Econophysics and the Distribution of Wealth: An Extended Comment

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Abstract

Some recent papers have proposed models of trading which produce realistic-looking distributions of wealth. While the models are technically correct, this Comment argues that they involve undeclared simplifications that limit their application. Though the papers claim persuasive empirical support for their models, many of the undeclared simplifications point to empirical propositions that are easily refuted. Since many of the simplifications depend on a dismissal of the economists' favorite price mechanism, it is important to realize why the models do not propose a coherent alternative. Thus, the paper suggests several directions for future research on this important topic.

Keywords: distribution of wealth, inequality, econophysics, trading mechanisms, price mechanisms, methodology, name-dropping
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* Though these people do not necessarily agree with what is written, this paper has benefitted from the comments of Richard Arnott, Bill Baylis, Stefan Bornholdt, J.-P. Bouchaud Bikas Chakrabarti, Eric Nodwell, Nicola Scafetta and Y. Sudhakar. Further comments are invited. Research assistance by Xuzhen Zhang is appreciated. Comments are welcome and can be sent to panglin@uwindsor.ca or to Department of Economics, University of Windsor, Windsor ON Canada, N9B 3P4. The most recent version of this Comment can be found at http://www.uwindsor.ca/PaulAnglin

Some recent papers[1] have proposed explanations for the distribution of wealth using a class of models that the authors describe as "simple", "rich", "intriguing" and "generic". The authors and certain commentators (Ball, 2002; Hayes, 2002; Buchanan, 2000) find it striking that such models can reproduce a statistical regularity which economists appear to overlook. They also use the models to comment on government policies that might improve the welfare of society. In my opinion, while the arithmetic is correct and the conclusions are seductive, the models are not sufficiently reliable to extrapolate their conclusions to the real world confidently.

These models focus on trading as the process which generates a distribution of W, something that is interpreted as a person's wealth. The trading process is vital since, without trading, wealth cannot change hands. These models suppose that, because markets do not fulfill all of the characteristics that economics textbook, the trading process favors traders who are already wealthy. Therefore, individual i's wealth may evolve according to:

$$dW_i/dt= \eta_i(t)\, W_i+ \sum_{j(\neq i)} J_{ij}\, W_j- \sum_{j(\neq i)} J_{ji}\, W_i$$

(Bouchaud and Mezard, eq. 2) where η is a random variable and $J= [J_{ij}]$ is an (asymmetric) matrix of terms describing "the amount of wealth that agent j spends buying the production of agent i". J_{ij} is assumed to not vary with W_i or W_j. Or the evolution of wealth may be determined by a difference equation (Chakraborti and Chakrabarti, 2000, p. 168)[2]

$$W_i(t+ 1)- \lambda\, W_i(t)= \epsilon(1- \lambda)(W_i(t)+ W_j(t))$$

where ϵ is a random variable and λ is an exogenous parameter used to create different processes. Both of these models preserve total wealth. These equations are sufficiently simple, and computers have become sufficiently fast, that it is easy to simulate these processes.

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[1] The relevant literature is large and the bibliography lists only some of the work. I think that the distinguishing characteristic of this literature is the use of a pairwise random interaction amongst a large set of traders based on a simple trading rule. Many of the comments below use the paper by Bouchaud and Mezard to illustrate specific concerns with this research program. This focus should *not* be interpreted as evidence that I think that it is weaker than others. In fact, my use of quotations indicate the clarity of its logic and why other authors have sought to comment on it or extend it in selected ways.

[2] The equation was amended from the published version following a personal communication.

These models are interesting because they offer an explanation of a macro-phenomenon based on unconsidered micro-process: the resulting stable distribution of wealth displays a power law. Many papers verify the resulting statistical regularity (e.g. Chakrabarti and Chatterjee, 2003; Reed, 2003; Souma, 2002) based sometimes on the entire distribution and sometimes on a part of the distribution. This Comment does not offer new mathematical insights or data because referees and editors have already determined the validity of the proofs presented in published papers.

I focus on a different aspect of the puzzle which may also be useful: the validity and relevance of the assumptions being used. As a scientific exercise which illuminates an unexplored aspect of a dynamic process or as a toy used to investigate a complex system, these models are interesting. As isolated models in a broader research program, their ultimate contribution is not yet known. The facts that the authors compare the outcome of these models to the distribution of wealth in specific countries and that they consider the implications of policy variables, such as taxes, suggest that the authors think that the models are almost realistic. Using incomplete models can be risky because of the possibility of unforeseen consequences. Given that existing research has demonstrated that the distribution of wealth is affected by many things, see Champernowne (1973), Sutton (1997) or the three volume collection of papers editted by Sattinger (2001) and wealth affects many other aspects of an economy, this Comment questions the sense in which these models appear realistic.

I think that any attempt to integrate other economic dimensions into these models would reveal the sense in which these models do not represent an economy of any sort. This stark statement is made with the full knowledge that the bibliographies of the papers often cite Adam Smith's *Wealth of Nations* and Paul Samuelson's textbook, which has introduced millions of undergraduate students to economics. I think that these models should not be regarded as serious comments on an economic issue for three reasons. First, some authors are confused or careless when using some key words. Second, the models overlook five specific principles, each of which has been refined by at least a century of economic debates. For this reason and to introduce readers to debates amongst economists that appear to have been overlooked, the

bibliography is relatively long.

The important of the third reason depends on difference between the methods of economists and physicists. Stanley, Amaral, Gabaix, Gopikrsihan, and Plerou (2001) noted that physicists are "fundamentally empirical" and use a model to highlight a new phenomenon. To the extent that a model is an analogy where the simplifications are irrelevant, criticisms of the model should not matter; only the data analysis should be important and it should precede the development of an explanatory model. Economists also theorize using simplified economies, test the predictions of models (of which there may be many) and compare different models of the same phenomenon. This step is necessary to link effect to cause and this new literature on the distribution of wealth asserts that a new kind of link has been discovered. I argue that the data analysis used in this literature uses a standard that ignores the kinds of differences between physical objects and economic objects that is especially relevant to the proposition advanced by this literature.

A simpler explanation, which must be at least part of the answer, has been overlooked. Using not-unrealistic parameter values, this simpler model implies that the richest 20 percent of the population owns more than half of total wealth and the poorest 20 percent of the population owns about 1 percent of total wealth. Further, the steps involved in constructing this model show why any distribution of wealth should be stable for reasons that nothing to do with the interaction between the rich and poor or the trading rule. Thus, the analysis of the data may not be as useful as some people hope and, maybe, the declarations should be more modest.

Five Basic Principles of Economics

The models focus on the evolution of a single variable, called "wealth" or "money",[3]

[3] Some papers perpetuate misunderstandings about the nature of money. For example, the *first* sentence of Bornholdt and Wagner (2002) claimed that Debreu's (1959) book studied the role of money in an economy. Debreu (p. 28 and endnote 3 to Ch. 2) claimed the opposite. A contemporary reference that focused on the nature of money is Patinkin (1956). Shi (2003, esp. section 1.5) offered a review of modern models.

Similarly, the *first* sentence of Manolova, Tong and Deissenberg (2003) claimed that

which cannot be produced or consumed but is used to procure items that are consumed. The dynamic process is described as scale-free.[4] When trying to relate the model to the real world, the importance of the measure is unclear: If W cannot be produced or consumed, then who cares if the distribution becomes so skewed that one person owns all of it? It is certainly true that many economists attempt to summarize personal or national well-being in terms of money or wealth or gross domestic product (GDP) but even introductory textbooks note deficiencies in this attempt.

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economists are "unable to explain ... the use of ... fiat money" without providing evidence or citations. This claim is noteworthy because these authors propose what an economist would describe as an "overlapping-generations model" where money is not "super-neutral" without referring to the extensive literature on either of these concepts. It is also noteworthy that p. 449 of their paper assumes that trades are coordinated by the same "Walrasian auctioneer" that they scorn on p. 446.

In a short paper, Yakovenko (2003, p. 1) states "Unfortunately, it is very difficult to find the data on distribution of money m. On the other hand, a lot of statistical data is available for distribution of income r (for revenue)". He then substitutes data on income to represent revenue. Similar claims of equivalence can be found in (Chatterjee, Chakrabarti and Manna, 2004, p. 161-162; Patriarca, M., A. Chakraborti, and K. Kaski, 2004, first paragraph). Dragulescu and Yakovenko (2000) is more careful in its introduction but not consistently. Anybody who uses a credit card quickly learns that there are important differences between the concepts of "money" "income", "revenue" and "wealth".

[4] The reasoning used to justify scale-free-ness fails to distinguish "real" and "nominal" wealth. Consider Bouchaud and Mezard (p. 537): "Since the unit of money is arbitrary, one indeed expects that the equation governing the evolution of wealth should be invariant when all W_i's are multiplied by the common (arbitrary) factor". Economists recognize that inflation, or a change in currency which changes all prices and incomes proportionately, has no real effect but any reasoning concerning inflation reveals little about interaction if some of us become rich in the sense that Bill Gates is rich. Although Bill Gates is about 80 times wealthier than the 400th ranked person on Forbes list (http://www.forbes.com), I think that few researchers would suggest that the effect of the relative bargaining power of these two people is the same as if one person were starving and the other were 80 times wealthier. Yet, the proposed processes study situations with this characteristic.

The essential problem is that proposal of scale-free-ness does not yield a unique process and this fact demonstrates one of the dangers of arguing by "analogy" (quoted in Dragulescu and Yakovenko , 2000, p. 723 and in Pianegonda, 2003, p. 668). For example, if two traders meet, trader i might receive a *fraction* $W_i^{\beta} / (W_j^{\beta} + W_i^{\beta})$ of the total wealth of the two traders where β is a fixed parameter. Dividing wealth in this way is also scale-free. Letting β vary from minus infinity (favouring the poor) to plus infinity (favouring the rich), the effects of scale-freeness could be studied independently of the other assumptions of the model.

I think that one source of confusion is that the authors view economic issues from a Mercantilist perspective. This perspective opposes what Adam Smith and David Ricardo identified as the true source of wealth in a nation: trading of goods for more-preferred goods, not the holding of money gold or wealth, enables a trader to become better off.[5] Many authors criticize this conclusion by noting that few markets display the conditions needed for the famous Invisible Hand result, or its precise formulation as the "First Welfare Theorem." Such authors seem to forget that trading can be mutually beneficial even when markets are not perfectly efficient and that the benefits of trading can be maximized even in the absence of perfectly competitive markets (e.g. consider a polluting industry controlled by a monopolist).

I think that the basic problem is that the models obscure ideas which would show whether their conclusions are robust. To answer my challenge, I think that the research program needs to be explicit about five Basic Principles:

Opportunity Cost,

Gains from Trade,

Margins,

Equilibrium,

Comparative Statics.

The following discussion illustrates how these models fail each of these principles in ways that

[5] Paul Krugman has written on why this important idea is so difficult to understand and to explain to non-economists: http://www.pkarchive.org then click on "International Trade," then on "Ricardo's Difficult Idea 3-96".

One implication of this Difficult Idea is that, even if monetary wealth is preserved, the transaction process is *not* a zero-sum game. This implication makes it difficult to understand the assumptions implicit in papers such as Pianegonda et al (2003). This paper was careful to note that individual welfare is not necessarily the same thing as wealth but it also noted (p. 669) that actions taken by poor agents to increase their own wealth, at the expense of their neighbour's, include such things as "changing production methods, borrow money" presumably to buy newer capital or technology. Each of these activities tends to increase aggregate wealth in addition to whatever effects they might have on their neighbours. In fact, Pianegonda et al (2003, p. 668) were careful enough to state that assuming an economy is similar to a zero-sum game takes issue with "economic orthodoxy". Their conclusion discussed some of the ways that this assumption might be remedied but, by assuming that the supposed gains in aggregate wealth are independent of the transaction process, the proposed model avoids the big question.

are empirically relevant.

Opportunity Cost and Gains from Trade

The principle of Opportunity Cost is so fundamental to economic analysis that its importance cannot be overstated. By contrast, most of these models assume that the only good has only one purpose. If the characteristics of an individual can be summarized by a single good, called money or wealth, then the models cannot allow for differences in taste. Such differences are important and relevant since economists often advocate a decentralized price mechanism *because* it enables traders with different tastes to consume different bundles of goods. If differences in taste are empirically relevant and if the models do not allow different traders to have different tastes, then the relevance of the models' predictions seems limited.

Some models have vaguely included more than one type of good. Though one can argue whether the exchange rate between goods is sensible, it seems to me that a sensible model of trading would not allow a trader to knowingly exchange one valuable item for a valueless item. To put this idea another way, without a sensible reason to trade, it becomes easy to "conclude" that any price mechanism produces a bad outcome (see Pianegonda, Iglesias, Abramson and Vega, (2003, p. 673) for some concluding language that could have been better chosen).[6]

Margins

Merely recognizing the existence of two goods, and including a measure of tastes to

[6] Chakrabarti, Pradhan and Chakrabarti (2001) offered a model with "money" and a "good" where, a trader who has no "money" or nobody else has any goods to sell cannot trade. A key result of this paper seems to be that a market's "self-organizing is seen to be significantly affected when the money supply becomes less than optimal" and that "this optimal money supply ... depends on ... scarcity in the commodity market" (abstract). Surprisingly, since the only benefit to holding money is to buy goods, the price of the good is fixed even if the aggregate money supply changes; if the price varied, and particularly if it varied proportionately with the money supply for reasons discussed in introductory economics textbooks, then there should *not* be an unique "optimal" money supply or excess demand. Thus, while this model recognizes the principle of Opportunity Cost, the resulting process appears not to be scale-free and it fails to address issues raised in the context of the principle of Equilibrium.

account for each trader's trade off between them, points to an empirical inconsistency in the models. For example, when the "price" of the consumable increases, a trader can reduce consumption without reducing that consumption to zero. In many cases, quantity demanded of a good is so sensitive to its price that expenditure on a good is *negatively* correlated with the price. (An increase in the price may cause a trader to want to sell more and I will comment on the selling dimension next.) The principle of a margin is especially important to the study of wealth since few people spend all or none of their wealth at one time.

While these models reject the economists' traditional ideas of a market process, it is unfortunate that the models also reject the notion of a price. Including price explicitly would make it easier to evaluate the empirical relevance of a model which assumes that "the amount of money earned or spent by each economic agent is proportional to its wealth" (Bouchaud and Mezard, p. 537): when the price varies, the value derived from spending a fixed sum of money varies. Similarly, it is difficult to evaluate the assertion that the value of an item is constant when there are many familiar counter-examples: e.g. a cup of coffee (early in the morning versus a second or third cup versus coffee consumed in the evening versus a cup of tea) or whether "value" is an objective or subjective concept. For this reason and because doing so needs to recognize the wealth disguised as a non-financial asset whose value is not only random but ambiguous, it is difficult to know how to verify the assertion that the evolution of wealth is partly due to accumulating differences between the price of an item and its value (Scafetta, Picozzi and West, 2004, p. 5). For all these reasons, noting the possibility of a variable consumption margin introduces several reasons why the models should be interpreted as focusing on a special case.

Equilibrium

Compared to other critics of economic processes, these models are careful about the concept of an equilibrium, usually meaning a stable distribution. But the models noted in the introduction do not represent an equilibrium in the sense of quantity supplied equaling quantity

demanded.[7] Since the models propose that trading occurs in pairs, the models must assume that any trader can satisfy the demands of any other trader at any time. This aspect of a model creates three problems that have not been acknowledged. First, if a trader can *always* produce enough stuff for any other trader then they should be able to produce enough for themselves. Clarifying the model in this way would have the unintended implication that poverty is not a barrier to consumption. Second, if a very rich trader met a poor trader, then the poor trader would realize that the rich trader wants to buy large quantity and the poor trader could say "Sorry, I haven't got any". Immediately and in contrast to the stated presumption of the models, this statement places the richer trader at a *disadvantage*.

Third, without a centralized market place, a rich trader incurs transaction costs in a way that is *not* necessarily proportional to W_i. The poorest trader always needs only one other trader to satisfy their demands while a rich trader must use several traders to satisfy their demand for

[7] Many authors assert that markets rarely attain a perfectly competitive equilibrium and use this assertion to justify an alternative model. Unfortunately, some models in this literature apply this assertion inconsistently, as noted elsewhere in this Comment. Paradoxically, in some models, an economist would predict either that *any* price could be an (perfectly competitive) equilibrium price or that there is no perfectly competitive equilibrium: i.e. if quantity demanded does not vary with price and quantity supplied does not vary with price. An equilibrium may have a second characteristic but the investigation of this characteristic, also discussed in connection with the principle of Comparative Statics, is incomplete in these models.

Economists continue to research this topic and curious readers can study many alternatives: e.g. Barro (1979), Benassy (1982) and Fisher (1983) offer three approaches that are detailed, different and internally consistent. List (2004) recently argued that markets converge to the perfectly competitive solution quickly; some readers may find it amusing that the same issue of this journal includes an article (Bils and Klenow, 2004) studying the prevalence of fixed prices. The evolution of thinking amongst economists can be seen most clearly in Krueger's (1997) presidential address to the American Economic Association.

The disagreement amongst economists tends to focus on how to "close" the model in a way that respects the independence of independent actors. Closure is important because each trader's expectations should be consistent with the other variables determined by the equilibrium and expectations affect the perceived purchasing power of any bundle of assets. Without this closure, the intentions of a trader may be infeasible and the model would have no reliable predictions. In passing, it is interesting to note that, in part, Vernon Smith is a Nobel Prize Laureate for using controlled experiments to demonstrate that markets are *more* competitive than they should be in theory.

goods. The number of other traders needed by a rich trader depends on how much each of the other traders has to trade, which is based on the distribution of trader types.

Different economists have proposed many different pricing mechanisms, and the debate amongst economists has not ended, but all economists agree that the process which determines prices cannot be separated from the process which determines quantities. Given the success of econophysics models in predicting stock prices, it may not be surprising that the models focus on the price dimension; but, because the models are being applied to a different question, it is disconcerting how many models ignore the quantity dimension. The easiest way to illustrate this connection is to consider a market which is thought to be out of equilibrium and to consider how to ration the implied imbalance between the intentions of buyers and the intentions of sellers. Without a market clearing price, the amount actually purchased by a buyer and the amount actually purchased by a seller would differ from their intentions and would depend on the mechanism used to distributed the imbalance. For this reason, the resulting distribution of wealth cannot be independent of this mechanism. A popular alternative is to suppose that any imbalance is rationed in a way that is consistent with a formal bargaining process (Pissarides, 2000; Shi, 2003).

Comparative Statics

Comparative static analysis emphasizes the idea that a careful study of the effect of a change in a parameter ("exogenous variable") requires comparing the equilibrium solution ("endogenous variables") before the change with the equilibrium solution after the change. Comparative statics analysis helps economists to answer the kinds of questions that people ask: Is "globalization" or the information revolution responsible for the *changes* in the distribution of income/wealth? Do free markets or central planning or some third way *increase* total wealth?

In the context of these models, comparative statics analysis could show the effects of different trading mechanisms. Being able to isolate these effects from other aspects of the model would show which parts of the simplified models are important. Some authors, e.g. Bouchaud and Mezard, claim that their mechanism permits many interpretations. Scafetta, Picozzi and

West (2004) claim that their model provides more support for a classical economic model rather than a neo-classical model. The writings of some authors (Das and Yarlagadda, 2004, first paragraph) can be interpreted as claiming that all other explanations of the distribution of wealth are irrelevant. Before accepting these claims, I wonder if this class of models permits the economist's textbook model as a special case.

The textbook model would be useful as a reference point, if only because it is commonly discussed. Consider a model which includes a parameter measuring the "degree of market imperfection." Call it c. If c equals 0, then the model would be the textbook model and we could study its properties. If c is not equal to zero, but the equilibrium outcome were a continuous function of c, then the economists' textbook answer would be approximately correct for c close to 0. Identifying the effects of a change in c would be a truer test of whether and how the transaction process affects the distribution of wealth. A complete study would also identify the kinds of subsidiary implications that help to distinguish competing hypotheses of a phenomenon and identify which simplifications of a model limit the generality of an analysis.

The most useful kinds of comparative statics experiments focuses on key parameters of a model which, for Bouchaud and Mezard's (2000) model, would focus on J. Unfortunately, J is not directly observed and using some other effect of J to infer J is likely to depend on whether other aspects of the model are realistic. Some models (e.g. Chakraborti and Chakrabarti, 2000) suggest a link between an observable parameter (savings rate) and the skewness of wealth. Unfortunately, a true test of this prediction reveals two reasons why economists are so concerned with a proper comparative statics experiment and why methods used in other fields may be less successful when used to study people. First, a test involving long-term differences in the savings rate would require comparing the distributions of wealth in different countries and countries differ in ways other than the saving rate; a true test would need to identify and control for the influences of these other ways. Second, as demonstrated below and in contrast to the model, the savings rate is not an exogenous variable: it is the result of a combination of other economic processes, each of which can affect the distribution of wealth in different ways.

A comparison shopping process, where each buyer (seller) meets many sellers (buyers) but selects only *low price* sellers (*high price* buyers) as trading partners, is an empirically relevant alternative model of an imperfect trading mechanism that seems to have been overlooked by this literature.[8] This margin of adjustment gives even the poorest trader a kind of market power that is easy to exercise and would limit the degree of price dispersion implied by many models. In such a model, c could be the cost incurred by a buyer to compare prices at different sellers; Diamond (1971) showed that the market equilibrium in such models is not necessarily a continuous function of c.

Saving, Age and Wealth

The models noted above are simple and simple models have the advantage of summarizing key information into a few parameters. In such cases, estimating those parameters imposes useful discipline on the logic. This section imposes this discipline on one of the models above and then investigates an alternative that necessarily explains part of the observed distribution of wealth.

Bouchaud and Mezard (2000) find that their model produces a limiting distribution of wealth described by a Pareto distribution with parameter $\mu = 1 + J/\sigma^2$ where σ^2 represents the variance of the rate of return on wealth. The literature suggests that $\mu = 1.5$ is not unreasonable. Evidence on the inflation adjusted rates of return on the S&P 500,[9] a relatively risky form of investment, for the last 75 years suggests that $\sigma^2 = 0.04$ per year. A safer investment in bonds over the same time period implies $\sigma^2 = 0.01$. Using these numbers, the model requires J to lie

[8] While it is easy to argue that price dispersion causes comparison shopping, it is also true that enough comparison shopping makes price dispersion unsustainable. Burdett and Judd's (1983) equilibrium model derived a distribution of prices with a compatible level of comparison shopping. Later work used these kinds of ideas to study labor markets (e.g. Pissarides, 2000), financial markets (Duffie, Garleanu and Pedersen, 2001; Duffy, 2001), real estate markets (Anglin and Arnott, 1999) and money markets (Shi, 2003).

[9] http://www.martincapital.com/longterm/LngTrm10.htm: This website also reports that the annual rate of return (inflation-adjusted) for the last 75 years averaged about 9.5 percent on the S&P 500 and 2.5 percent on bonds.

between 0.02 and 0.005 where, in this simplest model, J represents the rate at which each trader trades with all other traders.[10] This range of not-unreasonable values for a key parameter permits wide latitude when deciding whether the model accurately represents the data, if this model is the only explanation permitted.

Chakraborti and Chakrabarti (2000) and Chatterjee, Chakrabarti and Manna (2004) developed a model where an (exogenous) savings rate was the key parameter controlling interaction. People save for many reasons but one of the most important is to create a stock of wealth to be spent during retirement. Empirically, knowing how wealth is distributed across different ages is at least as important as knowing the overall distribution of wealth: Kennickell (2002, esp. Figure 3) showed that the percentage difference between the wealth of the median 30-year-old and the wealth of the median 50-year-old American is about the same as the difference between the 10^{th} percentile of 50-year-olds and the 90^{th} percentile of 50-year-olds.

To study this issue further, suppose that each person starts working at age 20 and lives for a maximum of 100 years. Income, y, is the same for all workers. While working, the person saves S per year. At date T, they retire and plan to spend their wealth at a constant rate R. Though R, S and T may appear to be arbitrary, two simple conditions show how they are linked by ideas commonly applied by economists.[11] First, the reason to save is to pay for consumption after retirement. Thus the present value of post-retirement consumption should equal wealth at the date of retirement. Second, there is a trade off between savings and consumption and the easiest resolution of this trade off invokes the idea of consumption smoothing: consumption

[10] I am indebted to J.-P. Bouchaud for pointing out an error in a previous draft.

[11] This model is consistent with the "Permanent Income Hypothesis" that is an integral part of most models in macroeconomics. Some econophysics models of trading seek to link their ideas to traditional economic models but, to me, the link is not as tight as it should be. In an otherwise ingenious paper, Das and Yarlagadda (2004) assert that they can derive a model of trading behaviour based on the behaviour of a single (price taking) trader without explaining the process which determines the price. Further, because the dynamics of an individual's wealth over time varies with the level of wealth in these models (see Pianegonda and Iglesias (2004) for careful analysis), their conclusion that the (endogenous) savings rate can be constant and consistent with the Permanent Income Hypothesis should be examined more carefully.

should not vary with age.

To apply these two conditions, more aspects of the investment process need to be specified. Suppose that an investment grows exponentially and without risk at a rate of r: saving S at age t becomes S exp(r(T- t)) at age T and a constant flow of saving means that wealth at an age of a is

$$W(a)= S (\exp(r(a- 20))- 1)/r.$$

After age T, wealth is used to pay for consumption until age 100; the present value of such consumption at age T is

$$R (1- \exp(-r(100- T)))/r.$$

To summarize, R, S and T are related by a desire by consumers to smooth consumption over time, i.e.

$$y- S= R$$

and the need for that consumption to be self-financing, i.e.

$$S (\exp(rT)- 1)/r= R (1- \exp(-rT))/r= W(T).$$

Some simple assumptions on r and the savings rate complete the calibration of parameters. Consider a (riskless) rate of return of 6.5 percent: r= 0.065 and a retirement data of T= 65. Computation shows that a savings rate of 0.048 during a working life maintains a constant rate of consumption over a lifetime *and* ensures that this consumption can be funded until death at a maximum age of 100. A change in r or in the savings rate would produce different solutions for R, S and T but these numbers are broadly consistent with observed data.[12]

[12] The conclusion will note that simplified models are sometimes too simple. In this model, most people die before reaching age 100. Integrating this fact into the algebra would either increase the skewness of the distribution of wealth, if wealth is passed from rich parents to rich children (Charles and Hurst, 2003), or lead to difficulties in interpreting the data, if people use pensions issued by insurance companies in place of personal wealth to finance consumption during their retirement, or both. Only one paper (Dragulescu and Yakovenko, 2000) considered the possibility of a trader temporarily going into debt (i.e. negative financial wealth), and only then with a borrowing rate of 0 percent which may be inconsistent with an equilibrium. The assumption that income does not vary with age also begs to be improved but most changes to this assumption would raise a question of whether it is proper to distinguish wealth disguised as

Though each person's wealth is a simple function of their age, deriving the implied distribution of wealth is complicated by the fact that wealth is not a monotonic function of age: wealth is 0 at age 20 and at age 100, and is maximized at age T. Thus the fraction of the population with wealth equal to Z is the fraction of young people with wealth Z, d(Y(Z)), plus the fraction of old people with wealth Z, d(O(Z)), where d(.) is the age distribution of the population. Inverting the equations above to solve for age as a function of wealth shows that

$$Y(Z)= 20+ \ln(1+ rZ/S) / r$$

$$O(Z)= 65+ \ln(1+ rZ/(W(T)- R)) / r.$$

By construction $Y(0)= 20$, $O(0)= 100$ and, since wealth is maximized at age T, $O(W(T))= Y(W(T))= T$.

With this introduction, it is possible to visualize the distribution of wealth for any given age distribution. The solid line in Figure 1 shows the Lorenz Curve for the distribution of wealth under a Cumulative Savings plan using information on the current age distribution of the US (Census 2000). As expected, it is skewed with the wealthiest people being near aged 65. Many of the models in this literature seek to compare the observed distribution of wealth to a Pareto distribution. The line with triangles shows the Lorenz Curve for that distribution using the parameter 1.5 which, some people claim, represents a reasonable value for the US. It is possible to compare these two distributions more precisely but, using an eyeball metric, the Cumulative Saving distribution looks more skewed for lower levels of wealth and less skewed for the higher levels of wealth.

A reader would be justified in believing that differences between the distributions might

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financial assets, which is greatest in older people, and wealth disguised as human capital, which is greatest in younger people. T= 65 is imposed artificially even though there are reasons to expect that the choice of T would vary with the realized rate of return. Integrating this choice into a model of wealth creation would require a more precise understanding of the trade off between labor and leisure.

Rather than offering a more complex model based on these added dimensions, my intention is to introduce a simpler model with more realistic behavioural foundations that also produces a realistic-looking distribution of wealth. None of these added dimensions were noted in the papers listed in the bibliography.

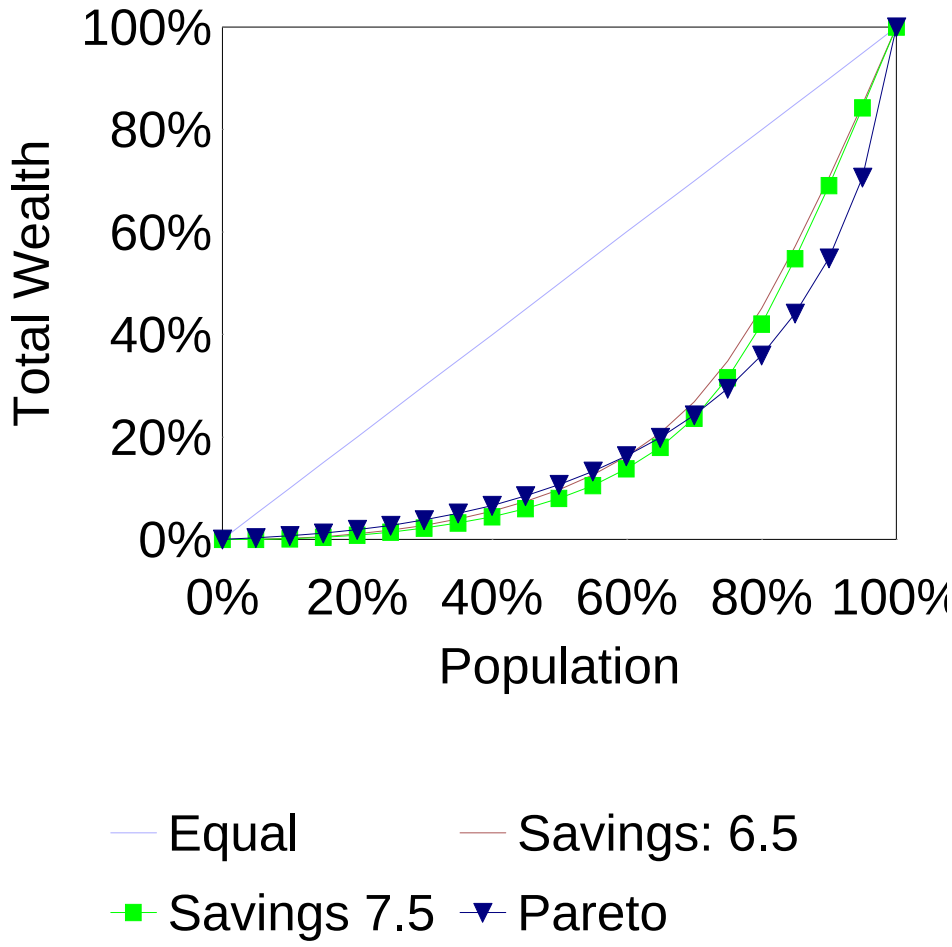

Figure 1: Comparing Distributions

be explained by one or more aspects omitted from both models. The principle of comparative statics shows how to investigate the significance of these omissions. To take a simple example, if the rate of return on investment had been full percentage point higher for everybody, 7.5 percent instead of 6.5 percent, then the higher rate of return disproportionately affects those who are already wealthy. But, since the reason to generate wealth is to fund consumption during retirement and since r is higher for a *lifetime*, there is also an indirect effect: the savings rate falls to 0.032. At low levels of wealth, the fact that wealth is nearly 0 implies that any change in the interest rate has little effect and, at high levels of wealth, the decrease in the savings rate offsets the increase in the rate of return. The resulting Lorenz Curve, using the current age distribution of the US, is shown with the line with squares.

It is also not unreasonable to suppose that people with more wealth would be willing to invest in riskier investments which, on average could produce even higher rates of return with less effect on the savings rate. Or, wealthier people may be older, have higher annual income and save more. Thus, this example illustrates why a process which produces something close to a Pareto distribution is not necessarily an unfair process. Using the eyeball metric again and to quote some authors out of context, these data "compare very well" or are a "very good" match or may "encourage" further work in this direction. More generally, this example illustrates why the distribution of wealth is stable regardless of why it is skewed. Further, while it is true that an eyeball metric is not very precise, it is also true that the power and size of other tests used in this literature are rarely reported.

The Search for Regularities

Learning about the field of econophysics has been an interesting and informative exercise for me. I know enough economics to know that economists do not have all of the answers and this Comment is partly an attempt by me to clearly state that we know more than we are given credit for. I also have some sympathy for opponents of academic economics who argue that we tend to give our greatest rewards to innovators in theory, as evidenced by the early winners of The Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel, to give the Nobel Prize in Economics its official title. The search by econophysicists for regularities in economic data (Roehner (2002) is a particularly passionate advocate) offers a balance to elaborate formalism but why economics should be balanced in this way remains a mystery to me.

It may help to understand why economists have mostly ignored this type of advice for a long time. I think the first reason is because it is possible to find somebody who claims to have found a regularity in economic data almost every day:

- the investment advice given by any stock broker is sensible only if there is a non-random pattern;
- suggestions for government policy depend on a stable relationship between the policy lever being pulled and the desired outcome.

So, by itself, the search for *new* regularities is not interesting exercise. A necessary second step

dismisses most claims: a claim should focus on well-defined variables, which footnotes 3, 4 and 12 demonstrate is not as easy as some may wish, and it should be verified independently for many times and places.

Second, a search for regularities seems to suffer from a flaw that is important in many economic issues. What is the contribution of a search which finds nothing? Colleagues would be very bored with your work if, every day, you found "nothing". Editors like to publish new findings. Yet many important ideas are based on "nothing":

- The idea that firms in a perfectly competitive market earn zero economic profit in the long run is important and used.
- The idea that the long run real money supply (i.e. the nominal money supply divided by the price level) does not vary with the growth rate of the money supply is important and used.
- The idea that income is approximately equal to expenditure regardless of prices is important and used.

Simon (1990) offered a list of 14 other regularities that he deemed worthy of knowing and of study.

Readers who assert that these (well-known) suggestions are not true regularities would demonstrate two more requirements for a regularity to be interesting. First, that it should not be constant. A feature of an economy which never varies for any reason is, in all senses of the word, exogenous.[13] Regularities which vary a little are more interesting. But, using the principle of Comparative Statics, it seems to me that the scientific contribution is to explain the variation, not the regularity itself.

[13] Note: it might be possible to assert that the distribution of wealth is a kind of universal constant which connects other variables. However, the cross-national evidence is not sufficient to justify this assertion and the discussion of the five basic principles shows that the existing class of models should be amended to remove the predictions for several important variables that are currently inconsistent with the data.

The second requirement is that any claim of a regularity needs to have a clear standard showing how much variation is acceptable and a rejection would represent a failure to meet this standard. In a related thought, it has surprised me that, of the many papers which claim a good match between their model's prediction and the observed distribution of wealth, none state a significance level on their tests and few attempt to establish whether their explanation is better than an alternative. It has also surprised me that the evidence used to validate a model focuses on a single aspect to the exclusion of other empirical implications. Finding nothing when something specific is expected advances Science but the value of this contribution depends on specifying the (reasonable) expected alternative. For this class of problems, there are many good reasons to expect the distribution of wealth to be skewed regardless of the trading process. Without a clear statement of the test, an independent observer cannot determine what would be necessary for a prediction to fail. Some papers (e.g. Das and Yarlagadda, 2004) repeat claims made by others that economists cannot fully explain the distribution of wealth; using the same standard of proof used in these papers might be a good place to start. As models of econophysics become progressively more established, the search for regularities will have to be replaced by another methodology which acknowledges that there are other explanations of the same phenomenon and *compares* them.

Finally, the history of economic science reveals several examples where an established empirical regularity has misled. The large scale Keynesian models of the 1960s and 1970s became unreliable at about the time that they were used extensively. This failure led to the insight now known as the Lucas Critique (Lucas, 1976). Game theory also reveals the importance of actions that are possible but not observed because they are "off the equilibrium path"; these actions would become relevant as soon as the economy changes.[14]

[14] For example, if all sellers charge the same price, such as in a perfectly competitive equilibrium, then the buyer's ability to compare prices at different sellers is not evident or does not need to be studied carefully. But, if the situation changes in a way that makes prices dispersed across sellers, such as when a market is out of equilibrium or in a different kind of equilibrium, then this aspect of a model cannot be ignored.

Concluding thoughts

Tools employed by physicists have created attention-grabbing profits for Wall Street firms, as discussed in Matenga and Stanley (2000), but the recent research on the distribution of wealth strays into areas where previously successful tools become unreliable. The ideas that economists have over-simplified their models of buying and selling and that a more realistic trading rule produces a more realistic distribution of wealth are interesting. The arithmetic of the models is simple, is correct and the results can be described with surprising ease. Though rarely acknowledged, this work has a history that dates back to a student of John Maynard Keynes (Champernowne, 1973) and earlier.

Any one model is a part of a larger research program where simple models are replaced by better models. As Einstein is supposed to have said: Things should be made as simple as possible *but no simpler*. The success of the models encourages further research but this Comment has tried to argue that, to study the behavior of real people who trade when they are willing and able, these models unintentionally propose a theory where traders are forced to trade and always can. I noted several instances where this class of models and their predictions ignore competing explanations as well as historically significant and empirically relevant basic principles. For these reasons and others, it is difficult for this economist to determine the power of the empirical tests used to investigate the hypothesis offered by this new literature.

Given the number of times that the name of Adam Smith is invoked, I am surprised that the models seem to reject his intellectual contribution. Only sprinkling words like “money” and “wealth” in the text make these models sound like a representation of an economy. I offer a process that is consistent with many of the ideas that neoclassical economists like, that helps to explain a phenomenon which is at least of equal importance and that has been overlooked as an explanation: the relationship between wealth and age. Analysing this process also creates a realistic-looking distribution. Moreover, it illustrates why questions concerning the distribution of wealth should not be isolated from other questions. Is the Cumulative Savings process too simple? Of course. As the bibliography and various footnotes show, both this model and the other models omit behavioral and technological features of an economy whose significance has

been repeatedly confirmed. For example, the difference between the distributions of wealth might be explained by differences in income where high income earners also save more. Or the difference between the distributions might be explained by wealthy people who avoid the costs of capital market imperfections to self-finance highly profitable ventures. Rather than seeking to explain why the distribution is skewed, a valid research program would identify features which *change* the skewness of a distribution expected to be skewed. *On the margin*, but not by themselves, these extra features are interesting.

Dynamic models would be appropriate and necessary to study the dynamics of poverty (Finnie and Sweetman, 2003; Charles and Hurst, 2003; Fujiwara et al, 2003). Dynamic models would also be appropriate to study whether an economy, where traders who interact repeatedly, would evolve toward using the kinds of trading rules assumed in these models, instead of using some other rule such as an auction or using intermediaries, or to study conditions under which trade using money would replace barter (building on the work of Manolova, Tong, and Deissenberg, 2003).

Another conclusion of this Comment should be that nobody needs to confuse the techniques of analysis with the principles that those techniques embody. Many academics and ordinary people claim that modern economics is merely mathematized ideology (e.g. McCauley, 2001) or that it is not a real science.[15] Like all scientists, economists try to ask questions that can be answered; other questions are left for future economists and amateur philosophers. Unlike researchers in the natural sciences, economists are less able to control the initial conditions of the experiments they use to test their hypotheses. Testing economic hypotheses also suffers from the fact that the actors being studied exercise freedom of choice. These facts make human behavior less predictable and make it harder to falsify some hypotheses. At the same time, this freedom of

[15] Physicists who suggest that economists should use more careful mathematical techniques might benefit from reading papers which study the relationship between economics and mathematics more carefully: e.g. Katzner (2003) and Wientraub (2002). The criticisms often focus on the presence of potentially-invalid value judgements embedded in the techniques or that certain ideas (and therefore the associated conclusions) are excluded because they cannot be expressed.

choice means that a human being can participate in a system *selectively* in a way that an atom cannot. These facts make certain methods of analysis inappropriate. They should also mean that an arithmetically correct result can be unimportant if it is not robust to perturbations from previously identified sources of ignorance. And they should change the relative value of theoretical analysis vs. analysis based on (limited) data. The five basic principles discussed above are easy to identify in the models which economists have found useful in the past but other models can also embody these principles.

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