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Misbehavioral Urban Economics*

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Abstract

Applications of the framework of behavioral economics to questions arising from urban economics are discussed. Directions for future research are outlined.

Keywords and Phrases: Behavioral urban economics, ambiguity aversion, loss aversion, regional art

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1 Introduction[1]

1.1 Context

A number of fads from economics have percolated to regional science. Some have resulted in new insights, some have not. It is not clear to anyone at the beginning

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[1]As an economist, the bias in my views should be obvious and accounted for by the reader in what I say below. This essay is meant to provoke. Who wants to write dull papers?

whether these ideas end up as fads or whether they end up affecting research in a substantial way. For example, game theory began as something of a fad (the reader might be too young to remember) studied by a small but devoted set of researchers; it was not even taught as a part of the core micro sequence when I was a first year student at Berkeley, one of the places to go to learn theory back in the dark ages. But now it is used by most everyone, and comprises about half of the core micro theory sequence here at Washington University in St. Louis. The New Economic Geography began as something of a fad developed by a small set of researchers, but ended up contributing greatly to the development of regional science.

Regional science has had close encounters with some fads that have not had staying power in the study of urban questions, such as computational economics, fractals, chaos and catastrophe theory.

Why have some fads met with success in regional science, and why have some failed? To address this question before getting into the meat of this paper, namely the potential impact of the behavioral economics fad on regional science, it is of utmost importance next to compare and contrast the methodology used in economics and regional science.

1.2 Methodology

There is methodology used in regional science, even contemporary regional science, that was settled as unreasonable by economists many years ago, justifiably so in my opinion. At a superficial level, economists require that models of markets be consistent with the use of prices (wages and rents) and with the optimizing behavior of agents. This applies whether the work is empirical or theoretical. Regional scientists seem oblivious to this, often using models implying that people (including themselves) don't care about the price of a house when they buy it, or models where non-zero price elasticities would falsify their theory. Moreover, as I have learned over the course of many years, regional scientists have issues with basics in economics such as supply and demand, and are in denial about it.[2]

[2] Thus, as I found out when serving as a discussant for the Presidential Address of the RSAI in 2007, a "transformative development" for regional scientists would be understanding the construction of supply and demand curves, and the meaning of perfect competition. Regional scientists embed

At a deeper level, economists generally adhere to scientific method. Economists take an interesting question motivated by observations, write down a model to address it, and then find the mathematics appropriate to apply to the model to answer the question. The empirical implications are then taken to data.

In contrast with their application in economics, applications of fads to regional science were motivated by excitement over the fad, often an area of applied mathematics, rather than motivation by economic questions that in turn generate models that require the application of this sort of mathematics. But I have harped on that elsewhere (see Berliant and ten Raa, 1994). One no longer sees sessions at regional science meetings or issues of regional science journals devoted to applications of computation, fractals, chaos and catastrophe theory. No wonder. A related phenomenon, apparent at the 2008 Regional Science Association meetings in Brooklyn, is that no matter the question, it must be addressed with a New Economic Geography model. This is, no doubt, a direct result of the 2008 Nobel prize in economics.

The stark difference here is in the use of scientific method, and the stage at which mathematics is brought into the research process. If the mathematics or a particular model itself is the motivation for the work, working backwards from the mathematics to obtain the economic assumptions usually results in nonsense.

At mainstream economics departments, the scientific method as applied to economics is an important part of training graduate students. In fact, that is the topic of the standard first lecture in a core graduate micro course. The reasons why mathematics is used by economists is not the subject of this essay; see Weintraub (2002) for more. But I shall nevertheless take this opportunity to express my frustration with many regional scientists about both their understanding of how and why mathematics is used in economics, and their misconceptions about basic economics, throwing jargon around in meaningless ways to try to impress. My goal here is not to rename the field "regional art," though it does have a nice ring to it.[3] I am hopeful that

market clearing conditions in the optimization problems of agents, a mistake usually reserved for undergraduates. So I ended up talking about material freshman learn in my discussion of the Presidential Address. Another "transformative development" would be if they learned about adverse selection and moral hazard; that is a subject for another essay/rant. But Savannah was nice in November.

[3]Thus, the readers of this journal are regional artists.

younger scholars will not follow in the footsteps of their elders.[4]

As can be seen in journals and at meetings, regional scientists seem oblivious to controversies that currently rage in economics. In contemporary economic research, there are clear controversies over empirical methodologies, namely whether one uses structural models, reduced form models, or calibration. There are some new fads, such as experimental economics, neuroeconomics, and behavioral economics, that might or might not have a long run impact on the field, but that regional science should at least be made aware of. The past for economics is often the future for regional science.

The use of behavioral ideas in economics springs not from their popularity or mathematical elegance; rather, it comes from either anomalies in data or in casual observations that are not captured by classical theory. I am hoping, but not optimistic, that the application of behavioral ideas to regional science will take a similar *scientific* rather than *artistic* tack. Realistically, I expect that the ideas will be brought in wholesale from economics and shoved into regional science models, whether the assumptions and motivating questions (if any) make sense in this new context or not.

Simply put, the goal of this essay is to bring one of the controversies currently raging in economics to the doorstep of regional science. I shall discuss some of the *motivating questions* that are important for regional science to consider before diving head first into the behavioral point of view. I shall also discuss my view of behavioral economics and its implications for regional science. Although it is interesting to speculate about whether it is just a fad or something more substantial, in technical terms whether or not it will "nuke the fridge," I'm going to leave that to the reader. But before getting into its application to regional science, we must first digress: What is behavioral economics?

2 Behavioral Economics

I shall not attempt even a partial literature review here, but in general, behavioral economics looks at decisions, either theoretically, empirically, or in the lab, where

[4]Some economists enjoy singing *Kumbaya* with the regional artists, but if the latter are addressing economic questions without understanding economics, and are in denial, then singing is counterproductive.

context or situation (Glaeser, 2004) matters. Rustichini (2008, p. F248) makes a nice attempt to define the field: "Behavioral economics may be defined as the research programme striving to give a psychologically realistic basis to the theory of economic behavior." Examples are: loss aversion, where a reference point such as current wealth has an effect on a person's evaluation of well-being under various scenarios; the endowment effect, where actually owning an item rather than having a budget creates greater attachment to the item; non-classical discounting that gives greater weight to the present than is possible under classical discounting; and various forms of altruism or envy. I should note that in the classical market setting, most of these possibilities were incorporated into general equilibrium theory many years ago. One of my favorite articles on this subject is Shafer and Sonnenschein (1975), that specifies a full blown general equilibrium model (with externalities and production) where preferences are only required to be irreflexive, namely no bundle is preferred to itself, continuous and convex. This paper gives the entire model, examples, and demonstrates existence of equilibrium *in four pages*. The assumption of convexity of preferences was of course dispensed with in large economies even before this. As I tell my first year graduate students, expected utility (for example) is not a requirement of the theory, but is useful in applications in some circumstances. General equilibrium theory has, with few exceptions, *never* required expected utility functions, or even a utility function!

Now consider a theory that is consistent with standard behavior and utility theory, but not an expected utility theory. One such theory is that of ambiguity aversion (e.g. Maccheroni et al, 2006). This theory allows one person simultaneously to have many ideas of the distribution of a random variable, for example utility levels or welfare in cities, and specializes to expected utility theory when there is only one distribution. A special case is the pessimistic person, who thinks that nature always chooses the worst case distribution for them (Gilboa and Schmeidler, 1989). In technical terms, these ideas are called "unexpected utility theory."

For our application to urban economics in the next section, it is useful to distinguish between decisions that a particular agent might consider to be big, such as the choice of city by a consumer or a firm, and decisions that a particular agent might consider to be small, such as the choice of input supplier by a firm. The small de-

cisions made by individuals can add up (across agents) to something much bigger. They might be the underpinnings of important phenomena, such as agglomeration economies; see for example Helsley and Strange (2002). However, the consistency of these small decisions with particular theories might be difficult to tease out in individual data, as agents might be satisficing. The choice might not matter much to any one of them. Thus, we focus on the big decisions.

Due to the presence of many other factors (to be discussed shortly) that can cause apparently anomalous behavior, to isolate the choice problem from these other factors, testing has been moved from the classical laboratory of the real world to the experimental lab. Thus, the thrust of the empirical tests of behavioral theories has been to isolate them in so-called context free environments. But this is troublesome for a couple of reasons. First, it is hard to actually implement a context free environment in the lab. Second, it is often context that is important in behavioral theories.

Some experimental studies have their focus on context, for example studying how a society's culture, market interactions and social interactions affect decisions. For small decisions, Roth et al (1991) and Henrich et al (2001) tell us that there might be something behavioral in the data, though it is not clear what it is and how situational effects are identified, as opposed to subjects misunderstanding the context of the experiment or using analogies to more complex real world situations that can be explained by standard decision or game theory. Plott and Zeiler (2005) can explain anomalous behavior in the lab by subject misconceptions arising from differences in the instructions given to subjects and differences in experimental design. The residual seems to be ambiguity aversion.

I confess that due at least in part to my training, I am a fan of the critics of the literature such as Rubinstein (2006) rather than of the literature itself. As already mentioned, the theories of preferences and general equilibrium do not exclude externalities, for instance, so if they are found to occur, it is unfair to say that we must reject our standard theories. Cox et al. (2007) show that if externalities are introduced so that a person's well-being depends on their relative status and the reciprocity of others (in the form of prior actions) as well as on their own income, then many apparent anomalies in experimental data can be explained.

If there is something to behavioral economics, we should consider revising our theories, of course. But this does not mean throwing the baby out with the bath water;[5] see Binmore (2008). It does not necessarily imply, for example, that we should not be using supply and demand (*correctly*) in a classical competitive market. Rather, it might imply changes in the theory in certain contexts. It is possible that these are at the fringes of applications of the theory, or are small enough in magnitude for large markets that the classical theory works well enough, perhaps due to aggregation across agents.[6] But we don't know. In any case, careful and scientific reading of the evidence does not imply that: the whole theory should be scrapped and replaced with something else, preferably not involving economists at all. Regional scientists seem to rejoice at the latter prospect, since it means they don't have to learn basic economics. But in my opinion, you've got to know something to criticize it.

When an agent makes a smaller decision, satisficing could easily be prevalent, as the agent doesn't care much about the outcome and thus doesn't invest much in the decision-making process. Thus, individual data on small decisions could be very noisy. When an agent makes a big decision of the type considered in the next section, such as location choice, satisficing is unlikely to be prevalent because a mistake could be costly to the agent. So at least the magnitude of behavioral effects should be measurable, in other words greater than epsilon, if they are present.

3 Misbehavior in Urban Economies

Storper and Manville (2006, footnote 13) lament that behavioral economics has not made its way into the urban literature.[7] However, it is very important to have specific

[5] For an explanation of this idiom and its origins, see http://www.answers.com/topic/throw-out-the-baby-with-the-bath-water

[6] The technical term is "smoothing by aggravation."

[7] It is impossible for me to read this article and not observe its collision with economic theory as taught in first year micro Ph.D. courses. We illustrate using the following examples. 1) The article discusses indivisibilities in location and housing, nonconvexities in preferences, and lack of transitivity of preferences as if they pose insurmountable obstacles, when in fact general equilibrium theory has allowed all of these features for at least 30 years. No problem. 2) The article discusses preference aggregation without mentioning Arrow's theorem, the Gibbard-Satterthwaite theorem,

ideas and questions in mind when considering this issue. I provide some next.

In the general setting, behavioral effects would be observable in the decisions of agents. Evidently, we are talking about the optimization problems of consumers and producers. What is special about the urban setting, as opposed to the setting in general economics, is the choice of location embedded in agents' optimization problems. To make matters more concrete, let us focus on consumers instead of firms. Some of behavioral economics (but not all) involves consumer behavior when facing uncertainty. The decision to change cities is a very good example of this.

When people consider moving or actually move to a new city, there generally remains much uncertainty about their new circumstances, for example their commute, their neighborhood and their schools. Although much information can be gleaned from the internet and from current residents, the residual uncertainty can be substantial. Given that there is less uncertainty associated with their current residence, a reluctance to move is understandable. The reasons include risk aversion and ambiguity aversion. The extant literature in urban economics, both empirical and theoretical, does not deal well with this kind of uncertainty. In general, it is simply assumed not to exist. What are its implications for the way we look at cities?

Relegating the choice of bundles of mobile commodities to the background, do we observe behavioral effects in migration choices? In other words, do people migrate less often than is predicted by standard models? Is such stickiness due to the presence of uncertainty, in the sense that there are unknowns about cities other than the location of residence that, in combination with risk aversion or ambiguity aversion, cause people to move less? Or are there situational effects, as Glaeser (2004) calls them, that might cause stickiness in the migration decisions of households? Here I refer to loss aversion and endowment effects, for example.

There is some preliminary evidence of loss aversion in the context of real estate markets; see Genesove and Mayer (2001) and Ong et al (2007). This research addresses listing prices of houses and default decisions, but not decisions such as whether or not to sell a house at all or move to another city.

Similar to applications of behavioral economics in other fields, in urban economics it is hard to isolate behavioral effects from unobservable variables, such as moving cost,

and the Sonnenschein-Mantel-Debreu theorem.

preferences for amenities, social networks, and the aforementioned risk and ambiguity aversion. In other words, it is hard to isolate the effects of interest from noise. In contrast with the general literature on tests of behavioral theories, urban economics is not context free. In fact, it is all about context, namely one's surroundings. So the experiment that would follow naturally, not context free, is to compare in the lab decisions of consumers without the endowment of a house and location (but just a budget, like a new migrant to a country), and consumers who are identical but endowed with a house in a community. One could also look at real world data, for example comparing locations chosen by new immigrants to a country with locations chosen by people already resident in a country, though the controls would have to be extensive. In other words, are location decisions sticky, and if so, why? If they are sticky, there might be a role for government to improve *ex post* welfare by subsidizing moves through the tax system. There are also applications to quality of life indices, that rely on no uncertainty in their calculation. Uncertainty itself under expected utility will add a little, as it's equivalent to a moving cost, namely the risk premium. But ambiguity aversion will add more, as I shall explain below. Let me be more specific about an example.

What are the implications of risk aversion and ambiguity aversion for the way we look at cities? Consider first a standard equilibrium model of cities, e.g. Eeckhout (2004). Suppose for simplicity of exposition that all people are identical. The standard model has no uncertainty or moving costs. Then people will move to the city where they are happiest. Those cities that offer a lower level of happiness will be empty.[8]

Now add to the model uncertainty about circumstances in cities other than the city of initial residence. It will not matter, in the end, whether people are risk averse or not. The result will be the same as the previous one. Here is why. Consider people in the city generating the lowest, or close to the lowest, level of happiness. As in the case without uncertainty, they know that any other city will generate a higher level, at least in expected utility, though they might not know the happiness level for each particular city. So that unhappy city will empty. This process will continue until only

[8]For the regional artists out there, all of these arguments apply whether or not there are prices, namely wages and land rents, in the model. But the equilibria will be different depending on whether or not there are prices.

the cities at the top of the distribution are left, and all consumers know that. Thus, under the assumptions of the standard model, and using expected utility theory, only the top cities survive. The resulting equilibrium allocation might not be first best due to externalities.

Now consider a theory that is consistent with standard behavior and utility theory, but one based on ambiguity aversion.

We conjecture that in this situation, many non-degenerate distributions of (welfare in) cities can be supported as equilibria. The reason is that pessimistic people do not think that moving will make them happier, so they stay put in the city that they know and never learn about other cities. Cities with relatively unhappy residents do not empty.

There are applications of this idea to the welfare economics of systems of cities and to explaining the size distribution of cities as well as to the interpretation of quality of life indices.

4 Conclusions

Are the location decisions of firms and consumers sticky, and if so, why? This seems to be a critical question in the future of misbehavioral urban economics. It can be addressed theoretically, empirically, or in the lab. The goal of this essay is to provoke on several levels, not to provide answers.

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