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Comments

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Inequality and Poverty

In "Halving Global Poverty," Tim Besley and Robin Burgess (Summer 2003, pp. 3–22) note that, conditional on mean real per capita national income, higher inequality (as measured by the standard deviation of income) is correlated across countries with higher absolute poverty as measured by the headcount of those with less than $1 a day (see their Table 3). This result is not surprising. Indeed, as stated, the observation might be little more than tautological. After all, the more of total national income is taken by the rich and mean income is held constant, then the less is available for the rest, and as such there are likely to be more absolutely poor people.

Is there more to the inequality-poverty link than this tautology? In fact, there is. Consider a regression in which the poverty headcount is the dependent variable, and the two explanatory variables are the mean income of the lower 90 percent of the income distribution and the share of income going to the top decile. One might expect that after taking into account the mean income of the bottom 90 percent of the income distribution, the share of income going to the top percentile should not affect the poverty headcount.

However, when I carried out this regression for using data for 89 countries and territories—that is, all for which these variables are available using the World Bank GPID database and World Development Indicators—I found that the share of income going to the top decile is large and statistically significant. In an ordinary least squares regression, if the share of income going to the decile rises by 1 percentage point, the percentage of the population below the $1 per day poverty line rises by about half a percentage point, after including the mean income level of the bottom 90 percent as another explanatory variable and adding a constant term. This finding survives inclusion of other variables such as financial depth, and measures of institutional quality (Honohan, 2004). Almost equivalent results are obtained by substituting the mean income of the top decile for their share in total income. These results can be found at ⟨http://econ.worldbank.org/programs/finance/library/⟩.

It is by no means clear why making some rich people richer should increase the number of absolutely poor people: as an empirical fact, this is rather startling. Admittedly, it is consistent with most of the functional forms that are used to fit the statistical distribution of incomes in any country. Two-parameter functional forms for income distribution (such as the log-normal) almost necessarily imply such a relationship, in the sense that mean-preserving parameter changes for any given functional form will send the share of the top decile and the poverty headcount in the same direction. Even a more flexible functional form such as the widely favored three-parameter Singh-Maddala (McDonald, 1984) tends to predict the positive association in this sense. However, this insight only pushes the question back one further step inasmuch as these statistical models lack any serious economic rationale. Besides, the empirical fit of these curves near the ends of the distribution have traditionally been their weak point; thus, there was no *a priori* assurance that this prediction would have been empirically robust.

Of course, there is no surprise that increasing mean national income reduces the poverty headcount. But what is it about societies where the rich are rich that tends to result in more people falling into poverty? This appears to remain something of an unresolved puzzle.

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■ *Without implicating him, I am grateful to Aart Kraay for helpful suggestions. This note reflects the views of the author alone and not those of the World Bank.*

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Fuel Economy Standards

In their article "The Economics of Fuel Economy Standards," Paul Portney, Ian W.H. Parry, Howard K. Gruenspecht and Winston Harrington (Fall 2003, pp. 203–217) note that engineering studies suggest a substantial opportunity to improve the energy efficiency of new vehicles using demonstrated, cost-effective technologies and that the failure of markets to exploit this potential is difficult to reconcile with economic theory. Given the gap between evidence and theory, the authors express skepticism about the evidence; that is, they criticize the use of engineering studies in justifying more stringent fuel economy standards. The authors do not, however, note that important econometric evidence also points to significant inefficiencies in markets for energy-using equipment.

In a seminal paper on home appliance purchases, Hausman (1979) found that consumers employ an implicit discount rate of 25 percent per year in evaluating the net benefits of improved energy efficiency. A subsequent literature found discount rates ranging from 25 percent to 300 percent in markets for refrigerators, heating and cooling systems, building shell improvements and a variety of other technologies (Frederick et al., 2002). These anomalously high discount rates provide reason to reject the hypothesis that markets for energy-using equipment are characterized by substantive rationality and efficient resource allocation. In a study that is directly relevant to the fuel economy debate, Dreyfus and Viscusi (1995) undertook a hedonic price analysis of the U.S. automobile market to assess consumers' willingness to pay for improved safety and energy efficiency. On the assumption that consumers use a common discount rate in evaluating both safety and fuel economy, the study calculates an implicit discount rate that ranges from 11 to 17 percent in alternative specifications. More tellingly, Dreyfus and Viscusi conclude that only 35 percent of the present-value cost savings provided by improved energy efficiency is capitalized in the purchase price of vehicles.

Attempts to interpret the so-called "energy efficiency gap"—the failure of real-world markets to implement energy-efficient technologies that are cost-effective at prevailing energy prices—have focused mainly on issues of information asymmetries (Howarth and Andersson, 1993), bounded rationality (Conlisk, 1996) and inefficiencies in the structure of large organizations (DeCanio, 1993). These explanations are consistent with empirical work in behavioral economics, which finds that a wedge often exists between observed behavior and the model of substantive rationality in the context of intertemporal decisions (Loewenstein and Thaler, 1989). A contrasting approach is taken by Hassett and Metcalf (1993), who seek to explain the use of high discount rates as a rational response to issues of risk and irreversibility. Sanstad et al. (1995), however, show that these effects are too small to account for the empirical magnitude of the efficiency gap.

Although Portney and his co-authors reason that car buyers are well-informed about fuel economy tradeoffs by the energy labels required for new vehicles, behavioral studies suggest that providing consumers with technically accurate information often has little influence on their decision making (Gardner and Stern, 2002, chapter 4). This observation does not imply that consumers are fundamentally irrational. As Conlisk notes, limitations on people's cognitive capabilities imply that (boundedly) rational agents must rely on simple decision heuristics that are subject to systematic bias. In the context of automobile purchase decisions, energy costs (a) constitute only a small fraction of the total cost of owning and operating a vehicle; and (b) are not tangibly apparent to consumers at the point of decision. These conditions match the circumstances under which bounded rationality most typically prevails, explaining why consumers fail to optimize fuel economy choices as they would given perfect information and infinite cognitive capabilities (Kempton and Layne, 1994).

Portney, Parry, Gruenspecht and Harrington are on firm ground when they note the importance of taxing gasoline to reflect the social costs of fuel consumption. The imperfections that appear to exist in vehicle markets, however, weaken the force of the authors' critique of enhanced fuel economy standards. As the authors note, the National Research Council (2002) found that strengthening the Corporate Average Fuel Economy (CAFE) standards could substantially raise the energy efficiency of new vehicles while maintaining or enhancing consumer welfare. Although this conclusion seems difficult to reconcile with the traditional theory of efficient markets, it is arguably consistent with recent developments in information and behavioral economics.

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Reply from Paul Portney, Ian Parry and Winston Harrington

Unfortunately, one of the most crucial issues in assessing the economic merit of tighter fuel economy standards is also one of the most contentious. A number of engineering studies suggest that there is a wide range of fuel-saving technologies that could be adopted by auto manufacturers for which the discounted fuel saving benefits over the vehicle lifetime would exceed the costs of vehicle production—see in particular NRC (2002). If so, tightening Corporate Average Fuel Economy (CAFE) standards on new passenger vehicles can easily be welfare-improving overall; if not, tightening CAFE could be welfare-reducing, due to its perverse effects on increasing the incentive to drive and on compounding distortions from pre-existing fuel taxes (Parry, 2004).

Is there a market failure that prevents auto manufacturers from installing fuel-saving technologies that consumers should be willing to pay for? The most common hypothesis is that auto buyers have very high discount rates and undervalue the true social benefits from future fuel savings. Richard Howarth draws our attention to a number of econometric studies suggesting that consumers may in fact discount future fuel costs at high rates. Howarth's underlying explanation is that when fuel costs are a small portion of total vehicle owning and operating costs and are not tangibly apparent to consumers at the point of purchase, then ". . .[boundedly rational] consumers fail to optimize fuel economy choices as they would given perfect information and infinite cognitive capabilities."

Maybe. But some of the evidence Howarth cites relates to energy savings from home appliances, and it is not necessarily clear that discount rates from these studies are applicable to automobiles, largely because the energy savings are so difficult for consumers to observe. Of most relevance is the Dreyfus and Viscusi (1995) hedonic analysis of car purchases, which finds a discount rate of 11 to 17 percent. The discount rate used in the NRC (2002) report is 14 percent—exactly the midpoint of this range.

Perhaps it is not that consumers misperceive or overly discount fuel-saving benefits, but rather that engineering studies underestimate the true economic costs of actually adopting fuel-saving technologies. The true economic cost is probably larger than the engineering cost estimates used by the NRC for two reasons. First, it ignores the possible opportunity cost of not using fuel saving technologies for other vehicle enhancements. That is, by forcing automakers to apply their technical expertise to more fuel-efficient engines, tighter CAFE standards could mean fewer of the improvements to which consumers have responded enthusiastically in the past—including such things as enhanced acceleration, towing capacity and so on. It is the implicit values of these foregone improvements that ought to be compared with the fuel economy savings that tighter CAFE standards would bring. A second point is that engineering studies may exclude various sosts of actually implementing a new technology that are difficult to observe—for example, marketing, consumer unfamiliarity and retraining of mechanics.

While it would be extreme to assume all manufacturers incorporate fuel saving technologies that pay for themselves the instant they become available, basing the case for substantially tightening fuel economy standards purely on results from engineering studies is also on rather shaky ground. Until a greater consensus emerges on the extent to which the true economic costs of tightening fuel economy standards differ from engineering costs, policymakers would be well advised to focus on other initiatives that are on firmer ground. We would advocate a moderate (economy-wide) carbon tax to reduce greenhouse gases, a broad oil tax (of perhaps $3 per barrel) to help reduce the economy's dependence on imported oil, encouragement of per-mile insurance (rather than annual lump-sum payments) to reduce driving and reforming the CAFE program by allowing manufacturers to trade fuel economy credits.

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Exchange Rate Regimes

Guillermo Calvo and Frederic S. Mishkin's article "The Mirage of Exchange Rate Regimes for Emerging Market Countries" (Fall 2003, pp. 99–118) illustrates a problem that has plagued discussion of exchange rate regimes: lack of precision, especially regarding fixed exchange rates. Like many other economists, Calvo and Mishkin use the terms "fixed" and "pegged" interchangeably. It is confusing to have two terms for the same thing.

Their definition of one type of fixed exchange rate, a currency board, is vague in a key respect. Calvo and Mishkin (p. 100) say only that a currency board has "enough" reserves "to exchange domestically issued notes for the foreign [anchor] currency on demand." A currency board does not hold just any amount of foreign reserves that may be "enough." Rather, it holds net foreign reserves equal to 100 percent of the monetary base. A currency board does not let its reserves fall below 100 percent, nor does it accumulate excess reserves beyond at most an additional 10 percent. The excess reserves, if any, serve as a cushion against possible losses in the capital value of assets, not as a source of funds for discretionary monetary policy. A currency board holds no significant financial assets other than its foreign reserves, hence it does not hold domestic financial assets.

Their vague definition leads Calvo and Mishkin to say that Argentina's monetary system of April 1991 to January 2002, known locally as "convertibility," was a currency board. However, the central bank held large amounts of domestic financial assets, and over the life of the convertibility system, the ratio of net foreign assets to the monetary base was often quite far above or below 100 percent. It is more accurate to view the convertibility system as the latest of Argentina's many attempts to combine a hard pegged exchange rate with central banking.

Calvo and Mishkin claim that the choice of exchange rate regime "is likely to be of second order importance" in developing good overall economic policies (p. 115). Here again, I think it is possible to be more precise, in a way that is useful for economic policy. Even the best exchange rate regime (whatever you may consider that to be in a particular case) is not enough by itself to ensure economic growth, but a very bad regime is enough by itself to reverse growth. A good exchange rate regime enlarges the potential scope for mutually beneficial trades, but high tax rates, insecure property rights or other factors may still discourage people from actually making the trades. A very bad regime shrinks the scope for mutually beneficial trades, in extreme cases making barter more attractive than monetary exchange. The difference between a good exchange rate regime and the best regime may be small in terms of its effect on economic growth, but there is abundant evidence that the difference between a good regime and a very bad regime is large.

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■ *The views expressed here are the personal views of the author, not necessarily the views of the Joint Economic Committee.*

School Accountability

In their excellent article on "The Promise and Pitfalls of Using Imprecise School Accountability Measures" (Fall 2002, pp. 91–114), Thomas J. Kane and Douglas O. Staiger point out that many accountability systems treat small schools in a capricious fashion. Because a small school has greater sampling variation, its average scores, or average gains, are likely to be volatile and occasionally extreme, even if the school itself is steady and average. Quite ordinary schools, if they are small, are likely to be praised in some years and censured in others.

To address this problem, Kane and Staiger suggest setting "different thresholds for schools of different sizes. For example, grouping schools according to size . . . and giving awards to the top 5 percent in each size class." This proposal solves the problem of comparing small schools to large schools, but it does not change the fact that, within the small school group, schools in the top 5 percent are likely to be there because of luck. Kane and Staiger's other suggestion—lowering the threshold so that more schools win rewards—has the same problem.

What Kane and Staiger overlook is that at least one government recognizes the small-school problem and has taken steps to avoid it. The danger is addressed by Sanders's influential value-added methodology, which is a mixed model focused on year-to-year gains. In Tennessee and other systems that have adopted Sanders's suggestions, school effects are treated as random and estimated using a "shrinkage" estimator known as the empirical Bayes (EB) residual or the best linear unbiased predictor (BLUP). This estimator "shrinks" school averages toward the system mean, with greater shrinkage for schools with smaller enrollments (Sanders, Saxton and Horn, 1996; Robinson, 1991; Raudenbush and Bryk, 2002). Kane and Staiger (2001) allude to empirical Bayes methods in describing their complex "filtered" estimates of school effects, but do not mention that empirical Bayes methods, with their shrinkage properties, are already part of the simpler accountability system in Tennessee.

Shrinkage ensures that a small school is unlikely to have a large estimated effect. For example, suppose that sampling variation made average scores 75 percent reliable for large schools, but only 50 percent reliable for small schools. If both a small school and a large school reported average scores that were two standard deviations above the mean, the large school's estimate would be shrunk to 2×75 percent $= 1.5$ standard deviations, while the small school's estimate would be shrunk to $2 \div 50$ percent $= 1$ standard deviation above the mean. The small school is shrunk more, because a smaller sample provides weaker evidence of extraordinary achievement.

A drawback of this approach is that shrinkage makes it hard for an exceptional small school to get much attention (Raudenbush and Bryk, 2002). From a policy perspective, however, it may be appropriate to focus attention on large schools, since large schools impact more students. In short, one of the pitfalls that Kane and Staiger have identified is something that certain governments have learned to sidestep. These governments should be commended, and others should be encouraged to follow their example.

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