistic, or the assumption of common knowledge of individual rationality is just too strong, is a different matter, although Guesnerie shows that alternative criteria – e.g. convergence of learning processes or ‘evolutionary’ stability – often coincide with the eductive approach). In other cases, however, several different strategy profiles are rationalizable; then, Guesnerie argues, rational expectation equilibria are more problematic, and predictions based on the concept should be considered with caution.

Again, Elster is certainly correct in stressing that, too often, equilibrium concepts involving strong (and typically implicit) requirements such as complex coordination of beliefs are mechanically applied without much concern for their micro foundations. But, again, sloppiness can hardly be considered as a mark of excessive ambition. If anything, we should be more demanding, not less.

Empirical practice

Elster’s sharp criticism of empirical practice is mostly well-taken, although it may be overly pessimistic in the end. That ‘a non-negligible part of empirical social science consists of half-understood statistical theory applied to half-assimilated empirical material’ (p. 19) may well be true, but one can also look at the full half of the glass, i.e. at the remaining part. The evolution of empirical practice over the last decades, as I see it, is (at least partly) characterized by a growing concern about robustness of the outcomes and generality of the procedures. To take Elster’s example of estimating utility functions, the trend in structural applied micro has been towards ‘flexible’ forms and non parametric identification; assumptions that used to be taken as obviously acceptable, if not just granted (e.g., homothetic preferences or normally distributed random shocks) are now considered with suspicion, and largely relaxed. Similarly, selection issues, and generally the distinction between correlation and causality are taken quite seriously, which explains in part the increasing use of ‘natural experiments’ and explicit randomization. The work of econometricians like Jim Heckman (quoted by Elster) and others is a perfect example of this evolution toward rigor and generality.

That said, Elster is absolutely right on a simple but (I think) essential methodological requirement – namely that any new theory should be able to ‘generate other predictions, preferably in the form of “novel facts”, over and above those it is supposed to explain’ (p. 7; although Elster addresses this issue in

his discussion of the identification of utility functions, I believe its validity is fully general, and I extend its scope accordingly). In a sense, this is the oldest requirement in the book – namely, that a theory should be independently testable – and few scientists would dispute its relevance; but it is often forgotten in the actual practice of the profession. At worst, testability issues are just disregarded; at best, they are often left to applied people – ‘let the theorists formulate new theories, let the empirical folks manage to test them if they can’. Such a repartition of roles strikes me as inappropriate; not that division of labor is inefficient (on the contrary, specialization is a source of progress), but because it forgets that the derivation of testable implications lies at the core of the *theorist’s* job, even if actual tests require in general a different and complementary type of expertise and will typically be performed by different specialists. That any new approach should be presented together with the original, testable implications it generates sounds like an elementary requirement, but one that, unfortunately, is not always part of economists’ practices. Here as before, I am arguing that the level of ambition has been consistently set *too low* by the profession.

2. Bounded rationality

I would now like to briefly comment on Elster’s analysis of what he nicely calls the ‘as-if rationality assumption’. Elster’s point is simple but powerful: the usual (‘full’) rationality assumption often requires from the agents under consideration analytic capabilities that go well beyond what can reasonably be expected. Elster gives four possible justifications, and quite convincingly argues that while each has its merits, none is sufficient to justify the practice. My feeling here is that Elster forgets a fifth explanation, which is both ‘embarrassingly simple’ and to some extent disheartening, but remains (at least in my view) the most convincing one: the absence of a satisfactory alternative.⁴ A trivial remark is that science does not abandon a theory simply because it has been empirically falsified – Feyerabend used to remark that ‘all theories are *born* falsified’ – but only when a better alternative becomes available. ‘Better’, here, is quite demanding: the new theory should maintain the advantages of the previous one (here, in the case of ‘as-if rationality’: generality, tractability, predictive power...) while correcting

⁴ After all, we should not forget that the very criticism discussed by Elster was already formulated by Herbert Simon in the 60s. Few economists would dispute the fact that, in many situations, economic agents are more likely to ‘satisfice’ than to optimize. But it is fair to say that, contrary to Simon’s hopes, satisficing has failed to replace optimization as the economists’ favorite tool, essentially because no comprehensive theory has (yet) emerged that would precisely characterize, in a tractable way, how satisficing outcomes systematically differ from optimal ones. As Elster puts it, satisficing theory ‘was largely descriptive, with neither prescriptive nor predictive implications’.

some of its flaws.⁵ In that sense, Elster discussion of behavioral economics is remarkably convincing. He very clearly describes the numerous ‘irrationality-generating mechanisms’ that have been considered in the literature. These mechanisms are each interesting in their own respect; but they are largely or totally unrelated with each other, often contradictory, and definitely hard to encompass within a unified framework. Replacing a unified theory with a collection of ad-hoc and local explanations certainly does not sound like a promising solution.

Faced with this conundrum, the profession actually seems to react in an epistemologically sound manner: on the one hand, sticking to the existing theory and checking whether (some of) its apparent violations can be solved by relaxing auxiliary assumptions (particularly those regarding either the shape of preferences or the nature of unobserved heterogeneity)⁶; on the other hand, trying to develop an alternative theory that would satisfy the requirements listed above – the recent works by Roland Benabou, Richard Thaler, Jean Tirole and many others are perfect illustrations of these attempts. The future will tell us which approach will ultimately prevail, but the general movement seems to be in the right direction. On this, however, we should take very seriously Elster’s warning that *the empirical standards have to be as demanding as for the traditional approach*. In particular, one should analyze more systematically ‘real-life’ data (behaviors ‘in the wild’), and go beyond the simple collection of a list of situations that *may* be explained by one of the various mechanisms at stake, to aim at the formulation of a consistent theory generating testable new implications.⁷ Again, the level of ambition should be set at a *higher* level.

In the end, my only (minor) disagreement is with Elster’s conclusions, which I find too pessimistic. To quote an old joke, we all accept that prediction is a difficult task, especially when it deals with the future. The main issue is that even if we are (or were) able to identify general laws, many or most complex phenomena involve several of them, and the analysis of their *interactions*

⁵ Ideally, the new theory should moreover explain the successes of the previous one. For instance, relativity theory can explain the predictive power of Newtonian mechanics because the Newtonian model is a very close approximation for speeds ‘well below’ the speed of light.

⁶ For instance, while the ‘favorite longshot bias’ has often been presented as a violation of standard models of decision under uncertainty, recent work argues that it can actually be reconciled with expected utility when preference heterogeneity between betters is taken into account; see Ghandi (2009) and Chiappori, Gandhi, Salanié and Salanié (2009).

⁷ Indeed, when alternative approaches are proposed that offer a consistent, reasonably exhaustive theory generating testable implication, they find quite rapidly their way into mainstream practices. A typical example is prospect theory, which has been applied to a variety of fields, and tested ‘in the wild’ (see for instance Jullien and Salanié 2000). Note, however, that the behavior described by prospect theory is by no means *irrational*; it differs from standard, expected utility models only in the assumptions it makes regarding individual *preferences*. De gustibus non disputandum...

generally requires very precise quantifications of each effect. Such quantifications may or may not be available in our current state of knowledge. Nothing new here: even in Newtonian physics, predicting the trajectory of a falling body depends on the law of gravitation but also the law governing air resistance, which in turn involves the object's mass, drag coefficient and relative surface area. If some of these characteristics are unknown, we can only state that the object's acceleration will never exceed the acceleration due to gravity, that it will decrease with the object's speed, and that if the object's fall is free it will ultimately reach a terminal velocity that depends on the object's characteristics. In the same way, a microeconomist will predict that an increase in payroll taxes will have an income and a substitution effect that affect labor supply in opposite directions, the ultimate impact on labor supply depending on the agent's preferences. Still, testable predictions are possible; for instance, we expect that if the change affects the marginal rate of taxation but not the average rate, the income effect should be negligible, and the substitution effect should reduce labor supply – although, again, the size of the impact depends on preferences. I would not object to calling these patterns 'mechanisms' instead of 'laws' - in a sense, the distinction between 'laws' and 'mechanisms' is somewhat semantic. However, we should try very hard to preserve an *empirical content* – i.e., the ability to discard some a priori possible outcomes as inconsistent with the theory. Elster's (admittedly extreme) examples present situations in which, given our knowledge of the mechanisms involved, we can explain ex post *any* outcome (following the King's repressive measures, subjects may be less likely, or more likely, or just as likely to rebel). But then our explanatory scheme has little value, if only because it is just impossible to either use it in a prescriptive way or even assess its validity. If the World Bank asks our advice on the best way to implement a conditional cash transfer program and we have no suggestion to make, the fact that, whether it succeeds or fails, we would be able to explain why ex post would be of little comfort. I hope economics is not doomed to exclusively formulating empirically empty ex post explanations; and from a normative perspective I would certainly recommend a more ambitious goal.

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