Commentary

Derek Neal

LEVITT

Steven Levitt makes a set of interesting observations concerning changes in the relationship between a given individual's income and the likelihood that he is a crime victim. The relationship between income and crime victimization is important because it contributes to overall inequality among our citizens. Work on this topic is timely because the measures of wages, earnings, and wealth that serve as our standard yardsticks of individual prosperity indicate that outcomes in our society have become less equal in recent decades. Levitt's main thesis contains two parts. First, he argues that property crime has become more concentrated among poor households. Second, he contends that violent crime has not become more concentrated among the poor and, in fact, he shows that murder—at least in one large city—may have become less concentrated among the poor.

Levitt argues that these preliminary results do not add much to our understanding of how the distribution of individual welfare has changed over time because we cannot observe what individuals spend in order to avoid crime. His argument is correct, and I admire his reluctance to jump to unwarranted conclusions. Here, I comment on his initial findings and pay particular attention to the results concerning the increasing concentration of property crime among the poor.

I begin by noting that in the National Crime Victimization Survey (NCVS) data, there is really no obvious pattern in the relative exposure of rich and poor families to violent crime. Aggravated assault has clearly become relatively more common among black families with incomes of more than $50,000, but no similar results appear for robbery or aggravated assault among whites. Furthermore, the homicide results from Chicago indicate that murder became less concentrated in neighborhoods with low median incomes over the 1965-95 period. But the results do not make clear the degree to which murder actually became relatively less common among poor families in Chicago. A comparison of the 1970 and 1990 results in Levitt's Table 7 shows that the relationship between community characteristics and community homicide rates is remarkably similar in the two periods once all the community characteristics are included in the regression. Thus, the change in the correlation between median family income and homicide rates may simply reflect different patterns of mixing by income across communities.

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My main point is that the evidence on changes in the incidence of violent crime is far from clear cut, while the evidence on the incidence of property crime is clear, consistent, and striking. For both auto theft and burglary, victimization rates among the poor rose relative to the rates among the rich. Furthermore, although burglary rates fell in all groups, the decline among the rich is much greater than the decline among the poor. For both blacks and whites, the ratio of burglary rates among the poor to burglary rates among the rich increased by more than two-thirds. Moreover, the levels of change in these victimization rates are quite impressive. All groups saw the annual probability of burglary victimization for a household fall by at least 2 percentage points, and rich blacks actually enjoyed a decline of more than 8 percentage points. In addition, for both races, the excess decline in burglary victimization rates for rich versus poor white families was more than 3 percentage points.

These seem like big numbers to me, especially given that the expected losses associated with burglary victimization should be larger for rich households than for poor households. If the expected loss for a rich family is even $1,000 per burglary, the 8-percentage-point reduction among rich black families represents an expected savings of $80 per year in the direct costs of burglary alone, and this figure does not even take into account the time costs or the nonpecuniary costs of victimization. As a check on these victimization numbers, I would be interested to know whether or not the patterns of victimization reported in the NCVS provide any insights into trends in the cost of property insurance over the same time period.

Levitt argues that the NCVS numbers may reflect a combination of improvements in crime avoidance technologies and more intensive use of these technologies among rich families. I have no quarrel with this conjecture, but I do believe that a complete analysis of these trends requires data on patterns of residential segregation and crime. We read a fair amount in magazines and newspapers about "gated communities." However, I do not think we really understand the role of residential segregation in determining crime trends or trends in the relative victimization of rich and poor.

Downes and Figlio

Thomas Downes and David Figlio provide an interesting survey of a relatively recent but rapidly growing literature on school finance reform. Because this literature deals with the relationship between government policies and the distribution of investments in human capital, it addresses some of the most important issues in modern research on inequality. The authors also include two sets of results from their own research. I will not provide a detailed commentary on the entire literature that Downes and Figlio review. Rather, I will focus my attention on their results and on a specific debate over methodology that is ongoing in this literature.

Their results in Table 1 indicate that in states where some type of equalization reform occurred between 1970 and 1990, relatively high-spending districts experienced relative reductions in the fraction of residents who have college degrees. The authors interpret this as evidence that well-educated parents leave high-spending districts when reforms restrict the ability of these districts to spend more than other districts.

The results in Table 2 examine the fraction of both public and private school students in central cities who come from families with high incomes or families with household heads who are college graduates. Downes and Figlio report that relative to central cities that are not affected by reforms, central cities that are included in equalization plans experience increases in the fraction of students from high-income or high-education homes. This is true for public school and private school students, but especially for private school students. The authors offer the following interpretation of the results in the tables: "The evidence is wholly consistent with the notion of highly educated families moving to central cities in response to school finance reforms and sending their children to private schools."

This scenario is only one of many that could be constructed to rationalize the numbers in the tables. It is possible that urban public schools gain students from two sources in response to equalization reforms. First, they could acquire some middle- and low-income students who would have attended urban parochial schools in the absence of reform. These students could be small in number relative to the public school population yet still represent a substantial portion of the urban private school population. Second, urban public schools could acquire a larger number of children from high-income families who would have migrated to the suburbs in the absence of reform. Given the relatively small size of the private school sector, these two effects could yield what we observe in Table 2—namely, modest increases in the fraction of urban public school students who come from high-income homes and large increases in the fraction of urban private school students who come from high-income homes.

I do not offer this scenario as the correct explanation for the results in Table 2, but rather as one of many that are equally plausible and yet indistinguishable based on the evidence that the authors present. This type of difference-in-difference analysis of composition measures will never provide clear answers to questions concerning patterns of residential mobility in response to reform changes. Composition changes in central cities alone cannot be used to pin down changes in the number of families that make specific types of choices concerning schooling and location.

However, I am more concerned about another methodological issue. Downes and Figlio, and many others who work on this topic, commonly employ an event-study approach. This approach treats all reforms as an occurrence of a specific and common event. In this framework, states or localities that have not experienced formal reforms serve as a control group, while those that have experienced reforms constitute the treatment group. The goal is to evaluate the average effects of reform (the treatment) on various outcomes. Downes and Figlio acknowledge that there are problems with using the event-study approach to evaluate school finance reforms, and I commend them for raising these important issues. However, we differ in our evaluation of the importance of these problems. I am firmly convinced that the event-study method is not appropriate for research on school finance reforms. A recent paper by Caroline Hoxby (1998) demonstrates that school finance equalization reforms are incredibly heterogeneous events. The changes in tax and expenditure regulations that accompany these reforms vary greatly among states, and the effects of these reforms on the incentives faced by local school districts vary significantly within and among states. No sensible economic model would ever predict that such varied reforms would yield similar impacts, and I am not sure that we learn much about how these reforms work or how an optimal reform should be designed by estimating the average impact of such a varied set of interventions.

The details are important here. Future research should focus on pinning down exactly how various features of school finance reforms affect the incentives and behavior of schools, parents, and students. Hoxby has taken the first step down this road. Others should follow her lead.

References

Hoxby, Caroline M. 1998. “All School Finance Equalizations Are Not Created Equal.” NBER Working Paper no. 6792, November.