POLICY OPTIONS AND CHALLENGES FOR DEVELOPING ASIA— PERSPECTIVES FROM THE IMF AND ASIA
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COMMENTS ON: BECK, DEMIRGÜÇ-KUNT & LEVINE, "FINANCE, INEQUALITY AND THE POOR"

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Comments on: Beck, Demirgüç-Kunt & Levine, ***"Finance, Inequality and the Poor."***

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This is a very carefully executed study with fascinating results and potentially important policy implications. The paper finds robustly negative relationships between financial development, on the one hand, and income inequality and poverty, on the other, suggesting that there is no trade-off between growth and equity in pursuing financial development. My assignment as a discussant given by the organizer is to comment on the paper from policy makers' points of view. With that in mind, I would like to raise two sets of questions/issues mostly intended to supplement the findings of the paper. One is about the robustness of the findings. Since policy recommendations need to be based on well founded knowledge rather than fragile results, policy makers would(should) be interested in how robust the results are. Second set of issues is: given the paper's findings, so what? The paper finds that financial development is likely to facilitate not only growth but smaller income inequality and poverty reduction. As noted in the paper in its conclusion, however, policy makers would need to know how such financial development can be facilitated. So, the second set of issues I would like to raise is how (pro-poor) financial development can be induced.

1. How robust are the results found in the paper?

Robustness questions are particularly important in determining how seriously a particular result should be taken in the context of cross-country regression studies since many empirical relationships that are found statistically significant in one study are often found not so in another depending on model specifications and particular datasets used. One potential issue in terms of the robustness of the *quantitative* findings in this paper is the relative contribution of mean income growth versus changes in income distribution poverty reduction. This paper finds that a larger proportion (60%) of the poverty reduction associated with financial development is due to growth effects but that a substantial proportion (40%) is also due to redistributional effects. This is consistent with the finding on the decomposition of cross-country poverty reduction rates into 'growth' (70%) and 'redistribution' (30%) components obtained by Kraay (2006) using his data on 'short-run' growth spells (with the average of 3years per spell). However, Kraay (2006) also cautions us by showing that the decomposition results are quite sensitive to the use of data; the same decomposition using the 'long-run' growth spells obtained the relative shares of 97% versus 3%, instead of 70% versus 30%. The latter finding suggests that ('long-run'?) poverty reduction comes almost exclusively through higher mean income growth. Such decompositions thus appear to be relatively fragile, and thus greater caution is likely to be in order for such estimates to be taken seriously.

Another potential issue in robustness may be the use of alternative measures of financial development. The cross-county effects of financial development on income inequality and poverty appears somewhat sensitive to how 'financial development' is measured. Cross-country variations in financial development, as measured by the 'private credit' measure of financial development as used in this paper, is significantly associated with both the level (Clarke et al) and the growth (this paper) in income inequality. On the other hand, Kraay (2006) finds no significant effects of financial development, as measured by M2 over GDP on income inequality or on poverty reduction. Also, as noted by the authors, Dollar and Kraay (2002) find no evidence of significant effects of financial development, as measured by the commercial bank assets over the total bank asset, on the growth of the poorest income quintile. The authors, in contrast, find that financial development has significant positive effects on the income growth of the poor whether financial development is measured either by 'private credit' or by 'commercial/the total bank assets'.

On balance, however, the main quantitative (as well as qualitative) results found in the

paper appear to be quite robust given the authors' dataset. Since financial development appears to have quantitatively important positive impact on both growth and greater income equality, while the decomposition of its effects on poverty reduction into the relative contributions between growth versus redistribution effects should perhaps be taken with greater caution, its relatively large impact on poverty reduction appears to be a reasonably robust finding.

Apart from cross-country regression studies, there are country-level empirical studies that also corroborate this paper's finding that financial development can have a quantitatively large impact on poverty reduction. Binswanger, Khandker and Rosenzweig (1993), for example, find that the expansion of commercial banks in rural areas had a significant positive impact on crop production through its quantitatively large effects on agricultural investments and fertilizer demand. Also a recent study by Rob Townsend on Thailand similarly finds that financial sector development had a large impact on poverty reduction mainly through helping household/small enterprises and through raising wage rates (Townsend 2006, Townsend, forthcoming). The main avenue for poverty reduction through financial development, as documented by Townsend, is by allowing farm households or worker households to shift their occupation to self-employment by starting non-agricultural household enterprises and also by allowing small scale enterprises to expand their investments.

2. Moving toward more concrete policy implications

So, the paper finds that there is a reasonably robust empirical relationships between financial development and greater income inequality/poverty reduction. This suggests that enhancing financial development would be unambiguously pro-poor, but a main question for policy makers would be, how? In the reminder of my comments, I would like to raise a few issues that hopefully make us help moving toward more concrete policy implications.

What do theories say?

We can note that different theoretical models typically focus on particular aspects of 'financial development' and specific channels through which economic growth and poverty reduction are facilitated. For example, it seems to me that the seemingly contrasting predictions regarding the relationship between financial development and income inequality, as noted by the paper, do not necessarily mean that those models are as contradictory as they may appear once a distinction is made in financial development in intensive versus extensive margin. One the one hand, there are models with credit market failures (plus some kind of indivisibility of investments) that predict that growth and income distribution dynamics are dependent on initial distribution of wealth (e.g., Galor and Zeira 1993, Banerjee and Newman 1994, etc.). In these models, typically credit market imperfections are *exogenously* imposed and fixed. They suggest that relaxing such credit market imperfection is likely be beneficial for the poor. Expanding (exogenously) credit access by previously constrained households is likely to help the poor and reduce inequality. Financial development in extensive margin, in other words, is likely to be pro-poor.

On the other hand, the model by Greenwood and Jovanovic (1990) illustrates patterns of *endogenous* financial development given some lumpy 'entry fees' into financial intermediation (but otherwise with no market imperfection) and suggests that incomes of those who are already with access to financial intermediation grow at higher rates. In other words, 'financial development' in intensive margin is likely to be anti-poor. All those models, therefore, seem to be consistent in pointing to the possibility that "pro-poor financial development," if such a thing exists, would mean financial development at extensive margin into increasingly poorer households. The measures of financial development used in the paper do not allow such a distinction, and thus it cannot be verified empirically based on the authors' data, but the point here is that existing models suggest that such a distinction may be potentially important in understanding the 'pro-poor' nature of financial development.

If pro-poor financial development requires expanding the access to financial intermediation by increasingly poorer households, then one of the main issues becomes: what are the main barriers for the poorer households against entering financial intermediation? In other words, what are the main sources of financial market imperfection? Various theoretical models suggest a number of potential sources, such as adverse selection, moral hazard of various kinds, transactions costs, 'entry fees,' connections, etc. Since the relative importance of such potential sources of financial market imperfections could vary from one country context to another, it essentially becomes an empirical question. This leads us to depart from cross-country studies and look into country-level studies.

Some insights from country-level studies

The studies by Townsend (2006, forthcoming) on Thailand address such a question and find that the nature of credit constraints, as well as the implied remedies for them, differs from region to region within Thailand and also across households with different levels of wealth holdings. This suggests that designing policy interventions to relax credit constraints for poor households would require local level information regarding the particular sources of market imperfections in different parts of a country.

Furthermore, country-level studies are also essential in understanding how financial development can be facilitated through various government policy instruments. Obviously there are a number of potential issues, but I would like to put on the table just one issue, potential complementarity between financial development and public investments. For example, the study on India by Binswanger, Khandker and Rosenzweig (1993) looks into the determinants of the expansion of commercial bank branches into rural areas and finds that significant determinants include market infrastructure (0.2) and road (0.8) but not primary schools (numbers in parentheses are estimated elasticities). The study also finds that availability of bank branches tend to have quantitatively larger impacts on crop production growth than do interest rates. This suggests that investing in infrastructure, particularly road, can be a powerful tool in expanding access to financial intermediation by poor rural households. The studies by Townsend (2006, forthcoming) cited earlier also contain insights regarding how financial development can potentially be facilitated, including their finding that physical and human capital tend to be complements to each other rather than substitutes. This suggests that investing in schools, while found to be not a significant determinant of financial development in the India study cited above, has positive impact on financial development in Thai contexts. In addition, Townsend's studies (2006, forthcoming) also have evidence on how various microfinance institutions work (or not work) in different parts of Thailand.

Finally, I would like to close with a somewhat cautionary note regarding the potential distributional consequences of (facilitating) financial development. For example, (once again) Townsend's Thai studies find that financial development had a large impact on poverty reduction in Thailand not only through allowing households and small enterprises access to much needed investment funds but also through raising wage rates. While higher wages are beneficial to relatively poorer households, Townsend finds, it also had negative welfare effects on some existing business owners, who may resist such financial development. As another example, a recent work on intra-household disparity in time allocation in rural India (Fuwa, et al 2006) finds that the gender disparity in schooling tends to be of larger magnitude among credit constrained households than among unconstrained households. This suggests that financial development to expand access to credit by poor households may potentially have negative effects on gender disparity between girls and boys in human capital investments (though such a development is likely to be temporary). Under such circumstances, facilitating financial development may need to be accompanied by complemantary interventions targeting girls' schooling in order for the financial development not to have adverse effects on gender inequality.

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