Comments

Eduardo Fernández-Arias: This paper is a welcome addition to the literature on the redistribution and poverty effects of economic crises. In addition to the four traditional channels conspiring against the less wealthy (namely, the reduction in labor demand, high inflation, the adverse relative price change associated with real depreciation, and the curtailment of public spending), the authors identify what they term the financial channel. This novel channel appears to consist of financial transfers (or bailouts) in banking sector crises.[1] The paper provides evidence designed to convince the reader that the financial channel is, like the other channels, regressive. It does so by isolating two aspects of bailouts associated with banking crises: the financial transfer from nonparticipants to participants in the banking system (depositors, debtors, and bank owners); and the internal distribution of the transfer among participants. The authors claims that each of these aspects is regressive, and they substantiate these claims with a straightforward accounting of the income profile of payers and beneficiaries of the transfers. Finally, the paper discusses the policy implications of these findings.

In my view, the main contribution of the paper is to raise awareness of this channel and to illustrate how substantial its distributional effects may be. The findings of this paper confirm what most economists (and pretty much everybody else) believe about the regressive characteristics of financial bailouts. These are not minor achievements in a field dominated by the efficiency effects of financial crises and their remedies. In this sense, this is an important paper that opens a research agenda with the potential of altering best-practice policymaking.

However, this paper is only a start, a first pass. It contains three main areas of weakness that future papers ought to strengthen in order to complete the research agenda opened by this paper: (a) the analytical framework

1. The paper hints at a possibly wider scope of applicability of this channel, beyond the banking sector, but does not attempt to substantiate it.

used to trace key distributional effects; (b) the quality of the quantitative estimations; and (c) the policy implications discussed. In what follows I elaborate on the analytical pitfalls and then quickly refer to the other two areas of weakness.

Analytical Issues

I highlight three key analytical dimensions that the paper largely overlooks: the economic effects of bailouts; the financing of bailouts; and the ex ante effect of anticipated bailouts.

The Economic Effects of Bailouts

The paper only accounts for the direct effects of bailouts, such as who is entitled to receive the financial bailout in the case of a banking crisis. It thus analyzes the income profile of recipients of deposit insurance schemes to see how depositors benefit; it asks whether debtors benefit from subsidized debt repayment programs, examining the kind of firms that see their debts diluted and the income profile of their owners; and it looks for bank financial support, which would prima facie represent a case of a regressive transfer to wealthy bank owners. As the taxation literature on incidence makes clear, however, these direct or first-round effects may be offset by indirect effects stemming from general equilibrium considerations. Economic modeling and analysis is required to uncover what goes beyond the straightforward, legal incidence taxes, and the same would be true in the case of financial bailouts.

The paper's description and empirical work provides an interesting illustration and is certainly a part of the picture, but redistribution and poverty estimations must deal with some key general equilibrium repercussions that the paper ignores. This weakness can be best appreciated by asking what would happen in the absence of a bailout intended to address a potential or actual banking crisis. The implicit answer of the paper is that the bailout recipients would be worse off (and the payers better off). Nevertheless, many other consequences can be expected, and bailouts exist precisely to alleviate these important consequences. For example, a successful bailout may prevent or contain a systemic banking crisis prompted by lack of confidence. Even within a declared banking crisis, a bailout that eases debt repayment or credit conditions would have

a first-order employment effect on the firms kept afloat, let alone further repercussions. These ignored economic repercussions may have progressive distributional effects and certainly have positive effects on poverty. The lack of economic, as opposed to accounting, analysis of the bailouts weakens the findings and vitiates the policy analysis of bailout instruments designed to address these very economic effects.

The Financing of Bailouts

The financing of bailouts represents half the story of the distributional effects of bailouts, but the authors devote little attention to it. The paper simply mentions that tax systems in Latin America are not progressive and that marginal spending is progressive, which would imply that financing by either more revenue or less spending is not an offset to the regressivity of bailout transfer. Apart from constituting far too sketchy an analysis, this approach leaves out two potentially important aspects of the financing half that can be analyzed empirically. The first is the distributional profile of marginal taxation, as opposed to average taxation. As in the case of spending, it is the change at the margin (and over the long term, I might add) that matters. The second aspect that is ignored in the analysis is the distributional implications of borrowing, which is a key source of bailout financing. Over and above the failures of Ricardian equivalence in practice, the existence of forced, tax-like public borrowing may make the analysis in the paper inapplicable to this case.

The Anticipation of Bailouts

The paper analyzes the ex post distributional consequences of bailouts, and so far I have constrained my comments likewise. However, the anticipation of bailouts also has important distributional effects that need to be incorporated in the analytical framework. This is especially important in the context of policy analysis. The evaluation of policies requires assessing not only their effects when applied, but also their prior effects when anticipated.[2] The anticipation of bailouts has significant ex ante effects that partially invalidate the ex post analytical framework used in the paper and cast doubts on the relevance of some of the findings.

2. Policy choices may be constrained by ex post time-consistency constraints. Nevertheless, if there is a policy choice among credible policies, which is the premise of any discussion on policy implications, then anticipation effects need to be assessed.

The anticipation of bailouts to participants in the banking sector affects the terms of the contracts among them, in particular interest rates for deposits and loans. For example, an anticipated bailout to depositors would lead to lower interest rates on deposits, because banks will need to offer less to depositors to entice them to save with them. If banks are competitive, this will lead to lower interest rates on loans and thus to more credit. The anticipation of bailouts to debtors would similarly expand credit, financed by higher interest rates on deposits. The anticipation of selective bailouts to particular classes of depositors or debtors can be expected to bring some offsetting effect on the financial terms of that particular class.[3]

In general, the anticipation of bailouts to the banking system would lead, not surprisingly, to more bank credit. How such increased credit is supported by changes in financial terms for deposits and loans, and therefore how the bailout transfer is effectively shared among bank participants, depends on the economics of the industry (for example, the elasticity of deposits, the profile of investment opportunities, and the degree of bank competition) much more than on the details of who is designated beneficiary of the bailout transfer. The economics of the industry dictates how participants play the banking game and share any anticipated transfer from an outside party. The formal assignment of the bailout to particular participants prompts offsetting changes to the contracts among them in such a way that overall payoffs tend to remain.

The bottom line is that the paper's formal assignment of the bailout to depositors, to debtors, and to banks, and to different segments within these classes is not that relevant for distribution once the ex ante, anticipatory effects are taken into account. What remains relevant is the first part of the analysis—namely, the aggregate distributional effect of a transfer of a given size from nonparticipants to participants in the banking sector.

Empirical Issues

The authors make an important effort to bring together various data sets and empirical analyses pertaining to this issue. The effort is in many ways uneven. For example, they provide much detail on bank depositors, but do

3. The sketchy empirical testing of these effects included in this version of the paper lacks the counterfactual needed for interpretation, as the authors note. It may be safer to stick to the predictions of economic theory.

very little work on the financing of the bailout, as noted above—and the latter, as argued above, is more important than the former for the issue at hand. Furthermore, the empirical work on bank deposits in tables 2 through 8 does not appear to match the question of income distribution addressed in this paper because the effect of income in the regression exercises is conditional on a number of controls (such as education level) that are foreign to it. For example, the regressions could have revealed that deposits decrease with income for each level of education (which would have been interpreted as negative evidence), while at the same time increasing with income overall (because education is associated with income), which is what really matters to support the finding that depositors are relatively rich. (Nevertheless, the work on bank deposits in tables 2 through 8 is interesting for its own sake.)

Policy Issues

As the authors recognize, the policy discussion is speculative and tentative. This paper does not offer a solid basis for assessing policy alternatives concerning financial crisis bailouts because, whatever its merits and drawbacks in examining distributional effects, it leaves aside the efficiency reasons that provide the core justification of actual bailouts. An informed policy discussion of the issues addressed in this paper requires an analysis of the trade-off between efficiency and equity.

Nevertheless, analyzing the equity effects of alternative policy designs in light of the analytical issues raised above suggests three general conclusions that are somewhat at odds with the points emphasized by the authors. First, what matters most for income inequality is the expected size of the bailout rather than the design of who receives the bailout. This conclusion applies to expected inequality as long as it is anticipated, as explained above. If this is true, then the analysis of the benefits of selective bailouts of the kind suggested in the paper is misguided, because the selectivity of recipients will be largely offset by anticipation. Bailout design and selectivity may be relevant for efficiency, but this is not the policy issue that this paper is set to address.

Second, given the size of the bailout, the key policy alternatives concerning equity effects relate to how the bailout is financed, an area the paper does not pursue.

Finally, in the context of this paper, if the bailout is the problem for inequality, then a financial tax would be the solution. A financial tax on bank participants to finance the expected bailout would undo the transfer and cancel its direct distributional impact. The details of the assignment of the financial tax would be unimportant for incidence among participants for the same reasons that bailout design would be unimportant. This financial tax would have the presumably negative consequence of reducing the level of bank credit . . . but only to offset the expansion produced by the anticipation of a bailout (moral hazard).

Ugo Panizza: Within the wide body of research studying the distributional impact of macroeconomic crises, this very interesting paper opens a new avenue of research by focusing on the redistributive impact of financial crises. The main finding of the paper is that financial crises lead to redistribution through a financial channel and that this redistribution process tends to hurt the poor and benefit the rich. Marina Halac and Sergio Schmukler should be commended for making an effort to study an effect of financial crises that, so far, has not been covered by the large literature on the topic. The authors demonstrate a very detailed knowledge of the evolution of banking crises in Latin America, and I found their description of the differential effects across depositors and borrowers extremely interesting.

The paper starts by pointing out that the existing literature suggests four channels linking crises to income distribution. In this setup, the authors need to clarify their definition of crisis, for at least two reasons. First, some of the channels discussed in the introduction do not apply specifically to financial crises, but to the relation between economic crises, which may or may not have a financial origin, and income distribution. Second, it is not clear what the authors mean by financial crisis. The title just mentions crises, the abstract indicates that the paper focuses on financial crises, and most of the discussion concentrates on banking crises. While these are related phenomena, it would be helpful to have a more precise definition.

The Transmission Channel

The paper argues that the transmission channel from financial crisis to inequality works as follows. First, at the time of a crisis, transfers are made both to participants in the financial sector and among participants in the

financial sector. Second, participants in the financial sector tend to have high income, and transfers within the financial sector tend to go from people with relatively low income to people with high income. Finally, the costs of the transfers to the financial sector fall on all income groups.

I have no problems with point one of the transmission mechanism highlighted above. The authors make a convincing case that financial sector participants receive transfers during a financial crisis. They document in great detail the fiscal transfers that are generated by a financial crisis and highlight the ways in which crisis resolution involves transfers to depositors (who are protected with liquidity support or deposit insurance schemes), borrowers (who are able to default on their debt or benefit from debt relief programs), and financial institutions (through recapitalizations and various other forms of bailout).

I also agree with point two of the transmission mechanism, but I have some comments on empirical strategy and the interpretation of the results. When the authors show that those belonging to the top decile of the income distribution are more likely to have bank accounts, they state that their figures underestimate the presence of the rich in the financial sector because they do not have information on the amount of deposits held by each household. This statement may be true in the absolute sense, but it may not hold in relative sense, because the middle class (and maybe even the poor) are likely to hold a larger share of their wealth in the domestic financial system compared with the rich, who may be able to keep a large share of their financial wealth in foreign bank accounts. The authors should be more careful in discussing whether the transfers are from the poor to the rich or from the poor to the middle class.

The finding of a positive correlation between income and participation in the financial sector is not a surprising result and, in this sense, I am fully convinced by their tables. I am not fully satisfied, however, by the functional form of some of the regressions in tables 2 through 8. For instance, I would have entered the square of age on the assumption that the probability of accessing the financial system is maximized for individuals between 35 and 50 years of age. I would also have explored nonlinearities (besides those implied by the probit structure) in the relation between income and the probability of participating in the financial sector. Is the positive income effect only due to the fact that poor people do not have bank accounts (such that the effect dies out for higher levels of income), or is the probability still increasing when we move from, say, the sixth to

the seventh quintile? This could be tested by repeating the regressions of table 2 and entering the income deciles as separate dummies.

Another issue has to do with the exclusion restrictions used in the selection model of tables 8 and 9. The model is identified by the nonlinearity of the first stage, which has no economic interpretation, and by excluding age and sex from the main equation (and also labor market status in table 9). Several authors suggest that when one has weak instruments or when the errors are not normally distributed, the costs of using selection models may largely outweigh the benefits.[1] In particular, Manski points out that very small misspecifications in the selection equation might generate very large biases in the estimates.[2] The authors' exclusion restrictions seem reasonable (although I think that age should belong to the main equation because it is always included in tables 2 through 8), but I would have found a robustness analysis helpful.

The discussion of table 9 should include a description of the total income effect. As shown in the first stage, very poor people are not affected by the deposit freeze because they don't have savings. Furthermore, the possibility of nonlinearities (besides those imposed by the probit specification) in the relation between income and the probability of being affected by the deposit freeze makes it necessary to look at what happens at different levels of income.[3]

The section in which the authors show that the costs of financial crises fall on all income groups is extremely important, and it should be further developed. The financing of the transfer is a key element of the transmission mechanism highlighted above, but the authors only dedicate a short subsection to it (probably because they would need a whole paper to cover the issue). Their argument goes as follows: (1) fiscal costs are partly financed by taxation; (2) VAT is the main source of taxation in Latin America; (3) VAT is not a progressive tax (the authors find that it is

1. Blau and Kahn (1996).

2. Manski (1989)

3. For example, consider three different experiences of the recent Argentine crisis: someone with a low income and savings of U.S.$1,100; someone with an intermediated income and savings of U.S.$5,000; and someone with a very high income and savings of U.S.$1,000,000 held outside the Argentinean banking system. Of these three individuals, the one most severely affected by the *corralito* (which froze deposits above U.S.$1,000) is the one with intermediate income.

roughly proportional in the case of Mexico) and hence the high-income people who receive most of the benefits of crisis resolution do not pay their full cost. I don't have major concerns about the second and third points. With regard to the first point, however, the "partly" qualifier does make the statement trivially true, but financial bailouts are also likely to be partly financed by the issuance of debt. It would be helpful to have evidence on what share of the cost is financed through higher taxes or lower public expenditure and what share is financed through borrowing. This may seem an irrelevant distinction in a world characterized by Ricardian equivalence. After all, bonds issued today will be financed by taxes tomorrow. However, today's borrowing could be financed by tomorrow's default, and the bondholders might be the same high-income people who benefited from the bailout at the time of the crisis, or they might even be foreigners. Even in the absence of default, if the economy is growing fast (that is, if the economy's growth rate is higher than the interest rate paid by government debt), then those who will pay the bonds tomorrow may be richer than today's poor. The intertemporal distributional consequences of the crisis will thus be smaller than what the authors imply.

Bond financing does not necessarily have a smaller distributional effect that tax financing. For all I know, it could be worse. The discussion is simply intended to demonstrate that the specific way in which banking crises are financed is a topic that deserves further research.

Another Way of Looking at the Issue

The authors are reluctant to provide a direct test of whether financial crises increase inequality or poverty, and they argue that it is difficult to directly detect the effect of the financial channel because the cost of transfers is financed over time.[4] While this may be true, it is still the case that if the mechanism highlighted in the paper is at work and the transfers are not fully financed by issuing long-term bonds, then macroeconomic crises that are also characterized by a banking crisis should have, in the medium term, a higher impact on inequality than macroeconomic crises that are not

4. This was not fully their choice, as the editors of *Economía* specifically requested that they not test the direct effect of the financial channel.

accompanied by a banking crisis.[5] Such a direct test would incorporate the effect of other transfer mechanisms that are likely to appear during financial crises but that are not considered by the authors (for instance, the collapse of bond and stock markets).[6]

Table 10 looks at this issue. The first four columns focus on the economic crises listed by Nora Lustig.[7] The dependent variable is either the change in inequality (measured by the Gini index) or the change in poverty (measured by the headcount ratio) between the period before and the period after the crisis. All the regressions control for the initial value of poverty or inequality and the depth of the recessions (measured as the maximum drop in GDP growth over a two-year period). The presence of a banking crisis is measured by a dummy variable (BANK_CRISIS) that takes the value of one if a recession was accompanied by a banking crisis (six of the nineteen recessions in the sample included a banking crisis) or by a continuous variable (COST_OF_BC) that measures the cost of the crisis expressed as percentage of GDP.[8] The regressions show that in three out of four columns (the exception being column one), the banking crisis variable has the expected positive sign (which indicates that a banking crisis increases poverty or inequality, after I control for the depth of the recession), but the coefficients are never statistically significant. The impact is fairly large, however. The point estimates suggest that the average Latin American banking crisis (with a cost of 26 percent of GDP) is associated with an increase in poverty of approximately 3.5 percentage points. The banking crisis with the highest cost—namely, Argentina in 1985, which resulted in costs on the order of 55 percent of GDP—would be associated with an increase in poverty of 7.7 percentage points.

The last two columns focus on a larger sample. The data have a panel structure and cover the 1960–2000 period. The dependent variable measures

5. This statement is valid under the assumption that the redistributive effects of the financial channel are not negatively correlated with the redistributive effects of the other channels highlighted in the previous literature.

6. The analyst may not want to run this direct test if he or she is interested in separating the distributional effects of banking crisis from the distributional effects arising from collapses in bond and stock markets. In that case, however, the study should specify that the research agenda focuses on a specific subset of the financial channel.

7. Lustig (2000).

8. The data are from Caprio and Klingebiel (1996).

TABLE 10. The Direct Effect of a Financial Crisis[a]

	Latin American countries				*All countries*	
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
GINI_1	−1.120	−0.956			−0.177	−0.165
	(0.90)	(0.80)			(1.35)	(1.45)
POVERTY_1			0.044	0.030		
			(0.26)	(0.20)		
REC_DEPTH	−2.453	−2.933	61.861	58.136	−3.957	−10.177
	(1.34)	(1.67)	(1.19)	(1.10)	(0.25)	(0.69)
BANK_CRISIS	−0.088		5.258		0.297	
	(0.55)		(1.19)		(0.14)	
COST_OF_BC		0.002		0.144		0.126
		(0.56)		(1.22)		(1.92)*
Constant	0.937	0.859	−4.373	−3.117	5.636	5.354
	(1.45)	(1.43)	(0.38)	(0.30)	(1.27)	(1.43)
Summary statistic						
R^2	0.18	0.17	0.21	0.20	0.15	0.19
No. observations	19	19	18	18	31	31

Source: Author's calculations, based on data from Lustig (2000) and Deininger and Squire (1996).

* Statistically significant at 10 percent.

** Statistically significant at 5 percent.

*** Statistically significant at 1 percent.

a. The dependent variable in columns 1 and 2 is the change in the Gini index between the period before and the period after the crisis; in columns 3 and 4, the change in the headcount ratio over the same period; and in columns 5 and 6, the change in the Gini index over a five-year period. The sample in the first four columns is based on Lustig (2000); the sample in the last two columns is drawn from Deininger and Squire (1996) and excludes from the regression all five-year periods that did not include an economic crisis (defined as a two-year period in which GDP growth is two standard deviations lower than the forty-year average). All the regressions control for the initial value of poverty or inequality and the depth of the recessions (measured as the maximum drop in GDP growth over a two-year period). The presence of a banking crisis is measured by a dummy variable (BANK_CRISIS) that takes the value of one if a recession was accompanied by a banking crisis or by a continuous variable (COST_OF_BC) that measures the cost of the crisis expressed as percentage of GDP. Robust *t* statistics are in parentheses.

changes in the Gini index over a five-year period, and the explanatory variables are defined as before.[9] To focus on crisis periods, I exclude from the regression all five-year periods that did not include an economic crisis (defined as a two-year period in which GDP growth is two standard deviations lower than the forty-year average). This reduces the sample to thirty-one observations.[10] The coefficients again have the expected positive sign. Furthermore, the variable measuring the cost of crises is statistically significant at the 10 percent confidence level. The point estimates indicate

9. The Gini index data are from Deininger and Squire (1996).
10. Lack of data on income inequality is a problem.

that the average banking crisis in the sample (with a cost of 14 percent of GDP) is associated with an increase in the Gini index of 1.8 points over the sample average of 37 points.

These are extremely simple experiments based on a small sample and plagued by several econometric and measurement problems, and the results should thus be taken with a lot of caution. They suggest, however, that banking crises may play a role in increasing poverty and inequality. Further research on this area could be fruitful.

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