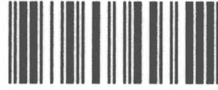
INEQUALITY AND INSTABILITY

A STUDY OF THE WORLD ECONOMY JUST BEFORE THE GREAT CRISIS

JAMES K. GALBRAITH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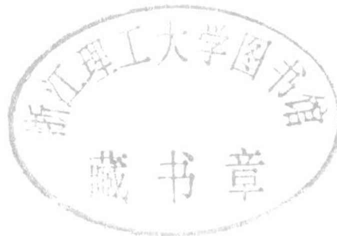

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Kepler undertook to draw a curve through the places of Mars, and his greatest service to science was in impressing on men's minds that this was the thing to be done if they wished to improve astronomy; that they were not to content themselves with inquiring whether one system of epicycles was better than another, but that they were to sit down to the figures and find out what the curve in truth was.

—Charles Sanders Peirce (1877)

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Austin, Texas
September 12, 2011

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Inequality and Instability

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CHAPTER 1

The Physics and Ethics of Inequality

In theory, theory and practice are the same. In practice, they aren't.
—Attributed to Yogi Berra

In the late 1990s, standard measures of income inequality in the United States—and especially of the income shares held by the very top echelon[1]—rose to levels not seen since 1929. It is not strange that this should give rise (and not for the first time) to the suspicion that there might be a link, under capitalism, between radical inequality and financial crisis.

The link, of course, runs through debt. For those with a little money, it is said, the spur of invidious comparison produces a want for more, and what cannot be earned must be borrowed. For those with no money to spare, made numerous by inequality and faced with exigent needs, there is also the ancient remedy of a loan. The urges and the needs, for bad and for good, are abetted by the aggressive desire of those with money to lend to those with less. They produce a pattern of consumption that for a time appears broadly egalitarian; the rich and the poor alike own televisions and drive automobiles, and until recently in America members of both groups even owned their homes. But the terms are rarely favorable; indeed, the whole profit in making loans to the needy lies in getting a return up front. There will come a day, for many of them, when the promise to pay in full cannot be kept.

The stock boom of the 1920s was marked by the advent of the small investor. Then the day came, in late October 1929, when margin calls wiped them out, precipitating a run on the banks, from which followed industrial collapse and the Great Depression. The housing boom of the 2000s was marked by a run of aggressively fraudulent lending against houses, often cash-out refinancings to the

small homeowner.[2] The evil day came again in September 2008, when Fannie Mae, Freddie Mac, Lehman Brothers, and the giant insurance company AIG all failed. Over the months and years that followed, home values collapsed, wiping out the wealth and financial security of the entire American middle class, accumulated for two-thirds of a century.[3] The associated collapse of the mortgage bond and derivatives markets precipitated a worldwide flight to safety, which in Europe developed into the crisis of sovereign debt for Greece, Ireland, Portugal, and Spain.

Thus in a deep sense inequality was the heart of the financial crisis. The crisis was about the terms of credit between the wealthy and everyone else, as mediated by mortgage companies, banks, ratings agencies, investment banks, government-sponsored enterprises, and the derivatives markets. Those terms of credit were what they were, because of the intrinsic instabilities involved in lending to those who cannot pay. Like any Ponzi scheme, or any bubble, it is a matter of timing: those who are in and out early do well and those who are not nimble always go bust. As Joseph P. Kennedy said in the summer of 1929, "Only a fool holds out for the last dollar."

Yet to those economists whose voices dominated academic discourse this was an invisible fact. Their models of "representative agents" with "rational expectations" treated all economic actors as if they were actually alike; even if all incomes were not equal, the assumption that consumption preferences were independent meant that relative position played no role in the theory.[4] Further, in their notions of "general equilibrium" financial institutions such as banks made no appearance. In the classification system of the *Journal of Economic Literature* there was (and is) no category for work relating inequality to the financial system. In other words, both inequality and financial instability were largely blank spots in dominant theory; neither concept was important to mainstream economics, and the relationship between them was not even thought of.

The economists in the tradition espoused, for example, by Professor Benjamin Bernanke at Princeton were devoted to the view that—except for occasional bouts of bad policy, caused by a central bank creating either too much money or too little—the economy always tends toward stability at full employment. Following the stabilizing prescriptions of Milton Friedman, bad policy could be avoided and crises of the sort we endured in the 1930s could not recur. Wise policy, inspired by wise principle, had given us a "Great Moderation"—a new world of stable output growth, high employment, and a low-and-stable inflation rate. This would not be disturbed in any serious way by credit markets. Until just a month before the crisis broke into public consciousness in August 2007, the official prognosis of the Federal

Reserve Board—by then chaired by the same Professor Bernanke—was that all problems in the housing sector were "manageable."

This was the pure product of something economists called the quest for "logically consistent microfoundations for macroeconomics": an economics completely disengaged from the sources of financial and economic instability. Not only was there no recognition of inequality, and not only was there no study of the link of inequality to financial instability; there was practically no study of credit and therefore no study of financial instability at all. In a discipline that many might suppose would concern itself with the problems of managing an advanced financial economy, the leading line of argument was that no such problems could exist. The leading argument was, in fact, that the system would manage itself, and the effort (by government, a human and therefore flawed institution) to "intervene" was practically certain to do more harm than good. In retrospect, it all seems almost unbelievably odd.

At the same time, there was (and is) a substantial group of economists who did (and do) study the problem of economic inequality. But they do so for other reasons, and they are not closely connected to the core of mainstream economic theory. This group is concerned primarily with poverty; with wage structures; with the conditions of family life; with the effects, efficiency, and adequacy of social policies, including education, training, child care and health care, and notably in comparative context between the United States and Europe. They do often-excellent work with large datasets, though usually only in cross-section. Given the limitations of their data, they have little capacity to explore the evolution of inequality over time; indeed, the making of a reliable comparison between countries may require factoring out the influences of the "stage of the business cycle." This group thus had no interest in the issue's macroeconomic dimensions and made practically no contribution to the study of inequality and credit relations. Their study of inequality was divorced, entirely, from the study of economic dynamics, and it therefore posed no challenge to the dominant doctrines.

Yet another group of economists had spent time and effort on the links between inequality and economic development in the wider world, in a way that might potentially have brought them into dialogue with the dominant theory. These economists were pursuing the lead provided back in 1955 by Simon Kuznets, whose work tied inequality to the level of income and stage of development, and they used the facilities of the World Bank and later of the United Nations to obtain greatly expanded data on inequality in countries around the world during the intervening decades. In recent years, this work concentrated on an attempt to discern how inequality influences the prospects

for economic growth, so it did have a dynamic aspect. But the dynamics were, at best, primitive: the question under investigation was generally whether an equal or an unequal society would do a more efficient job of savings, capital investment, and expansion of productive capacity over time. No analysis of finance, credit relationships, or the instability of the growth process entered into this work, and it does not appear that those involved ever seriously considered raising the point. So the dialogue with mainstream theories of growth and equilibrium, which might have happened, never did.

Further, analyses in this vein of development economics were hampered by the poor quality of the underlying measures, an artifact of the sparse and often-primitive surveys used to gather the underlying information on economic inequality over half a century or longer. Faced with noisy data and many missing observations, researchers were obliged to rely heavily on a compensating sophistication of technique, and the studies were often a triumph of complex econometrics over clear information. Perhaps not surprisingly, as well, consistent findings stubbornly refused to appear. Whatever the merits of each individual research project, the results often contradicted one another: some studies concluded that greater equality fosters growth, while others came to the opposite view. Thus a (modestly liberal) vision stressing the importance of broad-based development (and education, especially) contested with a neo-Victorian vision stressing the importance of enhanced savings, even if it should require highly concentrated wealth. No general consensus emerged, beyond agreement that Kuznets's simple insights would no longer suffice. As we shall see later, even this verdict was highly premature.

Thus although there was interest in inequality among economists—and there has been all along—neither major group of active empirical inequality researchers made a link between the micro- or developmental issues that they were pursuing and macroeconomic conditions. And so, like the macroeconomists, they too were unprepared to examine the relationship between economic inequality and the global financial crisis.

Apart from data quality, the study of economic inequality has faced another substantial limitation, not often remarked on because we tend to take it for granted. It concerns the frame of reference from which the available data are drawn. In most cases, this is the nation-state. We almost always measure and record inequality by country. We do this because (for the most part) only countries engage in the practice of sampling the income of their citizens. Thus only countries compile the datasets required for the calculation of inequality measures. Studies of inequality by smaller geographic units, such as American states or Chinese provinces or European regions, are rare. Studies of inequality

across multinational continental economies, such as Europe, are practically nonexistent, not for lack of interest but for apparent lack of information. This would not be a problem if all economies followed national lines, but they do not. In some cases (increasingly rare these days), a smaller unit is appropriate. In many more, economies now function smoothly across national lines, and the people in neighboring lands inhabit the same economic space. Thus as the economically relevant regions change—with the integration of Europe and North America or the breakup of the Soviet Union, for example—inequality studies tend to suffer an increasing mismatch between the questions one would like to answer and the information available to answer them with.

At the same time, a few researchers have taken on what is in some ways the biggest inequality data challenge, which is to measure economic inequality across the entire world. "Imagine there's no country" is the way one of these pioneers put it (Bhalla, 2002); let's try to determine just how unequal all the people of the world are when seen as a single group. The most distinguished efforts here belong to Branko Milanovic, who has carefully assembled the best information from a wide range of sources at the country level. But the limitation of this work lies in the fact that only a few years of comparable data are supported by the mass of underlying information. Most other studies purporting to assess inequality at the global level are actually based on a comparison of average income levels across countries (adjusted for purchasing power parity, PPP). This is useful work for some purposes, but it suffers from uncertainties associated with the comparative measurement of total income, and especially with PPP adjustments.[5] No one would take it as a substitute for the analysis of changing distributions within countries.

This book originated in dissatisfaction with an economics of inequality pushed to the backstage of comparative welfare analysis and development studies, and especially with the limitations of the evidence underlying these various lines of research. Without disparaging any of them—or even wishing to contradict their findings in most respects—it seemed to us more was required. And there was of course a greater dissatisfaction with the larger economics—with an economics that denied the possibility of financial instability, was unprepared for the Great Crisis, and takes no account of inequality at all.

Our premise has been that a new look at these topics requires new sources of evidence. One can talk about inequality as a moral or social or political problem, and one can philosophize about it, as many do, in the abstract. And there are inequalities affecting people by gender, race, and national origin that can be identified in purely qualitative terms. But you can't actually study economic inequality without measuring it.

For reasons explained in detail later, other researchers had already pushed the available data to the limits of their information content—indeed beyond those limits in many cases. Further progress, new insights, and the resolution of controversies would require broader, more consistent, and more reliable numbers. It would take, we thought, a considerable expansion of the measures of inequality by country and by year—or even by month—and also the capacity to calculate measures of inequality both at lower (provincial) and higher (international, continental, and global) levels of aggregation. This could not be done by conventional methods, which could not, by their nature, change the boundaries of their coverage or the inconsistencies of their method, nor escape the historical limitations on the times and places where surveys were actually conducted.

How, then, could we escape those limitations? New numbers were needed. Where might they be found? The answer rested on a simple insight: the major contours of inequality between people could be captured, substantially, by measures of inequality between *groups* to which those persons belong. Grouping is a very general idea. Individuals invariably belong to groups; they live in particular places, work in particular sectors or industries and can be classed by gender, race, age, education, and other personal attributes. And even though there is not much one can do to rectify a dearth of information about individuals, the archives are full of information about groups—publicly available and free for the taking.

Thus, for example, in China it is well known that a fair fraction of the economic inequality in that vast country reflects the difference in average income levels between city and countryside, and between the coastal regions and the interior. A simple ratio of the average incomes in the city to the countryside (say) would be an indicator—however crude—of the trends in inequality over the country as a whole. If this were all you had, it would still be better than nothing.[6] And one might be able to get a crude measure of this kind regularly—perhaps every year—permitting one to develop a portrait of movement over time. Therefore—so we thought—it would be much better to have ongoing (even if crude) measures of this kind than to insist on excellent measures that might be available for only a few years, if at all.

So much is true, but in fact we can do better than just taking crude ratios. To take China as an example: the country is divided into thirty-five provinces,[7] and the government routinely collects data on sixteen major economic sectors in each province, for a total of 560 distinct province/sector categories. Thus it is possible to know the average income and population size, every year, of all of these 560 categories. From this, it is easy to compute the dispersion of income

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