Economic School of Louvain. LECON 2608: Advanced Labor Economics

## Epistemology and labor economics: Choices made by LECON 2608

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Let us start with a general statement. Any scientific approach of a real phenomenon in a way "reduces" this phenomenon by the methodology it implements. Otherwise, this approach would remain at the level of a pure description of reality (keeping all aspects, all details of it). This "reduction" means that the researcher simplifies, ignores aspects of reality, streamlines the real phenomenon. For better or for worse.

People do not only work to get a monetary compensation and access to consumption (or more consumption than what can be bought thanks to social allowances). Having an occupation can give more than access to consumption, namely,

- The compensation provides a certain degree of autonomy in the conduct of one's life (in comparison with getting the money or the goods from parents, a husband, social security, ...);
- Having a job can mean being part of a social group, a community (creating social ties, possibly the feeling of being member of the "working class" and what not); Having a job contributes to the definition of "who we are" (our identity);
- Having a job can provide the feeling of being useful to others;
- Being hired by an employer recognizes the worker as someone who is worth to be recruited ("productive");
- If the job has specific characteristics (e.g. autonomy, creativity), it can be interesting *per se*.

Any worker may value these dimensions to some extent<sup>2</sup>. However, "mainstream labor economics" mostly<sup>3</sup> ignores the latter features and adopts

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 $<sup>^2</sup>$  In the 2015 "work orientation" wave of the International Social Survey Program conducted in 47 countries, about 77% of workers find that having a job that is useful to society is important. So, for less than a quarter of them it is not important (Dur and van Lent, 2018).

<sup>&</sup>lt;sup>3</sup> The theory of compensating wage differentials is a counter-example (Rosen, 1986). In this theory, workers do not only value their wage and the leisure time lost in employment, but also the non-monetary aspects of their jobs. These non-monetary aspects cover e.g. safety, the intrinsic quality of the occupation. This theory assumes perfect competition on the labor market.

- An *instrumentalist* perspective (i.e. the only reason why people work is because they care about the consumption they obtain by working),
- In combination with an aversion to labor (hours worked and/or effort on the job create a disutility)<sup>4</sup>.

As far as the consumption motive is concerned, it is well possible that some workers seek "happiness" through a form of asceticism. About the aversion to labor, it may very well be that some workers intrinsically like working. However, it is implicitly assumed that such tastes are rare and hence unimportant.

It is not difficult to acknowledge that the instrumentalist approach is very reductive. Depending on the phenomenon one hopes to analyze this reduction will or not lead us to miss key causes of what is observed in the real world. For example, to what extent can we explain the strike phenomenon by sticking to the above assumptions (and some others stated below)?

As Mainstream Economics, Mainstream Labor Economics adopts *methodological individualism*: The attempt to explain phenomena by analyzing how they result from actions (decisions) made by individual decision makers<sup>5</sup>. In addition, it is assumed that those decisions are taken on purpose by individual agents who pursue some objective. It is therefore implicitly assumed that all economic agents (workers, whether employed or not) are in a "position" such that *they can make choices*. For instance, in standard labor supply theory, one excludes (or again finds so rare that it is unimportant) the possibility that someone works all along its time endowment because this is the only way to survive, leaving no scope for any choice.

Mainstream labor economics also adopts standard assumptions of *rationality* of economic agents leading to the maximization of expected utility (often without error). In practice, with few exceptions, it typically does not depart from postulates of *self-interested maximization*.<sup>6</sup>

Mainstream labor economics analyzes the labor market thanks to the construction of formal (mostly mathematical) models. Their theoretical predictions can be tested on the basis of data sets.<sup>7</sup> Measurement issues are frequent. This is in particular the case of survey data. However,

<sup>&</sup>lt;sup>4</sup> Here again, mainstream economics has produced some analyses that do not follow this view. For instance, on pages 102-3, Polachek and Siebert (1993) show how standard labor supply theory can be extended to the case where "people enjoy at least some hours of their work" (p. 102).

 $<sup>^{5}</sup>$  There is sometimes a misunderstanding here. Assuming that ultimately individuals (and not groups) take decisions does *not* mean that those who take decisions are not influenced by others (what they say, claim, do...).

<sup>&</sup>lt;sup>6</sup> This can be contrasted with the so-called "Agent-Based modelling". To briefly describe the latter, I quote Farmer and Foley (2009): "An agent-based model is a computerized simulation of a number of decision makers (agents) and institutions, which interact through prescribed rules" (p. 685) "The major challenge lies in specifying how the agents behave and, in particular, in choosing the rules they use to make decisions. In many cases this is still done by common sense and guesswork, which is only sometimes sufficient to mimic real behaviour." To have an idea of progress made by this approach, the reader can look at the recent paper by Caiani *et al* (2016).

<sup>&</sup>lt;sup>7</sup> More and more, empirical analyses use "individual data", i.e. data about individual workers and/or firms.

the problem is not limited to surveys: Administrative data are not immune to measurement problems. There can be a gap between the concepts defined by labor economists and the actual measurement of the concepts. For instance, unemployment, participation to the labor market, and workers' compensation are not at all obvious to measure. The way they are measured can be context-specific (e.g. depending on administrative rules in a given country). Some variables that matter for the theory are sometimes simply not observed by the econometrician (e.g. the reservation wage i.e. the lowest wage needed to accept a job offer). Despite these difficulties, *labor economics has to a large extent become an empirical discipline*. Some labor economics courses put almost all the emphasis on empirical work with only rather intuitive or vague links to theory. LECON2806 adopts a different viewpoint. It develops first of all theories. This does not mean that empirical analyses (econometric analyses or more generally a confrontation with the data) are neglected. It means that empirical work disconnected from theory is as much as possible avoided.

These models are based on assumptions. The degree of "realism" (or put another way the degree of abstraction) of those assumptions can be questioned. However, what "realism" means is not so obvious! For Hausman (1998) and Mäki (2011), a prominent view defines scientific realism "in terms of truth about unobservables". A good theory has to be right about unobservables (like electrons in physics). For Hausman (1998), economics is not a theory that postulates unobservables.<sup>8</sup> So, the argument goes, economics is not in the purview of scientific realism. Mäki (2011) reduces the requirement of realism to a minimal version. This is however a complex philosophical exercise. We should at least keep in mind that the requirement of realism is not as obvious as it seems: After a bit of reflection, one should recognize that it is difficult to judge that one assumption is realistic while another is not. So, some theory based on "apparently unrealistic" assumptions can be of some use.

One still influential view escapes the difficulty of discussing what is realistic and what is not. This view states "theory is to be judged by its predictive power for the class of phenomena which it is intended to explain" (Friedman, 1953, p. 8). If an assumption brings fruitful predictions, there would be no need to worry about its realism. Following this view, one could adopt the assumption of perfect competition on the labor market – even if it is or at least it sounds unrealistic – as soon as it generates models whose predictive power is "high" about, say, wages. This view is still very influential. For instance, many macroeconomic models are created in order to match some statistical moments found in the data. "Nothing more".

 $<sup>^{8}</sup>$  In empirical work, econometrics recognizes and takes into account the presence of unobservables. However, the aim is to "explain" observables (income, employment and the like) taking the role of unobservables into account.

According to Mäki (2003), this *positivist* or *falsificationist*<sup>9</sup> approach is however not taken too seriously by Friedman himself. Mäki (2003) underlines that although defending a positivist view of economic models, Friedman (1953) "emphasizes the role of subjective judgment, of the background of economists, of tradition, and of consensus amongst them" (Mäki, 2003, p. 503). Hausman (2012) adds: "Economic theories, which have not been well tested, are taken to be well-established guides to policy, rather than merely conjectures."

Quite in opposition to Friedman's view, Mäki (2005) argues "many theoretical models are ('thought') experiments". In a related way, Gilboa et al (2014) establish a similarity between the reasoning in parts of economic theory and case-based reasoning. Provided that it has been developed with rigor, each new piece of economic theory enriches the set of cases. As such the case needs not to be 'real'; rather it is theoretical (a thought experiment as in Mäki, 2005). Gilboa et al (2014) provide a clarifying example. A theoretical analysis of the ultimatum game where players only care about their own payoff concludes that player 1 offers a minimum amount to player 2 and that the latter accepts. Next, an experiment in the lab reveals a totally different outcome. If one views the theoretical model as a "general rule", the experiment – if properly conducted - leads to the conclusion that the "rule" is violated. By contrast, if one views the piece of theory as a case, and similarly the experimental result as a case, the two can coexist. So, in a way, Gilboa et al (2014) claim a certain degree of autonomy of theoretical developments. There are other arguments for avoiding to reject theory too quickly on the basis of one or a few empirical studies. Each of them (being in the lab or "in the field") requires a specific protocol and more generally specific (identifying) assumptions. The population under study can be quite specific. Even if the analysis is an example of rigor, the external validity of its results can be questionable: "To what population, settings, and variables can this effect be generalized?" (Campbell, 1957, cited by Dehejia et al, 2015).

Facing these different views, what is the perspective adopted by these lectures?

As opposed the positivistic view defended by Friedman (or at least some reading of Friedman, 1953), I do not consider models as mere tools for accurate predictions. Being aware that theories simplify a lot reality, they are nevertheless seen as a way of getting some understanding of the mechanisms *actually* at work behind social phenomena. Although models often lead to predictions – and this is important-, they are not to be assessed on the *sole* 

basis of the accurateness of their predictions.<sup>10</sup>

<sup>&</sup>lt;sup>9</sup> According to Popper (<u>http://www.science.uva.nl/~seop/entries/popper/</u>), "scientists should formulate theories that are "logically falsifiable" — that is, inconsistent with some possible observation report" (Hausman, 2012).

<sup>&</sup>lt;sup>10</sup> This debate about whether theory should only aim at predicting or should explain part of what is deeply going on in the real world is not specific to economics. For instance in physics, Pierre Duhem wrote in 1906: "A theory of physics is not an explanation. It is a system of mathematical propositions, deduced from a small number of principles, which have for their aim to represent as simply, as completely and as exactly as possible, a group of

Because we hope that models help understanding mechanisms at work, we will care about the "realism" of assumptions (remembering however the difficulty, mentioned above, of ranking more and less realistic assumptions). We do not do so for the sake of realism as such, but because we think that "realistic" assumptions contribute to a better understanding of social phenomena. Some important phenomena in the labor market can hardly be understood without going beyond the assumption of pure competition. For instance, why are equivalent workers (on the basis of rich observable characteristics) durably paid differently? Why do we observe that substantial numbers of vacancies and unemployed people coexist and that this is not a temporary phenomenon? As we will see, introducing imperfect information on the labor market improves our understanding of the mechanisms at work.

So, the theories developed during the lectures are *a step* in the direction of taking seriously into account "features" that seem pervasive on the labor market: asymmetric and imperfect information, imperfect competition and the like.

Stylized facts coming from a good observation of reality are essential. We also need good empirical work to conclude for instance that observed characteristics only capture a limited part of the variance in individual wages. We need good measurement of vacancies, unemployment and labor market flows and the like. So, empirical results will be central in several parts of the lectures.

As far as empirical "testing" of theoretical predictions is concerned, my view is that caution is needed. There is a clear and natural desire to confront theory/theoretical predictions to data. And we will do it at some points. However, shortcomings in data sets measurement issues, and the specific context in which empirical studies are conducted should lead us not to conclude too quickly "data rejects the theory". Without going as far as Gilboa *et al* (2014) when they argue in favor of the peaceful coexistence of theoretical predictions and empirical results that are completely different, it is probably useful to see theory as case-based reasoning instead as the production of general/universal rules that should never be violated by "the data".

Our references, in particular Cahuc, Carcillo and Zylberberg (2014), are textbooks in mainstream labor economics. They adopt *most of the time* an *instrumental* approach and so are almost silent about those features mentioned at the very beginning of this note. During the lectures, some other references will be considered to take into account some of these above-mentioned features (at least partly).

experimental laws." (*Duhem, The Aim and Structure of Physical Theory, vol 13, p. 19*; More information on e.g. <u>https://en.wikipedia.org/wiki/Pierre\_Duhem</u>). So according to Duhem, in physics a theory is a system of mathematical propositions whose aim is to produce a representation (not an explanation) of experimental results (facts).

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