Comments on Abramitzky, Chaudhary, and Musacchio

The dissertations of Ran Abramitzky, Latika Chaudhary, and Aldo Musacchio differ in the questions studied as well as the methods used to analyze their chosen topics. Yet what all three have in common is a strong element of originality, and by virtue of either compelling theoretical arguments or empirical evidence, or both, their findings give us a new perspective on important questions, and challenge us to perceive the world in a different way. These dissertations have a heft to them—that comes in part from the authors having to negotiate the terrain of historical sources, but also from their fluency in the literature of labor economics, public finance, and law. In my comments, I reflect on some of the contributions of these essays, potential criticisms, and the questions that are left open for further research.

ABRAMITZKY

Ran's dissertation is an excellent study that interprets in economic terms an institution that seemed based on expressly noneconomic principles. The setup of the first Israeli Kibbutzim in 1910 was akin to a socialist vision of utopia: all members were guaranteed an equal share of the total output of the commune regardless of individual ability or productive input. There was common ownership of the means of production, and there were no individual bank accounts and no private property. The first main question he considers is why these communities persisted with its egalitarian principles for as long as they did. His answer is that the traditional Kibbutz is a risk-sharing organization that insured against fluctuations of income across its members, and that the institutions of the Kibbutz helped to enforce a self-sustaining equilibrium.

I do not doubt that mutual monitoring and peer pressure reduced moral hazard, or that adverse selection was limited through Kibbutz bylaws that prohibited members from taking any assets with them if they should decide to leave the commune. Casual observation, however, suggests that there were elements about the traditional Kibbutz that were contrary to the objectives of full risk sharing. For example, communes aimed to be self-sufficient in labor, not hiring in workers. Members rotated on tasks, instead of specializing on different tasks, which would have allowed greater diversification across the member's production. The communes as a whole focused on agriculture, an occupation with highly volatile returns from period to period. All this raises the question of just how good the insurance provided by the communes was once all aspects of the Kibbutz were taken into account, especially for the ones that were smaller and more isolated geographically.

Two chapters of the dissertation contain empirical analyses. In the first of these chapters, Ran constructs a model of the Kibbutz. In period one, members make a sunk contribution to the Kibbutz, and the Kibbutz offers an income to members that determines the level of equality. In period two, individuals learn about their type, and decide whether to stay in the Kibbutz. The equilibrium level of equality is endogenous in the sense that high equality increases insurance but excludes the high ability types, and low equality decreases insurance but keeps more high ability types around. Two pat-

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terns that emerge from the data seem particularly noteworthy. One is that the degree of egalitarianism within the Kibbutz is positively related to its wealth. The other is that in the late 1980s, in the aftermath of the debt crises of the Kibbutzim, many communes suffered a cut in their wealth; they did not maintain full equality, but even that was not enough to prevent their most productive members from leaving. However, the Kibbutzim that still had high credit ratings after the crises experienced growing membership and did not have very high exit rates. Both patterns conform to the prediction of the model.

The second empirical study uses individual-level data to track the performance of those who exited and entered the system of Kibbutzim across 1983 and 1995. These movements provide a convenient context for examining migration patterns, and the chapter is motivated by a hypothesis due to Borjas that positive selection may occur whenever the origin has a more equal income distribution than the destination, in that the best people leave and outperform the workers at the destination.

Ran finds partial support for the Borjas hypothesis: individuals who leave the Kibbutz are favorably selected compared to those who stay, and entrants are adversely selected. However, once in the city, former Kibbutz members with high-skills in fact underperform individuals with comparable characteristics in education and occupation (to give the number, wage earnings were nearly 17 percent less than average); meanwhile former Kibbutz members with low skills outperform comparable individuals in the city (wage earnings here were 15 percent more than average). That the distribution of earnings, conditional on education, of former Kibbutz members is different from the distribution of earning of members' from the general population is a cause for concern—what it suggests is that there are important "other factors" that determine Kibbutz membership. This part does not fit the Borjas prediction and the explanation for it is open to speculation.

One possible explanation is unobserved heterogeneity, which might appear in any number of ways. Suppose differences in ideological drive or enthusiasm for communal living introduces unobserved heterogeneity at the individual level. If higher education is negatively correlated with ideological drive, then this would be a reason why the more educated and more skilled may be more likely to leave the Kibbutz, and the less educated more likely to stay. Moreover, this may lead some high-skilled people to leave the Kibbutz even though they are not well adapted to the city and have to work in a low skill occupation. The result may be that former Kibbutz members of the high-skill type do not earn as much as the average for their type—leading to the compressed wage pattern for former Kibbutz members found in the data.

As a final comment, suppose that membership in the Kibbutz induces changes in worker behavior. In the Kibbutz, low ability types receive more income than they produce and so are pressured to "work harder," whereas high ability types, who receive less income than they produce, are permitted to slack off more. Is it possible that artificially forcing wages to equality might create changes in worker productivity, conditional on ability, and that these effects persist even after the workers move into the city? The question is indirectly related to the test of the Borjas hypothesis, but whether communal living produces changes in worker behavior and productivity seems to be worth thinking about.

CHAUDHARY

Latika's dissertation takes us to British India, where she attempts to distinguish the adverse impact of collective action problems from that of political inequality in the

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provision of public goods. The context seems particularly suitable for looking at such a question because the presence of the Hindu caste system underscored existing social divisions and allowed higher castes to take control of political bodies while marginalizing the lower castes.

In the first part of her dissertation, Latika creates a new data set from Indian gazetteers and colonial censuses of 1901 and 1911 that includes variables on schools and population descriptions at the district level. A central part of her contribution is the construction of an index that captures the degree of caste and religious fragmentation within districts. One claim that turns out to be very important for her argument is that the higher castes had private substitutes to public schools, so that their domination of local district decision making meant that they would try to reduce local funds in public school provision.

She regresses the number of schools per capita on the index of fragmentation and control variables. In British India there were many different types of public schools, which reflected not only a diversity of demand for schooling, but also differences in sources of funding and management. The key hypothesis is that greater caste and religious diversity may be negatively correlated with public school provision because of collective action problems, but if political inequality is an important influence on public school provision, then the negative impact of fragmentation ought to be stronger for those types of public schools that were most susceptible to the political influence of the higher castes.

I found this identification strategy convincing. At the same time, it is not entirely clear why some types of public schools were only weakly affected by the presence of the higher castes—Latika speculates that it was possible the Brahmans were not averse to attending public schools with lower castes. But that would seem to imply that some high caste groups did value public schools even if they had private alternatives. A table I would have liked to see would be the percentage breakdown of the actual pupil attendance at all public and private schools, by caste. On these questions, it seems also that some other dimension of direct historical evidence—for example, written documentary accounts or anecdotal evidence—could prove useful in buttressing some of the indirect inferences that emerged from the regression analysis.

The second section of her dissertation is an empirical piece that extends the argument and analysis on public goods for education expenditures to expenditures on roads and bridges. The estimation strategy builds on the earlier methodology, but this chapter is much more sophisticated in its consideration of the econometric problems, and in formulating alternative specifications of fragmentation. Her findings indicate that fragmentation within the district population is negatively related to education spending, but positively related to civil works spending. The question is why. Latika argues that when the entire population is fragmented, then the politically powerful are better able to direct local policy according to their preferences, which in this case was to favor roads and bridges over education. However, other than the sign of the regression coefficient, it is not entirely clear how we can be sure the politically powerful preferred road building. On this score, it would strengthen the argument to provide more details on how decisions on civil works expenditures were made.

When Latika's fragmentation index is redefined to capture only the diversity of members within the politically influential groups, the effect of fragmentation is weaker than when fragmentation measures based on the entire population are used. Latika attributes this to the improved ability of the politically powerful to influence local policy in a more heterogeneous population, and she concludes that fragmentation of the entire population is what seems to matter. To some extent this is not really an

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explanation, so much as a restatement of what the empirical correlations say. On this front, it would have been useful to establish clearer ex-ante guidelines on what the connection between the prediction and the empirical evidence ought to be.

Overall, I am somewhat more persuaded by the political exclusion argument than the political influence angle. Fragmentation leads to lower provision of public goods—not because of high coordination costs and free-rider problems, but because fragmentation facilitates the political exclusion of one group by a more powerful one.

MUSACCHIO

Aldo Musacchio's dissertation is as much a critique of the economics literature on financial development, as it is a study of the financial history of Brazil. The dissertation confronts some of the better-known claims about financial market development with a very impressive array of data work, including collections from the National Archives of Brazil. I cannot do justice here in summarizing all the ideas touched on in the nine chapters of this dissertation, but a substantial part of the work focuses on the protection of investors by the legal system. For his analysis, Aldo collects information on bankruptcy cases of joint stock companies and bankruptcy laws between 1850 and 2001. The core argument is that it is the political interaction between interest groups and the constituency of the incumbent ruler that are most relevant in determining the extent to which investments are safeguarded in Brazil, rather than major alternative explanations such as the extent to which laws protect the property and contractual agreements of investors, or the legal origin of those laws, or the persistence of early institutional forms.

Some of the chapters on investor protection are written as if there would be tests of many different hypotheses, but in fact there is no hypothesis testing in the econometric sense of estimating coefficients and determining their significance level. Instead, Aldo gathers data, and checks whether the empirical patterns found for Brazil are consistent with the general findings from cross-country studies. Invariably, he finds that they are not. While he is quick to conclude that these studies do not explain the historical events of Brazil very well, I would have welcomed more discussion on why Brazil deviated from the average country in the sample of a particular study, and the extent of the deviation. A related expositional point is that several chapters of the dissertation are written as if to provide an answer to the question: among the more influential ideas about financial market development, which ones are more applicable to Brazil? Just as useful would be an extended discussion on how the findings from Brazil ought to change some of the more influential ideas about financial market development.

One contribution of this thesis lies in refining what institutional persistence means. The notion of "good institutions" has turned out to be a fairly large black box that includes good financial institutions, protection against arbitrary power, jury trials, electoral representation, nondistortionary economic policies, the preservation of law and order, private property, and so on and so forth. Which one of these factors is more or less important is one of the big questions that remain unanswered in the field of economics. One of the lessons from the history of Brazil is that even when the supposedly critical factor emerges, such as strong rights in creditor protection, these rights may not be as valuable if agents understand that the situation is insecure and apt to change. A simple indexing of rights based on relative strength may not deliver the relevant information about the stability and hence the dynamic protection of property rights. But building the appropriate index would require knowing whether there is a cost to strongly favoring one group over another group—for example, investors over laborers.

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We need to know whether it is always a good thing to limit labor rights. And, we need also a good answer to the question: Is it always better to have stronger property rights, or is there an optimal level?

I would like to close by emphasizing that it was a pleasure to have the opportunity to think about the research questions addressed by these newly minted Ph.D.s. Their insights enrich the field of economic history, and indeed the entire discipline of economics. I look forward to reading their future contributions to the profession.

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