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CROSSING IN THE NIGHT OF THE COLD WAR: ALTERNATIVE VISIONS AND RELATED TENSIONS IN WESTERN AND SOVIET GENERAL EQUILIBRIUM THEORY

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This paper extends various arguments in the recent historical literature on Soviet mathematical economics during the Cold War. It examines some of the tensions associated with the attempt to blend Walrasian economics and Soviet planning. The main argument is that the two literatures crossed in the night of the Cold War. Given the two different political-economic and scientific contexts, the aspect of the Walrasian vision most emphasized in the Western literature was exactly the opposite of the aspect most emphasized in the Soviet literature. For Western economists the main concern was how (how possibly) competitive market prices could coordinate the actions of heterogeneous economic agents, while the main concern of Soviet scholars was how (how possibly) the theory of competitive markets could be used to help facilitate the efficient implementation of a central planning mechanism with a single (social) goal. Many features of the mathematics were the same, but the different goals and contexts created various tensions within the mathematical models produced by the two scientific communities.

> …modern economics lends little support to the notion of basic identity of all interests in the society with one another and with the over-all social interest, a fiction on which much of Stalinist theorizing was built.
>
> (Zauberman 1975, viii)

> There is no better example of how love turns into hate and imitation breeds contempt than the Arrow-Debreu model.
>
> (Mirowski 2002, 406)

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1. Introduction

It is fairly well known that during the 1960s Soviet economics went through what has been called a mathematical revolution. The distrust of the mathematical tools of bourgeois economics that prevailed during the Stalin years faded enough to allow the development of a significant research program in a variety of areas within mathematical economics. Although there is an extensive historical literature on Soviet mathematical economics[1] – both the early literature and the literature of the 1960s – most of the scholarly attention has emphasized areas such as input-output, linear programing, activity analysis, and optimal growth theory, rather than the Walrasian general equilibrium theory. This is surprising given the importance of Walrasian theory within the mainstream economics of the time and the potential historical, political-economic, and general scholarly interest in Cold War social science and related fields.[2]

Using the literature on Soviet mathematical economics as a point of departure, this paper will investigate the specific theoretical details of the Walrasian models produced on both sides of the Iron Curtain in a fairly close-focused way. In particular, I will examine how ideas from Walrasian general equilibrium theory were involved in the Soviet literature and how fundamental differences in the social context and problem situation of the two communities created tensions that manifested themselves in the specific models produced. The tensions I will discuss are noted in some of the existing research – particularly in two papers by Ivan Boldyrev and Olessia Kirtchik (Boldyrev and Kirtchik 2014 and 2016) – but they are not the main topic as they are here. I will also discuss how these same tensions emerged in a very different way in the Western Walrasian literature during the period 1950-1970. In fact, as the title suggests, they crossed in the night of the Cold War. Soviet and Western scholars working in the field of general equilibrium theory did share a certain set of interests, tools, and insights, but the two groups had entirely different visions of the relative importance/emphasis within this shared set of ideas. In particular, that which Western economists emphasized was precisely that which Soviet scholars downplayed, and vice versa. As Boldyrev and Kirtchik note: Walrasian economics could not

[1] For a sample covering a variety of perspectives see Bockman 2011, Bockman and Bernstein 2008, Ellman 1973, Leontief 1960, Prybyla 1963, Stiglitz 1994, Sutela 1992, Zauberman 1965, 1967, 1969, 1975.

[2] See for example Amadae 2003, Erickson *et al.* 2013 on rational choice theory; Klein 2000, Mirowski 2002, and Sent 1998 on mathematical economics; Reisch 2005 on philosophy of science.

be «'simply' extended to a different intellectual space … extension requires a work of *interpretation* and *adaptation* to a new context defined by a specific institutional setting, political environment, perceptions of economic reality, and other factors internal and external to economic science» (2014, 436). Walrasian general equilibrium necessarily involves optimization, equilibrium, and rationality, but which of these features is given priority, where the primary rational agency lives, and what is being optimized, can be, and was, quite different in the context of Soviet and Western mathematical economics during the Cold War.

Understanding these theoretical tensions will enrich our understanding of Cold War mathematical economics in at least three different ways. First, it explains why Walrasian general equilibrium theory had such a small impact on the Soviet literature compared to other areas of Western mathematical economics: linear programming, activity analysis, dynamic optimization, growth theory, etc. Second, it will draw attention to the content of the Walrasian-inspired Soviet models and relate it to the context in which it was produced and the tensions that it generated. And finally, it will draw attention to particular aspects of, and tensions within, Western Walrasian theorizing during this period that have not been sufficiently recognized within the existing literature.

Following Hamminga (1983), I will use the term 'set of elementary plausibility convictions' (SEPC) for the broad set of goals and constraints about what constitutes an interesting or worthwhile theoretical result within a particular scientific community. The SEPC involves social, political, epistemic and technical constraints on theorizing; the fundamental goals of the scientific research; what counts as a positive and/or negative result; the interests that condition the scientific research process; and a broad range of other factors. The SEPC associated with a particular scientific community is thus a broad category: including things that historians would consider to be both internal and external, the context and constraints discussed within the science studies literature, interactions with and constraints from other sciences, the available technology, and many other factors.[1] This means that it is difficult to draw crisp bound-

[1] There are always questions about how historians go about determining the convictions of a particular community (scientific or any other), but given the language barrier and the inaccessibility of some of the Soviet sources, it is particularly challenging in this case. All I can do is to rely on existing translations of the relevant research and the English language secondary literature for the Soviet side of the story. In addition to the question of how the SEPC becomes clear to historians employing the concept, there is also the question of how clear the SEPC are to the relevant scholars. It certainly seems that some scientific communities have fairly tight convictions and other scientific communities have fairly loose convictions and the difference is relevant to the impact of the SEPC's constraints and objectives. One might suspect, given the political constraints facing Soviet scholars, that the SEPC would be quite tight – and in some respects that was the case (the personal cost of SEPC violation could be quite high) – but in other

aries around a particular SEPC, but it is nonetheless a meaningful and useful distinction. For example, the SEPC of the economists working on Keynesian macroeconomics during the 1940s and 1950s is clearly different than the SEPC of the macroeconomists working on New Keynesian dynamic stochastic general equilibrium theory (DSGE) during the last two decades, and the SEPC of economists working on Samuelson-Bergson-Pareto welfare economics during the 1940s is quite different than the SEPC of economists currently specializing in behavioral welfare economics or the economics of happiness. So too it is with Soviet and Western mathematical economists engaged in research involving Walrasian general equilibrium theory. There was certainly an overlap between the SEPC of Western economists doing Walrasian economics[1] and Soviet scholars doing research involving Walrasian ideas during the period, however there were also significant parts of their respective SEPCS that were quite different. The ways in which these differences conditioned the Walrasian models of the two communities – and the associated theoretical tensions – are the focus of this paper.

The paper is arranged as follows. Section one will review the literature on Soviet mathematical economics. Most of the discussion comes from the existing literature, but I will emphasize certain aspect that are key to the argument in the rest of the paper. Section two will discuss Western Walrasian general equilibrium theorizing during this period. I will assume the reader is familiar with the basic ideas of Walrasian economics, but again I will emphasize features that are of particular

respects the Soviet SEPC was more loose than the Western version. Since there were so few Soviet scholars working on Walrasian-inspired mathematical economics (less than 4% of the work in mathematical economics according to BOLDYREV and KIRTCHIK 2016, 19), and since what a particular scholar was able to do depended on the degree to which the scholar, or the scholar's institute, was supported by, or could stay under the radar of, the political powers that be, there was in some ways more theoretical variation within the Soviet than the Western literature. If you were a Western economist doing Walrasian general equilibrium theory during the third quarter of the twentieth century it was entirely clear what you were doing (clear to you and clear to your colleagues doing other things) – the Walrasian SEPC was fairly narrowly circumscribed – but this was much less the case among Soviet economists.

[1] I will generally use the term 'Walrasian economics' rather than 'general equilibrium theory'. The problem is that general equilibrium theory is a very broad category of economic models. A closed Leontief model is a type of general equilibrium model, and so is a dynamic stochastic general equilibrium model, a Sraffian model, most optimal growth models, and a host of other economic models, but these are not the economic models that are the concern here. While the term Walrasian is some help in narrowing down the relevant literature, some ambiguity remains. Neither the Walrasian economics of Leon Walras himself in the 1870s, or the immediately following work by Vilfredo Pareto and others in the early twentieth century, nor contemporary Walrasian macroeconomic models, are exactly the same as the mid-twentieth century Walrasian economics relevant here. The Walrasian general equilibrium theory of concern here is generally called abstract Arrow-Debreu theory and its core features will be discussed in section two.

relevance here. The final section will bring these two things together and make the case for the crossing of the two literatures and the various tensions that ensued. The conclusion briefly notes the contemporary relevance of the argument.

2. Soviet Mathematical Economics

Prior to the so-called revolution in mathematical economics of the 1960s Soviet economics involved very little mathematics and essentially no input from mainstream Western economic ideas. This seems a bit surprising given that i) a centrally planned economy would seem to be a natural environment for the application of mathematical and quantitative methods of economic optimization and control (much more so, it would seem, than the decentralized environment of a market economy), ii) Pre-Soviet, and even early Soviet, Russia had a rich scholarly tradition in optimization-based mathematics and even mathematical economics (associated for example with names like Dmitriev, Slutsky, and Tugan-Baranovsky), and iii) many of those on the pro-planning side of the socialist calculation debate in the 1930s and 1940s grounded their arguments in Walrasian-inspired market mechanisms and the associated mathematical methods (Lange 1936, Lange and Taylor 1964, Lerner 1944). However, the intellectual environment of the Soviet union at this time was dominated by doctrinaire Marxism that was extremely hostile to anything associated with 'bourgeois economics.'[1] As Michael Ellman explains:

> Soviet political economists were mainly engaged in agit-prop, and in particular in demonstrating the virtues of socialism in general and in the latest statement by Stalin in particular, and in enlarging on the sins of capitalism … When in spite of all these obstacles, serious contributions to economic analysis were published, such as the well-known works of Kantorovich … they were ignored.
>
> (Ellman 1973, 2)

Leonid Kantorovich – co-winner of the 1975 Nobel Prize in Economics (with Tjalling Koopmans) – began developing his «method of resolving multipliers», which became linear programming, in 1938[2] and published his first Russian book on the subject in 1939, but «for two decades Kan-

[1] As Wassily Leontief put it in 1960: «Soviet economic science, has remained static and essentially sterile over a period of more than 30 years – a huge, impassive and immovable monument to Marx – with scores of caretakers engaged in its upkeep, fresh flowers placed in slightly different arrangements at its feet from time to time, and lines of dutiful visitors guided past in never-ending streams.» (Leontief, 1960, 261). See Chossudowski 1939 for a discussion of the various conceptions of economic equilibrium in early Soviet economics.

[2] In research he conducted for the Laboratory of the Plywood Trust (Kantorovich's biographical page on the Nobel website Nobelprize.org).

torovich's invention left no impact whatsoever on Soviet economics and economy» (Zauberman 1967, 8-9). A review of his work in the organ of the State Planning Commission criticized Kantorovich's deviation from Marxist value theory; he «substitutes cost relations … for value in its Marxist sense and unwittingly reproduces certain propositions of so-called 'marginalism' … Marxist criticism demonstrated long ago that the marginalist conceptions are built on sand» (*ibidem*, 11-12).[1]

Two partial exceptions to this absence of mathematics and Western influence were the Leontief input-output model (Leontief 1936, 1941) and the von Neumann growth model (von Neumann 1945). However, in both cases the models involved pure production – in Soviet parlance they were «objective» not «subjective» (*i.e.* utility) based systems – and both either were (von Neumann), or could be (Leontief), used in normative planning models focusing on a maximum rate of growth. The basic Leontief model shares structural features with aspects of Marxian economics (particularly the reproduction schemes in Volume two of *Capital*)[2] – and it is the framework used in various attempts to formalize Marxian economic theory in the second half of the twentieth century (*e.g.* Morishima 1978) – while the von Neumann growth model is a subsistence wage model that treats households as an industry with necessities as inputs and the commodity labor as the output (as Marx characterized labor power in the abstract model of volume one of *Capital*). These were mathematical, and equilibrium, economic models, but models with a (linear) production-side orientation that seem to fit relatively comfortably with the production-oriented SEPC of Soviet scholars. On the other hand, Walrasian general equilibrium theory – particularly in its popular pure exchange version, a model where all activity is driven by the utility-maximizing (*i.e.* subjective and self-centered) behavior of individual consumers – was a much less comfortable fit with the SEPC of Soviet theorists during this period.

After Stalin's death in 1953 – but even more after the Kosygin reform in 1965 – the attitude toward mathematical economics and Western economic ideas began to change. Even in 1960 Wassily Leontief could write that «what the Soviets are about to adopt is Western economic

[1] See ELLMAN 1973 (particularly Ch. 4) or GARDNER 1990 for additional discussion of Kantorovich's work.

[2] It also had a «respectable Soviet Russian ancestry» as Leontief himself explains: «A search through old economic journals revealed that in 1925 a short article on the then newly complied balance of the Russian national economy was published … over my signature. (Actually, I wrote this paper when still a student at the University of Berlin; it was first published in Germany and then translated and published in Russian).» (LEONTIEF 1960, 269). See LEVINE 1964 and the literature discussed therein regarding the relationship between Leontief's original input-output model and the early Soviet literature.

science» and note that «it is 'bourgeois economics' rather than physics that is about to be used to serve Soviet aims» (Leontief 1960, 265). There are many reasons for this, but perhaps the most important was the ineffectiveness of the existing Soviet economic planning and the desire to use the best available scientific knowledge to improve it (a strategy that had worked effectively in fields like engineering and natural science).

The context of objective economic planning proved to be fertile ground for the development of Soviet linear programing, activity-analysis, and operations research that mirrored similar developments in Western economic theory. In many cases these mathematical tools grew out of similar problems associated with Wartime planning and resource allocation. These practical problems were much the same for both Soviet and Western economists, and in both cases new technical tools were needed because these problems resisted representation in terms of the differentiable functions required for traditional calculus-based optimization. On the Western side much of the initial work in linear programing and activity analysis had been done during the 1950s – classics include Koopmans (1951) and Dosso (1958) – and on the Soviet side it became an important part of the broader field of 'economic cybernetics'. The cybernetics movement was a loose amalgam of a number of different fields within applied mathematics – including, but not restricted to, operations research, linear and nonlinear programing, optimal control theory, and management science – and it became the *mainstream* (although not the only) research program within Soviet mathematical economics. It was technocratic in orientation (focused on the development of practical mathematical tools that could be used to improve central planning), employed both home-grown intellectual resources as well as those borrowed from the West, and reflected «the aspirations of the Soviet officials to demarcate socialist mathematical economics from the ideologically dubious, 'bourgeois' marginalism and neoclassicism» (Boldyrev and Kirtchik 2016, 9).[1]

Given that economic cybernetics borrowed heavily from the Western mathematical economics of the 1950s and early 1960s, and given that those years were the heyday of abstract Walrasian general equilibrium theorizing, one would expect Walrasian economics to be represented along with the various economic and mathematical ideas associated with economic cybernetics. But this was not the case. The SEPC of

[1] An interesting example of the importance (and legitimacy) of cybernetics to the Soviet bureaucracy comes from the Leonid Hurwicz archives in the Rubinstein Rare Book & Manuscript Library at Duke University [Box 11]. There is correspondence about the plan for Kantorovich's visit to the US during fall 1978 and it was clear that the letter of invitation needed to come from the Director of the Center for Cybernetic Studies (not from the Economics Department).

economic cybernetics did not include Walrasian economics with its abstract approach, emphasis on heterogeneous utility-maximizing agents, decentralization, and competitive equilibrium. As Boldyrev and Kirtchik explain:

> The concept of general equilibrium … was considered part of a 'bourgeois,' and consequently erroneous, economic theory which had to be refuted in relation not only to socialist, but also to 'real capitalist' economies … The most common argument against general equilibrium models, mentioned in the Soviet literature, posits that these models are only relevant for analyzing markets with perfect competition, and hence are unrealistic.
>
> (2016, 13)

Despite this resistance, there were a few Soviet scholars – primarily in institutes of mathematics and engineering – who did explore Walrasian and other demand side and utility-maximization oriented modeling strategies. These individuals and the groups they were associated with remained marginalized, but they did produce research that was more Walrasian than that associated with the cybernetics mainstream. It is this Soviet literature that most clearly demonstrates the tensions within Walrasian economics – on both sides of the 'Iron Curtain' – that will be emphasized here.

One change in the practice of Soviet planning that contributed to an increased tolerance for utility-maximizing models was an increased emphasis on consumption. As Alfred Zauberman explains:

> The changing conditions of the Soviet economic environment have made it desirable, and the assimilation of input-output analysis has made it technically possible, to adopt the final bill of goods, rather than gross outputs as hitherto, as the starting-point in the construction of the plan. Thus consumption has come into the planner's focus.
>
> (Zauberman 1965, 323)

Zauberman also argues that the ordinal utility revolution of the 1930s – what he calls the «Slutsky treads» – also helped to create an opening for utility-maximization and consumer demand in Soviet economics (Zauberman 1965, 324; 1969, 18). As noted above, Soviet Marxism had no place for the subjective utility of early neoclassical economics (a hedonistic-feelings-based theory of value). The standard story about the ordinal revolution was that, by basing the theory exclusively on things that were interpersonally observable, it allowed the use of the concept of utility while disconnecting it from any association with individual subjective feeling: thus making it more scientifically (and in this case politically) acceptable. The problem is that even if ordinal utility is sufficiently objective, it is still the utility of individuals – potentially quite heterogeneous individuals – and Soviet emphasis on central planning required a single objective function that represented the social goal.

> The constraints … are the material ones, the manpower and the natural resources and the state of scientific-technical knowledge. The task of economic theory is to instruct how they should be optimally employed. The controversy … concerns every point of this proposition – the need and the very possibility of making explicit and quantifiable the criterion of the plan, as well as its uniqueness and its essence – *social utility*, a concept repudiated for decades by the Soviet tradition in economics.
>
> (Zauberman 1975, 19, emphasis added)

Zauberman discusses the work of V. S. Nemchinov in 1962 and L. M. Dudkin in 1963 as early attempts to use marginalist-based utility theory to underwrite a version of a social utility function or community preferences. In both cases fairly ad hoc assumptions were imposed on the preferences or utility functions of the individual agents to obtain the aggregation results necessary for a social objective function. In these models the «object of the planner is to find a price set that will clear the market, and, subject to given resources and a distribution constraint, yield the community ('all labour groups') a maximum level of satisfaction» (Mishan and Zauberman 1967, 42). In this and other research from the period the agent whose objective function represents the social utility is «not far removed from the Pigovian 'representative consumer'» (Zauberman 1965, 325).[1]

An excellent example of the efforts by Soviet mathematical economists to integrate ideas from consumer choice theory and Walrasian economics is given by the example of Victor Polterovich. He was involved with this project for a relatively long period of time, but probably the best example of his efforts to use Walrasian general equilibrium theory as a tool for analyzing the problem of the efficient allocation of resources in a planned economy was his «Economic Equilibrium and the Optimum» (1973). This paper is a self-conscious effort to bring Walrasian equilibrium and a planning optimum together into a single analytical framework.[2]

Polterovich begins by distinguishing equilibrium models from optimization models on the basis of their normative implications:

> Equilibrium prices permit each participant in the economic process to choose consumption and production levels in such a way that, without violating his own interests, the physical and financial constraints on the system as a whole will be observed. Thus, in equilibrium models we hypothesize in advance a price mechanism which

[1] Bockman 2012 discusses some of the literature by Eastern European economists that was also moving in this direction at the time.

[2] Other Polterovich papers from this period that have been translated into English and explicitly involved equilibrium and utility theory include Polterovich 1971, 1983, 2000 and Mityushin and Polterovich 1978 but none of these are as explicitly Walrasian as the 1973 paper.

integrates the local economic objects into a single whole. This assumption is the basic reason that the concept of economic equilibrium does not possess a sufficiently obvious normative content such as, for example, that which inheres in the problem of maximizing a consumption target.

(Polterovich 1973, 3)

Of course he is recognizing that the Walrasian system as a whole does satisfy a *certain type* of normative goal – no agent is acting in violation of his/her 'own interests' – but from the Soviet viewpoint individual interests are not the issue. The problem is that general equilibrium alone does not guarantee that any particular normative social optimum will be reached.[1]

Polterovich's solution to the problem is to place special restrictions on individual utility functions that guarantee the existence of an aggregate social utility function with the same properties – in other words, so that the economy behaves *as if* there existed a single representative agent – «thus representing equilibrium as a solution to one 'big' optimization problem» (Boldyrev and Kirtchik 2014, 447). He does this in two steps. The first is to assume that all of the consumers have linear utility functions. The second is to move to a bit more general case by assuming that all individual utility functions are homogeneous of some positive degree: specifically that «the degree of homogeneity of the utility functions is identical and incomes of all participants are equal» (Polterovich 1973, 14).[2] He then proves that the solution to the aggregate distribution problem exists and is unique. This is a technique that allows the transformation of a model of the actions of individual agents into an optimization model with a single social objective function.

Polterovich saw this problem as a normative interpretation of an equilibrium idea. This coordination mechanism inherent in general equilibrium models was not normative enough to be understandable and meaningful (primarily for the decision-making authorities in the USSR) in comparison to the optimization models, where the choice criterion is explicitly formulated.

(Boldyrev and Kirtchik 2014, 447)

Although this paper is explicit about its commitment to Walrasian equilibrium, Polterovich's papers that involve other approaches often follow this same representative agent strategy for shifting a decentralized

[1] One way to think of this as a 'normative' interpretation of Walrasian general equilibrium theory: essentially as using the Walrasian equilibrium as a standard for what an economy desiring an optimal allocation of resources 'ought to do.' This is a common interpretation of LANGE's 1936 position: *e.g.* INGRAO and ISRAEL 1990, 252-53.

[2] The linear part of his argument follows GALE 1960, 280-290 and the generalization to the homogeneous case uses EISENBERG 1961. Other models that involve a representative agent-based optimization problem and/or aggregation results will be discussed below (with additional references).

equilibrium model into centralized optimization model. For example Polterovich (1983) is explicitly an optimal growth model and thus less self-consciously Walrasian, yet it too «was marked by the same idea that governed the whole work of Polterovich at that time: to show the connections between well-known (and politically correct!) optimization results on the one hand, and equilibrium theory on the other» (Boldyrev and Kirtchik 2014, 449). This is also true of some of his more narrowly technical work such as Mityushin and Polterovich (1978). In this case the technical result involves additional restrictions on individual preferences that will guarantee monotone individual demand and in turn monotone market demand. This is not sufficient to reduce the model to a full single-agent optimization problem, but as with that case, the goal is find restrictions that will help get around the problems of heterogeneity and differences in income. As the authors explain:

> If there exists only one consumer … in an equilibrium model, then this model reduces to an extremal problem and is rather easy to study. Monotonicity criteria are useful in those cases, where there are many consumers and their total demand isn't generated by a single goal function.
>
> (Mityushin and Polterovich 1978, 5)

It should also be noted that some of the later research by other Russian, even post-Soviet, scholars, has continued to follow this same theme (Norkin 1999 for example). Although aspects of these various attempts to integrate Walrasian equilibrium and optimization theory – from Nemchinov and Dudkin, through Polterovich's work, and on to some of the post-Soviet literature – are quite different, the core strategy in all of this research remains consistently the same. As we will see in the next section, although this strategy is quite understandable, it is at odds with the SEPC of the Western Walrasian theorizing during this period: which makes its tensions and weaknesses understandable as well.

3. Western Walrasian Theory during the Middle of the 20th Century

The Walrasian economics that concerns us here is the abstract Walrasian general equilibrium theory of the 1950s and 1960s (generally called Arrow-Debreu theory). The models typically consisted of multiple agents and n-goods with a perfectly competitive market for each good. The demand side of the market was based on the optimizing behavior of budget-constrained (ordinal) utility-maximizing consumers, and in pure exchange models the supply was simply the total endowment of each of the n goods. In production models the supply side was based on the profit-maximizing behavior of perfectly competitive firms. Research focused primarily on the existence, stability, and uniqueness

of the general equilibrium price vector; the Pareto efficiency of the associated equilibrium allocation (the first and second fundamental theorems of welfare economics); and various comparative statics exercises. The canonical texts of the research program is Gerard Debreu's *Theory of Value* (1959), but most of the results available to Soviet economists during the 1960s came in the following decade and were summarized in the most systematic way in Kenneth Arrow and Frank Hahn's *General Competitive Analysis* (1971). A more recent summary of the results is contained in Lionel McKenzie's definitive *Classical General Equilibrium Theory* (2002).

There is an extensive literature in both the history and philosophy of economics debating its scientific adequacy, empirical relevance, politics and personalities, core foundations, and many other aspects of this type of Walrasian theorizing, however I will not add to (or take sides on) any of the literature on these wider debates. My focus here is much more narrow. I want to emphasize one particular feature of such Walrasian modeling – one aspect of its SEPC – that is particularly relevant to understanding the problems Soviet economists encountered when trying to apply this set ideas to their own problem situation.

These Walrasian models involved optimization, however all of the optimizing was done by *individual economic agents*. These agents were generally households or firms and not individual humans, but there was no central planner or single representative agent whose optimization generated the market demand, market supply, equilibrium price or output of any good, or the total output of the entire economy. Consumer utility-maximization was *behind* the demand for each good and firm profit-maximization was *behind* the supply of each good – and these underlying optimization problems imposed certain restrictions on the market level demand and supply functions – but optimization did not determine the general equilibrium. The general equilibrium was determined by a set of prices where demand = supply for every good and that involved the higher level, institutional, mechanism of a system of competitive markets. This was the way it was in Walras' original *Elements* in 1874 and it was an essential feature of the SEPC of mid-twentieth century Walrasian theorizing.[1] One of the main tasks of Walrasian economics has been to show how, in the context of an idealized mathematical model, it is possible, under conditions of perfect competition, that the individual maximizing behavior of a large number of different agents could (could possibly) lead to coordinated efficient production and distribution of goods and services, rather than the self-interested

[1] Although, as I will note later, it is not an essential feature of every body of economic theorizing calling itself Walrasian (particularly more recently).

chaos one might expect (*i.e.* Hobbes's war of all against all).[1] If the economy had only one decision-maker, there would be no coordination problem and no need for general equilibrium theory.

The traditional term for this coordination of individual action is of course the 'invisible hand,' but that is probably not a good term since it has become ideologically charged and also suggests a kind of optimality that goes beyond solving the coordination problem. The equilibrium price is not anyone's goal, or intention, or optimal – buyers would prefer the same quantity of the good at a lower price and sellers would prefer it at a higher price – it is simply an unintended consequence of individually rational action under a particular set of institutional constraints. This aspect of the Walrasian theorizing of the period is summarized nicely on the very first page of Arrow and Hahn (1971):

> Adam Smith's "invisible hand" is a poetic expression of the most fundamental of economic balance relations … Whatever the source of the concept, the notion that a social system moved by independent actions in pursuit of different values is consistent with a final coherent state of balance and one in which the outcomes may be quite different from those intended by the agents, is surely the most important intellectual contribution that economic thought has made to the general understanding of social processes.
>
> (Arrow and Hahn 1971, 1)

The abstract Walrasian models of this period demonstrated that such a coordinated action was possible under a certain set of conditions (which the models specified in detail). A general equilibrium model with a single representative agent whose optimization produced the equilibrium output of the entire economy (or even an entire market) was simply not a Walrasian model; its core features were inconsistent with the *Raison d'être* of Walrasian program. Assuming a single representative agent in other kinds of mathematical economic models, optimal growth theory for instance, might be just fine – different tools for different tasks – it just wasn't appropriate in Walrasian theorizing.[2]

Although Walrasian theory was supposed to shed light on how competitive markets could efficiently coordinate the interactions of different agents, and although models with a single agent shed no light on that problem, the fact is that models with a representative agent were mathematically much more tractable and easier to analyze than models

[1] Hands 2015 provides some arguments for why, given the impact of the Great Depression and WW II, Western economic theorists of this period were particularly interested in such questions.

[2] As Frank Hahn remarked to Paul Samuelson in a letter dated June 22, 1993 (Samuelson Papers in the Rubinstein Rare Book and Manuscript Library at Duke University [Box 36]): «… it is the use of the 'representative agent' – a disastrous concept … It is *not* General Equilibrium analysis.»

with a number of interacting agents with different preferences and incomes. There are several reasons for this. First of all, the multiple-agent Walrasian equilibrium model of supply = demand for each good has much less mathematical structure (hence the technically difficult task of proving existence, uniqueness, stability, etc.) than an equilibrium given by a single maximizing agent (the optimization, unlike the equilibrium, problem has second order conditions that provide additional mathematical structure). Secondly, in multiple agent models changes in prices cause income effects that change the behavior of the agents in the model, making analysis much more difficult. This is particularly the case in the analysis of stability, where income effects are notoriously problematic. Third, representative agent models have a natural social welfare function – the utility function of the representative agent – which makes normative welfare analysis much more straightforward than in multiple agent models with heterogeneous agents.

But even with all of these reasons why representative agent models might be much easier to work with, the assumption of the representative agent, or other assumptions that implied such an agent, were steadfastly resisted by Western Walrasian economists. As economists like Kenneth Arrow and Frank Hahn put it:

> … the homogeneity assumption seems to me to be especially dangerous. It denies the fundamental assumption of the economy, that it is build on gains from trading arising from individual differences. Further, it takes attention away from a very important aspect of the economy, namely, the effects of the distribution of income and of other individual characteristics on the workings of the economy.
>
> (Arrow 1986, 390)

> If the rest of economic theory proceeded on these [representative agent] assumptions, welfare economics, for instance, would become extremely simple and stability analysis would be child's play. Indeed, a competitive economy could always be studied as if it were maximizing a utility function. Much of what we have regarded as interesting and important would be lost.
>
> (Hahn 1983, 42)

And there is the additional problem of course that a single optimizer not only means no coordination problem, it also means no trade.

> This dilemma is intrinsic. If agents are all alike, there is really no room for trade. The very basis of economic analysis, from Smith on, is the existence of differences in agents.
>
> (Arrow 1986, 389)

In order to see how Western economists generally resisted employing the assumption of the representative agent it is important to recognize that such models need not (literally) have a single agent. By this I mean that it is not necessary to explicitly assume that a market or the economy consists of only one individual in order to obtain the convenient

mathematical results associated with the representative agent. There are a number of different assumptions that can be imposed on the agents in multiple-agent models that make it behave *as if* there were only one representative agent. These assumptions were often called aggregation assumptions since they guarantee that aggregate (market) demand functions have the same properties as the demand functions of individual agents, but there are also revealed preference assumptions that give the same result.

The word 'aggregation' can mean a variety of different things in modern economics (Hoover 2012), but in the case of the Walrasian theorizing during this period it meant the aggregation of individual demand functions.[1] The most common such assumption is that all agents have identical homothetic (or homogeneous) preferences. Under this assumption all indifference curves are radial blowups so consumers continue to buy goods in exactly the same proportions as income changes; this implies that market demand functions are unit income elastic, and most importantly, that all markets behave, and thus the market economy behaves, as if there were a single representative agent.[2] Another, but related approach is to impose a revealed preference assumption on the market demand (or excess demand) functions. Samuelson's original 1938 condition – what later came to be called the weak axiom of revealed preference (WARP) – was not completely equivalent to constrained utility maximization, but Houthakker's 1950 condition – the strong axiom of revealed preference (SARP) – was equivalent. So if a demand function satisfies SARP it is *as if* it were generated by a budget-constrained utility-maximizing agent. If the demand (or excess demand) function in question is a market, rather than individual, demand function, then that agent is a representative agent.

Because the single agent assumption was so powerful in producing results such as stability and uniqueness, assumptions were often used that were very close to aggregation assumptions. For example, the as-

[1] Such aggregation means that individual demand functions can be aggregated into market demand that is a function of the aggregate/total income. In other words, market demand functions would have the same properties as the demand functions generated by a utility-maximizing consumer: such as zero degree homogeneity, negative substitution effects, a negative definite Slutsky matrix, etc.

Aggregation of course allows the modeler to circumvent the Sonnenschein-Mantel-Debreu (SMD) results on market demand functions which say that in the general case market functions do not necessarily inherit the properties of the individual functions. See SHAFER and SONNENSCHEIN 1982 for a technical discussion, INGRAO and ISRAEL 1990 or RIZVI 1998, 2006 for a more historical discussion, and HOOVER 2010, 2012 or KIRMAN 1992 for criticisms.

[2] Early results for the uniform homothetic case were provided in EISENBERG 1953 and GORMAN 1953. See CHIPMAN 1974 for a detailed discussion of the technical results and the appendix to HANDS 2016 for a summary of the main results.

sumption that all goods are gross substitutes was often used, and considered to be empirically reasonable, but it implies that WARP holds on market demand functions which is close enough to having a representative agent to get the desired stability and uniqueness results. But the important point about the mid-twentieth century Walrasian SEPC is *not* that economists often used these backdoor methods to take advantage of the mathematics of optimization, it is that these are the *only* ways that something close to a representative agent was permissible. In particular, none of the main results of the Walrasian models of the period were derived by assuming that the economy had only one utility-maximizing agent, or assuming what Paul Samuelson called the 'Santa Claus' case of multiple agents with identical homothetic preferences (Hands 2016). In fact Samuelson is a good example since he was clearly willing to employ the representative agent in other classes of economic models but consistently resisted it in consumer choice theory or Walrasian general equilibrium theory. The economists doing Walrasian economics during this period were trying to investigate various properties of equilibrium prices in an economy with competitive markets, but one where it was, or at least could be, the case that «independent actions in pursuit of different values is consistent with a final coherent state of balance and one in which the outcomes may be quite different from those intended by the agents» (Arrow and Hahn 1971 above) and that is not a model with a single representative agent or a central planner.

4. You Can't Always Get What You Want (or Even What You Need)

This section will pull together the main arguments from the previous sections. The goal is to explain the fact that i) compared to the extensive literature on economic cybernetics, there was very little literature on Walrasian-based Soviet mathematical economics, and ii) the Walrasian literature that was produced was full of tensions, and one major tension was precisely the *inverse image* of one of the mains tensions at work in Western Walrasian theorizing. On the Soviet side, the goal was to use Walrasian equilibrium to help model a centrally planned economy with a single representative agent (to take advantage of the mathematical properties of equilibrium to help find the solution to an optimization problem). On the Western side, the goal was to use individual optimization to help model the general equilibrium of a perfectly competitive economy (to take advantage of the mathematical properties of individual optimization to help characterize the properties of a general competitive equilibrium). The Soviet economists didn't get much from the Western literature and wasn't able to do much with the models they

produced because what they were looking for was simply not there in the Western literature; on the other hand, Western Walrasian theorists didn't get any help in dealing with their main theoretical concerns from Soviet economists for the same reason. What each group was looking for was simply not in the literature of the other group. The core commitments of the SEPC of the two communities at that time prevented gains from trade or productive collaboration (as happened in other areas like linear programing and activity analysis). This section will elaborate on the details of this argument.

There seem to be at least *three basic reasons* why there was not much Soviet literature on Walrasian economics and why the same problematic issues frequently emerged in the literature there was. The first is well-known and non-controversial, so my discussion will be brief. The second is less well-recognized, but it seems to be noncontroversial once it is pointed out, so I will spend a bit more time on it. The third reason does not seem to be recognized at all by either historians or the relevant economic theorists, and it will be the main focus of the discussion.

The first thing working against the popularity or success of Walrasian approaches to Soviet mathematical economics is simply the fact that the Walrasian theorizing was primarily demand and utility-focused, while Soviet mathematical economics was supply and production-focused. To put it starkly, the Soviet mathematical tools were designed to improve the efficiency of the production of goods, not to improve the degree to which those goods satisfied the whims of individual consumers. Many Walrasian models were pure exchange models with no production at all, and even when firms were explicitly included, the production aspects of the models never did any of the analytical heavy lifting.[1] This seems to be a fairly straightforward reason why «most of the Soviet work is closer to 'operations research' than it is to variations on a theme by Walras» (Ellman 1973, viii).

The second issue is that Soviet scholars were more focused on practical application – empirically implementable models – than Western economic theorists. The overarching concern of the Soviet SEPC was reforming economic planning; they were involved in the grand project of building socialism and that fact weighed heavily on the community's decisions. They focused on *implementable* models because they needed to find the solution/equilibrium and to allocate resources on the basis of it. This led them to emphasize linear models and focus on parameters like input-output coefficients that could, at least in principle, be empirically determined; after all, if the point is planning, one needs quantita-

[1] This is particularly the case in the literature on stability and uniqueness where all of the problems came from income effects on the demand side.

tive answers about what should be produced under what conditions. An existence proof such Arrow and Debreu (1954) – arguably the single most important result in Western general equilibrium theory – that proves an equilibrium exists but doesn't provide any way to compute it isn't a very useful result from the Soviet perspective. On the other hand, the vast majority of the Western literature was not computationally oriented; it was more concerned with 'how possibly' than 'how actually.' The theoretical analysis focused on questions like: 'Is it possible that a competitive economy composed of individual maximizing agents could have an equilibrium price vector that simultaneously clears all markets? (the existence question), 'Is it possible that the law of supply and demand could adjust prices in a competitive economy in such a way as to converge to the equilibrium price vector?' (the stability question), and 'Do utility maximizing consumers and profit maximizing firms guarantee that the equilibrium price vector is always unique?' (the uniqueness question). The point of the Western literature during this period was that the competitive market would achieve general equilibrium; no individual or institution is needed to find, or implement, it. There was of course a literature on «computable general equilibrium» developed by Herbert Scarf in the late 1960s (Scarf 1967) and extended by Shoven and Whalley (1984) and others, and it was a very important body of research for certain applications, but it was never the mainstream focus in, or the source of the economics profession's commitment to, Walrasian economics during the period under consideration. The Soviet economists doing equilibrium analysis never lost sight of the question of empirical implementation, while for most Western economists the point of the exercise was that no such implementation was needed. Different SEPCS indeed. This, like the emphasis on demand and utility theory, created barriers to the adoption of Walrasian ideas among Soviet scholars.

These two factors certainly go a long way toward explaining the paucity of the Walrasian literature and the kind of Western mathematical economics that was adopted, but there is a third, and in some ways a deeper issue. The two issues discussed so far may explain the general lack of interest in Walrasian economics, but they do not address the technical details of the particular models produced by Soviet scholars. My argument is that recognizing the quite different views of the representative agent – and the associated differences regarding individual vs. collective choice and equilibrium vs. optimization – gives us this more close-focused understanding of the content of Soviet Walrasian models (and why the content was what it was).

So the third (and generally unrecognized) point is that single-optimizer general equilibrium models were not acceptable to Western general

equilibrium theorists, but a single-optimizer is exactly what the Soviet economists were looking for. This generates a tension that can be seen in the various Soviet attempts to combine the two approaches. It is particularly clear in Polterovich (1973). Here he is trying to use the results of Western theory, but cannot directly use the existing standard results because Walrasian theorists systematically resisted the single-optimizer framework. As explained above, Western economists certainly could have – and it would have made the analysis much easier (or as Hahn noted 'child's play') – but they didn't, they resisted the temptation. The decision to work in the field of Walrasian general equilibrium theory was a commitment to work within the constraints of the research program's SEPC and the reduction of general equilibrium to a single-agent optimization problem would have been in direct conflict with the program's *Raison d'être*.

Of course the focus was quite different on the Soviet side. Oskar Lange had suggested that notions of consumer choice from Walrasian economics could be adapted to socialist central planning as far back as the socialist calculation debate in the 1930s:

> … freedom of choice in consumption does not imply that production is actually guided by the choices of consumers. One may well imagine a preference scale fixed by the Central Planning Board while the price system is used to distribute the consumers goods produced.
>
> (Lange 1936, 70)

To the extent that Walrasian general equilibrium theory entered into the conversation of Soviet mathematical economics during the 1960s and 1970s it was primarily through this idea – the idea of a collective representative agent – which simultaneously i) avoided the «long suspect» (Zauberman 1967, 42) neoclassical notions of indifference curves and individual preferences, and also ii) held the promise of converting mathematical general equilibrium theory into a technique that could be used for practical application to socialist planning. In the Soviet context a general equilibrium should reflect the «basic identity of all interests in society with one another and with the overall social interest» (Zauberman quote in the epigraph) and so associating general equilibrium with social welfare through the maximizing behavior of a single representative agent (Central planner) came quite naturally and was entirely appropriate. In fact, to do otherwise – to model the social outcome as emerging from the self-interested actions of dissimilar economic agents – was to contribute to bourgeois obfuscation: to use mathematical economics to defend the invisible hand of capitalism rather than as a practical tool for building a socialist society. So how did Polterovich and others apply Western Walrasian theory? Where did they go to find models consistent with the Soviet SEPC? They went to those very few examples

in the Western literature where, for very specific reasons, aggregation assumptions had been used to reduce the problem to one with a single-optimizer. Polterovich in particular wanted to bring Walrasian equilibrium analysis to Soviet economics, but to do so he was forced to draw resources from an atypical corner of the Western Walrasian literature.

The bottom line is that although the difference between the way that Western and Soviet general equilibrium theorist treated the representative agent has not been explored within the existing literature, it provides a nice framework for additional insights into the interaction between, and modeling strategies employed by, the two communities of mathematical economists. Not only did the two have different conceptions of the theoretical importance of the representative agent because they held different beliefs about the core motivation for general equilibrium theory, these differences also provide an additional reason why there was less sharing of analytical tools in general equilibrium theory than in other areas of mathematical economics. Since representative agent-based modes were considered to be relatively uninteresting special cases within the Western literature, relatively few examples of such models were available and the analytic framework associated with them was not very well-developed. But this meant that when Soviet economists began looking to Walrasian economics as a possible resource for theoretical insights and mathematical tools, they found very little that was useful; and, on the other hand, the general equilibrium models they developed for their own theoretical purposes were not generally models that were of much interest to Western theorists. Differences in the political and economic context inhibited the mutually beneficial transfer of scientific ideas within general equilibrium to a greater degree than in other areas of mathematical economics.

4. Conclusion

In conclusion, I would like to note that even if the reason for an author's choice of title is not obvious to the reader at the start of paper, it almost always is by the end, and in this case, that may not be true. One could read, and perhaps even be persuaded by, the above story and still have the question about the title: So what exactly is the «crossing in the night»? The problem is that to understand it, we need one more piece of information not in the above discussion, and that is that Walrasian general equilibrium models with a single utility-maximizing representative agent are now quite acceptable, in fact standard, in modern macroeconomics. A dynamic stochastic general equilibrium (DSGE) model typically starts with a single representative agent (who both consumes goods and supplies factors) and is constrained by the available

technology and consistency with competitive general equilibrium.[1] The Walrasian SEPC has changed[2] and the kind of central planner's version of general equilibrium theory that the Soviet economists were looking for during the 1960s and 1970s – but didn't find (or at least didn't find easily or without additional special, and not very appealing, assumptions) – is now the standard framework in modern macroeconomics. So the crossing in the night is simply that during the Cold War when Soviet economists were looking for representative agent Walrasian models, Western mathematical economics had essentially nothing to offer, but now that the Cold War and the context of central planning are gone, models with the features they were looking for are readily available in Western economics. I will leave it to the reader to contemplate the irony of the fact that DSGE evolved out of New Classical macroeconomics and yet its core theoretical construct is a Soviet social planner's problem.[3]

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[1] See Woodford 2003 for the canonical text and Hoover 2010, 2012 or Kirman 1992 for a critical discussion.

[2] At least in macroeconomics, but with the rise of game theory, experimental economics, and behavioral economics in microeconomic theory, pure macroeconomic theory may be one of the only places where abstract competitive equilibrium theory still has a deep foothold within contemporary economics.

[3] As a final remark it should be noted that the argument in this paper is at odds with some of the accounts of the relationship between Western and Soviet mathematical economics in the recent literature. Bockman 2011 in particular argues that the representative agents in these recent macroeconomic models were always a standard feature of modern mainstream economics: that «In the minds of economists, all members of a community can be considered identical and thus replaceable with one individual, whom economists called the 'representative agent' or the 'social planner'» (174). I hope I have demonstrated that for a period of time during the middle of the twentieth century – a key time for the transference of ideas between Western and Soviet mathematical economists – this was clearly not the case. Not only was the representative agent not a part of Walrasian economics during this period, this fact helps explain why the transfers took place in the particular way they did.

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