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Decentralizing Eligibility for a Federal Antipoverty Program: A Case Study for China

Martin Ravallion

In theory, the informational advantage of decentralizing the eligibility criteria for a federal antipoverty program could come at a large cost to the program's performance in reaching the poor nationally. Whether this happens in practice depends on the size of the local-income effect on the eligibility cutoffs. China's Di Bao program provides a case study. Poorer municipalities adopt systematically lower thresholds—roughly negating intercity differences in need for the program and generating considerable horizontal inequity, so that poor families in rich cities fare better. The income effect is not strong enough to undermine the program's overall poverty impact; other factors, including incomplete coverage of those eligible, appear to matter more. JEL codes: H70, I32, I38, O18

The public finance literature generally recommends that redistributive transfers aiming to reduce poverty should be the responsibility of the central government in a federal system.[1] The main argument against decentralizing such programs is that doing so will induce migration responses, which will be costly and undermine the redistributive effort.

Many countries are not following this policy recommendation. It is quite common for central governments, particularly in developing countries, to decentralize key aspects of the implementation and funding of their antipoverty programs. Typically, the center continues to provide broad guidelines and at least partial cofunding, but is relieved of decisions on the specific beneficiaries of that funding. Informational asymmetries have been the main justification for such decentralized redistributive policies. Advocates argue that, for assessing eligibility, local agents are better informed than the center about local conditions. These informational problems are believed to have special salience in

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1. The classic exposition is Oates (1972), also see the more qualified view in Oates (1999).

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developing countries. However, the literature also points out that the same information problems create prospects for capture by local elites, subverting the center's aims.[2]

Another important stylized fact is large geographic disparities in average incomes in developing countries. As this article will argue, these disparities can be associated with perverse geographic inequities in the outcomes of a decentralized antipoverty program. Indeed, the induced interjurisdictional disparities in program spending can far exceed even large disparities in mean incomes. Then, under certain conditions decentralization can severely limit the scope for reducing poverty as judged by consistent national criteria. The gains to the center in devolving power over beneficiary selection may come at a high price in the program's impact on poverty.

The essence of the problem is that local agents, who must typically commit at least some resources to the program, need not share the center's goals. Their budget-constrained choices can then undermine the program's performance against poverty nationally. For certain preferences of local agents, the government of a poor area will deliberately understate its poverty as an adaptation to its budget constraint. Geographic inequity arises as poor areas spend less on their poor people. Horizontal inequity also emerges as equally poor people are treated differently depending on where they live. Developing countries might then be better advised to follow the more standard recommendations from the public finance literature to centralize the key design parameters of their redistributive policies—although for rather different reasons from the traditional efficiency arguments based on migration responses.

Such concerns are not new. In the past they have been seen to yield a compelling equity case for central action, aiming to ensure that ex ante equals are treated equally by the fiscal system (as advocated by Buchanan 1950). The idea is that the center should correct for inequities by differential cost-sharing or intergovernmental transfers.[3] However, the extent to which such corrective policies are feasible in practice remains a moot point, given the same information asymmetries that have motivated the decentralization of antipoverty programs. Indeed, as this article will show, the information needed to eliminate the bias against poor areas ex post is even more demanding than that needed to directly implement the center's preferred program. And the fact that poor areas tend to have poor services in so many developing countries hardly suggests strong geographic redistribution of spending and fiscal burdens.[4] Political influence on the outcomes can also be expected, and it would not be too surprising if

2. On the arguments and evidence for and against decentralization of antipoverty programs in developing countries, see Bardhan and Mookherjee (2000), Alderman (2002), Bardhan (2002), Mansuri and Rao (2004), and Galasso and Ravallion (2005).

3. See McLure's (1995) comments on Prud'homme (1995). Boadway (2003) provides a good overview of this topic.

4. In China, the redistributive impact of the system of intergovernmental transfers is known to be quite weak; see Tsui (2005), Shen, Jin, and Zou (2006), and Shah and Shen (2006).

this favored better-off areas.[5] The case for believing that cost-sharing or transfers can solve the problem is far from obvious.

The article studies these issues in the context of an antipoverty program in which means-tested transfers aim to bring everyone up to an ensured minimum income. In an effort to redress China's sharply rising income inequality and signs of weak social protection for vulnerable groups, the central government introduced the Di Bao program in 1999. The program aims to provide all urban households that are registered in a specified locality with a transfer payment sufficient to bring their incomes up to a predetermined poverty line. Obtaining registration in a new location is generally a difficult process in China (not least for the poor), so in practice program eligibility is confined to well-established local residents. The program started in Shanghai in 1993, and as it was deemed successful became a national (federal) program with formal regulations issued by the State Council in 1999. The program expanded rapidly and by 2003 participation had leveled off at 22 million people a year. The program is administered by the Ministry of Civil Affairs (MOCA).

Like many social spending programs in China, implementation of Di Bao is decentralized.[6] While the national and provincial governments provide guidelines and cofinancing, the selection of beneficiaries is under municipal control. Individual municipalities determine their Di Bao eligibility line and finance the transfers in part from local resources. The center provides some guidance on how these lines are to be set, not only mentioning the need to ensure that basic consumption needs are met, given prevailing prices, but also noting local fiscal constraints (O'Keefe 2004; World Bank 2007). Claimants must apply to the local (county-level) civil affairs office for Di Bao assistance, typically through their local residential committee, which administers the program's day-to-day activities. There is also a community vetting process, whereby the names of proposed participants are displayed on notice boards and community members are encouraged to identify any undeserving applicants.[7]

In 2003–04 about 60 percent of the program's cost was financed by the center. The share varied across provinces, although data are not available on the precise shares. A State Council circular in 2000 says that "central finance will render support to areas with financial difficulties at its discretion," and a 2001 State Council circular clarifies that central funding was available for

5. Khemani (2006) reviews the literature on political influences on intergovernmental transfers for regional equalization.

6. Generic concerns have been voiced in the literature about the implications of China's high fiscal decentralization for the country's poor areas; see, among others, West and Wong (1995), Park and others (1996), Kanbur and Zhang (2005), Shen, Jin, and Zou (2006), and Zhang (2006).

7. This raises concerns about stigma effects. World Bank (2007) reports results of a survey of Di Bao participants in Liaoning Province that found that only 10 percent were ashamed or uncomfortable with disclosure of their household information in the application process. However, there may well be a selection bias in this calculation, if those deterred by public disclosure chose not to participate.

provinces with financial difficulties and high demand for Di Bao.[8] World Bank (2007, p. 11) reports that "the share of central financing relative to in-province financing for 2002 ranged from zero in coastal provinces to 100 percent in Tibet and 88 percent in Ningxia." This suggests an effort to set higher central cost shares in poorer provinces. However, in the context of public spending, generally, intergovernmental transfers are also known to be subject to political negotiation that does not typically favor poorer provinces (Shen, Jin, and Zou 2006). It would be surprising if Di Bao were immune to these political effects, although little is known about their specific form.

That local authorities retained power over the Di Bao thresholds undoubtedly reflects in part the center's lack of information on differences in the cost of basic needs in different cities. Government officials (in interviews with the author) said that the advantage of involving local community groups is their greater knowledge of local conditions, including the cost of living. However, the center also likely believed that there were limits to how much it could credibly control the local authorities, even with good information. The history of the program—notably Di Bao's emergence from a local initiative—appears to have also influenced the extent of decentralization in implementing the scaled-up national version. Central officials said that local municipalities had the right to set their own thresholds, given that Di Bao had started as a local program and the municipalities cofinance the program.[9]

However, in interviews, central MOCA officials also recognized the likelihood that poorer municipalities might choose lower real Di Bao thresholds because of lack of resources. The central officials considered this to be an undesirable feature of the program. They appear to view the program's objective as reducing absolute poverty nationally, rather than relative poverty as judged by each locality. The authorities hoped that more favorable cost-sharing arrangements in poor cities would help avoid this problem. This article will try to see whether that is the case.

The article begins by outlining a stylized program model, which demonstrates just how much decentralized beneficiary selection can reduce the program's overall poverty impact as judged by consistent national criteria. In one example, a central budget sufficient to eliminate poverty leaves 90 percent of the problem untouched when program implementation is decentralized under a fixed cost-sharing rule; this holds even with perfect targeting (according to local eligibility criteria) within all jurisdictions. Furthermore, in this model, the vertical and horizontal inequities come hand-in-hand; the only way to ensure equal treatment of ex ante equals is to eliminate the inequality in provision between rich and poor areas.

8. This information is from correspondence with Philip O'Keefe, then lead social protection specialist for East Asia at the World Bank.

9. This type of central reliance on local governments is a long-standing feature of China's social policies.

The article then studies the Di Bao program using a household survey that is representative at the level of each of China's 35 largest cities, allowing city-level analysis. These data are used to explore intercity differences in spending and other program parameters and to examine the implications for the program's impacts on poverty. The results indicate that poorer municipalities tend to set less generous eligibility criteria, which diminishes, but does not eliminate, the program's efficacy in poor municipalities. Overall, the extent to which decentralized eligibility attenuated the program's impact turns out to be very small. While there are some concerns about measurement error, it appears likely that the program's operation in practice has reduced the cost of the decentralized eligibility criteria to the program's performance in reaching poor areas and poor people nationally. However, there is evidence of horizontal inequity in the form of large intercity differences in the probability of participation at given (observable) household characteristics.

I. Theoretical Model of the Antipoverty Program

The following model is a stylized version of the scheme that will be studied empirically later in the article. It is assumed that the central government's objective for the program is to provide cash transfers sufficient to bring everyone in municipality j $(=1,..,n)$ up to an income level Z_j^* sufficient not to be deemed "poor." The model deliberately ignores political economy considerations facing the center: it is obvious that if the center does not in fact aim to reduce poverty through this program—trading this objective off against a desire to placate middle-income groups, for example—then the outcomes will fall short of the maximum impact on poverty for a given budget. Instead, the aim here is to explore whether decentralization of the eligibility criteria could on its own attenuate the poverty impact, even when reducing poverty is the center's objective.

In keeping with the fact that this is a federal program, poverty is defined in absolute terms, so that two people with the same real income are treated the same way wherever they live. Thus Z_j^* is the cost of a reference level of welfare (utility), which is fixed nationally. By an appropriate choice of a cost of living index for normalizing both incomes and poverty lines, $Z_j^* = Z^*$ for all j.

The resulting public expenditure will be distributed across municipalities such that the higher their poverty gap, the higher their spending allocation. Spending per capita in municipality j with income distribution $F_j\,(y)$ is

$$C_j^* = \int_0^{Z^*} (Z^* - y)\mathrm{d}F_j(y) = (Z^* - \overline{Y}_j^{Z^*})H_j^* \tag{1}$$

where $H_j^* \equiv F_j(Z^*)(>0)$ is the proportion of the population below the poverty line (the headcount index or poverty rate), and $\overline{Y}_j^{Z^*}$ is the mean income of the poor when the poverty line is Z^*. The cost of the program is implicitly a function of all parameters of the distribution function, $F_j(y)$. These include the mean, $\overline{Y}_j$, and the distribution of incomes relative to the mean, which is taken to be fully described by a vector of parameters, L_j, representing the Lorenz curve in municipality j. C_j^* is also a function of Z^* at a given $F_j(y)$. It is convenient to rewrite equation (1) as

$$C_j^* = C(\overline{Y}_j, L_j, Z^*). \tag{2}$$

The problem is that the center does not have the information needed to implement this ideal program. It has access to a national sample survey that includes household incomes or expenditures, but it can observe the nominal distribution of income only in provinces or municipalities for which the sample size is large enough to be considered representative. It is implausible that most national surveys would be representative at the levels of government at which the central government would want to implement such a program to exploit local information for assigning eligibility. And there are differences across municipalities in the cost of living and other sources of heterogeneity in the money needed to achieve a given level of welfare—differences that are unobserved by the center. For example, it is still rare to have spatial cost of living indexes. Additionally, there are likely to be idiosyncratic differences in needs (even without price differences) because of differences in climate and the mix of other public programs, among other reasons.

With decentralized implementation the center gives each municipality the power to select beneficiaries, but requires cofinancing to help control the program. Local agents are instructed to fill poverty gaps but are free to determine the local poverty line. Total spending on the program in municipality j is given by $C(\overline{Y}_j, L_j, Z_j)$, where Z_j is the municipality's chosen Di Bao poverty line. The possibility of relocation in response to the variation in Z_j is closed off. This can be rationalized by either prohibitive costs of moving or residency requirements (only long-standing residents are entitled to the program).

How will local spending vary with mean income? Intuitively, two effects might be expected to be working in opposite directions. A poorer municipality will have fewer resources for fighting poverty—call this the "resources effect." But a municipality with low mean income will tend to have a high poverty rate—call this the "needs effect." The qualifier "tend to" is important, since there can also be a "distributional effect," potentially offsetting the tendency for municipalities with a lower mean income to have a higher poverty rate. To see the various factors that come into play more clearly, differentiate equation

(2) with respect to the mean as follows:

$$\text{(3)} \qquad \frac{dC(\overline{Y}_j, L_j, Z_j)}{d\overline{Y}_j} = \left[\frac{dC(\overline{Y}_j, L_j, Z_j)}{d\overline{Y}_j}\right]_{Z=\text{const}} + H_j \frac{\partial Z_j}{\partial \overline{Y}_j}$$

where $H_j = F_j(Z_j)$. The first term on the right side is the needs effect and the second is the resources effect. The needs effect can be broken down as

$$\text{(4)} \qquad \left[\frac{dC(\overline{Y}_j, Z_j, L_j)}{d\overline{Y}_j}\right]_{Z=\text{const}} = \left(\frac{\partial C}{\partial \overline{Y}_j}\right)_{L=\text{const}} + \frac{\partial C}{\partial L_j}\frac{dL_j}{d\overline{Y}_j}$$

where

$$\text{(5)} \qquad \left(\frac{\partial C}{\partial \overline{Y}_j}\right)_{L=\text{const}} = -\int_0^{H_j} \frac{\partial y_j(p)}{\partial \overline{Y}_j} dp = -\frac{H_j \overline{Y}_j^Z}{\overline{Y}_j} = -\omega_j$$

where $y_j(p)$ is the quantile function (inverse of the distribution function, $p = F_j(y_j)$) and ω_j $(0 < \omega_j < 1)$ is the income share of the poor.[10] The first term on the right side of equation (4) is unambiguously negative, but the second term—the distributional effect given by the product of the two gradient vectors, $\partial C/\partial L_j$ and $dL_j/d\overline{Y}_j$—could have either sign. The expansion path for spending can be said to be distribution neutral if this aggregate distributional effect is zero.

The direction and size of the resources effect depend on the scheme's design and the behavior of local agents. A key design feature is that the center sets the share of the program cost to be financed locally, α_j, where $0 < \alpha_j \leq 1$ for all j. The center chooses α_j to ensure that the central budget is not exceeded. (The differential cost shares can also be chosen to help control local choices, as discussed later.) Income of the municipality net of spending on the program is $\overline{Y}_j - \alpha_j C_j$, where $\overline{Y}_j$ is gross income. The program's local income share is $s_j \equiv \alpha_j C_j/\overline{Y}_j$.

In characterizing the behavior of local government agents, it can be presumed that they do not care solely about reducing poverty. Each municipality is assumed to have preferences over spending on the program and other uses of local income, both valued positively. These preferences can be taken to embody the local political economy, in that different local municipalities are taken to have different preferences, which reflect the local political and economic factors that influence the tradeoffs drawn between spending on the antipoverty program and other uses of public money.

10. The derivation of equation (5) exploits the fact that, on holding the Lorenz curve constant (intuitively, holding inequality constant), it must be the case that all income levels change at the same proportionate rate, implying that the quantile function has an elasticity of unity with respect to the mean: $\partial \ln y(p)/\partial \ln \overline{Y} = 1$. Also note that $C_j = \int_0^{H_j} (Z - y_j(p))\, dp$.

In rationalizing the assumption that local authorities value spending on poverty reduction, they can be thought either to care intrinsically about their impact on poverty or to view it as instrumentally important. The second case rests on the fact that the program attracts cofinancing resources from the center. Reaching a larger share of the local population through the antipoverty program may buttress the position of local authorities, making it more likely that they stay in power.[11] The program's local impact on poverty is measured by the poverty gap, consistent with the program's stated objective.

More formally, let each municipality have a preference ordering over local spending on the program and income net of local program spending as represented by the function:

$$W_j = W_j(\overline{Y}_j - \alpha_j C_j, C_j). \tag{6}$$

The function W is assumed to be strictly increasing in both arguments. The conditions for an optimum with respect to C_j (or, equivalently, Z_j) are that[12]

$$\alpha_j W_{jY}(\overline{Y}_j - \alpha_j C_j, C_j) = W_{jC}(\overline{Y}_j - \alpha_j C_j, C_j) \tag{7a}$$

$$\alpha_j^2 W_{jYY} - 2\alpha_j W_{jYC} + W_{jCC} < 0. \tag{7b}$$

(Subscripts on W denote partial derivatives.) Implicitly differentiating (7a) with respect to $\overline{Y}_j$:

$$\frac{dC_j}{d\overline{Y}_j} = \frac{\alpha_j W_{jYY} - W_{jYC}}{\alpha_j^2 W_{jYY} - 2\alpha_j W_{jYC} + W_{jCC}}. \tag{8}$$

The direction of the municipal income effect in equation (8) is ambiguous under the assumptions made so far. However, four special cases will help interpret the result in equation (8).

Case 1: Suppose that higher municipal income lowers the marginal welfare of program spending ($W_{YC} < 0$) and that the municipality's objective is linear in income ($W_{YY} = 0$) then it is immediately clear from equation (8) that $dC_j/d\overline{Y}_j < 0$; poorer cities will spend more on the program.

Case 2: Suppose instead that $W(\cdot)$ is separable between the two types of spending ($W_{YC} = 0$) and has strictly diminishing returns to income ($W_{YY} < 0$) (separability can be weakened to $W_{YC} > \alpha_j\ W_{YY}$), then $dC_j/d\overline{Y}_j > 0$; poorer

11. A city government in China that was widely seen to neglect its local population would be unlikely to stay in power very long.

12. The problem is formally identical to a model of consumer behavior in which α is interpretable as the relative price of spending on the poverty-reduction program. Without the cofinancing requirement the municipality will choose a corner solution in which all its residents are deemed to be "poor."

cities will spend less on the program, in marked contrast to the centralized program.

Case 3: Now add to Case 2 the assumption of linearity in spending on poverty ($W_{CC} = 0$). Then the income effect on spending is simply the inverse cofinancing share:

$$\frac{dC_j}{d\overline{Y}_j} = \frac{1}{\alpha_j} \geq 1. \tag{9}$$

Not only will the resources effect dominate, but the total income effect will be no less than unity. At a 50 percent cost share (say), program spending will rise \$2 for each \$1 gain in mean municipal income. Furthermore, local spending on the program could be highly income elastic; the income elasticity is simply the inverse of the share of local income devoted to the program (s_j).

Table 1 gives a numerical example. Consider two regions, one poor and one rich. Given the parameter values in table 1, filling the poverty gaps relative to a single national (real) poverty line would require \$135 in the poor region and \$10 per capita in the rich region. It can be seen that 90 percent of the national poverty gap (the population-weighted aggregate of $(Z^* - \overline{Y}_j^Z)H_j$ across the two regions) is in the poor region. Under the Case 3 welfare function, $400\ln(\overline{Y}_j - 0.5C_j) + C_j$ (with a 50 percent cost share), and decentralization, the entire program budget ends up going to the rich region, with none to the poor region. Instead of eliminating absolute poverty, as judged by the national poverty line, the decentralized program will leave 90 percent of the problem untouched.

Case 4: A further insight into just how powerful the resources constraint can be is obtained by combining Case 3 with the assumption of distribution neutrality ($dL_j/d\overline{Y}_j = 0$). Then one obtains the following simple formulas for the

TABLE 1. Numerical Example (Case 3)

	Rich region	Poor region
Population share (percent)	60	40
Mean income ($\overline{Y}$)	\$300	\$200
Center's poverty line (Z^*)	\$200	\$200
Headcount index (H)	0.10	0.90
Mean income of the poor ($\overline{Y}^Z$)	\$100	\$50
Spending under centralized program to fill poverty gaps ($C(Z^*)$)	\$10	\$135
Locally welfare-maximizing spending under decentralization ($C(Z_j)$)[a]	\$200	0
Center's cost	\$100	0

[a]Local agent's welfare function is $400\ln[\overline{Y}_j - 0.5.C_j(Z_j)] + C_j(Z_j)$, implying welfare-maximizing spending levels of $C(Z_j) = 2\overline{Y}_j - 400$. The center's aggregate spending is \$60 per capita in both cases.

Source: Author's calculations.

decomposition in equation (3):

$$\left[\frac{\partial C_j}{\partial \overline{Y}_j}\right]_{Z=\text{const}} = -\omega_j < 0 \text{ (needs effect)} \tag{10a}$$

$$H_j \frac{\partial Z_j}{\partial \overline{Y}_j} = \frac{1}{\alpha_j} + \omega_j > 0 \text{ (resources effect).} \tag{10b}$$

This suggests that differences in needs may play a modest role under Case 4. In a municipality with typical income inequality and a medium-size program, ω_j will be quite small—unlikely to exceed 0.05. With a 50 percent cost share, the resources effect will be 2.05, swamping the needs effect.

A further implication of the existence of a municipal income effect on the poverty line is that the decentralized program will generate horizontal inequity, meaning that people who are identical ex ante are not treated equally under the program ex post.[13] This happens in two ways. First, when the income effect on the poverty line is positive, there will be people living in poor municipalities who are left out of the program but would be covered if they lived in a sufficiently better-off area. This stems from the region of nonoverlapping support (in the income dimension) induced by the income gradient of the poverty line. Second, participants within the region of common support who are at the same pre-intervention income will have different poverty gaps and (hence) receive different transfers depending on where they live.

In principle, such geographic inequities (both vertical and horizontal) can be redressed by a differential cost-sharing arrangement. To see what would be required, note that Z_j satisfying equation (7a) can be written as: $Z_j = Z_j\ (\overline{Y}_j, \alpha_j)$. Consider the conditional cost share, $\alpha_j^* = \alpha_j^*(\overline{Y}_j, Z^*)$, defined implicitly by $Z^* = Z_j\ (\overline{Y}_j, \alpha_j^*)$. If the center sets α_j^*, it will ensure that under decentralization each municipality chooses the national poverty line, Z^*. (In the numerical example in table 1, local cost shares of 0.37 and 0.73 for the poor and rich regions, respectively, will induce them to choose the center's preferred spending levels under decentralization.) Note that when the center has set the cost shares α_j^*, $j = 1,..,n$, there will be no municipal income effect on the poverty lines.

However, the data requirements for such a cost-sharing formula are considerable. The function $Z_j\ (\cdot)$ varies across jurisdictions according to the distribution of income as well as any idiosyncratic factors in preferences. Indeed, with less information than is needed to work out the α_j^*'s, the center could impose its ideal program at the local level. This suggests that the cost-sharing arrangements found in practice may be subject to severe information and

13. By contrast, "vertical inequality" here refers to differences in transfer receipts between individuals at different levels of income (irrespective of where they live).

computational constraints on the extent to which the biases against poor areas can in fact be eliminated.

The rest of this article explores these issues in the context of China's Di Bao program. Section II describes the data. Section III examines the municipal income effect on program spending and implements the decomposition into a needs effect and resources effect as defined above. The key finding is that a strong local resources effect (operating through the setting of local eligibility criteria) is essentially neutralizing the program's ability to reach poor municipalities.

These findings raise two further empirical issues, which are taken up in Sections IV and V. The first concerns the implications for the program's overall goal of reducing urban poverty; Section IV shows that the resources effect attenuated the scheme's overall impact on poverty but that this effect was quantitatively small; incomplete coverage and too low a benefit level were more important reasons for the program's low overall impact on poverty. The second issue concerns the implications for horizontal equity. Consistent with the arguments above, Section V shows that the decentralization of eligibility criteria generated considerable horizontal inequity; the poor living in relatively rich cities received more help from the program than otherwise identical families in poor cities.

II. Data

The empirical analysis is based on two data sources. The first is the available set of (published and unpublished) administrative records for the program. Most important, the administrative records provided the data on the local poverty lines, which could be mapped to the city level for the largest 35 municipalities, which are the setting for this study. However, independent data were not available on Di Bao spending at the municipal level.

The second data source is China's Urban Household Short Survey (UHSS) for 2003–04. The UHSS was conducted by the Urban Household Survey (UHS) Division of the National Bureau of Statistics (NBS) as a first step in constructing the sample for the regular UHS, which has a much longer questionnaire, but much smaller sample size. This article uses the UHSS sample for the 35 largest cities—a total sample of 76,000 households. The big advantage of the UHSS over alternative survey data sets in this context is that its large sample size allows it to be representative of each of the 35 largest cities; the sample sizes vary from 450 (in Shenzhen) to 12,000 (in Beijing). Thus intercity comparisons are reasonably reliable, though (of course) sampling and nonsampling errors are still to be expected. For the 35 cities with adequate sample sizes, the definitions of geographic areas in the UHSS also coincide exactly with those for the Di Bao lines.[14] The entire data set has been cleaned by NBS

14. Outside these 35 cities the local Di Bao lines are not coded or use different codes, and in many cases use different boundaries to the geographic areas used by the UHSS; a further problem is that the bulk of UHSS data outside the 35 cities has not been cleaned.

staff and made available for this research. While the UHSS is a relatively short survey, it permits measurement of a fairly wide range of household characteristics, including income. Chen, Ravallion, and Wang (2006) describe the survey data in greater detail. Table 2 provides summary statistics by city. The UHSS did not exist when Di Bao was being designed. In particular, Di Bao poverty lines had been set prior to the survey.

Five data problems are notable. First, the urban surveys conducted by the NBS are thought to under-represent the urban poor, notably the "floating population"—rural migrants to urban areas who still have rural registration. This problem arises from the fact that the sample frame of the NBS surveys was based on registration rather than on street addresses. This problem has become less serious because street address sampling was introduced into the urban surveys after 2002, but some observers think that a bias remains. The problem is of less concern in the present context, given that rural migrants are not eligible for the program.

Second, the survey measured household income from responses to the single question "What is your household's total income?" (although respondents were also asked how much of their income comes from wages). Responses to this question are unlikely to give as accurate a measure of income as obtained from surveys that base the income aggregates on many detailed questions, such as the NBS's UHS, although this survey is too small for city-level analysis. To some extent, the measurement errors will average out at the city level, but errors are still to be expected. Some implications of this problem will be pointed out along the way, as well as some robustness tests.

Third, there is no municipal cost of living index for China. The Di Bao lines may reflect (at least in part) cost of living differences. The likely biases due to this problem will be discussed, and it will be argued that the main results are robust.

Fourth, given that municipal program data were not available, estimates for program spending were based on survey responses on income received from the program. This excludes administrative costs. But probably more worrying is that self-reported Di Bao receipts are likely to be measured with error. If these are classical (white-noise) errors, they will lower the explanatory power of the regressions reported below without creating biases. However, the possibility of nonclassical errors cannot be ruled out (see below for the implications of this).

Fifth, that this is a single cross-sectional survey limits the possibilities for allowing for behavioral responses at the household level to Di Bao payments (such as through effects on labor supply). Chen, Ravallion, and Wang (2006) provide several tests for behavioral responses, which do not suggest that they are present to any significant degree, although the lack of longitudinal data limits the power of these tests. In measuring poverty impacts of the program, the income gain is assumed to be the Di Bao transfer payment.

Related to these data concerns, there is an issue of whether, in studying city-level income effects on Di Bao spending (gross), mean income of a city or mean

TABLE 2. Summary Statistics by City

	Mean income (yuan per person per year)	Di Bao poverty line (yuan per person per year)	Di Bao participation rate (percent of population)	Di Bao payments per recipient (yuan per person per year)
Beijing	13,357	3,480	2.53	535.20
Tianjin	9,789	2,892	6.26	239.88
Shijiazhuang	8,001	2,460	3.29	162.76
Taiyuan	7,855	2,052	2.49	187.16
Huhehaote	7,441	2,160	1.08	260.72
Shenyang	6,345	2,460	4.74	249.51
Dalian	7,835	3,312	3.67	288.75
Chuangchun	7,380	2,028	4.40	146.80
Harbin	6,812	2,400	5.15	239.03
Shanghai	13,767	3,480	6.41	353.98
Nanjing	11,557	2,880	2.66	320.67
Hangzhou	14,882	3,420	0.65	549.09
Ningbo	15,846	3,120	2.42	596.35
Hefei	8,211	2,520	5.66	179.62
Fuzhou	10,452	2,520	0.93	213.97
Xiamen	14,615	3,480	2.13	245.90
Nanchang	7,227	1,980	4.44	153.10
Jinan	8,597	2,496	4.39	284.37
Qingdao	9,235	2,760	1.59	372.01
Zhengzhou	7,732	2,400	1.33	260.37
Wuhan	8,410	2,640	5.59	244.03
Changsha	10,770	2,400	6.02	212.28
Guangzhou	14,039	3,600	1.31	623.32
Shenzhen	26,036	3,600	1.08	497.90
Nanning	7,573	2,280	3.66	85.26
Haikou	8,039	2,652	1.58	139.33
Chongqing	6,007	2,220	12.13	236.99
Chengdu	9,701	2,136	1.84	182.13
Guiyang	7,521	1,872	6.20	206.62
Kunming	7,231	2,280	26.81	155.91
Xian	7,901	2,160	4.08	240.90
Lanzhou	6,895	2,064	5.08	232.62
Xining	7,505	1,860	3.92	165.51
Yinchuan	7,515	2,040	6.09	179.06
Wulumuqi	8,351	1,872	1.87	215.72
Sample Mean	9,951	2,715	3.91	270.19

Source: Mean income, Di Bao participation rate, and Di Bao payments per recipient are calculated from the UHSS conducted by China's NBS; Di Bao poverty line is from administrative records of the Di Bao program (see Section III).

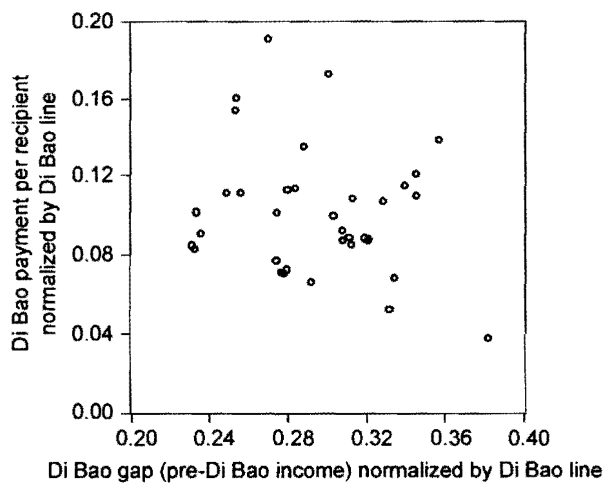
income net of Di Bao payments should be used. Net income is the obvious choice only if measurement errors are ignored. Given that gross income is obtained from a single question on income, it is unclear whether all income sources are properly accounted for in household responses. And the problems in measuring net income are compounded by the likely measurement errors in self-reported transfer receipts from Di Bao. Under these conditions, subtracting mean Di Bao spending at the city level from mean reported gross income may actually add to the bias in estimating the income effect on spending due to measurement errors. The following analysis, which tried both net income and gross income, found that the choice made negligible difference (given the size of Di Bao payments). The city-level results reported in Sections III and IV use gross income.

III. Cross-city Evidence for the Di Bao Program

The survey-based incomes and recorded Di Bao payments do not suggest that the program is working in practice as its design intended. This is evident in figure 1, which compares the estimated Di Bao gaps (distance below the Di Bao poverty line as a proportion of the line) with Di Bao spending across municipalities (also normalized by the Di Bao poverty line). If the program worked as designed and incomes were measured accurately, there should be a perfect positive linear relationship; instead there is a small negative correlation ($r = -0.20$). However, there is undoubtedly considerable noise due to measurement errors both in the estimated Di Bao gaps and in Di Bao spending based on self-reported receipts.

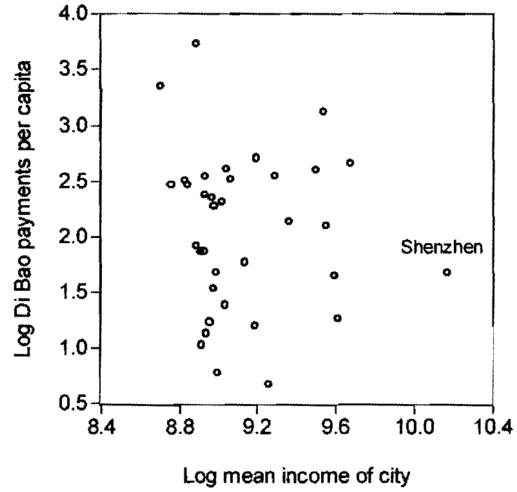
The model in Section I showed that if the program worked in practice as its design intended, then the income effect on program spending would be the net outcome of two opposing effects: the needs effect (whereby poorer municipalities have a greater poverty problem to be addressed) and the resources effect (whereby poorer municipalities have fewer resources for covering their share of the cost). The relative strength of these two effects depends on design of the

Figure 1. Di Bao Gaps against Payments



Source: Author's calculations based on data from China's MOCA and NBS.

FIGURE 2. Di Bao Payments Per Capita against Mean Income, 35 Main Urban Areas of China



Source: Author's calculations based on data from China's MOCA and NBS.

program and on the objectives of local agents. Because the program does not appear to be working as intended, there may be other sources of municipal income effects on program spending, such as differences in administrative capabilities or an income effect on the locally optimal level of redistribution (for example, under certain conditions poorer provinces will be less effective in targeting their poor; Ravallion 1999). A richer set of potential covariates for Di Bao participation using the micro-data will be introduced later (Section V), but for now the analysis focusses on the intercity relationship between program spending and mean income.

Considering the bivariate relationship first, across the 35 cities the regression coefficient of log Di Bao spending per capita on log mean income is -0.220, but it is not significantly different from zero ($t = -0.66$).[15] Figure 2 plots the data. (The correlation coefficient is -0.098.) Dropping the richest city, Shenzhen, the estimated income elasticity falls to -0.150 ($t = -0.31$). There is also a strong positive income effect on Di Bao expenditure per recipient, which has an elasticity of about unity to city income; the regression coefficient of the log Di Bao payment per recipient on log mean income of the city is 0.977 ($t = 5.18$).

In theory, Di Bao spending should also vary according to the program poverty line and differences in the distribution of incomes (Section I). To allow for distributional effects, the standard deviation of incomes within each municipality is used.[16] When a cubic in log Z was initially used, the higher-order

15. All t-ratios in this article are based on White standard errors corrected for heteroscedasticity.

16. With only 35 observations there are limits to how many distributional parameters can be allowed for. The coefficient of variation was also tried, but the standard deviation gave a better fit.

terms were individually and jointly insignificant (probability values around 0.5), so the choice became the following regression of log Di Bao spending per capita (S) on log mean income, the standard deviation (SD), and the log Di Bao poverty line:[17]

$$\ln S_j = \underset{(1.58)}{9.443} - \underset{(-2.63)}{2.386} \ln \overline{Y}_j + \underset{(2.15)}{0.113} \mathrm{SD}_j + \underset{(2.42)}{1.720} \ln Z_j + \hat{\varepsilon}_j \tag{11}$$

$$R^2 = 0.147; n = 35.$$

(The estimates changed very little on dropping Shenzhen.)

Equation (11) suggests the presence of both the needs effect (a lower mean income and more unequal distribution generate higher spending at a given Di Bao poverty line) and the resources effect (through the choice of the line). Recalling the theoretical analysis in Section I, the total income elasticity of spending combines three effects: a direct needs effect, a distributional effect (an effect through the variance of incomes), and a resources effect (through the Di Bao poverty line). Grouping the former two channels together as the needs effect, it is also of interest to estimate the "partial reduced form" regression of spending on mean income and the Di Bao poverty line:

$$\ln S_j = \underset{(-0.13)}{-0.468} - \underset{(1.89)}{0.925} \ln \overline{Y}_j + \underset{(2.06)}{1.401} \ln Z_j + \hat{\varepsilon}_j \tag{12}$$

$$R^2 = 0.075; n = 35.$$

On estimating a similar specification for log Di Bao payments per recipient (S/P, where P is the Di Bao participation rate), SD was insignificant ($t = 0.27$), so it was dropped giving

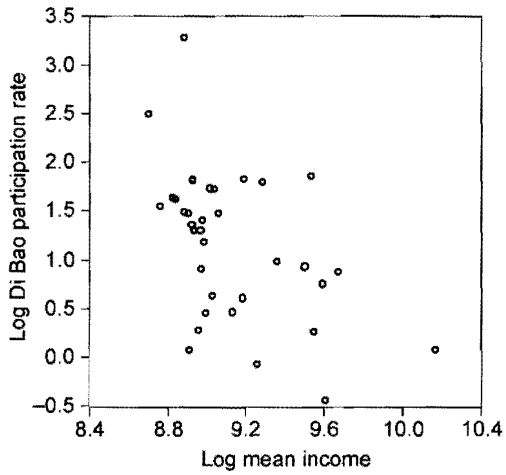
$$\ln(S_j/P_j) = \underset{(-3.59)}{-6.571} + \underset{(2.46)}{0.488} \ln \overline{Y}_j + \underset{(3.43)}{0.971} \ln Z_j + \hat{\varepsilon}_j \tag{13}$$

$$R^2 = 0.568; n = 35.$$

The income effect switches sign from equations (12) to (13). This clearly stems from a negative income effect on Di Bao participation. The estimated elasticity of the participation rate to mean income is -1.197 (with a t-ratio of -3.85). Figure 3 plots the relationship found in the data. The elasticity is even higher

17. The causal interpretation of this regression is questionable given that the Di Bao poverty line is jointly determined with program spending. Nor is there any valid instrumental variable, because anything that influenced the line would also presumably influence spending conditional on the line. However, the aim here is only to test for a conditional income effect at a given line.

FIGURE 3. The Municipal Income Effect on Di Bao Participation



Source: Author's calculations.

(in absolute value) when controlling for the Di Bao poverty line; the income elasticity of participation then rises to -1.413 ($t = -3.31$).

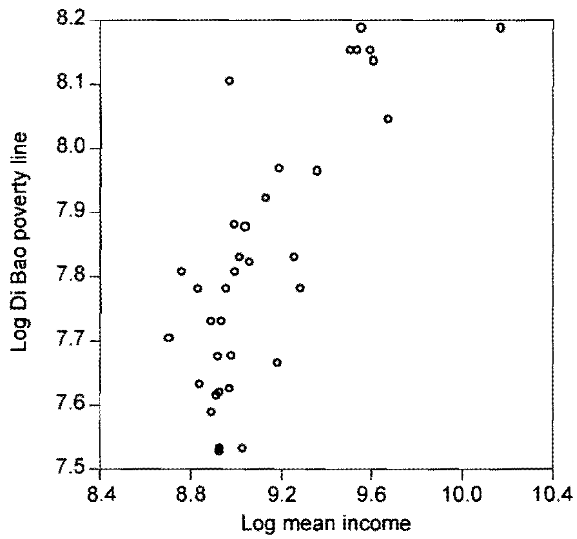
There is also a strong income effect on the Di Bao poverty line. The regression coefficient of the log Di Bao line on log mean income is 0.503, which is not only significantly different from zero at the 1 percent level ($t = 6.92$) but also significantly less than unity ($t = 6.84$). Figure 4 gives the scatter plot. Dropping Shenzhen, the income elasticity is 0.579 ($t = 8.36$).

Thus the small total income effect on spending is the outcome of a negative needs effect at a given Di Bao poverty line (an elasticity of about -0.9) and a positive resources effect operating through the local choice of a line (an elasticity of $0.704 = 1.401*0.503$, using equation (12)). On balance, half the income elasticity of Di Bao payments per recipient in equation (13) is attributable to the positive income elasticity of the lines.[18]

These regressions assume homogeneity in city size. Against this may be fixed administrative costs, yielding scale economies of city size, or congestion effects on the administrative capabilities, yielding diseconomies. While larger cities tend to have higher mean income, the correlation coefficient is small (the regression coefficient of log population size on log mean income is 0.220, with a *t*-ratio of 0.51), so only small biases can be expected in estimating the income effects on spending and the Di Bao lines. Controlling for city size, the income elasticity of spending is -0.335, but is still not significantly different from zero ($t = -1.08$), and the income elasticity of the Di Bao poverty line

18. The half is calculated as 0.971*0.503/0.977 (recalling the regression coefficient of the log Di Bao payment per recipient on log mean income is 0.977).

FIGURE 4. Di Bao Lines against Mean Incomes



Source: Author's calculations.

conditional on city size is 0.493 ($t = 8.82$). In both cases a significantly positive city-size effect was also evident, controlling for mean income.

The above results are based on the Di Bao payments recorded in the UHSS. As was clear from figure 1, there are large gaps between the observed levels of Di Bao receipts in the UHSS and the measured poverty gaps. This undoubtedly reflects both errors of targeting in the program's implementation and measurement errors. It is of interest to compare the regressions for recorded Di Bao spending above with the results expected if the program worked as intended and the measurement errors could be treated as white noise. Using the survey-based Di Bao gaps to estimate equation (1), the analogous results to equations (11) and (12) are[19]

$$\ln \hat{C}_j = \underset{(2.91)}{11.753} - \underset{(-5.06)}{2.974} \ln \overline{Y}_j + \underset{(2.61)}{0.094} SD_j + \underset{(4.73)}{2.374} \ln Z_j + \hat{\varepsilon}_j \tag{14a}$$

$$R^2 = 0.486$$

$$\ln \hat{C}_j = \underset{(1.11)}{3.561} - \underset{(-4.37)}{1.761} \ln \overline{Y}_j + \underset{(3.76)}{2.089} \ln Z_j + \hat{\varepsilon}_j \tag{14b}$$

$$R^2 = 0.364.$$

19. Squared and cubed terms in log Z were tried, but found to be (highly) insignificant.

Here $\hat{C}_j$ is the cost of filling the Di Bao gap based on income net of the program. On balance (factoring in the income effect on the Di Bao poverty line) the total income elasticity is negative and significant (-0.710, $t = -2.95$). The income gradient in the Di Bao gaps is larger than for recorded Di Bao payments.

These regressions imply that if the program had in fact filled the Di Bao gaps as intended, there would have been a negative income gradient, even allowing for positive income effect on the Di Bao eligibility thresholds. The needs effect would have dominated. This suggests that the ways in which the program in practice differed from its intended (theoretical) ideal acted to diminish its efficacy in reaching poor areas by enhancing the relative importance of the resources effect on spending.

However, caution should be exercised in interpreting regressions (11)–(14). The errors in measuring Di Bao spending based on self-reported Di Bao receipts in the UHSS will attenuate the income gradient if poor respondents tend to understate their true Di Bao receipts. And income measurement errors still influence the results (in all these regressions). The net bias is unclear. If mean income is over- (under-) estimated, the poverty gap is likely to be over- (under-) estimated, suggesting that equations (14a) and (14b) overestimate the true income gradient. However, measurement errors in the survey-based data on municipal incomes are likely to create an attenuation bias in the income elasticities of both the Di Bao gaps and poverty line.

One check for bias due to income measurement errors is to assume that these errors do not alter the income ranking of cities and that the income rank has no independent effect on spending (and so can be excluded from the regression for spending). Under these assumptions, the rank can be used as the instrumental variable for measured income. On doing so, the income elasticity of spending rises (becomes more negative). For example, the instrumental variables estimator for equation (12) is[20]

$$\ln S_j = \underset{(-0.16)}{-0.610} - \underset{(2.08)}{1.564} \ln \overline{Y}_j + \underset{(2.36)}{2.1641} \ln Z_j + \hat{\varepsilon}_j \tag{15}$$

$$R^2 = 0.043; n = 35.$$

The instrumental variables estimator for the income elasticity of the Di Bao poverty line rises slightly to 0.530 ($t = 7.27$). On balance, the total income elasticity of spending rises to -0.416 (from -0.220), but is still not significantly different from zero ($t = -0.97$). Other results were similarly robust to using income rank as the instrumental variable. Bias will remain to the extent that income measurement errors affect the rank order of cities by income.

20. The first stage regression (of $\ln \overline{Y}_j$ on the income rank) had an R^2 of 0.81.

There is another source of bias in the regressions reported in this section, due to omitted intercity differences in the cost of living. Consider the reduced form income elasticity of Di Bao spending; let the true income elasticity be δ_1 in

$$\ln(S_j/\mathrm{COL}_j) = \delta_0 + \delta_1 \ln(\overline{Y}_j/\mathrm{COL}_j) + \nu_j \tag{16}$$

where COL_j is the latent cost of living index for city j. Instead, $\ln S_j = \delta_0 + \delta_1 \ln \overline{Y}_j + \mu_j$, where $\mu_j = (1 - \delta_1)\ln\mathrm{COL}_j + \nu_j$. The ordinary least squares estimate of δ_1 is $\hat{\delta}_1 = \hat{\gamma} + (1 - \hat{\gamma})\delta_1$, where $\hat{\gamma}$ is the regression coefficient of $\ln \mathrm{COL}_j$ on $\ln \overline{Y}_j$. The bias goes to zero only as δ_1 goes to unity or as the income elasticity of the cost of living goes to zero.

A clue to the extent of this bias can be found in the provincial cost of living indexes estimated by Brandt and Holz (2006). These are not ideal; the most recent estimate is for 2000, and they are for all urban areas of a province rather than the 35 cities studied here. The ordinary least squares elasticity of the Brandt and Holz urban cost of living index across provinces to mean (nominal) income across the 35 cities studied here is 0.213 ($t = 6.44$). Deflating both Di Bao spending and mean incomes by the Brandt and Holz index shows an income elasticity of -0.486; this is higher (in absolute value) than the unadjusted estimate, although it is still not significantly different from zero ($t = -1.12$). Re-estimating equations (11) and (12) using the Brandt and Holz deflators yields:

$$\begin{aligned}\ln(S_j/\mathrm{COL}_j) = &\underset{(1.22)}{9.428} - \underset{(-2.37)}{2.357}\ln(\bar{Y}_j/\mathrm{COL}_j) + \underset{(1.79)}{0.117}(\mathrm{SD}_j/\mathrm{COL}_j)\\ &+ \underset{(2.69)}{1.682}\ln(Z_j/\mathrm{COL}_j) + \hat{\varepsilon}_j\end{aligned} \tag{17a}$$

$$R^2 = 0.152.$$

$$\ln(S_j/\mathrm{COL}_j) = \underset{(0.05)}{0.260} - \underset{(-2.06)}{1.035}\ln(\bar{Y}_j/\mathrm{COL}_j) + \underset{(2.26)}{1.433}\ln(Z_j/\mathrm{COL}_j) + \hat{\varepsilon}_j \tag{17b}$$

$$R^2 = 0.095.$$

The results in equations (11) and (12) are found to be reasonably robust, though the distributional effect is no longer significant at the 5 percent level.

Ignoring the cost of living differences probably leads to an overestimation of the true real income gradient of the Di Bao poverty lines, given that the cost of living is positively correlated with mean income. Using the Brandt and Holz index for the city's province as the deflator for each city gives an elasticity of real Di Bao line to mean real income of 0.384 ($t = 4.40$). The difference is not large; the income elasticity of the Di Bao line falls from about 0.50 to 0.37. Even if the true income gradient of the cost of living was 50 percent higher than implied by the Brandt and Holz deflators (elasticity of the Brandt and Holz index to mean income 0.32 rather than 0.21), the income elasticity of the Di Bao line would still be 0.27. The true income gradient of the cost of living would have to be more than double that implied by the Brandt and Holz deflators to yield zero real income gradient of the Di Bao lines.

Allowing for cost of living differences across cities will probably also yield a higher (real) income gradient in Di Bao participation. That will be the case if the cost of living has a (positive) income elasticity less than unity (so that cities are not re-ranked in terms of incomes when adjusted for cost of living differences).[21] Again, the provincial cost of living indexes estimated by Brandt and Holz provide a clue to the extent of the bias. The indexes give an elasticity of Di Bao participation to mean income rises of -1.410 ($t = 3.65$) (instead of -1.197 using the nominal data). The Brandt and Holz deflators suggest an income elasticity of Di Bao payments per recipient of 0.925 ($t = 4.18$), slightly lower than the unadjusted estimate of 0.977.

In summary, the above results suggest that both the needs and resources effects are present, but are roughly offsetting. At a given poverty line richer cities have lower participation rates and spend less on the program (though more per recipient). Although it does not dominate the needs effect, the countervailing resources effect is evident, in that a higher municipal mean income tends to come with a more generous Di Bao line. The resources effect is strong enough to roughly cancel out the needs effect—largely neutralizing the program's ability to reach poor municipalities.

IV. Impacts on Poverty

Despite the program's aim of eliminating urban poverty, the overall impact appears to be modest. In the same sample survey used here, Chen, Ravallion, and Wang (2006) find that the poverty-gap index, based on income net of Di Bao receipts, is 2.28 percent; adding Di Bao payments causes it to fall to only 2.06 percent.[22] (Among participants only the corresponding values are 19.92 and 14.23 percent; the higher index for participants reflects the program's targeting to the poor.) The mean poverty gap as a proportion of the Di Bao poverty lines (as given by the poverty-gap index divided by the headcount index) fell from 0.296 to 0.284.

21. To see why, suppose that the true income elasticity of the Di Bao participation rate is γ_1 in $\ln P_j = \gamma_0 + \gamma_1 \ln(\bar{Y}_j/COL_j) + v_j$, while the estimated regression is $\ln\bar{P}_j = \gamma_0 + \gamma_1 \ln\bar{Y}_j + \mu_j$, where $\mu_j = -\gamma_1 \ln COL_j + v_j$. The ordinary least squares estimate of γ_1 converges in large samples to $\gamma_1 (1 - \delta)$, where δ is the elasticity of COL_j to $\bar{Y}_j$. Thus γ_1 is underestimated given that $1 > \delta > 0$.

22. The poverty-gap index is the mean distance below the poverty line as a proportion of the line (where the mean is taken over the whole population, counting the nonpoor as having zero poverty gaps.) The national value of the index is thus the population-weighted mean of C_j/Z_j.

The scheme is underfunded relative to its aim; the population-weighted mean of the Di Bao payment (per recipient) as a proportion of the Di Bao poverty line is 0.108—slightly more than one-third of the aggregate Di Bao gap. Furthermore, the impact on poverty fell well short of the potential, given the budget outlay. These calculations imply that if all the payments made under the program had gone to the Di Bao poor, the aggregate poverty gap would have fallen by 36 percent (= 0.108/0.296), instead of the actual decline of only 4 percent (= 1−0.284/0.296). The scheme has clearly fallen well short of its potential.

What role has the program's decentralized eligibility played in this weak overall performance against poverty? In particular, how much greater would the program's impact on poverty have been if all the cities had used the same poverty line, set at a level that would have entailed the same aggregate level of public spending? Section III studied the relationship between program spending and the Di Bao line; for notational brevity the empirical relationship can be summarized by a function $S_j(Z_j)$ that gives the level of program spending in city j when Z_j is the local Di Bao poverty line. This assumes that the function $S_j(\cdot)$ remains the same for each j when a single national poverty line is imposed. In other words the municipalities behave the same way; all that changes is the poverty line.

What common poverty line would they confront? Define the budget-neutral national poverty line, Z^*, such that $ES_j(Z^*) = ES_j(Z_j)$. Thus, given the behavior of municipalities, the aggregate spending at Z^* is the same as under the decentralized eligibility thresholds. ($Z^* < \bar{Z}$, the mean poverty line, for $S_j(\cdot)$ strictly concave.) Suppose now that the level of spending S_j yields a poverty impact of $I_j(S_j)$. Define $\Delta_j \equiv I_j(S_j(Z^*) - I_j(S_j(Z_j))$, which is the impact gain (or loss) in j induced by the common poverty line. The contribution of variability in Z_j to the aggregate impact $E[I_j\ (S_j)]$ can then be measured by $E(\Delta_j)$. While $E[I_j(S_j(\bar{Z}) - I_j\ (S_j(Z_j))] > 0$ if I is strictly concave in Z, then $Z^* < \bar{Z}$ implies that $E(\Delta_j)$ could be positive or negative.

To implement this measure, an estimate of the poverty impact of program spending is needed. If the program worked exactly as intended, program spending itself gives the reduction in the aggregate poverty gap due to the program. However, although targeting is excellent, there is still sizeable leakage of benefits to the nonpoor. To allow for this, it is postulated that the program's actual impact on the poverty-gap index depends on program spending as

$$\ln(\mathrm{PG}_{0i}/\mathrm{PG}_{1i}) = \delta_0 + \delta_{1i} \ln S_i + \mu_i. \tag{18}$$

Here PG_{1i} is the post–Di Bao value of the poverty-gap index and PG_{0i}, the pre–Di Bao value. When an augmented version of this specification with controls for the (log) pre–Di Bao poverty measure was tested, it was found to be insignificant. When effects of differences across municipalities in the program's targeting performance were tested using (alternately) the share of Di Bao benefits going to the poor, the normalized share, and the overall concentration index, none was significant (including when interacted with Di Bao spending); this is consistent with the finding of Ravallion (2007) that the program's poverty impacts are uncorrelated with targeting performance across municipalities. When a specification including Di Bao spending per participant and (log) participation rate was tested as separate regressors, the null hypothesis that the coefficients are equal could not be rejected. A squared term in $\ln S$ and interaction effects with the pre–Di Bao poverty measure and with the measures of targeting performance also turned out to be insignificant. The only significant effect was an interaction effect between spending and the pre–Di Bao poverty rate, giving the estimated specification:

$$\ln(PG_{0i}/PG_{1i}) = \underset{(-4.38)}{-0.067} + (\underset{(10.93)}{0.110} - \underset{(-4.96)}{0.028}\ln PG_{0i})\ln S_i + \hat{\mu}_i \tag{19}$$

$$R^2 = 0.803.$$

The elasticity of poverty impact with respect to program spending varies from 0.101 to 0.221 with a mean of 0.154 and tends to be lower in poorer municipalities.

How much greater the poverty impact would have been without the variation in Di Bao poverty lines arising from decentralized eligibility can now be quantified. Using equations (11) and (19), the difference between the program's poverty impact ($\ln(PG_{0i}/PG_{1i})$) at the mean Di Bao line and its value at the actual line of each municipality is $\hat{\Delta}_i = 1.72(0.11 - 0.028 \ln PG_{0i})\ln(Z^*/Z_i)$. The value of Z^* is 2,666 (compared with a mean Z of 2,715 from table 2).[23] The value of $\hat{\Delta}_i$ varies from -0.06 to 0.06, with a mean of 0.007; by contrast the mean of $\ln(PG_{0i}/PG_{1i})$ at the actual poverty lines is 0.115.

The upshot of these calculations is that, while the post–Di Bao poverty-gap index would be lower without the variation in Di Bao lines (holding total program spending constant), the extra poverty impact is likely to be very small. The more important reason for the program's low overall impact on poverty is its incomplete coverage of those below the local Di Bao lines and that the Di Bao payments are too low to assure that the Di Bao line is reached. (Recall that the poverty measures reported at the beginning of this section imply that only 12 percent of the aggregate Di Bao gap is being filled by the program.) One can only speculate on the reasons for this weak coverage of the poor. The heavy reliance on self-selection by beneficiaries may have dulled the program's ability to cover all those eligible. Central political economy factors may also

23. The formula is $Z^* = [\bar{S}/M(S_i/Z_i^{1.72})]^{1/1.72}$, where $\bar{S}$ is (population-weighted) mean spending and $M(\cdot)$ denotes the (population-weighted) mean of the term in parentheses.

have played a role, whereby weak coverage of the poor (despite the programs' stated goal) stems from a desire to help other (nonpoor) groups instead.

V. Horizontal Inequity across Cities

Recall that horizontal inequality is an implication of a positive income effect on the Di Bao poverty lines across cities (Section I). The test for horizontal inequity is to see whether the probability of receiving help from the program varies between households that are equally poor but are located in different cities. The test is naturally constrained to the data; the possibility of some unobserved attribute relevant to welfare that is geographically correlated cannot be ruled out. It is important, however, that the test control for observed variables may be correlated with welfare. Excluding these variables from the test would raise the concern that what is being picked up as "horizontal inequity" is really just some geographically associated household characteristic that is correlated with welfare and not fully reflected in measured income.

To assess the extent of this problem, define a dummy variable, $D_i = 1$ if household i receives Di Bao and $D_i = 0$ if not, and let X_i be a vector of relevant "nonincome" factors, including location. The probability of participating in Di Bao is

$$\Pr(D_i = 1) = N[\phi(\bar{Y}_i) + \beta X_i] \tag{20}$$

where N is the standard normal distribution function (so that equation (20) is estimated as a probit) and $\phi(\cdot)$ is a parametric nonlinear function; on experimenting with different functional forms, a quadratic function of $\ln \bar{Y}_i$ provided the best fit.

The X's in equation (20) should include geographic effects, because location can influence living standards independently of other household characteristics, including income. A complete set of municipality effects is allowed by including 34 dummy variables for the 35 cities (Beijing is taken to be the reference).[24] The vector X also includes variables related to the dwelling and the observable characteristics of the household, as might be deemed relevant to local assessments of "need." Discussions with MOCA officials indicated that household assets play an important role independently of income.

The probit estimates of the municipality effects are given in table 3. Results are given with and without controls for other nonincome household characteristics.[25] However, the following discussion uses the results with those controls.

There is a positive correlation between the municipal effects in table 3 (the regression coefficients on the municipal dummy variables) and the Di Bao lines (figure 5). The regression coefficient of the municipal effect on the log Di Bao

24. The Di Bao line is constant within municipalities, so a regression coefficient for the Di Bao line cannot be identified separately from the geographic effects.

25. The coefficients on the extra control variables are omitted to save space. Complete results for the control variables can be found in Chen, Ravallion, and Wang (2006).

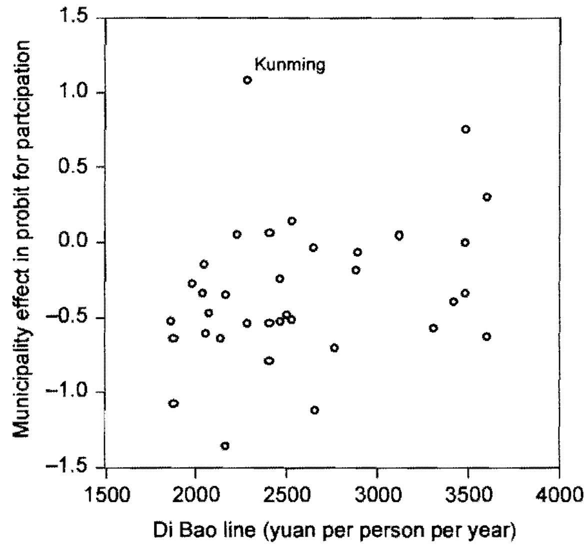
TABLE 3. Probits for Di Bao Participation

	Coefficient	*t*-ratio	Coefficient	*t*-ratio
Log income per capita (net of Di Bao)	0.9661	4.02	0.2725	1.09
Squared log net income per capita	−0.1404	−8.88	−0.0668	−4.08
Controls for household characteristics	No		Yes	
Beijing	Reference		Reference	
Tianjin	0.0994	1.75	−0.0681	−0.97
Shijiazhuang	−0.5859	−7.82	−0.2388	−2.78
Taiyuan	−0.8160	−8.77	−0.5976	−5.42
Huhehaote	−1.4190	−12.16	−1.3597	−11.08
Shenyang	−0.5663	−9.71	−0.5294	−7.89
Dalian	−0.4863	−7.33	−0.5717	−7.65
Chuangchun	−0.5863	−7.22	−0.3419	−3.74
Harbin	−0.6085	−10.16	−0.5388	−7.81
Shanghai	0.6629	11.78	0.7573	8.8
Nanjing	−0.3226	−5.07	−0.1851	−2.18
Hangzhou	−0.6560	−5.10	−0.3990	−2.72
Ningbo	−0.1917	−1.71	0.0490	0.36
Hefei	−0.2452	−2.77	0.1415	1.34
Fuzhou	−0.7722	−6.15	−0.5173	−3.8
Xiamen	−0.1928	−1.58	−0.3417	−2.32
Nanchang	−0.6285	−7.43	−0.2752	−2.77
Jinan	−0.5034	−7.39	−0.4849	−6.06
Qingdao	−0.8319	−8.02	−0.7061	−5.58
Zhengzhou	−1.0734	−10.47	−0.7907	−6.65
Wuhan	−0.2594	−4.48	−0.0319	−0.42
Changsha	−0.0598	−1.05	0.0645	0.84
Guangzhou	−0.4675	−4.13	−0.6260	−5.01
Shenzhen	−0.0389	−0.13	0.3040	0.97
Nanning	−0.9349	−8.41	−0.5367	−4.17
Haikou	−1.3432	−10.31	−1.1193	−7.41
Chongqing	−0.2114	−3.70	0.0532	0.7
Chengdu	−0.8323	−6.19	−0.6369	−3.95
Guiyang	−0.6388	−7.80	−0.6384	−6.52
Kunming	0.8216	10.20	1.0858	10.24
Xian	−0.3825	−3.49	−0.3491	−2.64
Lanzhou	−0.5356	−7.05	−0.4723	−5.36
Xining	−0.6486	−7.86	−0.5285	−4.84
Yinchuan	−0.3584	−4.24	−0.1434	−1.55
Wulumuqi	−1.1191	−10.51	−1.0720	−8.75
Constant	0.4066	0.45	0.3995	0.22
Number of observations	76,762		76,443	
Pseudo R^2	0.3704		0.4718	

Source: Chen, Ravallion, and Wang 2006.

line is 0.903 ($t = 2.93$). From figure 5 Kunming is an outlier; possibly the survey has oversampled Di Bao participants in Kunming. Dropping Kunming, the regression coefficient rises to 1.001 with a *t*-ratio of 3.40. However, it is also evident that there are location factors being captured by the city effects besides differences in the Di Bao lines; the last regression has an $R^2 = 0.249$.

FIGURE 5. Municipal Income Effect on Participation in the Di Bao Program from Table 3 against the Di Bao Poverty Line



Source: Author's calculations.

The municipal effects could well be picking up omitted, geographically associated, household characteristics.

While the (unconditional) participation rate falls as city income rises (Section III), the opposite is true for participation conditional on income and other characteristics. The regression coefficient of the municipal effects on log mean income is 0.502 and is significant at the 2 percent level ($t = 2.52$); when Kunming is dropped the regression coefficient rises to 0.605 ($t = 3.30$).[26]

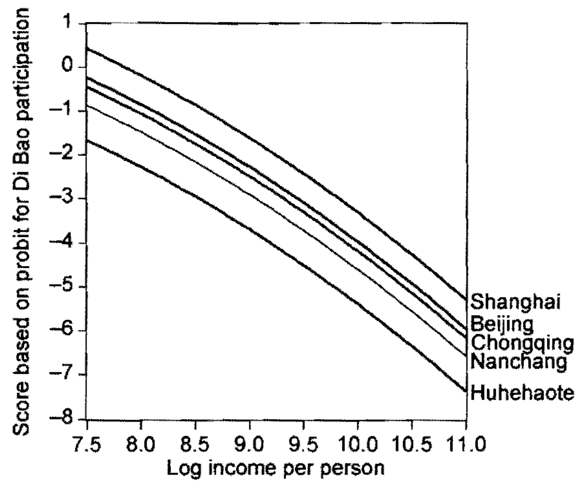
These effects remain reasonably robust when controlling for other "nonincome" factors (the second specification in table 3).[27] With the full set of controls, the regression coefficient of the municipal effects on the log Di Bao line is 0.709 with a t-ratio of 1.99, which is not quite significant at the 5 percent level. However, dropping Kunming, the regression coefficient rises to 0.814 with a t-ratio of 2.39. Again, the city effects are quantitatively large.

So one finds that, at given observed household characteristics, the higher the mean income of the city of residence, the better the chance of accessing the program. The differences in the size of the municipal effects on participation in table 3 are quantitatively significant. This can be seen when asking what

26. As noted, data are not available on the intercity differences in the cost of living. However, by similar reasoning to that in Section II, it can be argued that this data problem will lead to underestimating the real income gradient in the conditional city effects on Di Bao participation.

27. The control variables included the following household demographics: age of head; education attainments; size, age, quality, and ownership status of dwelling; selected consumer durables; health status of head; financial assets; occupation; and sector dummy variables. Details are available from the author on request.

FIGURE 6. Selected City Effects on Di Bao Participation as a Function of Income



Source: Tables 2 and 3.

income difference would compensate for the difference in the city coefficients holding the probability of participation constant. The existence of the quadratic term complicates the calculation, but simply graphing the predicted scores from table 3 is sufficient to demonstrate the point. Figure 6 gives the predicted scores for selected cities. Consider, for example, one of the richest cities, Shanghai, and one of the poorest cities, Nanchang (see table 2). Over the interval in which the scores overlap, the compensating difference in log income is about unity. In other words, a household in Shanghai with more than double the income of an observationally identical household in Nanchang would achieve the same probability of participation.

This effect largely operates through the fact that richer cities set higher Di Bao lines. There are no statistically convincing signs that the income effect operates independently of the Di Bao line; on including the Di Bao line as a control variable, the regression coefficient of the city effect on log mean income drops to about half its value and is not significantly different from zero.

So there are convincing signs in the data of horizontal inequity in the program. Holding other observed characteristics constant, people in better-off cities (in terms of mean income) are more likely to receive help from the program.

VI. CONCLUSIONS

Decentralized implementation of an antipoverty program relieves the center of the need to identify eligible recipients, which local authorities may well be in a better position to do. However, decentralization has its costs too—costs that may be hidden from the center. The literature has pointed to concerns about capture by local elites and migration responses to decentralized antipoverty programs.

This article has focussed on another concern, stemming from the fact that the choices made by local authorities in deciding who is eligible need not be consistent with the center's objectives and will typically be constrained by local resources. Even without local-capture problems, the geographic inequities under decentralization can so diminish a program's impact that the informational advantage of decentralization becomes moot. Furthermore, the information needed for setting corrective cost-sharing or interjurisdictional transfers is no less demanding than required for a fully centralized scheme. In short, there is no a priori reason to presume that decentralized implementation dominates centrally imposed eligibility criteria, albeit based on imperfect information.

It is an empirical issue just how much decentralized eligibility attenuates a program's ability to reach the poor nationally, though there has been very little research on that issue. China's Di Bao program provides an interesting case study. This is an ambitious attempt to eliminate extreme income poverty in urban China using geographically decentralized implementation of cash transfers aiming to guarantee a minimum income. Each municipality is free to decide who is eligible by setting its own minimum income.

On combining evidence from an unusually large household survey (representative for each of the 35 largest cities) with administrative data on the poverty lines chosen by local authorities, the article finds that better-off cities are able to support higher poverty lines for program eligibility and hence higher participation rates at given levels of need. The local resource constraint greatly diminished the program's ability to reach poor areas—roughly canceling the effect of the intercity differences in need for the program. The overall cross-city income gradient in program spending is still negative, although small and statistically insignificant. The variation in poverty lines associated with the decentralized eligibility criteria attenuated the program's overall poverty impact, but this effect turns out to be quantitatively small relative to the problems of leakage to ineligible households and (more importantly) incomplete coverage of those eligible.

As a consequence of the income effect on the eligibility thresholds, equally poor families in different cities have very different levels of access to the program, with the poor in poor cities typically faring the worst. This happens even though the center provides some degree of differential cost-sharing favoring poorer municipalities. The extent of this horizontal inequality suggests that it may create incentives for migration by China's poor. For now, the country's registration system and low Di Bao payments are constraining these incentives for migration. However, looking forward, likely reforms to the registration system (notably to free up the country's labor markets) and efforts to expand outlays and coverage will probably require a more unified, and horizontally equitable, program of social assistance.

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Mental Health Patterns and Consequences: Results from Survey Data in Five Developing Countries

Jishnu Das, Quy-Toan Do, Jed Friedman, and David McKenzie

The social and economic consequences of poor mental health in the developing world are presumed to be significant, yet remain underresearched. This study uses data from nationally representative surveys in Bosnia and Herzegovina, Indonesia, and Mexico and from special surveys in India and Tonga to show similar patterns of association between mental health and socioeconomic characteristics. Individuals who are older, female, widowed, and report poor physical health are more likely to report worse mental health. Individuals living with others with poor mental health are also significantly more likely to report worse mental health themselves. In contrast, there is little observed relation between mental health and consumption poverty or education, two common measures of socioeconomic status. Indeed, the results here suggest instead that economic and multidimensional shocks, such as illness or crisis, can have a greater impact on mental health than poverty. This may have important implications for social protection policy. Also significant, the associations between poor mental health and lower labor force participation (especially for women) and more frequent visits to health centers suggest that poor mental health can have economic consequences for households and the health system. Mental health modules could usefully be added to multipurpose household surveys in developing countries. Finally, measures of mental health appear distinct from general subjective measures of welfare such as happiness. JEL codes: O12, I10, I32, O15.

Mental health has received more attention in public health and policy spheres since the release of the World Health Organization's (WHO) flagship report, *Mental Health: New Understanding, New Hope* (WHO 2001). According to widely circulated estimates, unipolar depressive disorders are the leading cause of loss of disability adjusted life-years (DALYs) in the Americas and the third

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leading cause in Europe, but they also rank highly in lower income countries. They are the second leading cause in the Western Pacific, the fourth in South-East Asia, and the fifth in the Eastern Mediterranean (Ustun and others 2004).[1] While depression is not in the top 10 in Africa, it is recognized as a major source of disability, particularly in conjunction with HIV/AIDS epidemic (Freeman and others 2005).

The few low-income country estimates of poor psychological health suggest that prevalence is not systematically lower than it is in wealthier countries (Bijl and others 2003). The lifetime prevalence of psychiatric illness in rural Ethiopia was found to be 31.8 percent (Awas, Kebede, and Alem 1999). A community survey of 1,454 adults in Sao Paulo, Brazil, found a lifetime prevalence for any psychiatric disorder of 45.9 percent (Andrade and others 2002). And a review of five prevalence studies in four countries undergoing rapid socioeconomic change found lifetime prevalence rates of common mental disorders ranging from 23 percent in Pelotas, Brazil, to 46 percent in Goa, India (Patel and others 1999).

An economist's viewpoint adds to this literature in two ways. First, there are calls for broadening the concept of well being to include markers of "human development" other than income or consumption. Mental health could well be an important addition, as poor mental health directly affects well being. Understanding how (and whether) mental health indicators can be collected in the context of large-scale household surveys is thus important. Second, the impact of mental health on economic behavior is of inherent interest. To the extent that it affects labor force participation or productivity, there may be a direct economic rationale for public investments in improving mental health.

Incorporating mental health measures in household surveys would extend social science research on health, which attempts to understand the relations between poor physical health and economic and social outcomes (Strauss and Thomas 1998). The consequences of mental disorders such as depression and anxiety are presumed to be significant, yet are underresearched in low-income countries. A comparative research project of the World Bank's Development Research Group attempted to address this knowledge gap by investigating the socioeconomic context of poor mental health in low- and middle-income countries. The research produced three country studies from Tonga, India, and Indonesia (Stillman and others 2006; Das and Das 2006; Friedman and Thomas 2007). To study the correlates of mental health in a broad variety of low- and middle-income country settings, it used data from these countries as well as Bosnia and Herzegovina and Mexico.

This review argues that including mental health modules in multipurpose household surveys is relatively straightforward and informative in that they capture "real" underlying psychological illnesses. Validation studies from the

1. Such statements should be treated with caution given the sparse data on both physical and mental health available in many countries in these regions.

Bosnian and Indian data sets used here show that the mental health measures collected from surveys are highly correlated with clinical diagnoses of psychiatric disorders such as depression. In addition, associations between mental health and sociodemographic correlates are similar across all five countries, suggesting stability in what is being measured. The review also shows that mental health status is associated with labor supply and health care use, conditional on self-reported physical health status and other socioeconomic variables commonly collected in household surveys.

Two additional findings suggest that mental health measures complement traditional welfare measures, such as income, consumption, and poverty. Unlike studies in high-income countries, in low-income countries the correlation between mental health and levels of income or consumption is not strong. Poor mental health is not a "disease of affluence" in the developing world—nor is it a disease of poverty. But the country cases in the comparative research show that individual responses to long- or short-term shocks differ noticeably whether one looks at monetary measures of welfare, such as income and consumption, or at mental health indicators.

Section I of this article describes the survey instruments used to measure mental health and their validation. Section II examines the association between mental health and potential individual and household predictors. Section III considers the relations between mental health and two behavioral outcomes: labor supply and health care use. Section IV reviews the implications of these findings and contrasts them with the happiness literature, discusses potential response biases arising from self-reported mental health measures and how these may affect the findings here, and suggests probable productive paths of future research.

I. Data: Measuring Mental Health in Multipurpose Household Surveys

The psychological health literature has used two approaches to measure mental distress through surveys. The first attempts to diagnose specific psychiatric illnesses with data on symptoms collected through survey interviews. Used most widely is the Composite International Diagnostic Interview in its various formats, translations, and revisions. A second approach in low-income countries is to measure general psychological distress, rather than to diagnose specific manifestations of mental illness. Common examples include variants of the General Health Questionnaire (GHQ) of Goldberg (1972) and the Mental Health Inventory (MHI-5) of Veit and Ware (1983). These measures have been found to detect major depression, general affective disorders, and anxiety disorders (Berwick and others 1991; McCabe and others 1996). However, both approaches have found large differences in the overall case prevalence and proportion of respondents answering "yes" to specific questions across countries, differences ascribed in part to differences in cultural norms related to disease attribution and the stigma of mental illness (Patel, Pereira, and Mann 1998; Aidoo and Harpham

2001; Cross-National Collaborative Group 1992; WHO International Consortium in Psychiatric Epidemiology 2000; Demyttenaere and others 2004).

The multipurpose surveys used in this article follow the second approach, each fielding a widely used mental health screening instrument in the context of general household surveys. Table 1 summarizes the survey year, geographic coverage, mental health instrument, and sample size in each country. Nationally (or near-nationally) representative surveys of more than 10,000 households were carried out in Mexico and Indonesia, each using a variant of the GHQ to measure mental health. A nationally representative survey of more than 5,400 households in Bosnia and Herzegovina used the Center for Epidemiological Studies Depression Scale (CESD) of Radloff (1977), a 20-question self-reported depression scale. Also used here are two smaller special purpose surveys. The first is a longitudinal study of 300 households in Delhi, India, which do not differ in observable characteristics from a representative sample of households in this city (Das and Sánchez-Páramo 2003). It used the most comprehensive mental health instrument, the 90-question Symptom Checklist 90 Revised (SCL-90R). The second special survey is one of 230 Tongan households taken from villages in which some individuals had applied for an emigration lottery (Stillman, McKenzie, and Gibson 2006). It used the MHI-5 to measure mental health.

These data offer four advantages over those used in many previous studies. First, the surveys sample households from a representative population frame. Previous studies sample opportunistically, such as patients at health clinics (such as Patel, Pereira, and Mann 1998), with possible biases from nonrandom use of health clinics. Second, surveying households rather than individuals permits examining the mental health outcomes of different members of the same household. Third, the comprehensiveness of the surveys provides richer measures of socioeconomic characteristics, including expenditure- or income-based measures of poverty, rather than more blunt indicators, such as earning less than one-quarter the minimum wage or the presence or absence of electricity or tap water (such as Patel and Kleinman 2003). Fourth, mental health can be linked with behaviors in the labor and health care markets.

The exact content of the mental health questionnaires varies across countries, but they are similar in concept. Typical questions ask respondents the frequency in the last month of a similar range of internal states ("feeling sad or blue," "feeling anxious or nervous") or related behaviors ("difficulty falling asleep," "distracted from everyday activities"). The frequency of such states or behaviors is recorded on a four-point scale that ranges from "never" or "almost never" to "very often." In line with the standard analysis of the GHQ and MHI-5, the individual survey responses are scored by assigning a low ordinal value (1 point) to categorical responses of infrequency and high ordinal values (up to 4 points) to the categories with most frequent responses.[2] The average response

2. The MHI-5 used in Tonga had a five-point scale while the GHQ adapted in Indonesia had a three-point scale. Both were standardized to a 1–4 range for table 1.

TABLE 1. Overview of data sets employed in the Five Study Countries

					Mental health survey	Mental health measure	
Country	Year of survey	Number of households	Number of observations	Level of representation	Instrument	Mean	Standard deviation
Bosnia and Herzegovina	2001	5,409	12,956	National	CESD	1.495	0.502
India	2003	300	784	Seven neighborhoods in New Delhi[a]	SCL-90R	1.535	0.416
Indonesia	2000	>10,000	25,470	National	GHQ derived	1.413	0.508
Mexico	2002	>10,000	19,798	National	GHQ derived	1.341	0.358
Tonga	2005	230	714	Special sample of migrant-sending villages	MIH-5	1.745	0.337

Note: CESD is the Center for Epidemiological Studies Depression Scale; SCL-90R is the Symptom Checklist 90 Revised; GHQ is the General Health Questionnaire; MIH-5 is the Mental Health Inventory.

[a]Indistinguishable from a representative sample of the city.

Source: Authors' compilation.

across all questions (with each question given equal weight) constitutes the respondent's mental health score, often known as the Global Severity Index (GSI), which is higher for those reporting worse mental health.[3]

In contrast to subjective measures of well being, such as satisfaction with life, mental health measures can be validated against well-defined objectives—particularly whether answers to the questions predict clinical diagnoses of depression, anxiety, and other mental health disorders. Both the GHQ and MHI-5 perform well in this regard. Validations of the mental health modules for the specific populations used in the studies were attempted for the Bosnian and Indian data. In India, survey teams administered the survey to 38 individuals seeking outpatient treatment at two psychiatric facilities. They found the mental health scores to be significantly lower for the general population than for the selected subpopulation known to suffer from psychiatric disorders. In Bosnia and Herzegovina, a selected sample of 184 individuals—who had visited primary health care facilities in the Canton of Middle Bosnia—were administered the household survey module and then examined by a psychiatrist for known psychiatric disorders. The household survey had a 97.5 percent sensitivity rate and a 75 percent specificity rate (Kapetanovic 2004).[4]

Table 1 also presents the mean and standard deviation of the raw scores by country. Because of concerns with cross-national comparability, this review does not directly compare levels of psychological distress across national surveys. Instead, it explores the observed commonalities and differences in correlates of mental health, standardizing the mental health scores by subtracting the country mean and dividing by the country standard deviation. The standardized GSI then has a mean of zero and a standard deviation of one for each country.

II. Characteristics of Those with Poor Mental Health — Patterns and Divergences

Summarizing and extending the work in Das and others (2007), this section examines the correlation of mental health with individual, household, and community factors. It identifies demographic and socioeconomic characteristics that could be consistently measured across the different data sets and that the existing literature has found associated with mental health. These include age,

3. In addition, for each country except Tonga, three common questions ask whether the respondent has recently felt sad, felt anxious, and had trouble sleeping. An index formed using only these three questions has correlations of 0.84–0.90 with the overall index for each country, and similar results are obtained using this less comprehensive measure. To include Tonga in the analysis and use the most common indices, the authors analyze the more comprehensive measures.

4. Although these results are promising for the relevance of household survey modules to assessing mental health distress, validation exercises are made on a sample of individuals that might differ significantly from the larger population of interest. Field validations in future related studies may address this concern.

gender, marital status, physical health, and education. Added to these characteristics are household measures of expenditure (income for Tonga), household size, and the average mental health of other household members. Also included for the three large surveys are community-level mental health scores, the average mental health of other households in the community.[5]

Mental Health in the Cross-Section

Table 2 reports the results of ordinary least squares (OLS) regressions of the standardized mental health scores on these characteristics, with the analysis restricted to 15- to 80-year-olds and standard errors clustered at the household level. A possible concern is that much of the linear association between mental health scores and the covariates is driven by differences between individuals with very good mental health compared with average mental health, masking heterogeneity relevant to those with the worst mental health scores (presumably those in greatest need of treatment). Table 3 thus reports marginal effects from probit estimation of the probability of an individual being in the worst 10 percent or worst 5 percent of mental health scores in each country—typically referred to as the "caseness" category in the psychology literature.[6]

The results of both the OLS and the probit estimations confirm widely reported associations across countries between mental health status and demographic and household factors, with some exceptions. Consistent with the literature (Awas, Kebede, and Alem 1999; Andrews, Henderson, and Hall 2001), the tendency in all five countries is for mental health to worsen with age, significant in all countries except Tonga.[7] A second common regularity in the literature is a tendency for women to have worse mental health (Awas, Kebede, and Alem 1999; Kessler and others 2005), found in four of the five countries, with the effect strongest in Mexico, where women are 8 percentage points more likely to be in the worst 10 percent of mental health scores. Tonga is again the exception, with women having significantly better mental health in

5. The main results do not change appreciably with the inclusion of community fixed effects to control for unobserved community factors, so those results are not reported here.

6. Attempts to compare mental health prevalence across countries by the WHO International Consortium in Psychiatric Epidemiology (2000) yielded 12-month prevalence rates of 22.4 percent in Brazil, 12.6 percent in Mexico, and 8.4 percent in Turkey. Taking the worst 10 percent and worst 5 percent as cutoffs thus appears likely to include many individuals with diagnosable disorders. A more inclusive cutoff of 20 percent yields qualitatively similar results, as does a nonlinear Box-Cox transformation of mental health.

7. Das and others (2007) carry out semiparametric regressions to examine nonlinearities in age and other covariates. There is slight evidence for a nonlinear relation between age and mental health in Tonga, with mental health falling and then improving with age, while results for all other countries and variables are linear. Adding a quadratic term in age to the OLS regression in table 2 gives a p-value of 0.08. This quadratic relation appears to be driven by a few outliers, as the quadratic term becomes insignificant (p-value of 0.33) if the analysis is restricted to those aged 70 and younger. So, only linear terms are reported in this article.

TABLE 2. Correlates of Mental Health in the Five Study Countries

	Tonga		India		Mexico		Bosnia and Herzegovina		Indonesia	
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	0.000112	0.000635	0.00208	0.00324*	0.000656*	0.00116***	0.00442***	0.00614***	0.00152***	0.00155***
	(0.0016)	(0.0014)	(0.0019)	(0.0017)	(0.00039)	(0.00036)	(0.00069)	(0.00048)	(0.00037)	(0.00034)
Female dummy variable	−0.0483**	−0.0697***	0.105***	0.145***	0.156***	0.173***	0.0874***	0.147***	0.0767***	0.0837***
	(0.024)	(0.023)	(0.030)	(0.032)	(0.0066)	(0.0070)	(0.0095)	(0.0080)	(0.0073)	(0.0077)
Married dummy variable	0.159***	0.145***	−0.0171	−0.0484	−0.000198	0.00732	0.0736***	0.0641***	−0.0992***	−0.0910***
	(0.044)	(0.037)	(0.057)	(0.051)	(0.012)	(0.010)	(0.021)	(0.015)	(0.012)	(0.011)
Widowed dummy variable[a]	0.128	0.183**	−0.120	−0.170*	0.0583***	0.0626***	0.116***	0.133***	0.0465**	0.0452**
	(0.090)	(0.082)	(0.11)	(0.096)	(0.019)	(0.018)	(0.037)	(0.026)	(0.023)	(0.021)
Years of education	0.00418	0.00493			−0.00951***	−0.00653***	−0.0104***	−0.00496***	−0.00140	−0.00135
	(0.0039)	(0.0032)			(0.0012)	(0.0011)	(0.0016)	(0.00080)	(0.0011)	(0.00093)
Primary to high school			−0.0836**	−0.0720**						
			(0.039)	(0.035)						
High school or more			−0.153***	−0.109**						
			(0.051)	(0.043)						
Log household consumption per capita	−0.0438**	−0.0192**	0.0485	0.0281	0.000169	0.00502	−0.0853***	−0.0139***	0.00873	0.00541
	(0.017)	(0.0076)	(0.033)	(0.021)	(0.0054)	(0.0043)	(0.015)	(0.0053)	(0.0065)	(0.0048)
Household size	−0.0122*	−0.00141	0.0156	0.00968	0.00113	0.000610	−0.0314***	−0.00564*	0.00186	−0.0000531
	(0.0070)	(0.0034)	(0.012)	(0.0070)	(0.0026)	(0.0020)	(0.0077)	(0.0030)	(0.0018)	(0.0012)
Poor physical health[b]			0.236***	0.189***	0.379***	0.339***	0.723***	0.528***	0.378***	0.337***
			(0.058)	(0.050)	(0.021)	(0.021)	(0.097)	(0.059)	(0.015)	(0.014)
Elderly dependents[c]	−0.280**	−0.175***	−0.187	−0.169*	0.0282	−0.0205	−0.0919**	−0.174***	0.0398	0.0176
	(0.11)	(0.060)	(0.16)	(0.098)	(0.023)	(0.019)	(0.038)	(0.018)	(0.029)	(0.022)
Young dependents[d]	−0.255***	−0.192***	0.00626	0.00729	0.0572**	0.0273	−0.0920	−0.0358	0.0516**	0.0448**
	(0.098)	(0.049)	(0.10)	(0.070)	(0.023)	(0.018)	(0.059)	(0.023)	(0.026)	(0.019)
Household mental health		0.592***		0.404***		0.222***		0.544***		0.364***
		(0.045)		(0.067)		(0.017)		(0.020)		(0.022)

Community mental health						0.517***		0.354***		0.707***
						(0.042)		(0.020)		(0.039)
Constant	1.954***	0.730***	1.051***	0.968***	1.245***	0.187***	1.833***	−0.0904**	1.190***	−0.116
	(0.096)	(0.11)	(0.26)	(0.17)	(0.043)	(0.068)	(0.098)	(0.042)	(0.078)	(0.073)
Number of observations	685	685	738	738	17,905	17,905	11,870	11,870	19,697	19,697
R-squared	0.10	0.32	0.08	0.18	0.14	0.20	0.22	0.61	0.09	0.15

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Note: Higher score means worse mental health. Numbers in parentheses are robust standard errors clustered at the household level.

[a]Includes those separated and divorced.

[b]No individuals reported themselves in poor physical health in the Tongan survey, so this variable is not included in the Tongan analysis.

[c]Number of household members older than age 60.

[d]Number of members aged 15 and younger.

Source: Authors' analysis based on data described in text.

TABLE 3. Correlates of Severe Mental Health in the Five Study Countries: Marginal Effects from Probit Estimation of Being in the Worst 10 Percent or Worst 5 Percent of Mental Health Scores

Variable	Tonga Worst 10% (1)	India Worst 10% (2)	Mexico Worst 10% (3)	Mexico Worst 5% (4)	Bosnia and Herzegovina Worst 10% (5)	Bosnia and Herzegovina Worst 5% (6)	Indonesia Worst 10% (7)	Indonesia Worst 5% (8)
Age	0.0000921	0.000986	0.000522**	0.0000575	0.000783***	0.000298***	0.000688***	0.000137
	(0.0013)	(0.0010)	(0.00026)	(0.00020)	(0.00018)	(0.000063)	(0.00021)	(0.00014)
Female dummy variable	0.0000756	0.0437**	0.0789***	0.0443***	0.0260***	0.00731***	0.0306***	0.0150***
	(0.026)	(0.021)	(0.0057)	(0.0043)	(0.0043)	(0.0018)	(0.0049)	(0.0032)
Married dummy variable	0.0846***	0.0121	0.0122	0.00654	0.0206***	0.00294	−0.0514***	−0.0131**
	(0.032)	(0.033)	(0.0075)	(0.0059)	(0.0053)	(0.0019)	(0.0083)	(0.0052)
Widowed dummy variable[a]	0.125	−0.0472	0.0335**	0.0246*	0.0896***	0.00832	0.00519	0.0215**
	(0.15)	(0.034)	(0.016)	(0.013)	(0.033)	(0.0067)	(0.012)	(0.0097)
Years of education	−0.00182		−0.00435***	−0.00308***	−0.000773***	−0.000270**	−0.000558	−0.000528
	(0.0034)		(0.00081)	(0.00061)	(0.00027)	(0.00012)	(0.00063)	(0.00039)
Primary to high school		−0.0258						
		(0.024)						
High school or more		−0.0372						
		(0.024)						
Log household consumption per capita	0.000774	0.00228	0.0000798	0.00135	−0.00464**	−0.00148*	0.000546	−0.00441**
	(0.0093)	(0.014)	(0.0030)	(0.0023)	(0.0022)	(0.00088)	(0.0032)	(0.0021)
Household size	0.00774**	0.00764*	0.000174	−0.000830	−0.00148	−0.000405	0.000353	−0.000671
	(0.0038)	(0.0046)	(0.0014)	(0.0012)	(0.0011)	(0.00052)	(0.00085)	(0.00055)
Poor physical health[b]		0.0961**	0.181***	0.124***	0.155***	0.0959***	0.175***	0.102***
		(0.044)	(0.019)	(0.017)	(0.034)	(0.022)	(0.010)	(0.0076)
Old dependents[c]	−0.0492	−0.0182	−0.00969	−0.00225	−0.0201***	−0.0100***	−0.000732	0.000631
	(0.086)	(0.060)	(0.015)	(0.0100)	(0.0070)	(0.0027)	(0.015)	(0.0094)
Young dependents[d]	−0.124**	−0.0827	0.0141	0.00341	−0.0120	−0.00827*	0.0358***	0.00922
	(0.055)	(0.052)	(0.014)	(0.011)	(0.011)	(0.0046)	(0.013)	(0.0085)

Household mental health	0.414***	0.140***	0.0805***	0.0473***	0.0653***	0.0196***	0.138***	0.0637***
	(0.043)	(0.028)	(0.0085)	(0.0060)	(0.0055)	(0.0025)	(0.0096)	(0.0058)
Community mental health			0.182***	0.113***	0.0549***	0.0116***	0.267***	0.124***
			(0.027)	(0.019)	(0.0077)	(0.0031)	(0.026)	(0.016)
Observations	685	738	17,905	17,905	11,870	11,870	19,697	19,697

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Note: Marginal effects are the change in probability associated with a discrete change in dummy variables from zero to one and with an infinitesimal change in continuous variables. Numbers in parentheses are robust standard errors clustered at the household level.

[a]Includes those separated and divorced.

[b]No individuals reported themselves in poor physical health in the Tongan survey, so this variable is not included in the Tongan analysis.

[c]Number of household members older than age 60.

[d]Number of members aged 15 and younger.

Source: Authors' analysis based on data described in text.

the OLS regression. But much of this appears driven by subclinical differences, since the probit results show a small and insignificant effect of being female.

A third consistent finding in the literature is that respondents who are separated, divorced or widowed report worse mental health compared to those who are married (Andrade and others 2002; Andrews, Henderson, and Hall 2001; Kessler and others 2005; Weissman and others 1996). This is found to be the case in all countries except India, where widows report significantly better mental health. The difference in India appears driven by differences among individuals with relatively good mental health, because the coefficient becomes smaller and insignificant when the likelihood of being in the worst 10 percent of mental health scores is examined. In addition, small sample sizes (4 percent or 69 individuals are widowed) merit some caution in interpreting this result. The results here are also consistent with previous studies that have highlighted the links between poor physical and poor mental health (Kessler and others 1994; Bijl and others 2003). Physical health is measured as a binary variable based on self-assessed general health status, with a large and significant effect.[8] Individuals with poor physical health are between 10 percent (India) and 18 percent (Mexico) more likely to be in the worst 10 percent of mental health.

There is less of a pattern for household size and the presence of old and young dependents in the household. Individuals in larger households have marginally better mental health in Bosnia and Herzegovina, with no significant relation elsewhere. Elderly dependents in the household are associated with significantly better mental health in Bosnia and Herzegovina and Tonga, but no significant relation elsewhere, while the significant positive and negative associations with young dependents in the OLS results become less pronounced in the probit regressions.

The demographic patterns are similar to those found previously in the literature, but the association between mental health and per capita household expenditure is tenuous at best, in contrast to the view that poverty is strongly associated with mental health disorders (Patel and Kleinman 2003). In the OLS results, only Bosnia and Herzegovina and Tonga show a significant negative relation between per capita consumption and individual mental health. The effects are small, and even smaller in the probit results: a doubling of household per capita expenditure in Bosnia and Herzegovina results in a 0.4 percentage point greater likelihood of an individual being in the worst 10 percent of mental health scores. There is a small significant negative gradient in the probit results for those in the worst 5 percent of mental health scores in Indonesia. The OLS results for Mexico, India, and Tonga show a slight positive gradient, though this is not significant at conventional levels.[9]

8. No individuals reported themselves in poor physical health in the Tongan survey, so this variable is not included in the Tongan analysis.

9. The lack of association between mental health and poverty is not a result of the household average mental health status capturing a poverty effect. Results are similar when the household average mental health is omitted from these regressions.

The association between schooling and mental health is also small. All five countries have a negative coefficient on years of schooling in the probit regressions. But this is significant at the 5 percent level only in Mexico, where one additional year of schooling is associated with a 0.4 percentage point drop in the likelihood of being in the worst 10 percent of mental health scores—and in Bosnia and Herzegovina, where the equivalent association is a 0.08 percentage point drop. The OLS results show similarly small effects. The largest effect is in India, where those with high-school education have a mental health score 0.15 standard deviations better than those with less than a primary education.

All five surveys measured mental health for multiple adults within the household, enabling examination of the coprevalence of mental health. There are several possible channels for such a correlation. It may reflect omitted household variables, such as household-specific shocks or a lack of health services. It could also reflect unobserved individual traits, if assortative mating leads those with poor mental health to marry and perhaps pass on genetic factors influencing mental health of other family members. But it is also plausible that the presence of one household member with poor mental health creates a poor mental health environment for other household members, a "contagion" effect. Tables 2 and 3 show a strong and significant positive relation between an individual's mental health and that of the family.[10] A one standard deviation change in the mental health of household members is associated with a 0.22–0.59 standard deviation change in own mental health.

For Bosnia and Herzegovina, Mexico, and Indonesia it is possible to look at the relation between individual mental health and the mental health of others in the surrounding community. There is a significant and positive association, even after controlling for household average mental health, with the size of the coefficient up to twice that at the household level. Some of the explanations that account for the within-household clustering of poor (or good) mental health can also apply to the community.

Mental Health and Shocks

The cross-sectional analysis shows no significant relation between mental health and poverty, but it does find strong clustering of mental health outcomes within households and, to less extent, within communities. One interpretation of this intrahousehold clustering is that it reflects, in part, the effect of common household (or community) shocks. The three country-specific studies produced by the Development Research Group's comparative project examine in detail the impact of shocks on mental health and provide some support for this interpretation.

10. The household measure of mental health is the average across household members (excluding the individual respondent). Similarly, the average community mental health score can be defined as the average across the community (excluding the household in question).

Two of the studies demonstrate that shocks involving large changes in income led to changes in mental health. Friedman and Thomas (2007) consider the impact of a negative shock—the Indonesian financial crisis—finding that the crisis worsened mental health of households, more so for households more affected by the crisis. And they find that this effect persists up to three years after the onset of crisis, despite a rapid recovery in consumption and income to precrisis levels.[11] Stillman, McKenzie, and Gibson (2006) examine the effect of a positive shock—winning an emigration lottery allowing migration from Tonga to New Zealand—and find that it produces large increases in income and improvements in the mental health of the migrating households.

Although the Indonesian and Tongan case studies show that large shocks can have significant effects on mental health, the effect may be due only in part to changes in income. It is likely that the financial crisis led to many other covariate changes in household circumstances, including a reduced availability of community public services or household dislocation as a result of migration. Similarly, the reduced-form impact of migration on mental health possibly encompasses a host of changes other than income to the individual, which the authors explore.

The Indian study, using the well-documented difference in mental health scores between men and women as a motivating question, attempts to isolate exactly how a specific shock affects mental health, though the authors make less progress in identifying a causal impact. Das and Das (2006) show that the male–female difference in mental health scores is directly related to the number of pregnancies a women has lost, due to abortions, miscarriages, or infant deaths. In households without such losses, there is no gender difference in reported mental health scores. This finding can be interpreted in different ways. By combining the quantitative analysis with anthropological narratives, the authors present qualitative evidence on the pathways for producing these data.

Together, the three case studies provide strong evidence that while income and poverty are not strong predictors of mental health status, shocks that affect the economic or demographic nature of the household may have significant influences on mental health. The next section explores the possible consequences of poor mental health for economic behavior.

III. Possible Consequences of Poor Mental Health for Economic Behavior

The multipurpose surveys enable examining associations between mental health and two development-related outcome measures—labor force

11. Related evidence is found in de Mel, McKenzie, and Woodruff (2008), who show no relations among mental health recovery from the tsunami in Sri Lanka and income recovery of microenterprise owners.

participation and health care use. While a causal effect cannot be ascribed to mental health (except under the strong condition of selection on observables), this as an exploratory step intended to show how poor mental health status is correlated with economic behavior and how these patterns may vary around the developing world.

Labor Force Participation

Labor force participation is an important determinant of socioeconomic welfare, and many researchers observe a strong empirical association between mental illness and labor force participation in developed countries (Bjorklund 1985; Ettner, Frank, and Kessler 1997; Kessler, Turner, and House 1989; Dooley, Prause, and Ham-Rowbottom 2000). The association could reflect difficulties in finding and sustaining work for those with depression or mental health problems arising from a lack of work.

Table 4 presents the marginal effect of mental health on labor supply from probit regressions. Panel A shows the effect when the standardized GSI is used as the measure of mental health status; panel B shows the effect of being in the worst 10 percent of mental health scores within a country. Estimation is restricted to 18- to 60-year-olds, separately by gender because of gender differences in labor market behaviors.

Table 4 finds that the association between labor force participation and mental health status varies by gender and country. In both Mexico and Bosnia and Herzegovina, there is no significant association, and the coefficients are small. But in Indonesia both men and women exhibit significant if fairly slight associations. A one standard deviation improvement in mental health status increases labor force participation by 1.3–1.7 percentage points, while individuals in the worst 10 percent of mental health scores have 4.6–5.1 percentage point lower participation rates. In India, there is no significant effect for men, but a very large effect for women. The labor force participation rate of women aged 18–60 is only 30.4 percent, so the 17.7 percentage point lower participation rate for women in the worst 10 percent of mental health scores cuts labor force participation in half. The Tongan results show a sizable and significant negative effect of mental health on labor force participation for women when the standardized GSI measure is used, and a large but insignificant effect of being in the worst 10 percent of mental health scores. In contrast, there is no effect of the standardized GSI measure for men, but a large significant negative effect of being in the worst 10 percent.

The negative relation between psychological distress and labor force participation often observed in developed countries does not consistently translate to the developing country setting. Bosnian and Mexican respondents in psychological distress show no tendency to work less than others in their communities. A slight tendency is observed in Indonesia, and a much greater tendency exists for women in India.

TABLE 4. Impact of Mental Health on Labor Force Participation in the Five Study Countries: Marginal Effects from Probit Estimation (18- to 60-year-olds)

	Tonga		India		Mexico		Bosnia and Herzegovina		Indonesia	
Measure	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
Panel A										
Standardized mental health index	0.00765 (0.028)	−0.0863*** (0.029)	0.00910 (0.022)	−0.0462* (0.026)	0.000961 (0.0052)	−0.00455 (0.0074)	0.0192 (0.015)	0.0183 (0.013)	−0.0173*** (0.0043)	−0.0130** (0.0056)
Panel B										
Dummy variable for being in worst 10%	−0.2090** (0.097)	−0.1077 (0.131)	−0.0317 (0.079)	−0.1769** (0.061)	0.011 (0.017)	−0.017 (0.023)	−0.0362 (0.062)	0.0042 (0.046)	−0.0508*** (0.019)	−0.0463** (0.019)
Labor force participation rate	0.59	0.37	0.84	0.30	0.89	0.38	0.73	0.43	0.83	0.50
Number of observations	299	320	334	345	6,665	8,675	4,597	4,980	9,626	10,742

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Note: Marginal effects are the change in probability associated with a discrete change in dummy variables from zero to one and with an infinitesimal change in continuous variables. Probits also include age, marital status, education, household size, elderly and young dependents, and physical health status. Numbers in parentheses are robust standard errors clustered at the household level.

Source: Authors' analysis based on data described in text.

Use of Health Services

Large proportions of severely mentally ill populations in the developing world receive no treatment for their disorders, suggesting widespread underuse and poor access. The situation is further complicated when patients present with primarily somatic complaints for psychiatric conditions. A study of primary care attendees in Goa, India, revealed that 97 percent presented with physical symptoms, but roughly half of them were psychiatric cases according to biomedical criteria (Patel, Pereira, and Mann 1998). This has a direct bearing on diagnosis and treatment, because somatic symptom presentation is associated with lower recognition rates of mental disorders by primary care physicians (Paykel and Priest 1992).

The three large nationally representative surveys (Bosnia and Herzegovina, Mexico, and Indonesia) permit examining how much mental health status predicts use of health facilities in the month before the survey, after conditioning on self-reported physical health status, income, and other possible determinants of use. Panel A of table 5 depicts the probit marginal effect when the standardized GSI is used as the measure of mental health status; panel B depicts the marginal effect of being in the worst 10 percent of mental health scores within a country.

The results suggest that individuals with poor mental health are more likely to use health services. This effect occurs for both men and women in all three countries, with similar-sized absolute effects in Mexico, a stronger effect for men in Bosnia and Herzegovina, and a stronger effect for women in Indonesia. Comparing the size of effects and the proportions using health facilities reveals quite large associations. For example, a Mexican male in the worst 10 percent of the mental health distribution has a 0.06 larger probability of using health facilities, which is 54 percent of the 0.11 for all Mexican males using health facilities.

The common observation in both developed and developing economies that individuals with poor mental health present themselves to health facilities more frequently, independent of their (self-assessed) physical health, is also found in the data. This behavior may pose economic and social burdens on households with individuals in psychological distress and on the health system, especially if the underlying cause of illness is misdiagnosed.

IV. Discussion: Implications for Research and Policy

Household surveys in five low- and middle-income countries covering Latin America, Eastern Europe, East Asia and the Pacific, and South Asia reveal significant associations between mental health scores and gender, the physical health of the respondent, marital status, and the mental well being of other members in the household and community. These relations hold (with occasional deviations) across all the countries with roughly comparable

Table 5. Impact of Mental Health on Health Facility Use in the Five Study Countries: Marginal Effects from Probit Estimation

	Mexico		Bosnia and Herzegovina		Indonesia	
Measure	Men	Women	Men	Women	Men	Women
Panel A						
	0.0347***	0.0473***	0.0826***	0.0573***	0.00566***	0.0127***
Standardized mental health index	(0.0053)	(0.0053)	(0.0093)	(0.0085)	(0.0021)	(0.0022)
Panel B						
	0.0653**	0.0889***	0.272***	0.0752**	0.00991	0.0247***
Dummy variable for being in worst 10%	(0.026)	(0.019)	(0.051)	(0.029)	(0.0085)	(0.0092)
Proportion using a health facility in the last month	0.111	0.211	0.204	0.262	0.050	0.076
Number of observations	8,538	10,720	6,007	6,794	11,948	13,296

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Note: Marginal effects are the change in probability associated with a discrete change in dummy variables from zero to one and with an infinitesimal change in continuous variables. Probits also include age, marital status, education, household size, elderly and young dependents, physical health status, and log household consumption or income per capita. Numbers in parentheses are robust standard errors clustered at the household level.

Source: Authors' analysis based on data described in text.

magnitudes. In contrast, there is no consistent relation between mental health scores and socioeconomic measures, such as the respondent's education or the per capita expenditure (or income) of the respondent's household. There is evidence in some countries that lower mental health is associated with reduced labor force participation, especially for women. Mental health is also a significant predictor of health care use and thus perhaps of the burdens on a health system ill-equipped to diagnose and provide care.

These results provide the setting for a discussion of the validity of mental health modules in multipurpose household surveys, the possible use of mental health in a broader definition of welfare than traditional measures, and the implications for research and policy on mental health.

Including Mental Health in Multipurpose Household Surveys

Kahneman and Krueger (2006, p. 7) in a review of subjective welfare measures suggest that "the validity of subjective measures of well being can be assessed, in part, by considering the pattern of their correlations with other characteristics of individuals and their ability to predict future outcomes." The patterns of correlations between survey mental health measures and age, gender, widowhood, sickness, and the mental health of other family members are remarkably stable across countries and different surveying techniques. This is reassuring because it suggests that such measures reflect the real psychological condition of individuals. Moreover, unlike other subjective measures of well being, mental health measures have been further corroborated by studies showing that these general purpose mental health questions strongly predict clinical diagnoses of depression and anxiety disorders.

In addition, mental health status helps predict labor force participation and health care use, conditional on other socioeconomic variables typically collected in multipurpose household surveys. Further, the associations between mental health scores and individual and household characteristics are very similar in surveys where questionnaires were fielded on a first visit to households and where they were fielded after a period of acquaintanceship. Finally, shorter modules (such as the GHQ-12) reveal associations similar to those of the longer SCL-90R, which took one hour to field for nonliterate respondents.[12] The results thus suggest that mental health screening questionnaires can be meaningfully added to multipurpose household surveys, such as the Living Standards Measurement Studies. In addition to their intrinsic interest, they provide additional predictive power for economic behavior beyond the physical health measures and socioeconomic variables traditionally collected.

A concern with self-reporting of physical health status is that it results in measurement errors correlated with socioeconomic characteristics, including income (Strauss and Thomas 1998; Lokshin and Ravallion 2005). Strauss and

12. Indeed, most of the relevant information for the nine dimensions covered in the SCL-90R is contained in the depression and anxiety components (Das ans Das 2006).

Thomas (1998) note that reports of specific functions, such as activities of daily living, are believed to be more accurate and less subject to these sources of measurement error than questions about general health. In this regard, the questions used to construct the mental health indices—such as those about the frequency of having difficulty falling asleep, or of being distracted from everyday activities—may be less prone to reporting bias than are the questions about general health.

Even so, there may still be biases in the reporting of mental health symptoms. Sociologists and psychologists have identified three effects that may influence accurate reporting of internal states: the overall tendency to say yes or no, the need for social approval, and the perception of the desirability of a trait (boys don't cry, so men often do not report whether they tend to "suddenly cry without reason"). Although the literature is not extensive, an early study (Gove and Geerkin 1977) suggests that these biases are not correlated with demographic variables such as sex, race, education, income, age, marital status, and occupation—the categories examined in this article. Their results were corroborated by Vernon, Roberts, and Lee (1982) and Hunt, Auriemma, and Cashaw (2003). In all three studies, reporting bias tends to underestimate the prevalence of psychiatric disorders such as depression but not to alter observed correlations between mental health and demographic characteristics. One caveat: these studies were all in high-income countries, so there is a need for further research in developing country contexts.

Mental Health and Welfare Measurement: Going Beyond Measures of "Happiness"

An additional reason for including mental health measurement in household surveys is that it can allow consideration of a broader notion of welfare than is offered by the traditional focus on income and expenditure. Consider the burgeoning literature on happiness and its correlates (see Kahneman and Krueger 2006 for a recent review). World Value Surveys, for instance, collect information on global life satisfaction or happiness with the single question "All things considered, how satisfied are you with your life as a whole these days?" An extensive literature analyzes the correlates of positive reports, so a natural question is whether information revealed through mental health questionnaires are partially orthogonal to information contained in questions on life satisfaction and happiness and thus add value in their own right.

First impressions suggest that happiness and common mental disorders have to be closely related—it seems difficult for the same person to report high levels of mental distress *and* high levels of happiness. Indeed, studies report a high correlation between measures of life satisfaction and measures of psychological depression (Kahneman and Krueger 2006). But a deeper examination of the correlates of mental health and happiness imply the need for a more nuanced explanation. The differences arise both in correlations between mental health and happiness and an individual's *characteristics* (age, gender, income) and in correlations between mental health and happiness and an individual's *life events*.

The analysis in this article reveals several areas for the associations of individual characteristics with mental health to differ from the associations in the happiness literature. Women generally report worse mental health than men, whereas happiness is unrelated to gender. Layard (2005), arguing for a single dimension of happiness and mental health, points out that depression is higher among women due to biogenetic markers. Yet the findings here—that life events can affect male and female mental health in a different manner (see discussion below)—suggest that gender is one wedge for distinguishing happiness measures from mental health measures. Mental health almost universally worsens with age, whereas the relation between age and happiness is complicated and highly nonlinear—there is some indication that happiness is lowest in households with teenagers, at least in developed countries. The relations are more similar for education and poverty or income. More education improves mental health (albeit slightly) *and* makes one happier; more income does neither.

A second possible difference between mental health and happiness is in habituation and adaptation, measured through the lens of changing life circumstances. A consistent finding in the literature on happiness is that individuals habituate or adapt to new circumstances in their lives. While changes such as marriage and recovery from illness are associated with greater happiness in the short run, the effects vanish after a while. On this, the related studies in the comparative project are suggestive. In India, women who report child loss (through miscarriages, abortions, or deaths) are at significantly higher risk of mental health problems than those who do not. Indeed, the female mental health penalty observed in the data is entirely driven by the difference between men and women in households that experienced the loss of a child (Das and Das 2006). In Indonesia, the mental health of the population worsened dramatically following the economic crisis of the 1990s, but although consumption recovered by 2000, mental health did not (Friedman and Thomas 2007). And as shown earlier in all five countries, poor physical health, measured through self-reported health status, is strongly correlated with poor mental health.

Implications for Further Research on Mental Health and Development

Effective public health policy requires understanding the mechanisms that determine poor mental health and, in turn, the implications of poor mental health for individuals and their families. The descriptive analysis here provides suggestive evidence for some of these mechanisms and thus a possible role for public health interventions.

The lack of any relation between conventional economic welfare measures and mental health outcomes across a diverse sample of developing countries suggests that poverty alone is not a strong determinant of poor mental health. Undermining a straightforward equity rationale for public investments in mental health are the higher prevalence among the poor of other health problems, such as tuberculosis and malaria, and the continuing financing gaps for these illnesses. But the lack of a relation between consumption poverty and mental health certainly does not support arguments that suggest no scope for public interventions to improve mental health.

Country studies in India, Indonesia, and Tonga suggest that changes in life circumstances brought on by positive or negative events have fairly long-lasting implications for mental health. In addition, two of the strongest factors associated with poor mental health are poor physical health and widowhood. These findings are consistent with studies that report worsened mental health outcomes in populations that have suffered conflict or disasters (UNHCR 2005; Lopes Cardozo and others 2004). The Indian and Indonesian studies suggest that the trauma from adverse events may persist long after the recovery of more traditional measures of welfare, and there may very well be real individual and household costs to this persistence.

Possible impacts of mental health on labor force participation and health-seeking behavior have already been discussed. Additional examples of such costs along the health dimension include lower adherence to dietary recommendations and medication regimes among diabetics with depressive symptoms than among diabetics without (Ciechanowski, Katon, and Russo 2000); high comorbidity rates for smoking and psychiatric disorders, with smoking twice as common among mentally ill than mentally healthy populations (Lasser and others 2000); an association between maternal mental health and child welfare in two South African studies, with maternal depression significantly increasing the odds that a child will experience growth faltering (Harpham and others 2005; Patel and others 2004). There is also evidence of such costs in other dimensions of welfare, such as education (Kessler and others 1995).

If negative shocks lead to worse mental health, which in turn may reduce labor supply and increase spending on health care, as the comparative research project suggests, then this chain of events raises the possibility of a behavioral poverty trap. Clearly, more research is needed to better understand the causal mechanisms and dynamic phasing, including the short- and long-term effects of negative and positive shocks to mental health. Research on the cost-effectiveness of interventions targeted to those in poor mental health will also shed needed light on the rationale for publicly financed mental health interventions. The clustering of mental health outcomes within households and communities raises the possibility that such treatments targeted to the household or community may be more cost-effective than those targeted to individuals—and this should be investigated.

An important limitation of this study—and of the household survey-based methodology—is the inability to differentiate common from severe mental disorders. Although both cause personal misery, the literature notes a clear distinction, especially in the context of findings that the annual prevalence of common mental disorders exceeds 10 percent in many countries: 16.9 percent in Lebanon, 17.8 percent in Colombia, 20.4 percent in Ukraine, and 26.3 percent in the United States (WHO World Mental Health Survey Consortium

2004). Severe mental disorders (such as schizophrenia), brought on by biogenetic causes and possible interactions with the environment, have much lower prevalence and require specialized studies rather than multipurpose surveys to identify—and most likely a separate policy response. In several low-income countries, the institutional capacity for treating such disorders is very poor, with frequent human-rights violations of the severely ill (WHO 2001).

A final limitation of this study—one mentioned throughout the text—is that, without an experimental setup, the associations presented can have multiple interpretations. For instance, the concordance of mental health outcomes within households could reflect unobserved household shocks, assortative matching (people in poor mental health are more likely to marry each other), genetic links between parents and children, or a "contagion" effect (caring for a mentally ill person in the household leads to a deterioration in the mental well being of the caretaker). Longitudinal data and experimental mental health interventions are needed to separate these disparate channels and to explore the links between mental health outcomes and broader measures of welfare that incorporate risk and vulnerability in their construction.

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Psychological Health Before, During, and After an Economic Crisis: Results From Indonesia, 1993–2000

Jed Friedman and Duncan Thomas

The 1997 Indonesian financial crisis resulted in severe economic dislocation and political upheaval. Previous studies have established the detrimental consequences for economic welfare, physical health, and child education. The crisis also affected the psychological well-being of the Indonesian people. Comparing responses of the same individuals interviewed before and after the crisis, this study documents substantial increases in several dimensions of psychological distress among men and women across the age distribution. It shows larger impacts of the economic crisis on the more vulnerable groups, including those with low education, the rural landless, urban residents, and those in provinces most affected by the crisis. Elevated psychological distress persists even after the economy returns to precrisis levels, suggesting that the deleterious effects of the crisis may persist longer on the psychological well-being of the Indonesian population than on standard measures of economic well-being. JEL codes: I10, O12.

The 1997 Asian currency crisis, one of the most disruptive global economic events in decades, caused severe economic damage across much of East and Southeast Asia. No country was more affected than Indonesia. After several decades of sustained economic growth with low inflation and a stable exchange rate, and three decades of President Suharto in power, Indonesia's society and economy were fractured by the 1997 crisis. The Indonesian rupiah (Rp) collapsed, falling from around Rp 2,500 to the U.S. dollar in late 1997 to Rp 15,000 to the U.S. dollar in mid-1998. GDP fell 12 percent in 1998 alone. Prices spiraled up with inflation reaching 80 percent in 1998 while food prices rose by 160 percent. Economic upheaval was accompanied by political

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turmoil. President Suharto resigned after street protests in early 1998, presaging historic changes in the systems of national and local government.

The vast majority of Indonesian households struggled with the immediate economic adversity at the onset of the crisis and with the tremendous uncertainty over their economic, social, and political futures. Living through the crisis took a toll on the psychological well-being of the Indonesian people. This study identifies subgroups of the population that paid the highest price in terms of their psychological health.

The impacts of the crisis on economic well-being were far from uniform. By several measures, the crisis was centered in urban areas, with disadvantaged urban households bearing the brunt of the crisis (Frankenberg, Thomas, and Beegle 1999; Friedman and Levinsohn 2002). Household declines in consumption and increase in poverty rates were much greater in urban areas than in rural areas. For example, it is estimated that between 1997 and 1998, household per capita expenditure declined by 34 percent among the urban households and 13 percent among the rural households (Frankenberg, Thomas, and Beegle 1999).[1] Among the self-employed rural men—mostly farmers—real hourly earnings declined by 11 percent. But among those working in the market sector, real wages declined by more than 50 percent in one year. Rural wage earners are primarily landless laborers and government workers (whose wages were set in nominal terms before the onset of the crisis). In the urban sector, people's real hourly earnings fell by 50 percent in both the market and self-employed sectors. While hourly earnings collapsed, employment rates remained remarkably stable despite some migration from urban to rural areas (Smith and others 2002). These facts reflect the rise in the relative price of food, particularly rice, benefiting food producers, and the concomitant collapse of real wages, taking its greatest toll on urban workers and the rural landless.

The economic impacts of the crisis varied dramatically across the 27 Indonesian provinces and numerous island groups, even within urban and rural areas (Levinsohn, Berry, and Friedman 2003). More urbanized provinces in Java, such as Jakarta and West Java, suffered the largest contractions, whereas the deleterious economic effects of the crisis were substantially more muted in provinces that produced exports and export-related services such as tourism (Bali, for example) and that produced oil, timber, and fishing (Sumatra). Variation across provinces and between rural and urban areas was a fundamental aspect of Indonesia's economic crisis, and these patterns are important for understanding how the economic crisis affected psychological well-being.

Previous studies have described the effects of the crisis on the economic well-being and physical health of Indonesians. Together, the studies indicate a dramatic but short-lived decline in the economic well-being of most

1. Poverty rates more than doubled in urban areas while rising about 50 percent in rural areas (Suryahadi, Sumarto, and Pritchett 2003).

Indonesians, suggesting an enormously resilient population that took great efforts to weather the storm.[2] But to the authors' knowledge, the impact of the financial crisis on psychological and mental health has not been explored. This article provides empirical evidence on how much the upheavals and associated stresses of the crisis affected the psychological health of Indonesians and whether any effects were long lasting. It contributes new evidence on the psychosocial costs of economic insecurity in developing economies.

Das and others (forthcoming) review the limited population-level evidence of psychosocial disability in developing countries. Typically, the highest levels of psychological pathologies are found in countries emerging from conflict, where levels of posttraumatic stress reactivity are high among the general population (de Jong and others 2001). People in regions that have suffered from natural disaster also suffer significantly more psychological distress as indicated by depression, somatization, and anxiety (Wang and others 2000; Frankenberg and others 2008). Given this evidence, it is plausible to hypothesize that severe economic dislocation could have adverse effects on physical and psychological health. Tangcharoensathien and others (2000) show that, after the onset of the financial crisis in Thailand, severe stress, suicidal ideation, and hopeless feelings were more prevalent among the unemployed than the employed. However, in the absence of any information prior to the crisis, it is not known how much of these gaps are related to the crisis or what the impact of the crisis was on the psychological well-being of the general population. In contrast, this article uses population-based data that follow the same individuals before and after the Indonesian financial crisis, which was both deeper and longer lasting than the Thai crisis.

I. Data

Data from the Indonesia Family Life Survey (IFLS) are used to track the indicators of psychological distress of the same individuals, who were assessed in up to three interviews before, during, and after the onset of the crisis. The IFLS is an ongoing multipurpose longitudinal survey of individuals, households, and communities. The first wave of IFLS was fielded in 1993 and collected information on more than 30,000 individuals living in 7,200 households. The original sample, which covered 321 communities in 13 provinces, is representative of the population residing in those provinces, about 83 percent of the national population. (Outlying provinces were excluded from the sample for cost reasons.)

The same respondents were reinterviewed in 1997 (IFLS2), a few months before the beginning of Indonesia's currency crisis, and again in 2000 (IFLS3).

2. Studies have described the impact of the crisis on poverty, consumption, wealth, labor supply, wages, earnings, schooling, physical health, and health care use (Frankenberg, Thomas, and Beegle 1999; Suryahadi, Sumarto, and Pritchett 2003; Frankenberg, Smith, and Thomas 2003; Thomas and others 2004; Strauss and others 2004).

About one-quarter of the respondents were reinterviewed in 1998 (IFLS2+) to measure the immediate impact of the crisis.[3] Attrition in both resurveys was low.[4]

In addition to collecting extensive information on the socioeconomic and demographic characteristics of respondents and their families, the IFLS assesses the psychological well-being of its respondents using a common interview-based survey instrument of the type outlined in Das and others (forthcoming). The IFLS psychological health questions, adapted from the General Health Questionnaire (GHQ), measure symptoms of two globally common categories of psychiatric disorder—depression and anxiety (Goldberg 1972). Appendix table A.1 presents the IFLS questions used in this study.

The questions focus on general feelings of sadness or anxiety as well as specific symptoms of distress.[5] They have been translated and back-translated to ensure accuracy and extensively field-tested to ensure comprehension by study subjects. Appendix table A.1 also includes a question on self-perceived general health status, a summary measure of health that encompasses physical and nonphysical domains of well-being. As a broader summary of health, general self-reported health status provides a useful comparison with psychological health indicators and emphasizes the potentially unique impacts of the crisis on psychological well-being.

The psychological health questions were assessed in full in the 1993 and 1998 surveys, and a subset of questions were assessed in 2000. (The subset is marked with an asterisk in appendix table A.1.) Unfortunately, the psychological health questions were not included in the 1997 wave.

The panel dimension of the data is exploited to contrast the general psychological health of each individual at two points, 1993 and 2000. (Results from the 25 percent subsample interviewed in 1998 are also used.) This seven-year period brackets the financial, political, and social crisis—which, given its magnitude, is likely a key factor underlying changes in psychological health over this period. An important advantage of using 1993 for comparison is that the estimates will not be contaminated by expectations of impending crisis.

3. The sample for the 1998 wave covered 25 percent of the IFLS enumeration areas, selected to span the country's socioeconomic and demographic diversity. The 1998 sample achieved above 80 percent efficiency relative to the entire IFLS sample.

4. Considerable attention has gone to minimizing attrition in the IFLS. In each resurvey, about 95 percent of original households have been recontacted, ameliorating concerns about selective attrition. Around 10–15 percent of respondents moved from the location in which they were interviewed in the previous wave, and concerted efforts were made to track these respondents to their new locations. Individuals who moved out of their original households have also been followed, adding around 1,000 households to the sample in 1997 and about 3,000 households in 2000.

5. The questions ask about a broad array of symptoms. Validation studies with the U.S.-based GHQ have concluded that if a clustering of symptoms within an individual is identified, then psychiatrists would likely be able to diagnose a psychological disorder. However it should be stressed that the IFLS data do not provide diagnostic information per se and hence the measures are interpreted as indicative of general psychological well-being.

II. Psychological Distress Before and after the Crisis

The empirical discussion begins with an overview of the psychological well-being indicators before and after the onset of the 1997 crisis. Table 1 reports the overall prevalence of psychological distress and poor general health in the population of men and women aged 20 and older at the time of each wave of the survey.[6] Columns 1–3 report estimates that draw on all respondents in each wave of the survey, weighted to be representative of the underlying population.[7] Columns 4–5 report estimates for panel respondents who were interviewed in both 1993 and 2000.

Psychological distress indicators measured in IFLS are dramatically higher after the onset of the 1997 crisis. For both men and women, the prevalence of distress almost doubles between 1993 and 1998 for each indicator except one, difficulty sleeping, which increases by about 50 percent. To illustrate, in 1993 about 12 percent of men report feeling sad in the previous four weeks; in 1998, nearly 30 percent of men reporting feeling sad. Women are slightly more likely to feel sad than men are in 1993 (16 percent) and much more likely to feel sad in 1998 (41 percent). There were even larger proportionate increases in the prevalence of anxiety which rose threefold to fourfold (albeit from a lower base).

When both markers are combined into an index identifying those who report feeling either sad or anxious, about one in six respondents reported such feelings in 1993. In 1998, one in three men and almost one in every two women reported these feelings. In 1993, one respondent in five had difficulty sleeping, and in 1998 this affected one in three adults. The prevalence of fatigue nearly doubled, and short temper more than double . For both men and women, the prevalence of reported somatic pain tripled from around 20 percent of respondents to 60 percent.

The increase in prevalence of psychological problems between 1993 and 1998 suggests a substantial rise in underlying psychological and emotional

6. For ease of exposition, the psychological distress and general health status questions have been dichotomized. Respondents who report a particular psychological distress indicator either "often" or "sometimes" over the past four weeks are combined. Likewise, respondents who report their general health status as "somewhat unhealthy" or "very unhealthy" are recorded as being in "poor general health." An alternative approach to measurement of health problems with survey data is to aggregate responses to several questions and to create a summary index (see Das and others 2007 for an example in psychological health). Results are presented across all three survey waves for three domains of psychological problems—feelings of sadness or anxiety and suffering from sleep difficulties. Those are the only questions repeated across each available wave of IFLS. Additional psychological morbidities, such as shortness of temper, were recorded in the 1993 and 1998 waves. The results are qualitatively unchanged if all six questions are combined into an index to compare 1993 and 1998. Similarly, results are not changed if the three common questions across the three surveys are combined into an index to compare 1993 with 1998 and 2000.

7. The sample sizes vary across the waves of the survey because of changes in the survey design. In 1993, a subsample of adults were interviewed in each household. In 1998 and 2000, all adults were individually interviewed. Recall that the 1998 survey was restricted to a 25 percent subsample of enumeration areas.

TABLE 1. Percentage of Male and Female Respondents Aged 20 and Older Reporting Psychological Distress and Poor Health

Health indicator	All respondents 1993 [1]	1998 [2]	2000 [3]	Panel respondents 1993 [4]	2000 [5]
Men					
Psychological health					
Sad	12.0	28.9***	27.5***	11.4	24.2***
Anxious	4.6	19.7***	18.4***	4.6	15.5***
Sad or anxious	13.8	32.7***	32.4***	13.5	28.6***
Insomnia	18.3	31.4***	27.4***	18.6	28.4***
Fatigue	25.5	47.2***	—	25.2	—
Short temper	11.8	28.7***	—	11.7	—
Somatic pain	17.4	57.9***	—	17.0	—
General health status					
Poor general health	10.7	11.4	11.6***	9.7	15.1***
Sample size	5,629	2,928	10,128	4,598	4,598
Women					
Psychological health					
Sad	15.8	41.0***	37.5***	17.1	37.6***
Anxious	7.3	25.9***	24.4***	8.5	23.6***
Sad or anxious	17.9	44.1***	42.7***	19.9	42.2***
Insomnia	22.3	35.0***	32.6***	23.6	35.9***
Fatigue	26.3	54.0***	—	28.5	—
Short temper	16.6	39.5***	—	18.1	—
Somatic pain	20.1	62.0***	—	21.6	—
General health status					
Poor general health	10.9	13.4***	14.3***	11.3	17.1***
Sample size	6,892	3,222	11,271	5,957	5,957

***Significantly different from 1993 prevalence at 1 percent level.

— Not available.

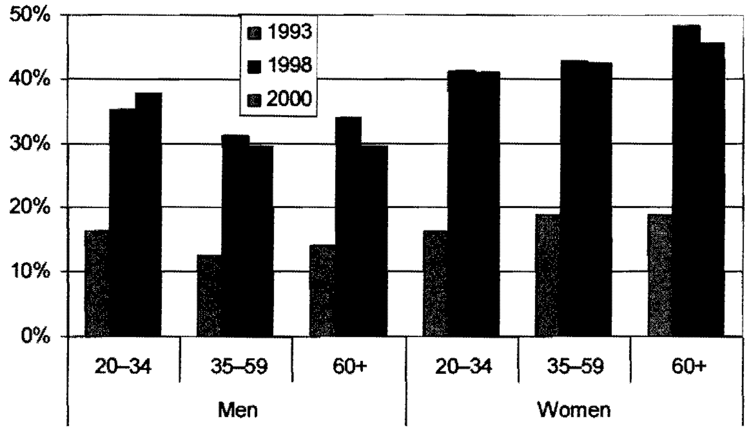
Note: Prevalence estimates are weighted to be population representative. Panel sample includes respondents who were at least 20-years-old in 1993 and who were interviewed in both 1993 and 2000.

Source: Authors' analysis based on data from IFLS waves 1 (1993), 2+ (1998), and 3 (2000).

distress, which persists well beyond the onset of the crisis (column 3 of table 1). There is only a small decline in the prevalence of psychological distress between 1998 and 2000 for both men and women. This persistence of distress is noteworthy since the Indonesian economy had begun to recover in 2000, and mean household consumption levels had already returned to the precrisis levels of 1997 (Ravallion and Lokshin 2007).

In contrast with the results for psychological well-being, self-reported general health status changed very little over the seven years. Around 11 percent of men and 11–14 percent of women report themselves in poor health. The relative stability of this general health measure suggests that the psychological distress

FIGURE 1. Variation in Psychological Distress (Prevalence of sadness or anxiety, percent)



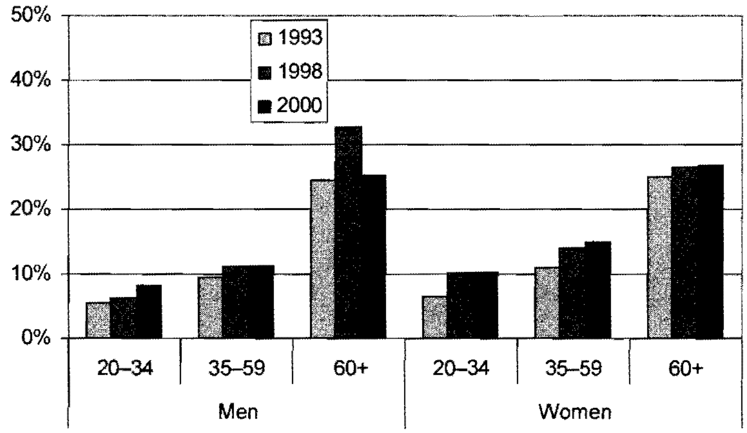
Source: Authors' analysis based on data from IFLS waves 1 (1993), 2+ (1998), and 3 (2000).

indicators identify a dimension of health and well-being different from general health and that the crisis affected these separate dimensions differently.

The analyses thus far provide evidence on the prevalence of health problems in the population at each point in time. It is also useful to focus on changes in health for the same group of individuals over time. The right panel of table 1 reports the prevalence of psychological problems and poor general health among respondents who were individually assessed in both 1993 and 2000 and were aged at least 20, in the 1993 baseline. More than 80 percent of the 1993 respondents were also interviewed in 2000. Comparing prevalence rates at baseline for the population (in the first column) with the rates for the panel sample (in the fourth column) provides insights into the representativeness of the panel sample. For both male and female respondents, the prevalence rates in 1993 are very similar across the columns. The rates in 2000 are not directly comparable in the cross-section and panel samples because the respondents are seven years older in the panel sample. For the psychological indicators, the rates are still close in value. Thus the panel sample replicates the large increase in psychological distress between 1993 and 2000, which was observed in the cross-section sample. In contrast, in the panel sample, a larger fraction of respondents report themselves in poor general health relative to the cross-section sample. This largely reflects the fact that poor health rises with age and provides further evidence that time effects are minor relative to age effects for general health.

To explore the variation in psychological distress over the life course, figure 1 provides a snapshot of the relation between reported sadness or anxiety and age for men and women. The prevalence of sadness or anxiety varies little with age, although prevalence rates are slightly higher among younger and older men in 1993 and among older women in all three years.

FIGURE 2. Variation in General Health (Percentage of respondents reporting poor general health)



Source: Authors' analysis based on data from IFLS waves 1 (1993), 2+ (1998), and 3 (2000).

These modest age differences were dwarfed by the dramatic overall increase in prevalence between 1993 and either 1998 or 2000. For every age group, the prevalence of sadness or anxiety roughly doubles between 1993 and 1998, indicating a profound increase in psychological distress for adults across the entire age distribution. The estimates for 1998 and 2000 are almost identical, indicating the persistence of crisis effects on psychological health across all ages, even as the Indonesian economy began to recover.

The results in figure 1 stand in contrast to those in figure 2, which presents the same snapshot for general health status. A clearer age gradient is observed for general health—as respondents age they are more likely to report poor general health. This result, consistent with the broader empirical literature, highlights the different domains of well-being identified by general health status and by the psychological health questions. Another difference concerns the relative lack of change in the general health status measure over 1993–2000. There does appear to be an increase in reported poor general health in 1998, although one not close in magnitude to that for the psychological distress measures. The general and sustained rise in the population's psychological distress is not mirrored in changes in the population's perceived general health.

III. INDIVIDUAL TRANSITIONS IN PSYCHOLOGICAL DISTRESS AND THE CRISIS

Taking advantage of the longitudinal nature of IFLS, the rates at which individuals transited into and out of psychological distress are examined in table 2. For each measure of psychological distress, the percentage of people in each of four possible categories is listed: those who report psychological distress in

TABLE 2. Transitions in Psychological Well-Being of Panel Respondents between 1993 and 2000 (percentage of respondents)

Transition status	Instance of measure 1993	Instance of measure 2000	Psychological distress measure: Sadness	Anxiety	Sad/anxious	Difficulty sleeping	Poor general health status
All panel respondents ($N = 10{,}524$)							
No transition	No	No	60.4	75.3	55.2	56.3	77.0
	Yes	Yes	6.8	2.2	8.6	10.3	3.7
Transition	No	Yes	25.0	17.9	27.7	22.3	12.5
	Yes	No	7.9	4.6	8.5	11.1	6.9
Male panel respondents ($N = 4{,}586$)							
No transition	No	No	69.0	81.1	63.9	61.7	78.7
	Yes	Yes	4.6	1.2	6.0	8.6	3.5
Transition	No	Yes	19.6	14.4	22.5	19.7	11.6
	Yes	No	6.9	3.4	7.5	10.0	6.2
Female panel respondents ($N = 5{,}938$)							
No transition	No	No	53.8	70.8	48.5	52.2	75.6
	Yes	Yes	8.4	2.9	10.6	11.6	3.9
Transition	No	Yes	29.2	20.7	31.7	24.2	13.2
	Yes	No	8.6	5.6	9.3	11.9	7.3

Source: Authors' analysis based on data from IFLS waves 1 (1993) and 3 (2000).

both survey years, those who report no distress in either year, those who transited into distress, and those who transited out of distress.

A substantial fraction of men and women transited into psychological distress. But more than half the population—and in some cases three-quarters—experience no transition, with the vast majority of these people never reporting psychological distress. A small fraction of the population moves out of feeling distressed. For example, of the 12 percent of men who feel sad in 1993, fewer than half still feel sad in 2000. Of the 17 percent of women who feel sad in 1993, half still feel sad in 2000. About 20 percent of men and 30 percent of women move from not feeling sad in 1993 to feeling sad in 2000. The patterns are broadly similar for the other indicators of psychological well-being.[8] Relative to men, women are more likely to transit into and out of distress.

In general, the fraction of the population that transitinto psychological distress is between two and five times greater than the fraction that transited out of distress. In contrast with the psychological indicators, relatively few people transited into poor health while comparatively more transited out. Of those in

8. The patterns are also similar for transitions in the 25 percent subsample over 1993–1998. The relative persistence of distress in 1998–2000 in contrast to either 1993–2000 or 1993–1998 is more pronounced, indicating greater stability in the psychological measures over this shorter period after the onset of the crisis.

poor health in 1993, two-thirds reported they were not in poor health in 2000, again underscoring the difference from psychological distress.

IV. Psychological Distress and Individual Characteristics

The combination of measures of psychological well-being and demographic and socioeconomic characteristics of individuals collected in the IFLS provides opportunities to identify population subgroups at elevated risk of suffering from psychological distress during economic and political uncertainty. Multivariate regression is used to identify susceptible groups before and after the onset of the crisis as well as groups in psychological distress in both waves of the survey and those likely to transit into or out of psychological distress.

Individual characteristics include gender, age, and education, all measured in 1993. Of course, not all observed changes between 1993 and 2000 may be due to the 1997 crisis. However, the fact that the impact of the 1997 crisis differed dramatically across the Indonesian archipelago is exploited to relate the extent of changes in psychological health to markers of the magnitude of the crisis for the area in which each respondent was living in 1993. Specifically, controls include province of residence, whether the respondent was living in an urban or rural area, and among rural dwellers, living in a household that owned a farm business or not. The latter included landless laborers, who relied on wage work as well as government workers such as teachers, public health workers, and local administrators, most of whose wages were set in nominal terms before the onset of the crisis and severe inflation. Because food prices rose faster than other prices, farmers were partially protected from the deleterious impact of the crisis, whereas rural wage earners and urban dwellers saw a significant reduction in their real hourly earnings. Under the plausible assumption that the crisis was unanticipated in 1993, the impact of location of residence in 1993 on psychological distress can be interpreted as capturing impacts unaffected by behavioral responses to the crisis.

Regression results are reported in table 3 for three indicators of psychological distress—sadness, anxiety, and difficulty in sleeping—and for poor general health using the sample of respondents who were at least aged 20 in 1993 and interviewed in both 1993 and 2000. Columns 1 and 2 in each block examine the correlates associated with psychological distress or poor health in 1993 and 2000, respectively. Odds ratios from logistic regressions are reported. The dependent variable is unity, if the respondent reports being in poor psychological or physical health. Columns 3–5 in each block report the results of a transition model across 1993–2000, estimated by multinomial logistic regression. Risk ratios, relative to not being in poor psychological or general health in both 1993 and 2000, are reported. Asymptotic *t*-statistics are based on estimated standard errors that are robust to heteroskedasticity of arbitrary form and take into account correlations of unobserved factors common within households. The table presents results for gender, age, education, and

TABLE 3. Demographic and Socioeconomic Influences of Psychological Distress and General Health, Cross-Sectional and Panel Estimates

Select demographic and socioeconomic measures in 1993	Sadness					Anxiety					Sleep difficulties					Poor general health				
	Cross-section association		Transitions across the two periods			Cross-section association		Transitions across the two periods			Cross-section association		Transitions across the two periods			Cross-section association		Transitions across the two periods		
	1993 [1]	2000 [2]	Enter [3]	Exit [4]	Persist [5]	1993 [1]	2000 [2]	Enter [3]	Exit [4]	Persist [5]	1993 [1]	2000 [2]	Enter [3]	Exit [4]	Persist [5]	1993 [1]	2000 [2]	Enter [3]	Exit [4]	Persist [5]
Male	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Female	1.57	1.89	1.91	1.59	2.38	1.95	1.67	1.63	1.89	2.85	1.36	1.45	1.48	1.41	1.67	1.21	1.22	1.25	1.28	1.21
	(8.04)	(14.95)	(13.71)	(6.21)	(10.18)	(7.88)	(10.11)	(9.23)	(6.30)	(6.69)	(6.41)	(8.76)	(7.94)	(5.23)	(7.41)	(2.94)	(3.57)	(3.66)	(3.07)	(1.82)
20–34 years	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
35–59 years	1.08	1.12	1.11	1.07	1.19	0.86	1.00	1.00	0.84	0.90	1.21	1.35	1.31	1.13	1.56	1.78	1.77	1.65	1.59	3.00
	(1.25)	(2.29)	(1.95)	(0.74)	(1.90)	(1.75)	(0.03)	(0.03)	(1.65)	(0.74)	(3.41)	(6.09)	(4.72)	(1.67)	(5.46)	(6.97)	(8.44)	(6.73)	(4.86)	(7.03)
60 years and older	1.09	1.38	1.41	1.12	1.37	0.79	1.23	1.23	0.75	1.02	1.37	1.88	1.79	1.20	2.30	3.96	3.98	3.76	3.66	9.66
	(0.91)	(4.22)	(4.04)	(0.83)	(2.23)	(1.55)	(2.34)	(2.25)	(1.54)	(0.06)	(3.71)	(8.50)	(6.83)	(1.53)	(7.19)	(13.18)	(15.26)	(12.82)	(10.05)	(13.01)
Less than primary	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Primary graduate	0.89	0.87	0.86	0.86	0.83	1.06	0.90	0.88	1.02	1.08	1.01	0.95	0.94	0.99	0.98	0.79	0.90	0.95	0.84	0.70
	(1.74)	(2.71)	(2.63)	(1.66)	(1.93)	(0.64)	(1.83)	(2.07)	(0.14)	(0.48)	(0.08)	(0.94)	(1.10)	(0.16)	(0.24)	(2.98)	(1.56)	(0.70)	(1.93)	(2.79)

(Continued)

TABLE 3. Continued

Select demographic and socioeconomic measures in 1993	Sadness					Anxiety					Sleep difficulties					Poor general health				
	Cross-section association		Transitions across the two periods			Cross-section association		Transitions across the two periods			Cross-section association		Transitions across the two periods			Cross-section association		Transitions across the two periods		
	1993 [1]	2000 [2]	Enter [3]	Exit [4]	Persist [5]	1993 [1]	2000 [2]	Enter [3]	Exit [4]	Persist [5]	1993 [1]	2000 [2]	Enter [3]	Exit [4]	Persist [5]	1993 [1]	2000 [2]	Enter [3]	Exit [4]	Persist [5]
Secondary graduate	0.82	0.82	0.80	0.77	0.76	1.49	0.80	0.79	1.55	1.17	0.79	0.72	0.70	0.78	0.65	0.53	0.61	0.71	0.66	0.23
	(1.99)	(2.72)	(2.73)	(2.04)	(1.94)	(3.35)	(2.58)	(2.57)	(3.11)	(0.73)	(2.76)	(4.56)	(4.15)	(2.35)	(3.48)	(5.02)	(4.88)	(3.21)	(2.98)	(5.26)
Rural landed	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Rural landless	1.04	1.19	1.19	1.02	1.20	0.97	1.18	1.16	0.86	1.32	1.08	1.05	1.09	1.17	1.05	1.06	1.13	1.12	1.01	1.22
	(0.41)	(2.60)	(2.36)	(0.12)	(1.45)	(0.25)	(2.16)	(1.84)	(0.87)	(1.19)	(1.04)	(0.68)	(1.09)	(1.59)	(0.42)	(0.54)	(1.44)	(1.18)	(0.09)	(1.23)
Urban	1.34	1.14	1.13	1.36	1.43	1.48	1.12	1.09	1.44	1.66	1.17	1.08	1.09	1.20	1.19	1.03	1.03	1.02	1.03	1.06
	(4.02)	(2.31)	(1.92)	(3.31)	(3.49)	(3.86)	(1.75)	(1.37)	(3.12)	(2.79)	(2.47)	(1.48)	(1.34)	(2.28)	(2.04)	(0.37)	(0.39)	(0.27)	(0.26)	(0.42)

— Excluded category.

Note: Sample includes all male and female panel respondents' aged 20 and older in 1993. Columns 1 and 2 report odds ratios for logistic regression results. The dependent variable is one if respondent reports condition, and zero otherwise. Columns 3–5 report relative risk ratios for multinomial logistic regression estimates. Entrants report condition in 2000 but not in 1993, exits report condition in 1993 but not in 2000, and persisters report condition in both periods; excluded outcome is no report of condition in both periods. Regressions include province of residence controls. Numbers in parentheses are asymptotic t-statistics robust to heteroskedasticity and that take into account within-household correlations. Sample size is 10,555 respondents.

Source: Authors' analysis based on data from IFLS waves 1 (1993) and 3 (2000).

rural–urban location. (Differences by province of residence are summarized in the next section.)

Gender

Gender plays an important role in both the prevalence of psychological distress and in transitions across states of distress over time. In both 1993 and 2000, women are significantly more likely than men to report feeling sad and anxious, suffering from sleep difficulties, and being in poor general health, after controlling for other characteristics. Relative to men, the omitted category, there is significantly more churning among women, who are much more likely to transit between states of psychological or general health (in either direction). These transition rates are not large enough to offset the higher risks of being in poor health, so women are also more likely than men to be in poor health in both 1993 and 2000.

For example, women are 57 percent more likely than men to be sad in 1993, and 89 percent more likely in 2000. This is reflected in the transition rates, reported in columns 3 and 4, which indicate that women are 91 percent more likely than men to become sad but 59 percent more likely to stop being sad. Hence, the larger gender gap in 2000. In contrast, the gender gap in risk of feeling anxious declines between 1993 and 2000 from 95 percent to 67 percent. Women are 89 percent more likely to stop being anxious and 63 percent more likely to become anxious. Clearly there is more mobility into and out of psychological distress among women than men, though women remain much more likely than men to be sad or anxious in both waves (column 5).

Age

Age is specified as a categorical variable with three groups, ages 20–34, 35–59, and 60 and older (the youngest age group is the omitted category in table 3). The influence of age varies with the psychological distress indicator. For example, feelings of sadness appear unrelated to age in 1993, conditional on other observed characteristics. But by 2000, a clear age gradient emerges, with respondents in the oldest category 38 percent more likely to report sadness than those in the youngest category. This is largely because the likelihood of transitioning from not being sad to being sad is significantly higher for those ages 60 and older, as well as for those in the middle age range. A similar pattern holds for feelings of anxiety—no clear age gradient in 1993 yields to a significantly positive gradient in 2000, when the oldest group is 23 percent more likely to report anxious feelings (and more likely to transition into feelings of anxiety).

These general results echo the information in figure 1, where there is little evidence that age is associated with the risk of being sad or anxious before the onset of the crisis, but that an age gradient appears, at least for women, by 2000. Both sleep difficulties and poor general health status tend to increase with age, and older adults are more likely to transit into suffering from sleep difficulties or poor health, as expected.

Education

Education, an indicator of socioeconomic status, is also measured as a categorical variable. Respondents are grouped into three categories: incomplete primary school (including those with no formal education), primary school graduates (including those with some years of secondary schooling), and secondary graduates or higher. Education appears to have a slight negative relation to feelings of sadness in 1993, with secondary graduates 18 percent less likely to report sadness relative to the omitted category of those who have not finished primary schooling. This gradient is largely unchanged in 2000, though it becomes more precise with even primary graduates 13 percent less likely to report sadness than those without primary schooling.

Feelings of anxiety follow a different pattern. In 1993, there is a significant positive gradient between education and anxiety, with those in the highest education category 49 percent more likely to report anxiety than those without primary schooling. In 2000, a significant negative gradient is observed, with secondary school graduates being 20 percent less likely to report anxiety than those without primary schooling. Over the crisis period, those who have not completed primary school are significantly more likely to transit into anxiety, while secondary school graduates or higher are significantly more likely to transit out of anxiety. Thus the crisis period witnessed a disproportionate increase in anxiety among the less educated. Studies cited earlier suggest that the most vulnerable groups during the 1997 crisis were the poorer and less educated groups. This is apparent in the evidence for anxiety, which likely reflects concerns about economic insecurity.

The pattern for sleep difficulties and poor general health are similar to each other. The better educated are less likely to have difficulty in sleeping both before and after the onset of the crisis, and they are less likely to experience a transition into or out of sleeping difficulties. The better educated tend to report better general health in 1993 and 2000, and they are less likely to experience a transition into or out of poor general health.

Area of Residence

The last three rows of table 3 investigate the relation between psychological health and area of residence, as well as landed status, all measured before the onset of the crisis. Rural dwellers who owned land in 1993 are the reference category. They are, on average, the group most protected from the deleterious impact of the crisis because they tend to be food producers and the relative price of food rose dramatically during the crisis. The landless are substantially more vulnerable to the negative impact of the crisis since they relied on (mostly nonfarm) wage labor at a time when wages collapsed.

This difference in vulnerability is apparent in the increasing relative likelihood of sadness and anxiety for the rural landless. Before the crisis, the rural landless were neither more nor less likely to report feelings of sadness or anxiety than the rural landed. After the crisis, the landless were significantly more likely to be sad (19 percent more likely) or anxious (18 percent more likely) and significantly more likely to transition into sadness or anxiety. The buffer of land assets most likely helped to protect landed households from severe income shocks and greater psychological distress.[9]

Urban residents have a consistently higher likelihood of reporting sadness, anxiety, or sleep difficulties than do landed rural residents before the crisis, and this remains true for feelings of sadness after the crisis as well. (For both anxiety and sleep difficulties in 2000 urban residents still have a higher likelihood of distress, although the difference does not meet standard levels of significance.) Urban residents are also more likely to transition across states of psychological distress and nondistress and to remain in a state of psychological distress, than their rural counterparts. In contrast with the result for psychological health, urban location does not influence the self-reporting of poor general health (nor does rural landless status).

Urban status clearly influences psychological distress indicators and is an important conditioning variable. But the relative importance of urban residence in determining psychological health does not change over the crisis period, at least in relation to landed rural households. If urban areas are more adversely affected by the crisis than rural areas, this impact does not translate into higher psychological distress conditional on other observed characteristics. This is in part due to the separation of the rural population into landed and landless in the regression analyses. The results in table 3 suggest that the rural landless bore the heaviest psychological burden of the crisis and that overall psychological distress was lower in rural areas in 1993. It is also possible that a high degree of heterogeneity in crisis impacts across Indonesia's cities may mask any crisis effects in the regression framework.[10]

9. An alternative explanation for this finding may arise if landless rural respondents are more likely than landed respondents to migrate to urban areas, where psychological distress is higher in general. When the analysis is conditioned on individuals who have never migrated, the results are essentially identical.

10. The data afford more generous regression specifications that can include a broader set of socioeconomic characteristics in the models, such as marital status and religion of respondents, living arrangements, wealth, household resources, and employment status. But it is not clear how to interpret estimates from these models, since these characteristics are potentially correlated with other unobserved factors that also influence psychological well-being. While marital status, work status, and wealth accumulation have all been shown to be associated with psychological well-being, the direction of causality has not been established (Ettner, Frank, and Kessler 1997; Dooley, Prause, and Ham-Rowbottom 2000). The preference here is to report a parsimonious specification under the assumption that the characteristics included—educational attainment, location of residence, and ownership of land measured in 1993—are largely fixed before psychosocial well-being in 1993 is determined.

V. Regional Variations in Psychological Health Transitions

Provincial location is another factor that mediated the severity of exposure to the crisis. Previously cited studies note large geographic variations in crisis indicators, such as inflation or declines in real household resources. For example, provinces on the island of Java were among the most affected, while Bali, with its reliance on tourism, and certain provinces in Sumatra and in the east of Indonesia, with their resource-intensive export industries, were less affected.

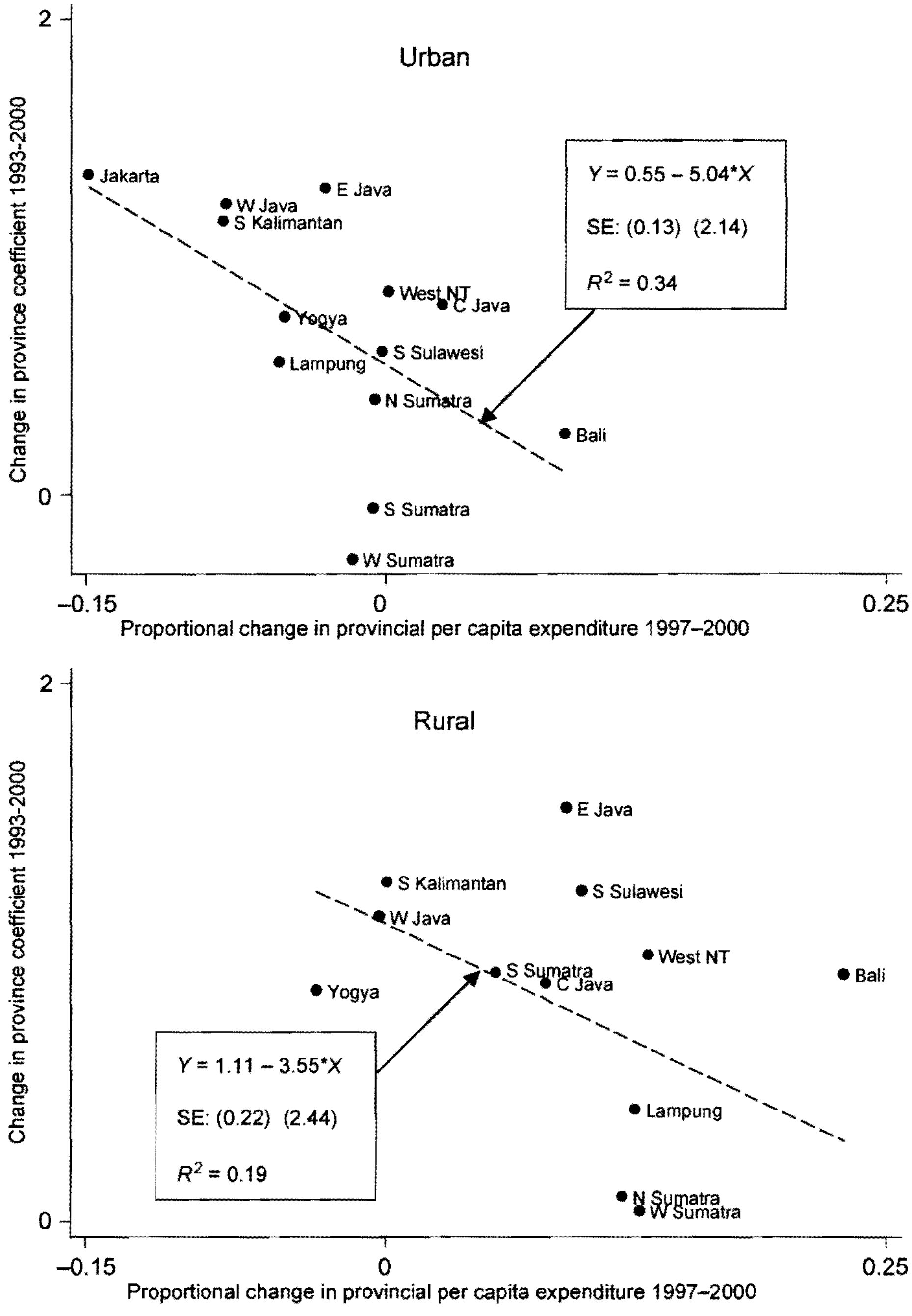
The spatial pattern in the changes in psychological distress indicators largely reflects these geographic differences in crisis severity. The models reported in table 3 also included controls for province of residence in 1993 (results not shown). There are pronounced differences in the psychological distress indicators by province of residence. This is the case even after controlling for gender, age, education, and urban–rural location. To provide a context for interpreting the differences in estimated risk ratios for provinces, those differences are related to a common measure of the impact of the financial crisis—the proportional change in mean real per capita household expenditure for each provincial urban or rural area.

These results are graphically summarized in figure 3, which relates the change in the estimated province effect between 1993 and 2000 to the proportional change in regional mean real per capita household expenditure between 1997 and 2000. The change in the estimated province effect can be interpreted as the change in mean psychological distress in that province, conditional on population observables. The province effects are based on logistic regressions of the form reported in table 3, except that here they are estimated separately for urban and rural households. The measure of psychological distress is the combined measure of sadness and anxiety.[11]

The results show a similar pattern for rural and urban areas, though the relation is more pronounced in urban areas. Moving from left to right on either x-axis in figure 3 indicates greater growth in mean real per capita household expenditure and thus a milder impact of the crisis. Almost all urban areas experience a decline in mean real per capita household expenditure between 1997 and 2000. For example, expenditures decline by 15 percent in Jakarta, and increase only in urban Central Java and urban Bali. Mean real per capita household expenditure growth is generally greater in rural areas; only three provinces—Yogyakarta, West Java, and South Kalimantan—experience a decline in rural areas. Moving from bottom to top on either y-axis indicates an increase in the conditional provincial mean of sadness or anxiety from 1993 to 2000.

11. The conclusions are unchanged when looking at the prevalence of sadness or anxiety separately or at pooled urban and rural households. Similar analysis that investigates the association of changes in general health status with provincial measures of crisis impacts does not find any relationship, consistent with results reported above.

FIGURE 3. Change in the Province Mean Conditional Prevalence of Sadness or Anxiety



Source: Authors' analysis based on data from IFLS waves 1 (1993), 2+ (1998), and 3 (2000).

Even at this aggregate level, there is a clear positive relation between the severity of crisis and changes in the relative prevalence of psychological distress. The urban areas that experience the largest declines in mean income also exhibit the largest rise in sadness or anxiety. People living in Jakarta were hit hardest by the crisis in both economic and psychological terms. Bali fares relatively well over the crisis period and also posts one of the smallest rises in overall prevalence. This general relation is also apparent in the fitted regression line, which has a significant negative slope (despite the small sample size of 13) and an R^2 statistic of 0.34. While the crisis affects the psychological health of many urban Indonesians, those who live in cities most affected by the crisis experience the greatest increase in distress.[12]

Rural residents in areas most affected by the crisis also experience the greatest increases in psychological distress, though the relation is not as pronounced as that for urban residents, even after excluding the outlier of rural Bali (where relative distress increases considerably even though mean household expenditures increased by almost 25 percent). The rural areas that experience negative or zero mean income growth over 1997–2000 witness some of the largest increase in psychological distress. In contrast North and West Sumatra, which experience healthy growth in mean real per capita household expenditure, also experience the smallest increases in distress. The overall relation between growth in mean real per capita household expenditure and psychological distress is slightly weaker in rural areas than in urban areas. Indeed, the slope of the rural fitted regression line is negative but not significant at conventional levels. This weaker relation may be due in part to the fact that population in rural areas overall did not fare as poorly over the crisis period for the reasons discussed earlier.

VI. CONCLUSIONS

The 1997 financial crisis was the most disruptive socioeconomic event to confront Indonesians for at least three decades. The effects of the crisis were wide-ranging. Although some households prospered from the new opportunities afforded by rapid price changes and shifts in the structure of the economy and the political landscape, overall poverty increased and mean income fell. This study is the first to look at the impacts of the crisis on psychological health, using a high-quality longitudinal socioeconomic survey. It finds that the severe economic dislocation and political uncertainty engendered by the crisis increased psychological distress in the overall population.

12. An alternative to the analysis described here is to regress individual changes in psychological health directly on the measured changes in mean provincial income. This approach finds similar results—respondents in provinces with larger contractions in mean income are significantly less likely to transition out of sadness or anxiety, given an initial state of psychological distress, than their counterparts in regions less affected by the crisis.

There was substantial increase in distress indicators at all ages and among men and women. The imprint of the crisis on psychological well-being can be seen in the greater prevalence of poor psychological health for groups most adversely affected: the less educated, the rural landless, and residents in hardest hit cities. To avoid the complications of co-determinacy between psychological health and economic outcomes, such as labor force participation or income, no attempt was made to estimate and interpret correlations with characteristics that might respond to the crisis. Instead, the analysis focused on characteristics such as age, gender, education, and location before the crisis.

Also important is the persistence of psychological distress from the immediate postcrisis period in 1998 to the recovery period in 2000. By 2000 mean national per capita household consumption had already recovered to 1997 levels and the overall economy had returned to precrisis growth rates. However, psychological distress remained elevated, suggesting that economic dislocation has longer lasting effects on psychological well-being than on static measures of economic status. The evidence here suggests that psychological well-being does not necessarily go hand in hand with standard measures of welfare based on economic status. It also suggests that examining the impact on psychological health provides a more complete picture of the consequences of the economic crisis for individuals and their households and communities.

Appendix

TABLE A.1. IFLS Psychological and General Physical Health Status Questions

Question	Possible responses			
Indicators of psychological distress				
In the last four weeks, have you...				
a. Experienced sadness?	1. Often	2. Sometimes	3. Never	
b. Experienced anxiety or fear?	1. Often	2. Sometimes	3. Never	
c. Had a hard time sleeping?	1. Often	2. Sometimes	3. Never	
d. Felt fatigue or exhaustion?*	1. Often	2. Sometimes	3. Never	
e. Been short-tempered or hypersensitive?*	1. Often	2. Sometimes	3. Never	
f. Felt bodily pains?*	1. Often	2. Sometimes	3. Never	
Indicator of general health status				
In general, how is your health?	1. Very healthy	2. Somewhat healthy	3. Somewhat unhealthy	4. Very unhealthy

*Not asked in 2000

Source: IFLS waves 1 (1993), 2+ (1998), and 3 (2000).

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Infrastructure and Public Utilities Privatization in Developing Countries

Emmanuelle Auriol and Pierre M. Picard

Should governments in developing countries promote private ownership and deregulated prices in noncompetitive sectors? Or should they run publicly owned firms and regulate prices at the expense of rents to insiders? A theoretical model is used to answer these normative questions. The analysis focuses on the tradeoff between fiscal benefits and consumer surplus during privatization of noncompetitive sectors. Privatization transfers control rights to private interests and eliminates public subsidies, yielding benefits to taxpayers at the cost of increased prices for consumers. In developing countries, where budget constraints are tight, privatization and price liberalization may be optimal for low profitability industries but suboptimal for more profitable industries. And once a market has room for more than one firm, governments may prefer to regulate the industry. Without a credible regulatory agency, regulation is achieved through public ownership. JEL codes: D82, H54, L33, L43, L51, O10

Over the last 25 years developing countries have drastically reduced their share of state ownership.[1] In most cases governments have privatized public assets because of critical budgetary conditions. During the 1980s debt crisis international financial institutions such as the World Bank and the International Monetary Fund made privatization a condition for economic assistance. Governments have continued to use privatization proceeds to relax their budget constraints.[2] The fiscal benefits of privatization are not limited to the

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1. Megginson and Netter (2001) estimate that output from state-owned enterprises in developing countries shrank from 16 percent of GDP in 1980 to 8 percent in 1996.

2. Using a panel of 18 developing countries, Davis and others (2000) show that privatization proceeds have been used to reduce domestic financing on a roughly one-for-one basis.

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divesture proceeds of public firms, which have been estimated at around $50 billion a year in countries outside the Organisation for Economic Co-operation and Development (OECD) (Mahboobi 2000; Gibbon 1998, 2000). The benefits also encompass the possible termination of recurrent, inefficient subsidies to state-owned enterprises. This article studies the impact of macroeconomic fiscal objectives on the decision to privatize infrastructure and public utilities in developing countries.

Privatization involves well-known economic costs for industries with strong economies of scale. Infrastructure and utility owners benefit from market power. By giving up direct control of a firm's operations, governments lose control over prices—to the disadvantage of consumers. In theory this could be avoided by auctioning off markets on the basis of the lowest product or service price (see Estache, Foster, and Wodon 2002). However, in a survey of 600 concession contracts from around the world, Guasch (2004) shows that in practice contracts are tendered for the highest transfer or annual fee. Because fee payments rise with the profitability of the privatized firms, many governments choose policies that increase a firm's profitability, such as exclusivity periods and price liberalization.[3] Prices are sometimes increased before privatization, to reduce a state enterprise's financing gap and to attract buyers. This was the case in Kenya, Senegal, and Zimbabwe, where governments increased electricity prices 10 percent at the time of the agreement with Vivendi Universal (AfDB and OECD 2003). An unaccounted for part of price increases stems from the termination of illegal connections (Birdsall and Nellis 2002; Estache, Foster, and Wodon 2002; AfDB and OECD 2003).

This article studies the privatization decision as the result of government's cost–benefit analysis. The social benefits from the cash flows generated by a public firm's divesture and from the termination of subsidies to an unprofitable public firm are balanced against the loss in consumer surplus induced by higher prices in the privatized industries and forgone revenues from profitable public firms.

The model used here is static and therefore does not address the transition problem between public and private ownership. It compares social welfare under private and public ownership in industries and market segments with large investment costs. To obtain clear-cut results, privatization in this model corresponds to a situation where prices are free. Privatization is thus close but not equivalent to laissez-faire because entry remains regulated (through license and entry fees). By contrast, public ownership corresponds to a situation where both entry and prices are regulated. This approach is robust from a theoretical point of view. Indeed if, as is shown, privatization with free price setting dominates state ownership with benevolent regulation, privatization also dominates

3. Wallsten (2001) studies the impact of the exclusivity period on the privatization price of 20 telecommunication firms in 15 developing countries. Two-thirds of those countries chose to allocate exclusivity periods for an average of 7.4 years. Exclusivity more than doubled the price private investors paid for the firm—but at the cost of high prices and lower network growth for consumers.

in situations where prices are liberalized to a lesser extent and regulation is not benevolent.[4]

The dominance of privatization over benevolent regulation is not obvious. Indeed, the deadweight loss created by monopoly pricing is the rationale for setting up public ownership in the first place. Under perfect information governments are able to mimic the outcomes of private monopolies so that privatization is never optimal. However, under asymmetric information between governments and firms, privatization may dominate public ownership because the presence of information rents raises the social costs of subsidies.

In this article a main factor in privatization decisions is the opportunity cost of public funds, which captures the tightness of the government budget constraint. The privatization decision is shown to be a monotonic function of this opportunity cost of public funds when the profitability of a market is low, as it is with infrastructure such as roads or utility services to poor people. For low opportunity costs (as in the case of wealthy governments) public ownership dominates privatization, and for high opportunity costs (as in the case of financially strapped governments) the opposite holds. Consider the case where the government cannot finance an infrastructure project (for example, a water distribution network). Privatization is an appealing alternative because it is better to have a privately owned and operated infrastructure, even with monopoly distortion, than no infrastructure at all. By continuity the result still holds when the government is able to finance the infrastructure.

This monotonic relationship between privatization and the budget constraint breaks down, however, when natural monopolies are sufficiently profitable and when governments are not able to recoup large enough franchise fees or divestiture proceeds. Such situations often stem from developing countries' difficulties attracting investors when auctioning off profitable state enterprises.[5] With underpriced public assets the privatization decision is shown to be optimal only for intermediate values of the opportunity cost of public funds.

The intuition is as follows: as before, for low opportunity costs of public funds, government bailouts of firms are cheap, and it is optimal to keep firms public, set prices close to marginal costs, and subsidize the firms so that they break even. For intermediate opportunity costs of public funds, bailout becomes costly, and governments prefer to privatize the public firms, cash the proceeds, and let private entrepreneurs manage firms. But for high opportunity costs of public funds, the privatization decisions differ because governments find it valuable to "hold up" on industries' rents. Governments do not privatize

4. Developing countries have generally failed to establish credible regulatory bodies because of governments' inability to commit. For instance, the concessions granted to private operators following the divestiture of Latin American public firms were renegotiated after an average of only 2.1 years (Laffont 2001; see also Guasch 2004).

5. According to Trujillo, Quinet, and Estache (2002), rarely do more than two bidders participate in auctions for major concession contracts in developing countries, so state enterprises are often sold at a discount to avoid the embarrassment of unsuccessful sales (see Birdsall and Nellis 2002).

profitable segments, choosing instead to operate them and to set private monopoly prices to reap maximum revenue.

This nonmonotonic result has important policy implications. While divestiture of profitable public firms may be optimal in developed countries, it is not necessarily so in developing countries, where budget constraints are tight and market institutions are weak. More specifically, the model suggests that public utilities in developing countries should focus on market segments where incomes and willingness to pay are high and should set prices high so that the public firms' profit can be used to subsidize new connections or other public goods.

Finally, when a firm's profitability rises substantially, the market has room for more than one firm. For large, profitable industries regulation of duopoly is shown to always be better than privatization with price liberalization. Market liberalization thus corresponds to the divestiture of a historical monopoly and the introduction of new entrants according to a regulatory scheme. It does not correspond to laissez-faire. This is a major concern in developing countries, which usually lack the human resources and institutions to implement effective regulation.

The article is organized as follows. Section I describes the model's relation to the literature. Section II presents the model and the main assumptions. Section III compares the performance of private and regulated monopolies, while section IV briefly discusses the duopoly case. Section V derives the optimal industrial policy. Section VI summarizes the results and offers some concluding remarks. For conciseness, all proofs are included in a supplementary appendix available at http://wber.oxfordjournals.org.

I. The Model's Relationship to the Literature

It is well known that public ownership generates inefficiencies because it encourages governments to bail out or subsidize money-losing firms. Kornai (1980) first termed such inefficiencies the "soft budget constraint" problem. This term explains many of the inefficiencies that occur in socialist economies, such as shortages or low price responsiveness.[6] Since less efficient firms can rely on government funding, they lack the financial discipline for efficient management. For instance, under incomplete contracts soft budget constraints affect the level of investment by public managers. By hardening a firm's budget constraint, privatization helps restore investment incentives. The transfer from public to private ownership is therefore often advocated as a remedy for the poor economic performance of public enterprises (see, for instance, Dewatripont and Maskin 1995; Schmidt 1996; and Maskin 1999).

Another concern about public ownership is government lack of economic orientation. For instance, in Kornai and Weibull (1983), Shleifer and Vishny

6. Interesting surveys are available in Kornai (2000) and Kornai, Maskin, and Roland (2002).

(1997), and Debande and Friebel (2003) governments demonstrate "paternalistic" or political behaviors as they seek to protect employment; in Shapiro and Willig (1990) governments are malevolent. The main conclusion of these two strands of literature is that privatization improves firms' internal efficiency.

Megginson and Netter's (2001) review of 65 firm-level empirical studies confirms that private firms are generally more productive and more profitable than their public counterparts. However, in industries with increasing returns to scale the efficiency gains are not automatically passed along to consumers.[7] Changing the ownership structure does not solve the problem of lack of competitive pressure (see Nellis 1999).

This article belongs to the traditional literature on regulation with adverse selection (see Laffont and Tirole 1993). It ignores the moral hazard issue discussed at length in the studies on the soft budget constraint and focuses instead on allocative efficiency and macro-fiscal balancing issues. A utilitarian government maximizes a weighted sum of consumer surplus and transfers from and to firms. The weight on transfers is the opportunity cost of public funds. As is standard in the regulation literature, the government is assumed to be able to commit to and offer complete contracts. When such contracts can be offered to both private and public firms, ownership is irrelevant.[8]

This article draws the line between public and private ownership by modeling the possibility of offering incentive contracts that regulate prices and production in public firms and of compensating those firms with subsidies. Private firms do not receive such contracts or subsidies and are therefore unregulated. Since the government is the residual claimant of a public firm's profits or losses and since it wants to avoid service interruption, under asymmetric information money-losing firms are subsidized while more productive firms earn informational rents. Production is distorted to reduce these information costs, which in turn diminishes consumer surplus. Privatization reduces the need to subsidize low profitability firms and to distort their production below the monopoly level (due to adverse selection). Privatization is used for firms that have low profitability or low social benefits. To avoid the technicality of an additional principal–agent problem, the private owner is assumed to be the firm's manager. The welfare comparison is thus between a benevolently regulated firm and a private monopoly charging the standard monopoly price.

Finally, the model can be related to the theory of public–private partnerships, which has received attention recently in national and international funding institutions (Vaillancourt Rosenau 2000; IMF 2004). The idea behind public–private partnerships is to make governments purchase the

7. Estache (2002) shows that technical and productive efficiency gains generated by Argentina's 1990s utilities privatization have not been transmitted to consumers. The benefits were captured by the industry because of inefficient regulation.

8. When private and public structures have the same degree of contract completeness, ownership is irrelevant. This happens when the government is able to offer the same contracts to public and to private firms, as described in Baron-Myerson (1982) and Laffont-Tirole (1993).

service rather than the asset associated with providing a public good or a good for which there is a potential market failure. On the one hand, governments view public–private partnerships as a vehicle to shift investment costs off their books and to safeguard the execution of projects that would otherwise hardly materialize given their budget constraints. On the other hand, public–private partnerships are praised for their potential productive efficiency benefits.[9] As the possible productivity inefficiencies are ruled out to focus on the allocative inefficiencies, it is no surprise that the benefits of privatization are aligned with this first view that emphasizes the fiscal benefits of privatization.

II. The Model

The government has to decide whether an industry with increasing returns to scale should be under public or private control. *Regulation regime* is used here to refer to the case of government control of the production of a public firm. The government's control rights are associated with accountability for profits and losses. That is, the government subsidizes the firm in case of losses and taxes the firm in case of profits. *Private regime* refers to the case in which the government imposes no control on the operations of a private firm and takes no responsibility for the firm's profits or losses. That is, no transfer between the government and the private firm is possible once production has begun. This is a simplification, since in practice government might subsidize the private sector. However, subsidies are lower under private ownership than under public ownership, which is what matters for the results.[10] Similarly, private firms do not pay a tax on profit, but they may pay an entry fee.[11]

The model considers a normal good. It is common knowledge that the inverse demand function for $Q \geq 0$ units of the commodity is given by[12]

$$P(Q) = a - bQ \tag{1}$$

9. Public–private partnerships can be used to harden a firm's budget constraint, as discussed earlier, and to bundle complementary tasks, such as constructing and operating infrastructure projects (see Hart 2003 and Martimort and Pouyet 2006).

10. For instance, in Burkina Faso government subsidies to state enterprises dropped from 1.42 percent of GDP in 1991 to 0.08 percent in 1999 as a result of privatization (AfDB and OECD 2003).

11. This is an artifact of the formalization. In the static model it is optimal for the government to sell the firm ex ante (that is, while it is in a position of symmetric information with respect to the firm) rather than to tax its profit ex post (that is, once the firm has learned its cost parameter and has an informational advantage). Empirical evidence shows that developing countries rely on entry fees to raise revenues from firms (see Auriol and Warlters 2005).

12. To keep the analysis simple a linear product demand is considered. However, the results are robust to a more general demand function. For instance, models with isoelastic demand functions require numerical simulations but yield similar results. Computations are available from the authors on request.

where a > 0 and $b > 0$. The gross consumer surplus is therefore

$$S(Q) = \int_0^Q P(x)\mathrm{d}x = aQ - \frac{b}{2}Q^2. \tag{2}$$

The analysis here focuses on infrastructure and utilities, which require firms to sink large investments. Technically, they involve increasing-returns-to-scale technology so that cost functions are subadditive. As in Baron and Myerson (1982), this is modeled by simply assuming that the cost function includes a fixed cost, $K > 0$, and an idiosyncratic marginal cost, β_i. To produce q_i units of the commodity, firm $i = 1,\ldots,N$ has the following cost function:

$$C(\beta_i, q_i, K) = K + \beta_i q_i. \tag{3}$$

Firm i must make investment K before discovering β_i. The β_i 's are independently and identically distributed on the interval $[\underline{\beta}, \overline{\beta}]$ according to the density and cumulative distribution functions $g(\cdot)$ and $G(\cdot)$. This law is common knowledge. The expectation operator is denoted by E, the average marginal cost by $E\beta$, and the variance of marginal cost by $\sigma^2 = \mathrm{var}(\beta)$. Neither the government nor the competitors of firm i observe β_i.

The fixed cost K is large, so that the maximum number of firms N that can survive under laissez-faire is small. More specifically, the following assumption is made:

$$K \geq \frac{(a - E\beta)^2}{16b} + \frac{\sigma^2}{4b} \tag{4}$$

which implies that $N \leq 2$.[13]

The firms are profit maximizers. The profit of firm $i = 1,\ldots,N$ is

$$\Pi_i = P(Q)q_i - C(\beta_i, q_i, K) + t_i \tag{5}$$

where t_i is the net transfer that the firm receives from the government (subsidies minus taxes and franchise fees).

The government is utilitarian and maximizes the sum of consumer and producer surpluses minus the social cost of transferring public funds to the firm. The transfer to the firm can be either positive (a subsidy) or negative (a tax).

13. To see how this assumption is computed, see endnote 3 in the supplementary appendix at http://wber.oxfordjournals.org.

The government's objective function is

$$W = S(Q) - \sum_{i=1}^{N} C(\beta_i, q_i, K) - \lambda \sum_{i=1}^{N} t_i \tag{6}$$

where λ is the opportunity cost of public funds. For λ close to 0 the government maximizes the consumer surplus; for larger λ the government assigns more weight to taxpayer surplus (that is, on transfers).

The term $1 + \lambda$ measures the social cost of transferring one unit of money from the government to the firm. That is, government pursues multiple objectives, such as producing public goods, regulating noncompetitive industries, and controlling externalities, under a single budget constraint. The opportunity cost of public funds is the Lagrange multiplier of this constraint. It tells how much social welfare can be improved when the budget constraint is relaxed by one unit of money; it includes forgone benefits of alternative investment choices and spending.[14] In practice, any additional investment in infrastructure or public utilities implies a reduction of the production of essential public goods, such as national security and law enforcement, or any other commodities that generate externalities, such as health care and education. It may also imply a rise in taxes or public debt. All these actions have a social cost that must be compared with the social benefit of the additional investment.

In developed countries λ is usually assumed to be equal to the deadweight loss due to imperfect income taxation. It is estimated at around 0.3 (Snower and Warren 1996). In developing countries low income levels and difficulties implementing effective taxation are large constraints on the government budget. The ratio of tax revenue to GDP for 1995, for example, was 36.1 percent for OECD countries (see OECD.org) compared with 18.2 percent for developing countries (based on a sample in Tanzi and Zee 2001). All else being equal, the opportunity cost of public funds is higher when government revenue is lower, and as a result, the opportunity cost of public funds in developing countries is likely to be higher than 0.3. The World Bank (1998) suggests an opportunity cost of 0.9 as a benchmark. But the value is much higher in heavily indebted countries.

III. Privatization of a Natural Monopoly

When K is large, a natural monopoly emerges: $N \in \{0,1\}$. Since there is at most one firm, the firm index can be dropped temporarily, leaving the

14. The opportunity cost of public funds is different from the marginal cost of public funds, which measures the deadweight loss from the marginal increase of a specific tax rate (see Warlters and Auriol 2006). Assuming an exogenous constant opportunity cost of public funds has proved useful in discussing the relationship between incentive and budget balance issues (see, for example, Laffont and Tirole 1993 and Picard 2001a).

production of the monopoly equal to total production, Q. Regulation aims to correct the distortion associated with monopoly pricing. Theory suggests that welfare should never be smaller under benevolent regulation than under laissez-faire. This is shown to not always be the case under asymmetric information.

Private Monopoly

The production level of a private monopoly is not controlled by the government, but the government can control firm entry by auctioning the right to operate. Let $F(\lambda) \geq 0$ be the (exogenous) franchise fee that a private firm pays to the government to operate in the product market. This fee depends on λ [see assumption (13) below]. The firm faces the following sequential choices. First, it chooses whether to enter the market by paying the franchise fee, $F(\lambda)$, and making the investment, K. After entry, nature chooses the marginal cost, β, according to the distribution function, $G(\cdot)$. The private firm learns β and chooses a production level, Q. No transfer is made after entry and realization of the marginal cost, β: the firm never receives a subsidy from the government, nor does it pay a tax.[15] The firm's profit is

$$\Pi^{PM} = \max_{Q} P(Q)Q - \beta Q - K - F(\lambda). \tag{7}$$

The optimal production is independent of K and $F(\lambda)$:

$$Q^{PM} = \frac{a - \beta}{2b}. \tag{8}$$

If a is smaller than the firm's marginal cost, β, production falls to 0. To rule out a corner solution in the paper, a is assumed not to be too small:

$$a \geq \max\left\{2\overline{\beta}, \overline{\beta} + \frac{G(\overline{\beta})}{g(\overline{\beta})}\right\}. \tag{9}$$

Substituting Q^{PM} in equations (5) and (6) yields the ex ante profit and welfare of a private monopoly:

$$E\Pi^{PM} = V - K - F(\lambda) \tag{10}$$

$$EW^{PM}(\lambda) = \frac{3}{2}V - K + \lambda F(\lambda) \tag{11}$$

15. Auriol and Picard (2005) discuss the privatization of a monopoly with ex post renegotiation and an endogenous franchise fee.

where

$$V = \frac{E(a-\beta)^2}{4b} \tag{12}$$

is the firm's operating profit. A monopoly is privately feasible if it is ex ante profitable. This requires that $V \geq K$ and that $F(\lambda) \in [0, V - K]$. Similarly, a monopoly is socially valuable if it yields ex ante positive welfare. Comparing equations (10) and (11) shows that monopolies are socially valuable but privately infeasible when $V < K < \frac{3}{2}V$.

Because public funds are costly, the ex ante welfare, $EW^{PM}(\lambda)$, increases linearly with $F(\lambda)$. The maximum entry fee that the government can collect is the maximum price a risk-neutral entrepreneur would agree to pay for the monopoly concession: $F^* = \max\{0, V - K\}$. In practice, international capital flows depend on country risk ratings, so that developing country governments do not collect F^* (see Brewer and Rivoli 1990).[16] Because of debt service, social instability, perceived corruption in the administration, and lack of transparent and predictable political and judicial institutions, private investors, especially foreign ones, are reluctant to invest in developing countries.[17] In this model a large λ translates into a bad rating. That is, countries characterized by a large λ are also countries that get low privatization proceeds. To capture this idea, the following assumption is made:[18]

(13) $F(\lambda) \in [0, F^*]$ is nonincreasing and weakly convex in $\lambda \geq 0$.

Regulated Monopoly

Under public ownership the government is accountable for the firm's profits and losses and monitors the production of the regulated monopoly. The timing is as follows: The government first chooses to make investment K. Nature chooses the marginal cost, β, according to the distribution function $G(\cdot)$. The firm's manager learns β. The government proposes a production and transfer contract, $(Q(\cdot), t(\cdot))$. Finally the regulated firm chooses and implements a production level in this contract.

16. The ratings reflect the ability and willingness of a country to service its financial obligation. See, for instance, the Global Risk Assessments website, www.grai.com/links.htm.

17. For instance, in 1999 foreign direct investment inflows to the 49 least developed countries (10 percent of world population) was 0.5 percent of world flows. Since less than 10 percent of this investment was cross-border merger and acquisition (including privatization), privatization proceeds are lower in poor countries than in rich ones, even with many privatizations.

18. The theory of predatory governments is another justification for assumption 13 (see, for instance, Evans 1989).

Symmetric information. Suppose that the government observes the realization of β. It then solves $\max_{\{Q,\, t\}} W$, such that $\Pi \geq 0$, where W and Π are defined in equations (5) and (6). Since λ is positive, transfers to the regulated firm are costly and must be reduced to the breakeven point, $\Pi = 0$. That is, $t^{RM*} = -P(Q)Q + K + \beta Q$. Substituting this expression in equation (6), and maximizing W with respect to Q yields

$$Q^{RM*}(\beta) = \frac{1+\lambda}{1+2\lambda}\frac{a-\beta}{b}. \tag{14}$$

Inserting Q^{RM*} in equation (6) and computing the expected value of W yields the ex ante welfare under symmetric information

$$EW^{RM*}(\lambda) = (1+\lambda)\left(2\frac{1+\lambda}{1+2\lambda}V - K\right) \tag{15}$$

where V is as defined in equation (12). The government invests K in a regulated firm only if equation (15) is positive. The ex ante welfare increases linearly in V and is nonmonotonic in λ if $V > K$: that is, it decreases for small λ and increases for large λ.

For small λ the government incurs few social costs transferring money to the regulated firm, which then produces quantities close to the first-best level (at a price that is close to marginal cost); that is, $\lim_{\lambda \to 0} Q^{RM*} = (a - \beta)/b$ and therefore $P[(a - \beta)/b] = \beta$. At this price the regulated firm cannot recover its fixed cost, but this loss is compensated for by a public transfer to the firm, $t = K > 0$. And the government will continue to subsidize the regulated firm as long as λ remains small enough. By contrast, for large λ the government is more interested in receiving transfers from the firm than in maximizing consumer surplus. In the limit it seeks the maximum revenue from the firm, and it chooses the production level of a private monopoly, $\lim_{\lambda \to \infty} Q^{RM*} = (a - \beta)/2b = Q^{PM}$, mimicking private firm behavior.

Asymmetric information. Under asymmetric information the government does not observe β. To entice the firm to reveal its true cost, an incentive compatibility constraint is needed. Taking this constraint into account implies that in the government objective function the marginal cost, β, is replaced by the virtual cost (see Laffont and Tirole 1993):

$$c(\beta, \lambda) = \beta + \frac{\lambda}{1+\lambda}\frac{G(\beta)}{g(\beta)}. \tag{16}$$

The virtual cost includes the marginal cost of production, β, and the marginal cost of information acquisition, $(\lambda/1 + \lambda)(G(\beta)/g(\beta))$. To avoid the technicalities of "bunching," the standard assumption of monotonic hazard rate is made:[19]

$$\frac{G(\beta)}{g(\beta)} \text{ is nondecreasing.} \tag{17}$$

Thus $c(\beta, \lambda) \geq \beta$, and by assumption (17), $c(\beta, \lambda)$ increases in β and λ. Let

$$V^{RM}(\lambda) = \frac{E(a - c(\beta, \lambda))^2}{4b}. \tag{18}$$

It is the function V in equation (12) evaluated at $c(\beta, \lambda)$ instead of β. This implies that $V^{RM}(\lambda)$ decreases in λ. Following Baron and Myerson's (1982) approach yields the following proposition, which proof is standard (see Laffont and Tirole 1993):

LEMMA 1. *Under asymmetric information the optimal production and the ex ante welfare under a regulated monopoly are those under the symmetric information case evaluated at the virtual cost,* $c(\beta, \lambda)$:

$$Q^{RM}(\beta) = Q^{RM*}(c(\beta, \lambda)) \tag{19}$$

$$EW^{RM}(\lambda) = (1+\lambda)\left(2\frac{1+\lambda}{1+2\lambda}V^{RM}(\lambda) - K\right). \tag{20}$$

Since $c(\beta, \lambda) \geq \beta$, $Q^{RM}(\beta) \leq Q^{RM*}(\beta)$ for any β. Moreover, since $c(\beta, \lambda)$ increases in β, the distortion is larger at higher marginal costs. Indeed, by lowering the production of inefficient firms, the government reduces the overall incentive to report inflated costs. This strategy lowers the firm's informational rent and the cost of information revelation. Comparing equations (12) and (18) shows that $V^{RM}(\lambda) \leq V$ for all $\lambda \geq 0$. Hence, the ex ante welfare of a regulated monopoly is lower under asymmetric information than under symmetric information, $EW^{RM}(\lambda) \leq W^{RM*}(\lambda)$.

Regulation or Privatization?

This section compares the welfare level generated by a private monopoly with that generated by a regulated monopoly. First is the symmetric information case.

19. When the hazard rate is not increasing monotonically, the virtual cost [equation (16)]—and thus the regulated output [equation (19)]—are not monotonic. Output is thus not an invertible function of the type β, and the government cannot infer each firm's type by observing its output level. Unable to distinguish the types of firms, the government must bunch various types in a same contract.

PROPOSITION 2. *Under symmetric information public regulated monopoly dominates privately feasible monopoly regardless of whether the privately feasible monopoly is franchised.*

This proposition is intuitive. Under symmetric information a benevolent government cannot generate less welfare than a private monopoly because, for any realization of β, the government can at least replicate the outcome of a private firm. Nevertheless, for large opportunity costs of public funds a regulated monopoly under symmetric information brings barely more welfare than a private monopoly when a private monopoly pays the maximum franchise fee, F^*.[20] In other words the welfare of a regulated monopoly is almost equal to the welfare of a private monopoly for large λ. From this argument it can be inferred that, for large enough λ, the asymmetry of information in the regulated monopoly gives rise to additional information cost, which makes this configuration less attractive for the government. More formally, it is readily shown that the welfare function of the regulated monopoly has an asymptote with slope equal to $V - K$ under symmetric information and to

$$\text{(21)} \qquad \lim_{\lambda \to \infty} \frac{EW^{RM}(\lambda)}{\lambda} = V^{RM}(\infty) - K$$

under asymmetric information. Because the former is larger than the latter, it can be deduced that privately feasible monopolies dominate regulated monopolies for large enough λ. Let the fixed cost, K, satisfy the following condition:

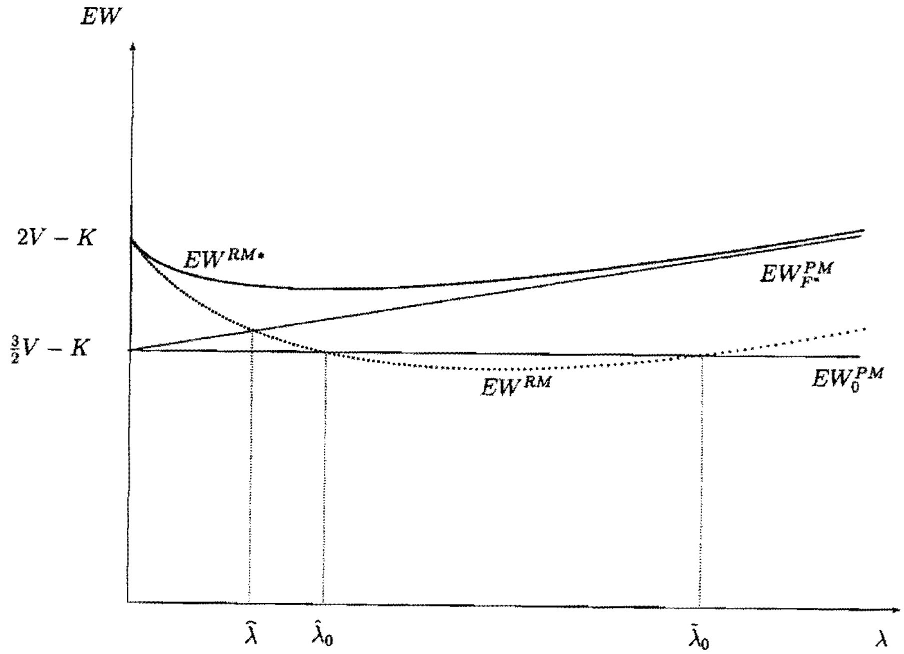
$$\text{(22)} \qquad V \geq K \geq V\left(2\sqrt{\frac{B + V^{RM}(\infty)}{V}} - \frac{B+V}{V}\right) \text{with } B = E\left[\frac{a - \beta}{b}\frac{G(\beta)}{g(\beta)}\right].$$

The interval defined in condition (22) is nonempty. Indeed, $2\sqrt{(B+V^{RM}(\infty))/V} - (B+V)/V < 1$ is equivalent to $B^2 + 4V(V - V^{RM}(\infty)) > 0$, which is always true since $V > V^{RM}(\infty)$. The left side of condition (22) implies that the fixed cost is small enough so that a monopoly is privately feasible [see equation (10)]. The right side implies that the fixed cost is large enough so that the monopoly is not too profitable.

Proposition 3 is the main finding of this article: under condition (22) privatization dominates benevolent regulation for at least some value of the opportunity cost of public funds.

20. When $F = F^*$, $EW^{RM*}(\lambda) = ((1+\lambda)/(1+2\lambda))V + (1+\lambda)(V-K)$, whereas $EW^{PM}_{F^*}(\lambda) = V/2 + (1+\lambda)(V-K)$. The two functions have a common asymptote with slope $V-K$ (figure 1).

FIGURE 1. Welfare for Private and Regulated Monopoly



Source: Authors' analysis.

PROPOSITION 3. *If assumptions (4), (9), (13), and (17) hold and if the fixed cost, K, lies in the nonempty range defined by condition (22), two cases are possible*:

(1) $lim_{\lambda\to+\infty}F(\lambda) \geq V^{RM}(\infty) - K$: *there exists a unique threshold,* $\hat{\lambda}$, *such that privatization dominates regulation if and only if* $\lambda > \hat{\lambda}$.

(2) $lim_{\lambda\to+\infty}F(\lambda) < V^{RM}(\infty) - K$: *there are two thresholds* $\hat{\lambda}$ *and* $\tilde{\lambda}$, $\hat{\lambda} < \tilde{\lambda}$ *such that privatization dominates regulation if and only if* $\lambda \in [\hat{\lambda}, \hat{\lambda}]$.

In other words for any franchise fee function $F(.)$, which includes the case $F(.) \equiv 0$, there exists a range of fixed costs, K, and costs of public funds, λ, so that the government prefers privatization (see figure 1). The bold solid curve depicts the ex ante welfare of regulated monopoly under symmetric information (RM^*), and the bold dotted curve depicts the ex ante welfare under asymmetric information (RM). The ex ante welfare of regulated monopoly is nonmonotonic in λ and it is higher for low and high values of λ than for intermediate ones. The thin solid straight lines represent the two boundaries of the ex ante welfare of a private monopoly (PM) (that is, for $F(\lambda) \equiv F^*$ and for $F(\lambda) \equiv 0 \ \forall \ \lambda \geq 0$). Depending on the franchise fee function, $F(\lambda)$, the welfare function associated with a private monopoly varies between these two bounds.

Privatization with price liberalization dominates a benevolent regulation under public ownership for (at least) intermediate values of opportunity costs of public funds. On the one hand, when the franchise fee, $F(\lambda)$, is large (that is, $F(\lambda) \geq V^{RM}(\infty) - K, \forall \lambda \geq 0$), the opportunity costs supporting privatization belong to an unbounded range $[\hat{\lambda}, +\infty)$. The optimal industrial policy is monotonic in λ. On the other hand, when the franchise fee falls below the threshold $V^{RM}(\infty) - K$, the optimal industrial policy is nonmonotonic in λ. For intermediate values of λ, privatization with price liberalization dominates regulation under public ownership. The opposite conclusion holds for lower and larger values of λ.

The preference for private feasible monopolies is not explained by the possibility of collecting franchise fees. As shown in the supplementary appendix, even with no fee, $F(\lambda) \equiv 0$, the interval $[\hat{\lambda}_0, \tilde{\lambda}_0]$ where privatization dominates regulation is nonempty (see figure 1). The intuition for this result is as follows. Private entrepreneurs enter the business if their firm is ex ante profitable. After the investment the private firm makes a large or a low operating profit depending on the realization of technical and demand uncertainties. Private entrepreneurs, who bet their own assets (or shareholders assets) on the firm, are accountable for these profits and losses. By contrast, under regulation accountability lies with the government, which bears the business risk and must grant ex post subsidies to unprofitable firms. Under asymmetric information the regulated firm uses the transfers to acquire a positive informational rent. The government prefers that the private sector take over when the social cost of the information rent outweighs the social benefit of controlling the firm's operation. As suggested by condition (22) and shown in section V this ultimately depends on the profitability of the industry market segment.

Numerical Assessment for $\hat{\lambda}$

Independent of the privatization proceeds and fees, privatization with price liberalization dominates a benevolent regulation under public ownership for intermediate values of λ. The relevance of this result depends on what "intermediate" means. If λ is very high, privatization will never be optimal. The lowest value of the opportunity cost, $\hat{\lambda}$, for which privatization becomes attractive, is obtained when the highest franchise fee F^* is applied (see figure 1). It solves $EW^{RM}(\lambda) = EW^{PM}_{F^*}(\lambda)$, which is equivalent to

$$4(1+\lambda)^2 V^{RM}(\lambda) = (3+2\lambda)(1+2\lambda)V. \tag{23}$$

To obtain an explicit value for $\hat{\lambda}$, β is assumed to be uniformly distributed over $[\underline{\beta}, \overline{\beta}]$.[21] Using equations (16) and (18), under the uniform distribution equation (23) is equivalent to $4E((1+2\lambda)(a-\beta) - \lambda(a-\underline{\beta}))^2 = (3+2\lambda)(1+2\lambda)E(a-\beta)^2$. Both the right and left sides can be divided by a^2 to show that $\hat{\lambda}$ depends only on $\underline{\beta}/a$ and $\overline{\beta}/a$. Under the uniform specification

21. The simulation results are robust to other statistical specifications (for example, normal distribution).

TABLE 1. Minimum Opportunity Costs, $\hat{\lambda}$, above which Privatization is Preferred

$\hat{\lambda}$	$\underline{\beta}/a=0.0$	0.1	0.2	0.3	0.4
$\bar{\beta}/a=0.1$	1.14				
0.2	0.71	1.07			
0.3	0.52	0.66	0.99		
0.4	0.42	0.48	0.60	0.90	
0.5	0.35	0.38	0.44	0.54	0.81

Source: Authors' analysis.

the demand intercept a satisfies assumption (9) if and only if $a \geq 2\ \bar{\beta}$. This implies that $0 \leq \underline{\beta}/a < \bar{\beta}/a \leq 0.5$. Table 1 displays $\hat{\lambda}$ for the admissible values of $\underline{\beta}/a$ and $\bar{\beta}/a$.

The opportunity cost of public funds is generally assessed to be around 0.3 in developed countries (see, for instance, Snower and Warren 1996) and higher in developing countries. If demand and cost functions are reasonably approximated by linear functions and satisfy assumption (9), which is an empirical issue, $\hat{\lambda}$ lies below the range of the opportunity costs prevailing in developing countries. The results in table 1 also show that privatization is more likely as technological uncertainty rises (that is, $\hat{\lambda}$ decreases with $(\bar{\beta} - \underline{\beta})/a$). Indeed, larger cost uncertainty implies more information asymmetry between firms and governments and hence larger information rent in the regulated structures.

IV. LIBERALIZATION REFORM: THE DUOPOLY CASE

This section briefly explores the optimal industrial organization when the fixed cost, K, becomes smaller or, equivalently, when the value of operating the firm after investment, V, becomes larger.[22] Following Auriol and Laffont (1992), a regulated duopoly is compared with a private duopoly, modeled as a Cournot duopoly with asymmetric information between firms.[23] To simplify the exposition, franchising is ruled out as well:

$$F(\lambda) \equiv 0. \tag{24}$$

In this model the benefit of choosing a regulated duopoly originates from the sampling gain, as first analyzed by Auriol and Laffont (1992). That is, variable costs are lower in a duopoly because the regulator can choose the most efficient supplier of two firms. Monitoring a regulated duopoly is thus equivalent to monitoring a regulated monopoly for which the investment level is $2K$

22. In the last two decades some industries (such as telecommunications) have experienced dramatic technological and demand changes because of decreased fixed costs and increased demand.

23. For conciseness, mixed duopolies with a regulated and a private firm are excluded here. See Cremer, Marchand, and Thisse (1989) and Picard (2001b) for a policy discussion of mixed duopolies.

and the marginal cost is min $\{\beta_1, \beta_2\}$. Since β_1 and β_2 are assumed to be independently and identically distributed, min $\{\beta_1, \beta_2\}$ is distributed according to $g_{\min}(\beta) = 2(1 - G(\beta))g(\beta)$. Let

$$V^{RD}(\lambda) = \int_{\underline{\beta}}^{\bar{\beta}} \frac{(a - c(\beta, \lambda))^2}{4b} g_{\min}(\beta) \mathrm{d}\beta \tag{25}$$

which is the monopoly expression $V^{RM}(\lambda)$ in equation (18) with the density function $g(\beta)$ replaced by $g_{\min}(\beta)$. The next result is established under the assumption that $G(\beta)$ is the uniform distribution, yielding a proposition that applies for a more general distribution.[24]

PROPOSITION 4. *If the firms' marginal costs are independently and uniformly distributed over* $[0, \bar{\beta}]$ *and assumptions (9) and (24) hold, a private duopoly is never optimal.*

In a regulated duopoly only the firm with the lowest marginal cost produces, which maximizes productive efficiency. By contrast, in a private duopoly there is excessive entry and inefficient allocation of production. The advantage of private structures thus disappears once more than one firm can enter the market.[25] For very profitable market segments the optimal choice is thus between regulated monopoly and regulated duopoly. Let $K^{RM/RD}(\lambda)$ be the value of the fixed cost such that the government is indifferent between a regulated monopoly and a regulated duopoly (that is, such that $EW^{RM}(\lambda) = EW^{RD}(\lambda)$):

$$K^{RM/RD}(\lambda) = \frac{1+\lambda}{1+2\lambda}(V^{RD}(\lambda) - V^{RM}(\lambda)). \tag{26}$$

Under asymmetric information the sampling gain is measured by $K^{RM/RD}(\lambda)$ so that a regulated duopoly is optimal whenever the entry fixed cost, K, is lower than $K^{RM/RD}(\lambda)$.[26]

24. See the supplementary appendix at http://wber.oxfordjournals.org.

25. This result may seem at odds with theories where private structures perform better with more entrants (see, for instance, Vickers and Yarrow 1991 and Segal 1998). A basic difference in this model lies in the intensity of competition within private and regulated structures. Private firms compete in quantities so that the addition of a firm does not fully eliminate market power and profits. By contrast, information costs fall when a second firm is added in the regulated market (see Auriol and Laffont 1992).

26. Since the distribution function $g^{min}(\beta)$ stochastically dominates $g(\beta)$ and since $(a - c(\beta, \lambda))^2/4b$ decreases in β, it can be deduced that $V^{RD}(\lambda) \geq V^{RM}(\lambda)$. However, the larger λ is, the lower is the impact of the sampling gain and the smaller is the government's preference for regulated duopoly.

V. Optimal Industrial Policy

Under complete information the government can always replicate the production decisions of private firms so that privatization is never optimal. The optimal industrial policy varies according to whether the investment cost, K, is large (no production), medium (regulated monopoly), or small (regulated duopoly). Under asymmetric information, information costs alter this result. Still, the optimal decision depends on the fixed cost, K. Let $K^{RM}(\lambda)$ be the threshold such that the government is indifferent between a regulated monopoly and no production; that is, such that $EW^{RM}(\lambda) = 0$. It is easy to check that

$$K^{RM}(\lambda) = \frac{2+2\lambda}{1+2\lambda} V^{RM}(\lambda) \tag{27}$$

where $V^{RM}(\lambda)$ is as defined in equation (16). Similarly, let $K^{RM/PM}(\lambda)$ be the value of the fixed cost such that the government is indifferent between a regulated monopoly and a private monopoly; that is, such that $EW^{RM}(\lambda) = EW^{PM}$. It is easy to check that

$$K^{RM/PM}(\lambda) = \frac{2(1+\lambda)^2}{\lambda(1+2\lambda)} V^{RM}(\lambda) - \frac{3V}{2\lambda}. \tag{28}$$

Proposition 5. *If the firm's marginal cost is independently and uniformly distributed over* $[0, \bar{\beta}]$ *and assumptions (9) and (24) hold, then optimal industrial policy under asymmetric information is to set:*

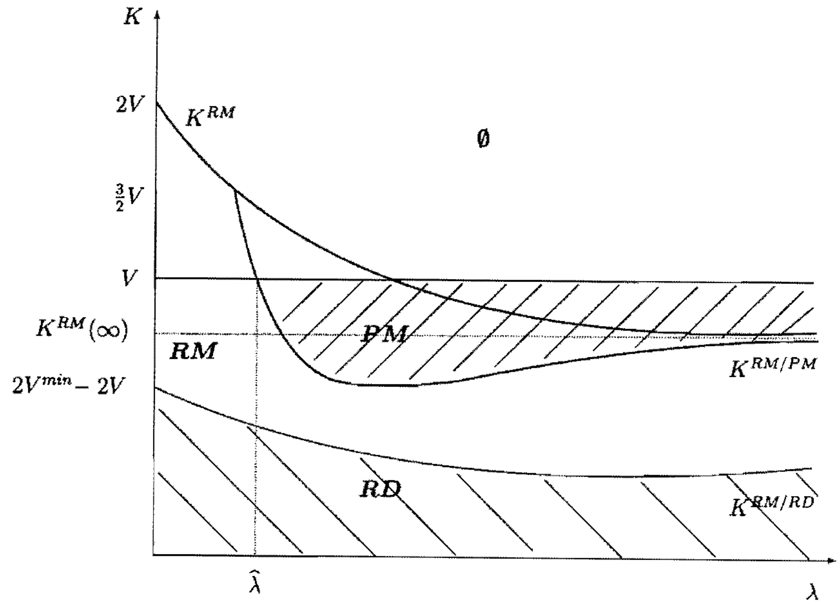
- *no production if* $K > \max\{V, K^{RM}(\lambda)\}$
- *a private monopoly if* $K^{RM/PM}(\lambda) < K \leq V$
- *a regulated monopoly if* $K^{RM/RD} < K \leq \min\{K^{RM/PM}(\lambda), V\}$ *or if* $V \leq K < K^{RM}(\lambda)$
- *a regulated duopoly if* $K \leq K^{RM/RD}(\lambda)$.

Although this proposition assumes that $G(\beta)$ is the uniform distribution, the result applies to more general distributions.[27]

Because developing countries have large opportunity costs of public funds, they can implement industrial policies that strongly differ from those implemented in developed countries (see figure 2). The discussion here is limited to four cases that depend on the profitability of the market segment. Profitability is assessed by the difference between the operating profit of the private firm, V, and the fixed cost level, K. In the following discussion V is fixed to a constant and K is successively decreased.

27. See the supplementary appendix at http://.wber.oxfordjournals.org.

FIGURE 2. Optimal Industrial Policy



Source: Authors' analysis.

The first case occurs for large fixed costs, $K > V$. The market segment is not privately profitable and is socially beneficial only if the opportunity cost of public funds, λ, is small enough. The optimal industrial policy is thus to establish a public regulated firm for low λ or to supply nothing at all for high λ. In figure 2, public regulated monopolies that are desirable under asymmetric information are depicted by the white area denoted *RM*, while the case for no production corresponds to the area denoted Ø. This is a case for public provision and ownership of firms in unprofitable segments. Examples are rural infrastructure projects (secondary roads or rural electrification) that are supplied only by wealthy countries and that are usually priced at marginal cost to rural populations. In poor countries the opportunity costs of subsidizing such infrastructure are higher than its social returns. As a result, poor countries choose not to offer such infrastructure or try to get rid of the unprofitable public firms in charge of them.

The second case occurs for smaller fixed costs that belong to the range $[K^{RM}(\infty), V]$. In this case a private firm finds it profitable to enter and to supply its output at the monopoly price. In contrast to the first case, the government can now organize supply through a private firm. The optimal industrial policy is monotonic in the opportunity cost of public funds, λ: a public regulated firm is preferred if λ is small enough, and privatization is preferred otherwise. In figure 2 the case for a public regulated firm is depicted by the white area denoted *RM*, and the case for privatization by the hatched area above the curve $K^{RM/PM}$ and denoted *PM*.

To understand why privatization can be a better alternative than public provision, consider a poor country government that is unable to finance an infrastructure project, such as a small water network or electricity generation facility (that is, a case where K lies below V and above the curve K^{RM} with λ high enough). The optimal solution is for a private firm to invest in the infrastructure in exchange for being allowed to charge monopoly pricing because it is better to have a privately owned and operated infrastructure with monopoly price distortion than no infrastructure at all. By continuity, this conclusion holds when the government gets a (moderate) benefit from financing the infrastructure.

Developing countries offer many examples of such privatization through concession, lease, and greenfield contracts. For instance, many developing countries have started build-operate-and-transfer road programs, wherein private firms finance the sunk costs associated with building highways in exchange for a long-term license to exploit a monopoly position.[28] China, Malaysia, and Thailand have implemented such programs in water, and Chile and Mexico in sanitation (World Bank 1997). In many places the privatization process is less formal. For instance, in Sub-Saharan Africa water and electricity services are offered by an informal sector made up of thousands of small-scale private and unregulated providers (see Auriol and Blanc forthcoming). As predicted by theory, they serve the middle class and the poor at prices that are much higher than public utility prices. Kariuki and Schwartz (2005) estimate that nearly half the urban population in Africa relies on such private services for water.

The third case occurs when K is lower than $K^{RM}(\infty)$. In contrast to the second case, the optimal industrial policy is no longer monotonic in λ. This property, discussed earlier, is reflected in figure 2 by the fact that curve $K^{RM/PM}$ is nonmonotonic in λ. For exposition $\underline{K}^{RM/PM}$ is defined as the minimum of $K^{RM/PM}$ (that is, $\underline{K}^{RM/PM} = \min_\lambda K^{RM/P\overline{M}}(\lambda)$), and the discussion is limited to fixed costs in the interval $[\underline{K}^{RM/PM}, K^{RM}(\infty)]$. Then, as the opportunity cost of public funds, λ, increases, the optimal industrial structure successively switches from a public regulated firm to a private firm and then back to a public regulated firm. The difference from the second case is that when λ is large enough the government seeks to extract the maximum revenue from the public firm by setting high prices. This case shows that, while the divestiture of a profitable public firm may be optimal in countries with intermediate costs of public funds, it is not necessarily optimal in developing countries, where budget constraints are tight and market institutions are weak.

The fixed-line and long distance segments of the telecommunication industry illustrate the nonmonotonic result. Anania (1992) shows how developing countries collected revenues from the profitable segments of public

28. Trujillo, Quinet, and Estache (2002) show that transport privatization leads to a reduced need for public investment.

telecommunication companies, such as international calls, to subsidize mail service and to ease their budget deficits. This experience indicates that the interests of developing countries in the privatization of the telecommunication sector strongly differed from those of developed countries.

Although governments in developed countries also care for the revenues generated by their utilities,[29] their effective taxation systems make them less greedy about the potential revenue of natural monopoly markets.[30] In developing countries privatization of profit centers of public utility is socially inefficient. By eliminating cross-subsidies between various market segments or industries, privatization generally increases the fiscal costs related to unprofitable segments and reduces political support from harmed (usually poor) consumers (Estache and Wodon 2006; Trujillo, Quinet, and Estache 2002).

The fourth case takes place at low enough fixed costs. With a large surplus at stake, a private Cournot duopoly is never optimal. Governments choose between regulated public structures with one or two firms, depending on whether opportunity cost of public funds is small or large. In figure 2, a regulated duopoly is preferred to a regulated monopoly in the hatched area below the curve $K^{RM/RD}$ denoted *RD*. This sheds light on the relationship between market liberalization on the one hand and technological improvement and product demand growth (illustrated by a fall in the ratio K/V) on the other hand. Market liberalization corresponds to the divestiture of the historical monopoly and the introduction of new entrants but is not equivalent to laissez-faire. Prices and entry should remain regulated to protect consumers against firms' tendency to reduce competition by setting low capacity levels or even by organizing collusion (not modeled here). With a large surplus at stake, ownership is not the key to the allocative efficiency problem; regulation is. Empirical evidence supports this result.[31]

VI. Conclusion

This article compares the welfare of a public firm with regulated prices and the welfare of a private firm with liberalized prices for different values of opportunity costs of public funds. It shows that the privatization decision nontrivially depends on the value of opportunity costs of public funds and on the

29. The United States adopted a federal excise tax on telephony services in 1898. Opponents of the tax argue that it is distortive; proponents insist that the revenue is needed. The 3 percent tax yielded \$5.185 billion in 1999.

30. In most countries the bulk of government revenue is raised through taxation. Governments do, however, obtain substantial revenue from a number of other sources. "On the whole this non-tax revenue is more important for developing as opposed to industrial countries, comprising about 21 percent compared to 10 percent of total revenue" (Burgess and Stern 1993, p. 782).

31. For instance, for telecommunications in Africa and Latin America, Wallsten (2001) found that privatization does not yield improvements except when accompanied by an independent regulator. For more on telecommunication reforms in developing countries, see Auriol (2005).

profitability in the market segment where the firm operates. Since the opportunity cost of public funds is higher in developing countries than in developed countries, optimal privatization policies are likely to differ between those countries, as highlighted in the four following cases.

First, a market segment can have such low profitability that no private firm is able or willing to cover it. This situation is typically encountered in secondary road or electrification projects in low-density areas. A public firm is then the natural option, provided that the opportunity cost of public funds is not too high. Otherwise, the service is not offered. Empirical evidence is consistent with this result. The share of people in poor rural areas that do not have access to any service is larger in developing countries than in developed ones.

Second, the market segments can be profitable enough to allow a private firm to enter. Privatization with price liberalization then dominates regulation if the opportunity cost of public funds is large enough. As a result, the provision of utility services and infrastructure is more market oriented in developing countries than in developed ones. This is consistent with empirical evidence. For instance, Kariuki and Schwartz (2005) estimate that nearly half of urban dwellers in Africa (that is, the middle class and the poor) rely on private providers for water service. The private (informal) providers are bridging the utility service gap at a high cost; their prices are up to 10 times those of public providers. The result is also consistent with developing countries' use of concession, lease, and greenfield contracts, such as build-operate-and-transfer programs for highways, sanitation, and water networks.

Third, when market segments are more profitable, privatization choice is restricted to intermediate opportunity costs of public funds. The government indeed finds it optimal to set up a public firm for large enough opportunity costs of public funds. Very poor countries are plagued with financial problems and welcome the potential revenue that can be extracted from a public firm. Privatization of profitable public utilities, such as fixed-line or international telecommunication services, is therefore not efficient.

Fourth, the market segment can be so profitable that a second firm is able to enter. Then privatization with price liberalization is not optimal. As shown in the booming mobile telecommunication industry, regulation is a keystone for successful liberalization reforms.

In contrast to many contributions on privatization, the discussion here focuses on the two issues of allocative efficiency and macroeconomic financial constraint. The empirical literature in development studies provides ample evidence of the relationship between those two issues in natural monopoly and oligopoly markets in developing countries. Nevertheless, as noted, improvements in productive efficiency associated with privatization have also been highlighted in the theoretical and empirical literature. The authors hope that this article helps readers find a balance between those issues.

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Systemic Risk, Dollarization, and Interest Rates in Emerging Markets: A Panel-Based Approach

Edmar L. Bacha, Márcio Holland, and Fernando M. Gonçalves

This study investigates the impact of systemic risks and financial dollarization on real interest rates in emerging economies. Higher systemic risks induce both higher real interest rates and increased dollarization. Using appropriate instruments for the dollarization ratio, the study overcomes the simultaneous equation problem and correctly estimates a negative coefficient for the dollarization ratio in the interest rate equation. It confirms the theoretical prediction that a strategy of "dedollarizing" the economy will raise the equilibrium domestic real interest rate if the strategy fails to address fundamental macroeconomic risks. Even so, it also finds that this effect is small, after controlling for the risks of dilution and default. The results bring to light the systemic-risk reasons for high interest rates in emerging economies—and contribute to evaluating the difficulties of dedollarization policies. JEL codes: E43, F31, O16, O23, O54

In a study of financial contracts and risks in emerging economies, De la Torre and Schmukler (2004) argue that dollar contracts at home and abroad are rational responses of agents trying to cope with high systemic risks. Such risks include interest rate and exchange rate volatility, default risk, loss given default due to poor contract enforcement, and dilution and confiscation risks. In an environment of high systemic risk, currency mismatches can thus be understood as risk-mitigating mechanisms. This contrasts with the well-known "original sin" hypothesis developed by Eichengreen and Hausmann (1999), who posit that currency mismatches are the result of international market failures that prevent the issuance of local-currency-denominated bonds abroad.

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This article follows De la Torre and Schmukler in emphasizing systemic risks as the main culprits behind currency mismatches. It differs, however, in also considering the potential role of interest rate differentials in compensating for risks. De la Torre and Schmukler explicitly assume that investors are not compensated through the return on a given financial contract for risks that are diversifiable by the use of other contracts. For example, if the interest rate on a long-duration local-currency contract does not compensate investors for the risk of unexpected changes in inflation, such risk will be hedged through, say, a U.S. dollar contract. Similarly, if the interest rate on a domestically executed contract fails to compensate investors for the confiscation risk, such risk will be diversified away by writing the contract in a foreign jurisdiction.

This lack of attention to interest rate differentials as part of risk-coping in emerging economies is also evident in the literature on financial dollarization—defined as the domestic use of a stronger foreign currency as a credit instrument and a reserve of value.[1] The dominant paradigm in this literature is the minimum variance portfolio hypothesis, in which the volatility of returns is the key to explaining financial dollarization. In this framework, the local-currency interest rate is usually assumed to be given by an interest parity condition unrelated to the degree of financial dollarization. Thus, Ize and Levy-Yeyati (2006, p. 39), although recognizing cases in which deviations of the dollarization ratio from minimum variance portfolio allocations are associated with high real domestic interest rates, categorically assert that: "financial dollarization is immune to systematic differences in rates of return (through arbitrage, interest rates adjust to equalize ex ante rates of return). Instead, financial dollarization is all about risk differences."

Another view of financial dollarization relates to the quality of institutions as a key driver of contract dollarization (Levy-Yeyati 2006). A weak institutional environment may boost dollarization in many ways. When institutional quality is low, the government may be unable to assure debt holders that it will not inflate away the real burden of local currency debt. In such cases, a credible commitment mechanism may be achieved by issuing dollarized debt (Calvo and Guidotti 1990). On a related interpretation, implicit government guarantees about the exchange rate's value may generate risk mispricing and excess dollarization. De la Torre, Levy-Yeyati, and Schmukler (2003) argue that government guarantees were an important determinant of contract dollarization during Argentina's currency board regime, but the argument also

1. See Armas, Ize, and Levy-Yeyati (2006); Barajas and Morales (2003); De La Torre and Schmukler (2004); De Nicoló, Honohan, and Ize (2003); Galindo and Leiderman (2005); IADB (2005); Ize and Levy-Yeyati (2003); Levy-Yeyati (2006); Reinhart and Nozaki (2006); and Reinhart, Rogoff, and Savastano (2003). The terms *dollarization*, *financial dollarization*, and *deposit dollarization* are used here interchangeably to express the same empirical concept: the ratio of foreign currency deposits to total banking deposits in a given country. The use of the term *dollarization*, which refers to asset substitution, should not be confused with its earlier use in the literature on currency substitution.

applies to countries with flexible exchange rates that exhibit "fear of floating" (Calvo and Reinhart 2002).

Irrespective of particular theoretical models, it stands to reason that the same systemic risks that explain dollarization—yield volatility, default, loss given default, dilution, and confiscation—could also generate high real local-currency interest rates. Some emerging market economies such as Brazil notably avoided deposit dollarization and developed diversified local financial markets almost entirely in domestic currency. Short durations are pervasive but these countries' high real interest rates draw the most attention. So, it is somewhat surprising that not a single study in the empirical dollarization literature deals with the local-currency interest rate as an associated dependent variable.

This article expands the scope of the financial dollarization literature to analyze the effects of systemic risks and deposit dollarization on the real interest rate in emerging economies. Higher systemic risks induce both increased dollarization and higher real interest rates. So, it is not surprising that the raw data here indicate a positive correlation between the local-currency interest rate and actual dollarization. Using an instrumental-variable procedure, the analysis overcomes the simultaneous equation problem and correctly estimates a negative coefficient for the dollarization ratio in the interest rate equation. When investors are offered the possibility of holding more dollar-denominated deposits, they charge a lower spread to hold domestic-currency-denominated deposits.

The econometric investigation here uses the most recent cross-country multiyear data sets developed by international agencies and other researchers. The panel-based results, obtained with instrumental-variable and panel-data econometric techniques, confirm the presumption that price-dilution and debt-default risks increase the real interest rate. They also verify the tradeoff between real interest rates and deposit dollarization—for given systemic risks, an increase in the relative supply of local dollar assets reduces the real returns that investors require on home currency assets, but the magnitude of this effect is small.

The policy implications of the findings are embedded in estimates of the prices that investors charge to hold financial assets, the values of which can be diluted by volatile and accelerating inflation or a high probability of default. The study also finds that investors' memories of past deeds find expression in the yield spreads that emerging market governments are currently required to pay on their debts. In other words, it unveils the numerical magnitudes of the systemic risks underlying high interest rates in emerging market economies. Finally, it provides tentative estimates of the impact of dedollarization policies on domestic-currency interest rates.

Section I describes the panel-based data and empirical methods of estimation. Section II discusses the econometric results. Section III discusses some implications of the findings. The appendix provides details on data sources and procedures.

I. Data and Estimation Methods

In the empirical model here, the interest rate is assumed to be a function of risk- and policy-related variables that are suggested in the financial dollarization literature. It is also assumed that deposit dollarization is negatively associated with the real interest rate. The argument can be summarized as follows. Consider a simple portfolio-allocation model that allows agents to choose between a domestic-currency-denominated financial asset and a dollar-denominated asset (either locally or abroad), assumed to be imperfect substitutes. In equilibrium, a certain amount of dollar assets will be held and a domestic-currency interest rate determined. If the government introduces a restriction on the dollar assets agents can hold, one consequence would be a higher domestic-currency interest rate, to induce agents to hold a large amount of domestic-currency-denominated assets.

The regressors for the real interest rate considered here can be grouped into three types:[2]

- Price-dilution risks, captured by the minimum variance portfolio variable discussed earlier[3] and by a delta-inflation variable (this year's inflation minus the previous year's inflation). The delta-inflation variable is designed to capture a possible inadequacy of the measured real local-currency interest rate, which subtracts ongoing inflation from the nominal interest rate. Suppose that investors are concerned with next-period wealth and extrapolate current inflation trends. Then, when inflation accelerates, they would demand a higher real interest rate as protection.[4]
- Sovereign default risks, captured by a dummy variable indicating whether the country is investment grade or not according to Standard & Poor's[5] and by the country's per capita income, a variable often used in the dollarization literature as a proxy for governance quality (Levy-Yeyati 2006).
- Policy environment variables, captured by a 0–5 scale measuring the degree of legal restrictions to onshore dollar deposits, by a 0–100 index

2. See the appendix for sources and construction details of each variable.

3. Theoretically speaking, Ize and Levy-Yeyati (2003) develop the minimum variance portfolio as a measure of the portfolio share allocated to foreign currency assets that minimizes the variance of a portfolio constituted with local and foreign currency assets. Recently Rennhack and Nozaki (2006) use the minimum variance portfolio in estimates of a dollarization equation for Latin American economies.

4. More specifically, suppose that the extrapolation of inflation by investors implies that expected inflation is a weighted average of current and future inflation rates: $\pi_t^e = (1-\alpha)\pi_t + \alpha\pi_{t+1} = \pi_t + \alpha(\pi_{t+1} - \pi_t)$, where α is the weight. Thus the real interest rate is $RIR \equiv NIR - \pi_t^e = NIR - \pi_t - \alpha(\pi_{t+1} - \pi_t)$, where NIR is the nominal interest rate. The real interest rate can be approximated by subtracting the current inflation rate from the nominal interest rate, and α can be estimated by including the term $(\pi_t - \pi_{t-1})$ among the regressors, as a proxy for the term $(\pi_{t+1} - \pi_t)$.

5. Standard & Poor's specific country ratings, converted into a numerical sequence, were also tested, but the results were poorer and are not reported here. It seems that for real interest rate determination, the speculative grade of the sovereign matters far more than its progress within this specific grade.

TABLE 1. Basic Statistics—Full Sample of 66 Economies

Variable	Mean	Median	Maximum	Minimum	Standard deviation
Real interest rate, average annual (percent)	3.5	3.1	66.2	−38.2	6.3
Nominal interest rate, average annual (percent)	12.7	7.7	92.0	0.1	15.0
Dollarization index (0 − 100, percent)[a]	18.6	9.3	88.4	0.0	22.0
Capital control index (0 − 100)[b]	68.8	62.5	100.0	37.5	21.1
Restrictions (0 − 5)[c]	0.6	0.0	5.0	0.0	1.3
Jurisdictional uncertainty (0 − 100)	35.6	33.0	86.1	0.0	23.9
Minimum variance portfolio (in decimal fraction)	0.5	0.5	1.2	0.0	0.3
Change in inflation rate[d] (percentage points)	−1.4	−0.4	58.1	−65.0	9.7
Inflation, average annual[d] (percent)	9.1	5.2	85.7	−0.8	14.0
Standard & Poor's sovereign ratings (0 − 16)[e]	8.1	7.0	16.0	0.0	4.5
Per capita income (US$)	9,129.3	4,366.5	37,164.6	752.3	9,931.2

[a]Ratio of dollar deposits to total deposits.

[b]A higher number denotes a higher degree of capital mobility.

[c]A higher number denotes more restrictions on residents' holdings of foreign currency deposits.

[d]Consumer price index.

[e]The rating scales were converted to values of 0 to 16, where 0 to 6 are speculative grades and 7 to 16 are investment grades.

Source: Authors' analysis based on data from World Bank (Various years); World Bank (2007); Worldwide Governance Indicators database; IMF (Various years); Edwards (2005); Levy-Yeyati (2006); and Standard & Poor's (2005).

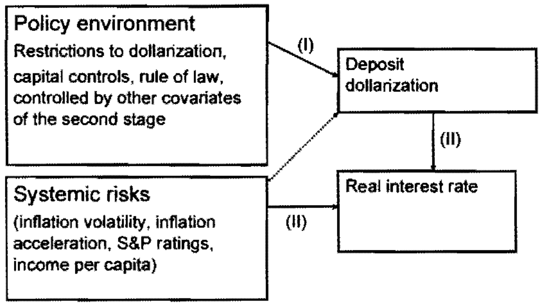
of capital account liberalization constructed by Edwards (2005), and by the complement of the World Bank 0–100 rule-of-law index as in the Worldwide Governance Indicators (World Bank 2007)[6]—the third as a proxy for the "jurisdictional uncertainty" concept proposed in Arida, Bacha, and Lara-Resende (2005) to capture government-related uncertainties besieging financial investors in weak jurisdictions.

In addition, because preliminary analysis of the data indicated that the real local-currency interest rate was a strongly autoregressive variable, its one-year lag was included as a further regressor. Generally, many economic relationships are dynamic, but this inclusion implies some difficulties for the estimations here, soon to be described.

The data set spans 1996–2004 for 66 economies from different parts of the world, including emerging and high-income market economies, so there are

6. Other World Bank institutional quality indicators were tested, with poorer results.

FIGURE 1. Two-step Empirical Strategy



relatively few time-series observations in an unbalanced panel.[7] Table 1 presents basic statistics for the variables in the model. Also reported in this study, they are estimations for dollarized emerging market economies, excluding all advanced economies, even with some dollarization, and also nondollarized emerging market economies. This data set spans 1996–2004 for 33 economies.[8]

Two steps are summarized in figure 1.[9] Step I uses the policy-environment variables, along with other covariates, to generate an instrument for the deposit dollarization ratio. This instrument subsequently enters the equation that determines the real interest rate together with the systemic risk regressors (step II). The two-step procedure is necessary because the real interest rate and the dollarization ratio are jointly determined variables in a supply and demand model for local currency and dollar-denominated assets. Because the dollarization ratio is positively correlated with the error term of the interest rate equation, its coefficient will be positively biased if the dollarization ratio is not properly instrumented using exogenous variables that are noncorrelated with

7. The 66 economies in the sample are, for speculative grade: Argentina, Bolivia, Brazil, Bulgaria, Colombia, El Salvador, Grenada, Guatemala, India, Indonesia, Morocco, Mozambique, Pakistan, Paraguay, Philippines, Romania, Russian Federation, Sri Lanka, Turkey, Ukraine, Uruguay, and Venezuela; and for investment grade: Australia, Austria, Bahrain, Belgium, Canada, Chile, China, Hong Kong (China), Croatia, Cyprus, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Republic of Korea, Kuwait, Latvia, Lithuania, Malaysia, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Singapore, Slovak Republic, Slovenia, South Africa, Spain, Sweden, Switzerland, Thailand, Tunisia, United Kingdom, and United States.

8. The 33 economies in this sample are, for speculative grade: Argentina, Bolivia, Bulgaria, Croatia, El Salvador, Grenada, Indonesia, Mozambique, Pakistan, Paraguay, Philippines, Romania, Russian Federation, Sri Lanka, Turkey, Ukraine, and Uruguay; and for investment grade: Chile, China, Hong Kong (China), Cyprus, Estonia, Hungary, Israel, Kuwait, Latvia, Lithuania, Malaysia, Mexico, Poland, Slovak Republic, Slovenia, and South Africa. According to the basic statistics, the dollarization ratio averages 30 percent, with standard deviation of 23 percent, for this sample of dollarized emerging market economies.

9. The authors are indebted to Fernando Velloso for suggesting this two-step procedure. When they ran instrumental variable estimations, they included in the first stage all the variables that were also included in the second.

TABLE 2. First Step Estimation

Variable	(1)	(2)	(3)
Constant	38.4** (0.045)	37.6** (0.05)	39.1** (0.042)
Restrictions (*R*)	−7.55** (1.75)	−7.50** (2.01)	−7.255** (2.01)
Jurisdictional Uncertainty (*JU*)		0.22** (0.080)	0.25** (0.080)
Capital Control (*CAPLIB*)			−0.095 (0.04)
R^2	0.28	0.31	0.33
No. of countries	66	66	66
No. of observations	369	358	358

Dependent Variable: Dollarization Index (DOLLAR)

*Significant at the 10 percent level.

**Significant at the 5 percent level.

TABLE 3. First-Step Estimation for the Sample of 33 Dollarized Emerging Market Economies

Variable	(1)	(2)	(3)
Constant	36.63** (0.038)	41.16** (0.044)	48.36** (0.057)
Restrictions (*R*)	−9.18** (2.65)	−9.64** (2.57)	−9.41** (2.05)
Jurisdictional uncertainty (*JU*)		0.33** (0.05)	0.33** (0.05)
Capital control index (*CAPLIB*)			−0.03 (0.05)
R^2	0.33	0.35	0.35
Number of countries	33	33	33
Number of observations	285	283	283

Dependent Variable: Dollarization Index (DOLLAR).

* Significant at the 10 percent level.

** Significant at the 5 percent level.

Note: Numbers in parentheses are standard errors. Coefficients and standard errors of the other covariates, as in equation (1), are not reported for convenience. Countries in the sample are, for speculative grade: Argentina, Bolivia, Bulgaria, Croatia, El Salvador, Grenada, Indonesia, Mozambique, Pakistan, Paraguay, Philippines, Romania, Russian Federation, Sri Lanka, Turkey, Ukraine, and Uruguay; and for investment grade: Chile, China, Hong Kong (China), Cyprus, Estonia, Hungary, Israel, Kuwait, Latvia, Lithuania, Malaysia, Mexico, Poland, Slovak Republic, Slovenia, and South Africa.

Source: Authors' analysis based on data from World Bank (Various years); World Bank (2007); IMF (Various years); Edwards (2005); Levy-Yeyati (2006); and Standard & Poor's (2005).

the error term of the interest rate equation and strongly correlated with the dollarization ratio. Appropriate instruments already appear to be in the regressor set as the three policy environment variables—restrictions to dollarization, degree of capital-account liberalization, and rule of law index.

Previous researchers (Levy-Yeyati 2006, for example) showed the fundamental importance of the restrictions-to-dollarization variable in determining actual dollarization, while the initial estimates here (not reported) indicated that dollarization restrictions do not belong in the interest rate equation. The results in tables 2 and 3 indicate the relevance of capital-account controls and the rule of law for the degree of dollarization.

It could be argued that rule of law and capital-account liberalization are not good instruments because of their likely impact on interest rates. Low rule of law could make contract enforcement more difficult. So, other things equal, it could increase the cost of borrowing and be associated with higher real interest rates. Similarly, capital-account restrictions could affect the availability of credit and thus the cost of credit. But, as Fraga (2005) argues, such effects may be relevant for bank credit spreads but are unlikely to affect a country's basic local short-term real interest rate (the interbank or money-market rate), the left-hand variable here, which would normally be firmly anchored on the central bank's repurchase rate. Furthermore, previous panel-based studies (Gonçalves, Holland, and Spacov 2007; Salles 2007) have found that capital controls and rule of law do not belong in the real money-market interest rate equation. It may thus be tentatively concluded that the instruments used here are not correlated with the disturbance term in the equation of interest.

The first step generates an instrument for deposit dollarization, the fitted value of the auxiliary regression:

$$dollar_{it} = \beta_0 + \beta_1 R_{it} + \beta_2 JU_{it} \beta_3 CAPLLIB_{it} + \beta_4 \mathbf{X}_{it} + \eta_{it} \tag{1}$$

where *dollar* is the share of bank deposits in US dollars, t indexes years and i indexes countries, R is the index of restrictions on holdings of foreign-currency deposits by residents (computed by De Nicoló, Honohan, and Ize 2003 and also used by Levy-Yeyati 2006), JU, or jurisdictional uncertainty, is the complement to the World Bank rule of law index, $CAPLIB$ is the capital-account liberalization index constructed by Edwards (2005), $\mathbf{X}_{it}$ is a vector of covariates used in the second stage of the two-stage estimation, and η is the error term. This equation was estimated according to a random-effects model[10] to generate the instrumental variable for the dollarization ratio (D^*) subsequently used in the second-step regression for the interest rate equation.

The general equation for the second-step estimation of the real interest rate (r) is as follows:

$$\begin{aligned} r_{it} = \gamma_t + \omega_i + \beta_1 r_{it-1} + \beta_2 D^*_{it} + \beta_3 MVP_{it} + \beta_4 \Delta\pi_{it} + \beta_5 IGRADE_{it} \\ + \beta_6 y_{it} + \varepsilon_{it} \end{aligned} \tag{2}$$

where γ_i and ω_i are time- and country-specific effects, D^* is the instrument for the dollarization ratio, MVP is the minimum variance portfolio, $\Delta\pi$ is the change in the consumer price index inflation rate, $IGRADE$ is sovereign risk measured by the Standard & Poor's ratings as captured by a dummy variable for the investment grade category, y is per capita income, and ϵ is the

10. A fixed-effects model was also estimated, but a Hausmann test showed that a random-effects model fitted better, possibly because some explanatory variables of this first stage have limited time variation.

error term. Further details on each of these variables are provided in the appendix.

For dynamic panel-data models, the ordinary least squares estimator is known to be biased and inconsistent. In dynamic panel-data models, where the autoregressive parameter is moderately large and the number of time series observations is moderately small, as in the data set here, Blundell and Bond (1998) find the widely used linear generalized method of moments (GMM) estimator obtained from the first differences of the sample variables to have large finite sample biases and poor precision in simulation studies. Lagged levels of the series provide weak instruments for first differences in this case (see Alonso-Borrego and Arellano 1999 and Blundell and Bond 1998).

When estimating a dynamic model for the equation of the real interest rate, the authors were interested in transformations that allowed using lagged endogenous variables as instruments in the transformed equation. Thus, to estimate the real interest rate equation with its one-year lagged value as one of the regressors, they adopted a two-step GMM system estimation (level and difference combined) proposed by Blundell and Bond (1998), and based on Arellano and Bond (1991) and Arellano and Bover (1995). In this procedure, the one-year lagged real interest rate is treated as an endogenous variable, and the two-year lagged real interest rate is an additional instrument.[11] Also used was the variance of the two-step estimation to deal with the downward bias in variance estimation in small samples (Windmeijer 2005). The consistency of GMM estimators depends on whether lagged values of the explanatory variables are valid instruments. This was addressed by considering two specification tests. The first is a Sargan test of overidentifying restrictions, which tests the overall validity of the instruments.[12] The second examines the null hypothesis that the error term is not serially correlated.[13] In both tests, the model specifications are supported because the null hypothesis is not rejected (tables 4 and 5).

II. Empirical Findings

Statistical results are reported in tables 2–5. Consider initially the results of the instrumental regression for the dollarization ratio in table 2, for the full sample

11. Several types of panel unit root tests were run, but for convenience the results are not reported here. In general, they strongly fail to accept the null hypothesis of the presence of unit roots. The results are available on request.

12. The two-step version of the GMM system estimator was used to obtain the Sargan test statistics, because the one-step version of the Sargan test overrejects the validity of the set of instruments in the presence of heteroskedasticity. It is well known, however, that the Sargan test may have low power in finite samples. To have some indication of the power of the test, the real interest rate equations with the endogenous one-year lagged value was estimated as an additional (but invalid) instrument in the transformed equations. This test overwhelmingly rejects the null hypothesis of instrument validity.

13. Arellano and Bond's (1991) test of serial correlation suggests that the error terms are white noise.

TABLE 4. Second-Step Estimation for the Full Sample of 66 Economies

Variable	(1)	(2)	(3)	(4)	(5)
Lag of real interest rate (r_1)	0.491** (0.146)	0.501** (0.15)	0.511** (0.149)	0.509** (0.15)	0.510** (0.148)
Dollarization instrument (D^*)	−0.013** (0.005)	−0.0129** (0.005)	−0.0129 (0.005)	−0.012** (0.005)	−0.011** (0.005)
Investment grade dummy ($IGRADE$)		−1.710** (0.551)	−1.714** (0.554)	−1.850** (0.550)	−1.902** (0.741)
Minimum variance portfolio (MVP)			0.288** (0.144)	0.270* (0.133)	0.285** (0.129)
Delta inflation ($\Delta\pi$)				0.011** (0.006)	0.0112** (0.007)
Per capita GDP (Y)					−0.001 (0.001)
Number of countries	66	66	66	66	66
Number of observations	456	456	456	456	456
Specification tests (p-value)[a]					
Sargan test	0.98	0.97	0.98	0.97	0.98
First-order serial correlation	0.001	0.002	0.002	0.000	0.000
Second-order serial correlation	0.455	0.454	0.452	0.543	0.575

Dependent Variable: Real Interest Rate (RIR).

* Significant at the 10 percent level.

** Significant at the 5 percent level.

Note: Numbers in parentheses are standard errors. Coefficients and standard errors of the other covariates, as in equation (1), are not reported for convenience. Countries in the sample are, for speculative grade: Argentina, Bolivia, Brazil, Bulgaria, Colombia, El Salvador, Grenada, Guatemala, India, Indonesia, Morocco, Mozambique, Pakistan, Paraguay, Philippines, Romania, Russian Federation, Sri Lanka, Turkey, Ukraine, Uruguay, and Venezuela; and for investment grade: Australia, Austria, Bahrain, Belgium, Canada, Chile, China, Hong Kong (China), Croatia, Cyprus, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Republic of Korea, Kuwait, Latvia, Lithuania, Malaysia, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Singapore, Slovak Republic, Slovenia, South Africa, Spain, Sweden, Switzerland, Thailand, Tunisia, United Kingdom, and United States.

[a]The p-values of the tests for the first- and second-order serial correlations report the probability of rejecting the null of no serial correlation. Windmeijer (2005) derives a small-sample correction, implemented here.

Source: Authors' analysis based on data from World Bank (Various years); World Bank (2007); IMF (Various years); Edwards (2005); Levy-Yeyati (2006); and Standard & Poor's (2005).

TABLE 5. Second-Step Estimation for the Sample of 33 Dollarized Emerging Market Economies

Variable	(1)	(2)	(3)	(4)	(5)
Lag of real interest rate (*r_1*)	0.520** (0.151)	0.543** (0.149)	0.551** (0.150)	0.548** (0.152)	0.550** (0.160)
Dollarization instrument (D^*)	−0.0155 (0.005)	−0.0145 (0.005)	−0.0151 (0.004)	−0.0158** (0.0045)	−0.0157** (0.0051)
Investment grade dummy (*IGRADE*)		−1.548** (0.401)	−1.550** (0.412)	−1.545** (0.423)	−1.550** (0.450)
Minimum variance portfolio (*MVP*)			0.295** (0.129)	0.275* (0.132)	0.292** (0.128)
Delta inflation ($\Delta\pi$)				0.0241** (0.0001)	0.0225** (0.0001)
Per capita GDP (*Y*)					0.0009 (0.0011)
Number of countries	33	33	33	33	33
Number of observations	283	283	283	283	283
Specification tests (*p*-value)[a]					
Sargan test	0.87	0.82	0.85	0.86	0.89
First-order serial correlation	0.003	0.004	0.002	0.0012	0.0014
Second-order serial correlation	0.389	0.401	0.402	0.405	0.411

Dependent Variable: Real Interest Rate (RIR).

* Significant at the 10 percent level.

** Significant at the 5 percent level.

Note: Numbers in parentheses are standard errors, coefficients and standard errors of the other covariates, as in equation (1), are not reported for convenience. Countries in the sample are, for speculative grade: Argentina, Bolivia, Bulgaria, Croatia, El Salvador, Grenada, Indonesia, Mozambique, Pakistan, Paraguay, Philippines, Romania, Russian Federation, Sri Lanka, Turkey, Ukraine, and Uruguay; and for investment grade: Chile, China, Hong Kong (China), Cyprus, Estonia, Hungary, Israel, Kuwait, Latvia, Lithuania, Malaysia, Mexico, Poland, Slovak Republic, Slovenia, and South Africa.

[a]The *p*-values of the tests for the first- and second-order serial correlations report the probability of rejecting the null of no serial correlation. Windmeijer (2005) derives a small-sample correction, which is implemented here.

Source: Authors' analysis based on data from World Bank (Various years); World Bank (2007); IMF (Various years); Edwards (2005); Levy-Yeyati (2006); and Standard & Poor's (2005).

of economies (66 economies). All coefficients are significant at the 5 percent level, except capital control, and the coefficient of determination (R^2) is 0.33 for the complete model (column 3 of table 2). Local restrictions to dollar holdings have by far the strongest impact on dollarization: as they rise from a minimum of zero to a maximum of 5, dollarization declines by 36.25 percentage points, a figure similar in magnitude, but opposite in sign, to the equation's constant term (which means that, as restrictions reach a maximum, dollarization is practically equal to zero). Jurisdictional uncertainty, as captured by the complement to the World Bank 0–100 rule of law index, is also relevant: as the index rises from zero to 100, dollarization increases by 25 percentage points. In addition, the 0–100 capital-liberalization index has a significant impact: as capital controls drop from a maximum of 100 to a minimum of zero, the dollarization ratio declines by 9.5 percentage points.[14] Estimation results for the restricted sample of 33 dollarized emerging market economies show slight differences (see table 3). Restrictions to dollar and jurisdictional uncertainty remain statistically significant at the 5 percent level, with slightly larger coefficients than for the full sample of 66 economies.

For both the full sample of 66 economies and the restricted sample of 33 dollarized emerging market economies, the instrumented dollarization ratio has a significant negative impact on the real interest rate, as indicated in regression 5 of tables 4 and 5, which includes all variables specified in equation (2). The coefficient of D^* for the full sample is -0.011, which means that as dollarization rises from zero to 100, the interest rate declines by 1.1 percentage points on impact and by 2.3 percentage points in the long run (this last figure is obtained by dividing 100 times the impact coefficient by 1 minus 0.510, where the 0.510 is the coefficient of the one-year lagged interest rate). The coefficient increases slightly to -0.015 for the restricted sample of countries. When actual dollarization is substituted for instrumented dollarization in the regression (data not shown), the coefficient of this variable becomes positive (though not significant). This is not surprising given that higher systemic risks would induce both increased dollarization and higher real interest rates. Thus, the raw data should and does indicate a positive correlation between the local-currency interest rate and actual dollarization. The policy environment variables used as instruments for the dollarization ratio are able to reverse its "wrong" positive sign in the interest rate equation. So, using the instrumental procedure overcomes the simultaneous equation problem and correctly estimates a negative coefficient for the dollarization ratio.

Also tested was whether the policy environment variables—restrictions to dollarization, rule of law, and capital-account liberalization—had some direct explanatory power on the real interest rate. The results were negative,

14. As in equation (1) a vector of covariates used in the second stage of the two-stage estimation was also included. Their coefficients are not reported here for convenience. They are available on request.

confirming and expanding the finding by Gonçalves, Holland, and Spacov (2007) that—contrary to a presumption in Arida, Bacha, and Lara-Resende (2005)—jurisdictional uncertainty (measured by rule of law as here) and capital account controls (measured by a different index from here) were not significant explanatory variables for the real interest rate.[15]

Consider now the effect of the price-dilution risk variables on the interest rate. First, the real interest rate is positively associated with the minimum variance portfolio variable. As that variable increases from 0 to 1, the real interest rate rises by 0.3 percentage point on impact and by 0.6 percentage point in the long run for the full sample of countries. When the sample is restricted to the 33 dollarized emerging market economies, the size and the significance of the coefficient remain quite similar. The coefficient of the inflation-acceleration variable indicates that as annual inflation increases by, say, 10 percentage points (approximately one standard deviation of this variable in the sample), the real interest rate increases by 0.1 percentage point on impact and by 0.2 percentage point in the long run. The median real interest rate in both the full and the restricted samples is 3.1 percent (table 1 for the full sample), so the price-dilution effects do not seem very large.

Both proxies for sovereign-default risk work very well, indicating that they probably capture different aspects of this risk. Particularly significant are the results for *IGRADE*—the dummy variable indicating whether Standard & Poor's rates a country as investment grade. Investment grade status in the complete sample reduces the real interest rate by 2 percentage points on impact and by 4 percentage points in the long run. Per capita income (measured in units of $1,000) also has a very strong impact—an increase in per capita income of $1,000 reduces the real interest rate by 0.8 percentage point on impact and by 1.6 percentage points in the long run.[16,17] For the restricted sample, *IGRADE* as a proxy for sovereign-default risk shows a smaller but still statistically significant coefficient, at 5 percent. Per capita income shows a similar coefficient size, but is not significant.

15. An anonymous referee expressed doubts about the true exogeneity of the policy-related instruments. If those doubts are correct, the instrumental-regression estimate of the (negative) impact of dollarization on the real domestic interest rate may be considered a lower bound for the true (more negative) coefficient.

16. This effect could be highly nonlinear, fading away for the largest per capita income figures, but this nonlinearity could not be captured using either the inverse of per capita income or its squared value.

17. Preliminary estimations also included the ratio of public debt to GDP as a regressor, with disappointingly small estimated coefficients. This might be a consequence of not properly accounting for domestic debt (which is important in many countries—such as Brazil, where it represents more than 80 percent of debt). It may also be that debt to GDP ratios do not capture important fiscal characteristics for each country, such as the degree of flexibility of revenue, expenditure pressures, and contingent liabilities. While fiscal positions are likely important determinants of interest rates, their effects are better summarized by the sovereign default-rating dummy variable used in the regression, as described in Beers and Cavanaugh (2006). So, the debt to GDP ratio is not included directly in the regressions.

In summary, appropriately instrumented financial dollarization has a significant negative impact on the real interest rate, but the economic magnitude of the impact is small. The real interest rate variable is significantly autoregressive, indicating the importance of using a dynamic model. Also established is the negative effect on the real interest rate of price-dilution risks as measured by inflation volatility and acceleration. Investment grade status and per capita income have large negative effects on the real interest rate. No direct effects were found on the real interest rate of rule of law, capital controls, and dollarization restrictions. These variables were shown, however, to be significant as instruments for the dollarization ratio that enters the real interest rate equation. It is important to stress that the results remain similar regardless of the sample of countries. This is likely due to the fact that some advanced economies experience dollarization while some emerging market economies do not allow it. So, the full sample, including advanced economies, appears accurate enough to allow drawing inferences.

III. Conclusions

This article expanded the scope of the financial dollarization literature by analyzing the systemic-risk determinants of the real interest rate in emerging market economies. Of particular interest was investigating the negative relation between deposit dollarization and local-currency real interest rates for a given set of fundamentals. The findings, obtained with panels of 66 and 33 countries for 1996–2004, indicated that deposit dollarization, properly instrumented, has a negative but small impact on the real interest rate.

A dynamic specification accounted for the fact that real interest rate changes are typically smooth (so, it is in general a strongly autoregressive variable). Also established were the negative effects on the real interest rate of price-dilution risks measured by inflation volatility and inflation acceleration. Generally speaking, investment grade status was shown to have large negative effects on the real interest rate: obtaining an investment grade rating would reduce the real interest rate by 2 percentage points on impact and by 4 percentage points in the long run. While no direct effects on the real interest rate were found for rule of law, capital controls, and dollarization restrictions, these policy environment variables were critical instruments for the dollarization ratio entering the determination of real interest rates.

The policy implications are embedded in the estimates of the high prices that investors charge to hold financial assets whose values may be diluted by volatile and accelerating inflation and by a high probability of default. Investors' memories of past deeds find expression in the yield spreads that governments of emerging market economies are required to pay on their newly issued debt. In other words, the numerical magnitudes of the systemic risks underlying the high interest rates in emerging market economies were unveiled.

A weak spot in the analysis is that the sample is short in the time dimension and includes only a limited number of emerging market economies. But the restricted sample of only dollarized emerging market economies can be considered a kind of robustness check on sample size. The estimation results stood when the same sample of countries was used in the first- and second-stage regressions. Future research with an expanded sample should thus seek to uncover dynamic relations not contemplated by the results here. Furthermore, only two of the possible financial consequences of systemic risk were considered: domestic financial dollarization and high real local-currency interest rates. Future research could incorporate financial shallowness, offshore dollarization, short-termism, and indexation as alternative systemic-risk-coping mechanisms in emerging economies.

Appendix. Data Sources and Procedures

Real interest rate (*RIR*). Ratio of one plus the average of the annualized end-of-month money market interest rate in the *International Financial Statistics* (IFS, line 60B, ZF) to one plus the average of the annualized monthly consumer price index variation (IFS, line CPI), minus one, in percentage terms.

Dollarization ratio (*DOLLAR*). Ratio of dollar (or euro) deposits to total bank deposits. *Source*: Levy-Yeyati (2006), who draws on IMF Staff Reports, central bank bulletins, Bolino, Bennett, and Borensztein (1999), De Nicoló, Honohan, and Ize (2003), and Arteta (2005).

Delta-inflation ($\Delta\pi$). Difference between this year's and last year's inflation, both calculated as the average of the annualized monthly consumer price index variation (IFS, line CPI), in percentage terms.

Investment grade (*IGRADE*). Equal to 1 for a sovereign investment grade rating and zero for a speculative grade rating. This variable was maintained constant for each country on the basis of its status in 2004. *Source*: Standard & Poor's.

Jurisdictional uncertainty (*JU*). Equal to 100 minus the World Bank rule of law index, ranging from 0 to 100. With only even-year values for this variable, odd-year values were interpolated. *Source*: World Bank, *Worldwide Governance Indicators.*

Capital account liberalization index (*CAPLIB*). Described in Edwards (2005) and provided by the author. It is a 0–100 scale, with higher values indicating increasing degrees of capital account liberalization.

Per capita GDP (*Y*). Measured in constant 2000 US dollars. *Source*: World Bank, *World Development Indicators.*

Restrictions (*R*). Index of restrictions on holdings of foreign currency deposits by residents, ranging from zero (no restrictions) to 5 (maximum restrictions). *Source*: Levy-Yeyati (2006), based on data from IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions*, revised and expanded by Levy-Yeyati from De Nicoló, Honohan, and Ize (2003) using the same methodology.

Minimum variance portfolio (*MVP*). This variable is derived from a portfolio choice model in which risk-averse local investors choose between a local-currency-denominated and a dollar-denominated asset. As shown in Ize and Levy-Yeyati (2003), if the uncovered interest-parity condition holds, the dollar share of the optimal investment portfolio, which replicates the minimum variance portfolio, is equal to:

$$MVP = [Var(\pi) + Cov(\pi, q)]/[Var(\pi) + Var(q) + 2Cov(\pi, q)]$$

where π is the inflation rate in local currency and q is the real exchange rate. To estimate a country's *MVP* for a given year, monthly data were used on inflation (CPI) and exchange rate changes for that country in that year. *Source*: IMF's *International Financial Statistics*.

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Quantitative Approaches to Fiscal Sustainability Analysis: A Case Study of Turkey since the Crisis of 2001

Nina Budina and Sweder van Wijnbergen

This case study of fiscal sustainability in Turkey after the crisis in 2001 reviews and extends quantitative approaches to fiscal sustainability analysis and brings them together in a user-friendly tool applicable in a data-sparse environment. It combines a dynamic simulations approach with a steady-state consistency approach. It also incorporates user-defined stress tests and stochastic simulations to deal with uncertainty. And it derives the future distribution of debt-output ratios, evaluating the fiscal adjustment required to stabilize them. Value at Risk analysis shows that considerable risks remain unless explicit feedback rules from debt surprises to the primary surplus are implemented. JEL codes: E61, E62, F34, C15

Long-run sustainability has moved to center stage in the analysis of fiscal policy. This reorientation has been part of rethinking the role of government, with less emphasis on active involvement and more on providing a stable environment for the private sector. Unsustainable policies will change and will be expected to do so; they are a natural cause of instability. Fiscal sustainability analysis has thus become a key element of macroeconomic analysis. This study reviews and extends various recent approaches to fiscal sustainability analysis and combines them into one model for a study of fiscal sustainability in Turkey since the 2001 crisis. The analysis is of interest in its own right, but it also demonstrates that the model is useful for widespread application in low-income and semi-industrialized countries.

Fiscal sustainability analysis has, for example, become an important part of the design and evaluation of anti-inflation programs. Many elements of the analysis can be found in the International Monetary Fund's (IMF) workhorse for standby programs, the Polak monetary programming model. And a long

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series of balance of payments crises has been linked to a lack of fiscal sustainability, particularly the series of failed stabilization efforts in Latin America in the late 1970s and 1980s. There, unsustainable fiscal policies, or the anticipation of policy changes because of a perceived lack of sustainability, triggered the balance of payments crises that brought several stabilization programs to a crashing halt. Even the Asia crisis in 1998, where the main protagonists had no major visible fiscal imbalances, has been linked by some to off-budget fiscal liabilities related to implicit or explicit bailout guarantees in the financial sector (Burnside 2005; Valderrama 2005). There is little doubt that countercyclical fiscal policy (allowing deficits to increase in "bad" times) could backfire seriously if fears of fiscal sustainability were to lead to expectations of runaway deficits in response to a deficit increase.

There are thus many reasons to assess the consistency of the various policies and measures brought together under the heading of fiscal policy. Yet in practice the approaches are often ad hoc and qualitative. A standardized framework is not yet available, though there has been substantial progress in various directions compared, for example, with the IMF's early monetary programming model, which can be interpreted as an attempt at fiscal sustainability analysis, with a short-run focus.[1] A major objective of this study and associated software development is to combine and extend the various strands. It uses a modern tool for fiscal sustainability analysis that reflects progress on this topic, links easily with existing data sources, and is user-friendly for the practitioner and country economist.[2]

I. Recent Approaches to Fiscal Sustainability Analysis

So, what are these "various directions" of substantial progress that will be brought together in the model presented here? An early approach to fiscal sustainability analysis is presented in Anand and van Wijnbergen (1988, 1989) and van Wijnbergen (1989). Anand and van Wijnbergen, like the Polak model, tightly link inflation, monetary policy and fiscal deficits, an approach that requires consolidating the central bank into the public sector accounts (Anand and van Wijnbergen 1988). But the focus is more on long-run consistency of inflation targets (and their fiscal impact if achieved), fiscal deficits, and debt management policies. Anand and van Wijnbergen pay a great deal of attention to the structure of the financial system as a key determinant of the link between inflation and inflation tax "revenues." But there is no serious attention to out-of-steady state (debt) dynamics, and no attention at all to uncertainty.

The IMF's efforts (IMF 2002, 2003) have taken a different direction. The Polak model's integration of the central bank and the public sector, more

1. See Anand and van Wijnbergen (1988) for a discussion of the link between the Polak model and fiscal sustainability analysis.

2. The model and associated documentation are available from the authors.

traditionally defined, has apparently been abandoned, and the focus has shifted toward public debt dynamics. Without the integration of the central bank, seigniorage income and the inflation tax have to be dropped from the analysis, so the link with anti-inflation programs cannot be made within the new IMF approach.

There have recently been several extensions to the IMF approach. Celasun, Debrun, and Ostry (2006) introduce uncertainty using simulation methods. They use stochastic properties of key variables determining fiscal deficits to simulate debt dynamics using Monte Carlo simulation techniques and to derive the probability distribution of debt stocks at given moments in the future. The authors also introduce fiscal reaction functions, an important shortcut to modeling the dynamic properties of fiscal policymaking (see Bohn 1998). At the World Bank, the IMF's emphasis on debt dynamics has been combined with Anand and van Wijnbergen's integration of public sector and central bank accounts to reintroduce seigniorage to the analysis, but without the long-run consistency approach and detailed financial sector modeling characterizing the Anand and van Wijnbergen models (see Burnside 2005).

The academic literature has focused mainly on techniques to establish whether historical debt and deficit processes are characterized by unit roots (see Hamilton and Flavin 1986 for an early example). A disadvantage of this line of research is that it is of necessity backward looking, limiting its usefulness after policy reform. More recent work has extended the analysis by resorting to full-fledged dynamic general equilibrium models, in practice at the expense of empirical verification (dynamic, stochastic general equilibrium models are typically "calibrated" rather than estimated). Mendoza and Oviedo (2006) present an interesting variant, deriving maximum debt levels below which governments can be expected to both maintain acceptable expenditures and service their debts. Other research has focused more on detailing the distribution of shocks using modern techniques such as bootstrapping to explore the existence of fat tails and asymmetries. Perotti (2007) provides an overview of the academic research on sustainability.

The approach here combines all these strands into a simple template usable for country economists yet sufficiently rich to incorporate the results of research. The model follows Anand and van Wijnbergen (1988) by tightly integrating the central bank into the public sector, which reintroduces seigniorage income and the inflation tax into the analysis. This allows analysis of the consistency and sustainability of inflation targets within a given set of fiscal plans, a crucial point since structural reform packages are often embedded in a macroeconomic stabilization program. But the study goes beyond the steady-state analysis of Anand and van Wijnbergen by also introducing debt dynamics, as in IMF (2002, 2003) and Burnside (2005). This permits analyzing how serious the deviations from consistency are, and what the tradeoff is between adjusting now and adjusting later, buying time at the expense of a larger required adjustment.

Possibly even more important is that explicitly introducing debt dynamics into the analysis also permits introducing debt structure and the attendant exposure to risk. Vulnerability to sudden stops of external financing is often related to debt structure, especially if there are high peaks in refinancing needs. This can occur when a substantial part of the debt is issued at short maturity or indexed to foreign exchange. Debt issued in or linked to foreign exchange leads to large capital losses after devaluations of the exchange rate. Analysis of these issues requires explicitly introducing the structure and composition of public sector debt.

Moreover, the vulnerability of market-access countries to sudden stops and reversals of capital inflows makes it critical to incorporate uncertainty in the analysis of fiscal sustainability. Uncertainty surrounding public debt dynamics is often related to uncertainty about medium-term projections of the economic growth rate, the primary balance, the cost of public sector borrowing, and the existence of either explicit or implicit guarantees of debt or bank deposits.

This study uses two approaches to introduce uncertainty and risk to the analysis. To deal with vulnerability to specific shocks, the template provides a variety of single-factor stress tests, as used for example in IMF (2002, 2003). To get a broader view on the riskiness of the basic projections, the study also introduces the possibility of a full set of stochastic simulations using the stochastic properties of key variables in debt dynamics.

Stress tests provide valuable insights into the robustness of the projections to specific shocks to individual exogenous variables and allow explicit analysis of the consequences of extreme events. The stochastic simulations approach has the advantage of deriving the entire distribution of future debt stocks, based on stochastic realizations of key debt determinants, and accounting for their variances and covariance structure. The tool can incorporate any number of distributional assumptions on fat tails and asymmetries, though the default assumption is a multivariate normal. The simulations allow using a Value at Risk approach, or calculating the likelihood that in a given period maximum "safe" debt levels, like the ones derived in Mendoza and Oviedo (2006), will be exceeded with more than a certain threshold probability. Fan charts represent the results, as in Celasun, Debrun, and Ostry (2006). The fiscal sustainability tool goes beyond Celasun, Debrun, and Ostry (2006) in presenting not just the distribution of debt-output ratios at various moments in the future, but also the distribution of the fiscal adjustment necessary to restore consistency and stability. This measure could be more useful to policymakers.

Stochastic simulations can be misleading if they assume unchanged primary deficits after stochastic shocks while the government has a record of responding to debt shocks by tightening its belt. So, the analysis here also incorporates fiscal policy reaction functions and endogenous debt feedback rules for the primary surplus as an additional option for stochastic simulations and other stress tests, as suggested in Bohn (1998) and Celasun, Debrun, and Ostry

(2006). Bohn (1998) shows that fiscal reaction functions are a key requirement for the stability of fiscal programs.

Section II outlines the analytical framework, and sections III and IV demonstrate the application of the fiscal sustainability tool for the case of Turkey.

II. Analytical Framework

Important to stress from the outset is that the fiscal sustainability analysis presented here is not based on a fully specified model. Rather, an accounting framework is the basis of fiscal sustainability analysis, with the exception of the fiscal policy reaction function parameter and the parameters of the stochastic processes used in the simulations. The fiscal sustainability analysis template thus does not provide a tool to set policy variables optimally, such as maximizing a particular welfare function. The more modest goal is to assess the sustainability of whatever policy package is chosen: What is the likelihood that solvency limits will be breached at unchanged policies?

Solvency and Debt Dynamics

Solvency is not much more than an intertemporal extension of staying within one's means: a government is solvent if it does not intend to spend more than its income and initial wealth (or minus net debt). The intertemporal equivalent of staying within one's means implies that the discounted value of current and future income plus initial wealth should at least be equal to the discounted value of all current and future noninterest expenditure. Interest expenditure and income are incorporated through the discounting procedure. Formally, this comes down to:

$$\begin{aligned} b_0 + \sum_1^{\infty} \frac{g_i}{(1+r)^i} &= \sum_1^{\infty} \frac{t_i + s_i}{(1+r)^i} \\ b_0 &= \sum_1^{\infty} \frac{ps_i + s_i}{(1+r)^i}. \end{aligned} \tag{1}$$

Equation (1) states that initial debt, b_0, plus the discounted value of all noninterest expenditure, g_i, should (at most, but equality is assumed in what follows) equal the discounted value of all public sector noninterest income, here summarized as the sum of tax revenues, t_i, and seigniorage revenues, s_i. Seigniorage is the net income the public sector derives from issuing money. This can be rewritten as the second line in equation (1): initial (net) debt should at most equal the discounted value of the primary or noninterest government surplus, ps_i, plus seigniorage revenues, s_i.

To understand the implications and structure of equation (1), it helps to write a simpler construct, the so-called flow budget constraint:

$$b_t = b_{t-1}(1+r) - (ps_t + s_t)$$

(2) or, equivalently:

$$b_{t-1} = \frac{ps_t + s_t}{1+r} + \frac{b_t}{1+r}$$

Equation (2) states that initial debt plus interest payments plus the primary deficit (or rather minus the primary surplus), and, finally, minus seigniorage revenues, equal the new level of debt. This can be rewritten as an expression for initial debt, as is done in the second line of equation (2), where initial debt equals the discounted sum of the primary surplus plus seigniorage and the end of period debt. This way of writing the flow budget constraint should also show how discounting takes care of interest payments. Including them in the deficit definition would essentially count them twice. Substituting equation (2) repeatedly into itself, for t starting at 0, yields:

$$\begin{aligned} b_0 &= \frac{ps_1 + s_1}{1+r} + \frac{b_1}{(1+r)^1} \\ &= \frac{ps_1 + s_1}{1+r} + \frac{ps_2 + s_2}{(1+r)^2} + \frac{b_2}{(1+r)^2}\,. \\ &= \lim_{t\to\infty} \sum_{1}^{t} \frac{ps_i + s_i}{(1+r)^i} + \lim_{t\to\infty} \frac{b_t}{(1+r)^t} \end{aligned} \qquad (3)$$

Combining equation (3) with equation (1) shows that solvency requires:

$$\lim_{t\to\infty} \frac{b_t}{(1+r)^t} = 0. \qquad (4)$$

In words, the debt should ultimately not grow faster than the rate of interest.

This result is behind the econometric approaches to testing for solvency mentioned in the introduction. Hamilton and Flavin (1986) use equation (4) as a starting point for a series of unit root tests to establish the compliance of a given time series of debt stocks with equation (4). Passing the unit root test (first for the budget surplus, then for the debt stocks) means that the process, if it continues to conform to the econometrically recovered structure, will be within solvency limits. Failure does not necessarily mean insolvency, however, since the test will also fail when b_t converges to a positive number, but at a rate that is positive but lower than r. A variant on the Hamilton–Flavin approach uses regression analysis to see whether coefficients on terms proportional to $(1+r)^t$ are significant in an equation linking debt to past deficits and to past debt stocks (see Burnside 2004 for a survey of research in this area). This second econometric approach also has weak power against near-alternatives.

But there is a bigger problem with econometric approaches: they are not really useful for policy analysts looking at the stability of reformed budget processes. The reason is that both approaches are backward looking and can by construction say nothing about recently reformed budgetary policies and the resulting debt stocks, since the new processes are without a track record.

Forward-looking Approaches: Dynamic Simulations

Because of the problems with backward looking approaches, the approach here is to take a forward-looking, dynamic simulations approach. The first step is to create a baseline scenario of the likely future time path of the public debt to GDP ratio. The baseline is derived using the flow budget equation (2) to update future debt stocks based on macroeconomic projections of key determinants of public debt dynamics, such as growth, inflation, projected primary surpluses, and interest rates.

To ensure consistency among debt stocks, deficits, and revenues from seigniorage, it is necessary to consolidate the general government accounts with the central bank's profit and loss account (see Anand and van Wijnbergen 1988). Otherwise seigniorage, an important source of revenue in many developing countries, will not show up in the budget dynamics, and debt may be mismeasured by failing to count assets held by the central bank.[3] Public foreign debt should be measured net of the (net) foreign asset holdings of the central bank, and public domestic debt, net of holdings of such debt by the same central bank.

Seigniorage $(g+\pi)m$ equals the real value of the nominal increase in base money, $\Delta M/(PY)$, where π equals the target inflation rate. The first term, gm, equals the increase in real balances that people are willing to hold simply to keep the money-output ratio constant in the face of growth, g. The second term, $\tilde{\pi}m$, equals the increase in nominal money balances necessary to keep the real value of money constant given inflation, $\tilde{\pi}$. The relation between demand for base money and inflation depends, among other things, on the financial structure and regulation (such as reserve requirements). So, it is likely to change after financial sector reform (see Anand and van Wijnbergen 1988 for an extensive discussion).

But given the lack of long time series for many developing countries, and the requirement to make a generally usable template for fiscal sustainability analysis, the fiscal sustainability tool simplifies estimation of the revenue from

3. In Turkey the seigniorage revenues were about 2 percent of GDP in 1991–93, then rose to 2.5–3.5 percent of GDP in the high inflation years 1994–99, and dropped to around 1 percent in the post-crisis period after 2001.

seigniorage by using a simple Cagan money demand function. The only additional requirement then is to estimate the coefficient of the elasticity of money demand to the nominal interest rate (or inflation), which represents the opportunity cost of holding money. The estimated coefficient will determine the amount of seigniorage to be expected given the assumed inflation targets. Of course tracing the budgetary impact of inflation through seigniorage does not constitute an argument to raise inflation. Inflation has high costs, which are not the focus of this model. So, outlining the fiscal consequences of inflation through seigniorage should not be construed as an argument to raise inflation in a response to debt concerns.

Steady-State Consistency and the Required Deficit Reduction Measure

A major disadvantage of the debt simulation approach is that it does not tell policymakers how much adjustment is required to keep the debt-output ratio stable. So, a second indicator is introduced, the required deficit reduction measure *rdr* that gives precisely that information.[4] The *rdr* indicator equals the deficit reduction necessary to stabilize the debt-output ratio at its current value—that is, the value it has in the year for which the *rdr* is calculated. Reducing the primary deficit by *rdr* restores consistency between projected growth rates, interest rates, and inflation targets on the one hand, and the requirement of a stable debt/output ratio on the other.

Incorporating Uncertainty

So far, deterministic paths have been assumed for the underlying variables. But there is little doubt that all input projections are surrounded by a great deal of uncertainty and so are the results of any deterministic analysis. Uncertainty leads to two separate questions, requiring separate approaches for their answer. First, with uncertainty attached to projections of such variables as interest and growth rates and exchange rate developments, how sensitive are the results to a given shock in any of the variables used as input in the exercise? Second, again given the uncertainty surrounding almost all variables, how much confidence can there be in the outcome of the base run? Or, phrased differently and more in line with the Value at Risk approach now commonly used by commercial banks, what is the probability that certain thresholds will not be exceeded in a given period? For the first question stress tests are introduced dealing with specific risks (in the next section). For the second question stochastic simulation methods are resorted to, using empirical information about the distribution of the input variables.

The sensitivity of the results to specific shocks can be tested by conducting sensitivity tests to the baseline scenario, assuming for example that the underlying variables swing away from their means by one or two standard deviations. Examples are stress tests for real interest rates, real output growth,

4. For extensive discussion of this measure see Anand and van Wijnbergen (1988, 1989).

primary balance, changes in the real exchange rate, and unanticipated realizations of contingent liabilities.

Stress tests can also be used to assess robustness to extreme events, possibly involving adverse changes in a variety of input variables ("crisis scenario"). Varieties of such scenarios are included and can be run at the discretion of the user. Under a full Monte Carlo simulation, extreme events are just one of many realizations and will largely be averaged out. Demonstrating robustness under extreme events may contribute importantly to credibility, so stress tests focusing on extreme events are a useful tool.

An alternative to stress tests is a full-scale Monte Carlo simulation (IMF 2003; Celasun, Debrun and Ostry 2006). Using estimated parameters of the joint distribution of all input variables, the distribution of these variables can be jointly simulated using Monte Carlo methods. This implies that for n input variables and a horizon of T years, $n \times T$ random numbers are generated repeatedly until the generated and empirical distribution are sufficiently close (5,000 runs are generated by default). And for each run, the model is applied to derive the full path of debt stocks and values of the required deficit reduction measure *rdr*. In this way, the full probability distribution of debt-output ratios and the *rdr* measure at each future point in time is derived. The probability density of the outcomes of the debt ratio and of the *rdr* measure of necessary fiscal adjustment in each year can be plotted from the stochastic simulations, generating a "fan chart" for the debt to GDP ratio and the *rdr* variable.

One way of obtaining the relevant variance–covariance information is to run a Value at Risk on historical variables and transform the generated random numbers in such a way that the resulting distribution conforms to the Value at Risk estimates of the true distribution of the input variables.[5] For a multivariate normal, a transformation using the Cholesky decomposition of the empirical covariance matrix can transform independent and identically distributed generated random variates into variates corresponding to the empirical distribution (Bandiera and others 2007). Alternative distributional assumptions can also be incorporated.

Both stress tests and Monte Carlo simulations may overestimate the impact of shocks, since the government may take deliberate corrective actions as its debt stock rises. Bohn (1998) shows that, if all other determinants of fiscal policy are stationary, a positive correlation between the primary surplus and the past level of the public debt to GDP ratio is sufficient to guarantee fiscal sustainability. The fiscal sustainability template therefore also provides the option to simulate with a fiscal policy reaction function, introducing feedback from deviations from base-level debt stocks to deviations from base-level primary surpluses.

5. These days, most middle-income countries have data series long enough do to meaningful simple Value at Risk regressions, particularly if quarterly data can be used.

A cautionary note may be in order. The Monte Carlo analysis outlined here presumes that the stochastic structure prevalent in the past will persist in the future. That may not be the case, particularly after reform periods, although the Value at Risk is not used for the budgetary processes and debt stocks are most likely changing. Moreover, simulating empirical distributions based on point predictions to get the mean and using estimated covariance matrices to generate the random terms around those predictions, do not incorporate uncertainty about the empirically obtained estimates for the parameters of that distribution. So, the distributions are simulated conditional on obtained prediction means. Another way of introducing uncertainty in modeling would be to explicitly model risk premia and their impact on asset returns (see Budina and van Wijnbergen 2007, where that is done, but it is also explained that the data requirements of doing so with sufficient empirical content would preclude application to most developing countries of interest).

III. Sustainability of Public Debt in Turkey: Stress Tests

Throughout the 1980s and 1990s there have been many attempts to stabilize inflation in Turkey, but all of them failed until recent successes changed the pattern. After each failed attempt, inflation jumped to a higher rate. Similarly, the debt to GNP ratio increased steadily from 1990 onwards, doubling through 1999, and tripling through 2001 (from 30 percent in 1990 to 90 percent).

The latest financial crisis seems to have triggered a break in the cycle of high inflation, failed stabilization attempts, rising debt burdens, and higher inflation. Following the crisis, Turkey adopted economic reforms to address the underlying economic vulnerabilities and regain macroeconomic stability. These reforms led to high primary surpluses (about 6 percent of GDP for the past five years), which reduced the overall fiscal deficit substantially, as improved credibility led to lower interest rates. The large fiscal adjustment, with robust economic growth and a strong real appreciation of the currency, led to a sizable reduction in the public debt ratio. The composition of public debt also improved considerably, as the share of foreign-currency-denominated debt fell. And the average annual inflation rate fell to single digits in 2004 and declined further in 2005.

An important question is whether these positive developments are likely to continue. Can Turkey grow out of its debt problem? Is the current fiscal policy sustainable and consistent with single-digit inflation? To answer these questions, some stress tests are presented in this section, and the results of a full Monte Carlo simulation in the next.

Public debt dynamics are derived using a variant of equation (2), after first consolidating the central bank and distinguishing foreign and domestic debt. Then increases in net public debt (measured net of the net foreign asset and public debt holdings of the central bank) can be decomposed in various contributing factors. Switching to ratios to GDP and indicating them by a tilde above the relevant variables, public debt dynamics can be broken down into several components: the primary fiscal deficit net of seigniorage revenues, growth-adjusted real interest payments on domestic debt, and growth-adjusted real interest payments on external debt, including capital gains and losses on net external debt due to changes in the real exchange rate:

$$\dot{\tilde{d}} = (\tilde{pd} - \tilde{s}) + (r - g)\tilde{b} + (r^* + \hat{e} - g)(\tilde{b}^* - \tilde{nfa}^*).e + OF \quad (5)$$

where $\tilde{d}_t$ is the net public debt to GDP ratio, $\tilde{pd}_t$ is the primary deficit as a share of GDP, r is the real interest rate on domestic debt, g is the real GDP growth rate, r^* is the real interest rate on external debt, $\hat{e}$ is the change in the real exchange rate (with $\hat{e} > 0$ denoting a real exchange rate depreciation), and e is the real exchange rate. A catch-all term, other factors, OF, collects residuals due to cross-product terms arising from the use of discrete time data (see annex A.1 in Bandiera and others 2007 for explicit discrete time formulas) and the impact of debt-increasing factors that in a perfect accounting world would be included in deficit measures but in the real world are not. Examples are contingent liabilities that actually materialize, such as the fiscal consequences of a bank bailout and one-off privatization revenues.[6] Note that in this single-equation exercise, debt levels are generated, but all other variables are considered exogenous (feedbacks from shocks to debt levels are not incorporated).

To derive public debt dynamics for the consolidated public sector, the fiscal sustainability tool also provides an estimate of the revenue from seigniorage during the projection period. Key inputs in the calculations of seigniorage revenue are projections for average inflation rate, average price level, and real GDP. A simple Cagan money demand function is assumed for base money, which because of the presence of unit roots had to be estimated in an error correction model framework (see table 1, with obvious definitions of variables).

The implied long-run value of the semi-elasticity of money demand to inflation is $-(-0.21039/-0.669) = -0.31$. Seigniorage revenue is projected at about 0.6 percent of GDP on average during the projection period. The value of 0.31 gives a maximum seigniorage inflation rate (the inverse of the semi-elasticity of inflation) equal to 318 percent, very close to the value found with the detailed financial sector model estimates in Anand and van Wijnbergen (1988).

The baseline scenario for public debt dynamics assumes continued commitment to the high primary surpluses of 2000–05 over the projection period (6.5 percent of GDP). Consumer price index inflation is expected to decline further, consistent with the recently adopted inflation targeting rule. Real interest rates on domestic public debt are projected to stabilize at about 10 percent a year and those on external public debt at about 8 percent. The real exchange rate is

6. See Brixi and Schick (2002) and IMF (2005) for a more detailed discussion on contingent liabilities.

TABLE 1. (Base) Money Demand Estimates

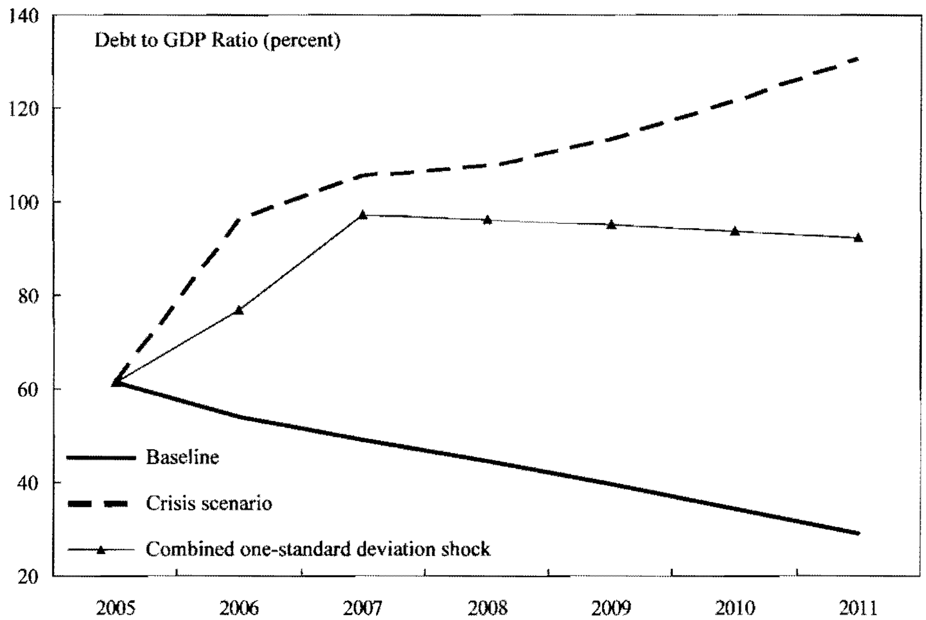
Variable		Coefficient	Standard Error	*t*-Statistic	Probability
C		−0.787	0.149	−5.284	0
INF(−1)		−0.210	0.138	−1.523	0.132
LM0Y(−1)		−0.669	0.126	−5.301	0
D(INF)		−0.315	0.097	−3.253	0.002
T		−0.005	0.002	−2.556	0.013
R-squared	0.381				
Adjusted *R*-squared	0.347				
Standard error of regression	0.231				
Sum squared residual	3.849				
Log likelihood	6.087				
Durbin–Watson statistic	1.862				
Mean dependent variable	−0.005				
Standard deviation dependent variable	0.286				
Akaike info criterion	−0.028				
Schwarz criterion	0.124				
F-statistic	11.076				
Prob(*F*-statistic)	0				

Source: Authors' analysis based on data from International Monetary Fund, International Financial Statistics database.

also assumed to remain broadly stable. Following the strong performance in 2002–04, growth rates are expected to stabilize at 5 percent, the potential growth rate. As a result, in this baseline estimation, the net debt to GDP level continues to decline, falling below 60 percent in 2006 and below 50 percent in 2008 and reaching 29 percent in 2011 (figure 1). The projected trajectories for the exogenous variables are arbitrary in that they are not derived from any underlying model (the Value at Risk estimations were not used to generate them).

The baseline scenario may be rather optimistic, particularly on maintaining the very high primary surplus. To assess the robustness of the base case, various stress tests were performed. One scenario is a structured "extreme event" scenario corresponding to a recurrence of the 1999–2001 crisis (labeled "crisis scenario" in figure 1). This scenario was formulated to illustrate risks related to a possible loss of market confidence. The key assumption is that the political will to continue the fiscal program falters, so the projected primary surplus does not exceed 3 percent of GDP. Weaker fiscal efforts will again trigger a loss of market confidence, exchange rate instability, and thus a worsening debt dynamics. A loss of market confidence is simulated through a sudden stop in the availability of foreign financing, assuming no new disbursements in 2006. This means that all the interest and principal falling due on external debt has to be repaid, so the government has to issue new domestic debt not just to finance its current deficit but also to pay back external debt falling due.

FIGURE 1. Baseline Projections for Net Public Debt to Gross Domestic Product Ratio



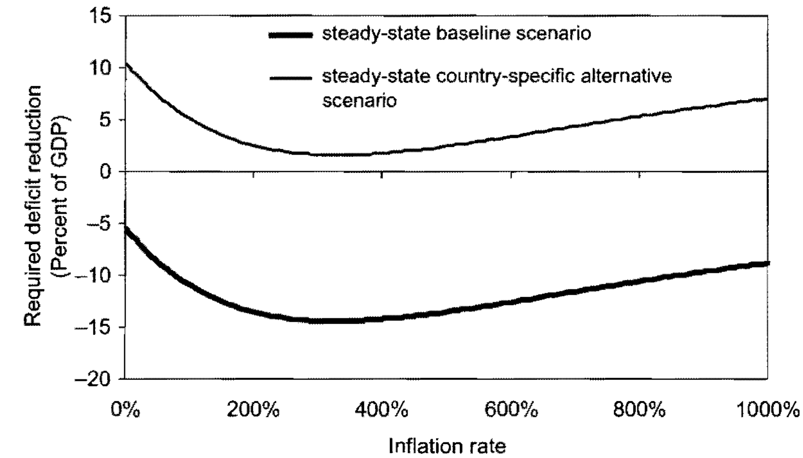
Source: Authors' analysis based on data from the International Monetary Fund (IMF) *World Economic Outlook* (various years) and IMF staff estimates.

In the crisis scenario, exchange rates overshoot initially, as they did in 1999–2001, leading to a substantial real depreciation (30 percent in 2006 and 15 percent in 2007). The depreciation puts pressure on domestic real interest rates, which increase to 40 percent in 2006 and 20 percent in 2007, again following the recent crisis pattern. Similarly, the loss of market confidence is reflected in a higher default premium, peaking at 12 percent in 2006 and 2007. As a result, the economy is assumed to contract by 6 percent in 2006 and then recover, but at a much slower pace. The sizable depreciation is also assumed to create pressures in the banking system, adding up to some 15 percent of GDP with the recognition of contingent liabilities in public debt. As a result, the public debt ratio is projected to increase to 130 percent of GDP by 2011.

An alternative scenario is more mechanical: four key variables deteriorate by one standard deviation for two years (GDP growth, the two real interest rates, and the primary surplus). In those two years the debt-output ratio deteriorates rapidly, to stabilize afterward at a high 100 percent of GDP (combined one standard deviation shock in figure 1).

The stress tests show that the favorable baseline scenario is not robust. Adverse shocks that are not all that different from the recent shocks could rapidly reverse the favorable debt trends of the baseline scenario.

FIGURE 2. Required Deficit Reduction for Different Inflation Rates



Source: Authors' analysis based on data from the International Monetary Fund (IMF) *World Economic Outlook* (various years) and IMF staff estimates.

Finally the *rdr*, the fiscal consistency measure, is calculated for the baseline and the crisis scenarios. The baseline assumptions of continued large fiscal primary surpluses and strong growth create substantial headroom for seigniorage and debt carrying capacity, given stable debt-output targets (figure 2). If the favorable macro data persists as assumed in the baseline scenario, there is substantial overadjustment: more than 5 percent of GDP, an overshooting policy that seems wise given the continuing problems of external credibility.

There should be concern about the robustness of the favorable baseline scenario. The *rdr* measure in the crisis scenario turns positive for all inflation rates and reaches 10 percent of GDP at the currently (2007) targeted inflation rate of 3.5 percent.

IV. Sustainability of Public Debt in Turkey: Stochastic Simulations

Stochastic simulations are used to account for the high volatility of key variables, such as real interest rates, real growth rates, and changes in the real exchange rate. A Value at Risk model is estimated, using the time series for 1990q1–2004q4: the log-difference of the real effective exchange rate (dlog (*reer*)), real GDP growth (dlog(*gdpr*)), and real domestic interest rates (*rdom*) from the International Finance Statistics database and IMF staff estimates. For foreign interest rates, the US interest rate on treasury paper (*rtreas*) is used.

Unit roots of individual time series are checked to make sure that they are stationary, performing augmented Dickey–Fuller tests on each series. Unit roots at the 5 percent level are rejected for all series (table 2). With all variables

TABLE 2. Unit Root Tests, Quarterly Data, 1990q1–2004q4

Variable from Value at Risk	With Intercept Only	With Intercept and Trend
dlog*(reer)*	−6.591**	−6.555**
dlog*(gdpr)*	−8.227**	8.167**
rdom	−8.821**	−9.212**
rtreas	−3.198*	−5.558**

*Reject unit root at 5 percent level; **reject unit root at 1 percent level.

Source: Authors' analysis based on data from International Monetary Fund (IMF), International Financial Statistics database and IMF staff estimates.

TABLE 3. The Appropriate Lag Structure

Lag	Schwarz's Bayesian Information Criterion
0	23.9938
1	19.9415*
2	20.5332
3	21.204
4	21.485

*Indicates lag order selected by the criterion at 5 percent level of significance.

Source: Authors' analysis based on data from International Monetary Fund (IMF), International Financial Statistics database and IMF staff estimates.

stationary, ordinary least squares can be used without concern about spurious regression results.

Since the tool performs stochastic simulations based on annual data for the public debt-updating equation, these variables had to be annualized before estimating a Value at Risk model, using an appropriate lag structure on the basis of Schwarz's Bayesian information criterion, which selected just one lag (table 3).

Table 4 shows the estimated coefficients of the Value at Risk model. The normality of the error structure was tested by performing a Cholesky transformation and testing the resulting four orthogonal empirical distributions for skewness and kurtosis. The Jarque-Bera test was also performed on each. The tests in the four dimensions separately rejected skewness and kurtosis for all (transformed) variables, and the Jarque-Bera test also accepted normality. On the four together the skewness and Jarque-Bera test accepted normality at a 5 percent level, and the kurtosis test at a 2.5 percent level (but on the four transformed variables individually at the 5 percent level). So, normality is assumed in what follows.

The estimated Value at Risk model corresponds to the variance–covariance matrix in table 5.

TABLE 4. Estimated Value at Risk Model for Turkey for 1990–2004

	Depreciation of RER	Domestic Interest Rate	Foreign Interest Rate	Growth Rate
Depreciation of RER (−1)	0.6164132***	−0.0764374	0.0071392***	−0.0163632
	(0.1214663)	(0.0745745)	(0.0032883)	(0.0578173)
Domestic interest rate (−1)	0.5183742**	0.7703966***	−0.0034098	0.2292575***
	(0.1300432)	(0.0798403)	(0.0035205)	(0.0618999)
Foreign interest rate (−1)	2.613194**	0.1130593	0.9929505***	0.8727535
	(1.310771)	(0.8047509)	(0.0354851)	(0.6239205)
Growth rate (−1)	0.0078362	−0.0719824	−0.0050722	0.6440931***
	(0.2706376)	(0.1661585)	(0.0073267)	(0.1288221)
Constant	−17.6484**	3.862517	0.0658478	−5.609101*
	(6.057141)	(3.718795)	(0.1639787)	(2.883169)

*Significance at the 10 percent level.

**significant at the 5 percent level.

***significant at the 1 percent level.

Note: Numbers in parentheses are standard errors.

Source: Authors' analysis based on data from the International Monetary Fund (IMF), International Financial Statistics database and IMF staff estimates.

TABLE 5. Estimated Variance–Covariance Matrix ($\hat{\Sigma}$)

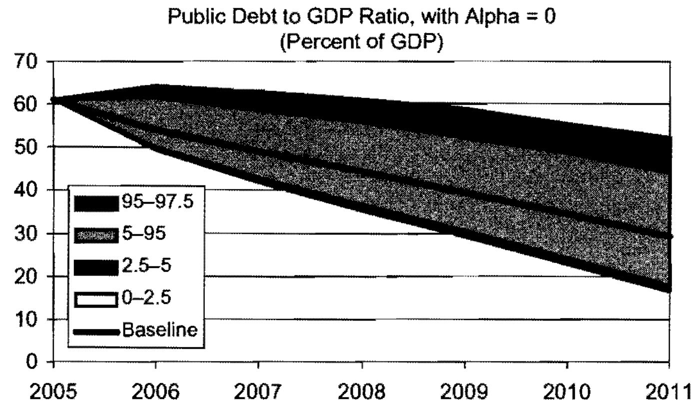
	Depreciation of RER	Domestic Interest Rate	Foreign Interest Rate	Growth Rate
Depreciation of RER	81.1591	−6.30625	−0.165489	23.1178
Domestic interest rate	−6.30625	30.5919	−0.164606	7.36655
Foreign interest rate	−0.165489	−0.164606	0.059481	0.112955
Growth rate	23.1178	7.36655	0.112955	18.3883

Source: Authors' analysis based on data from the International Monetary Fund (IMF), International Financial Statistics database and IMF staff estimates.

The Cholesky decomposition of the covariance matrix of table 4 is used in the stochastic simulations. A series of orthogonal independent variables is generated on [0,1] and transformed into orthogonal normal variables with variance one by applying the inverse normal density function. These variates are then transformed into normal variates with the appropriate covariance matrix by multiplying each vector of drawings by the inverse of the Cholesky matrix. This procedure produces a distribution matching the empirical estimates of the Value at Risk results. The fiscal sustainability tool then simulates the model in response to the shocks generated that way and produces a "fan chart" of the resulting debt to GDP ratios (figures 3 and 4).

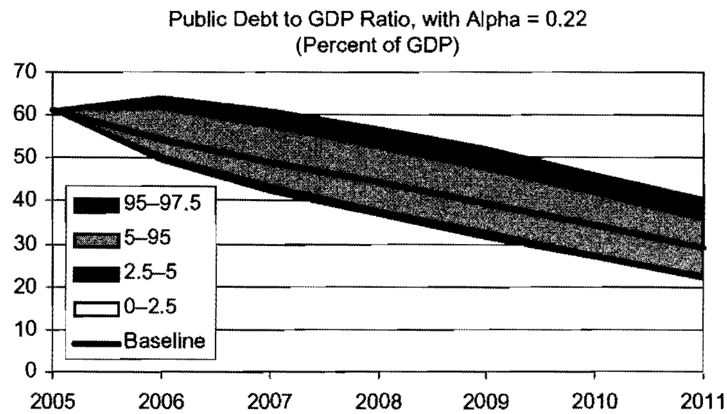
Figures 3 and 4 show the distribution of debt stocks as a function of time that comes out of the simulations. Figure 3 indicates a 50 percent chance that public debt will be below 29 percent of GDP at the end of the projection period: the chance that public debt will be below 50 percent at the end of the projection period is a high 95 percent. Although extreme events may throw the

FIGURE 3. Fan Chart for Public Debt to GDP Ratio, with Alpha = 0



Source: Authors' analysis based on data from the International Monetary Fund (IMF) *World Economic Outlook* (various years) and IMF staff estimates.

FIGURE 4. Fan Chart for Public Debt to GDP Ratio, with Alpha = 0.22



Source: Authors' analysis based on data from the International Monetary Fund (IMF) *World Economic Outlook* (various years) and IMF staff estimates.

economy off track, the variance around the baseline due to uncertainty from interest rates, exchange rates, and growth does not really change the basic optimistic note that comes out of the baseline scenario.

With debt staying as high as the upper end of the distribution shown in figure 3, the assumption of unchanging primary surpluses may not be realistic. There is strong evidence that the primary surplus in Turkey is influenced by total government debt: higher debt tends to lead to higher primary surpluses. A simple regression of the primary surplus on past debt and the output gap (and a dummy variable for the crash stabilization program adopted in 2001) confirms that result (table 6), showing a very substantial response coefficient of about 20 percent.

TABLE 6. Fiscal Reaction Function

Dependent Variable: Primary Balance

	Coefficient	*t*-statistic
Lagged debt	0.227***	4.94
Output gap	−0.005	−0.02
Dummy variable for 2001	3.247	0.85
Constant	−10.837***	−4.34
Number of observations	15	
F (3, 11)	8.95	
Prob > *F*	0.003	
R-squared	0.710	
Adjusted *R*-squared	0.630	
Durbin–Watson *d*-statistic	1.392	

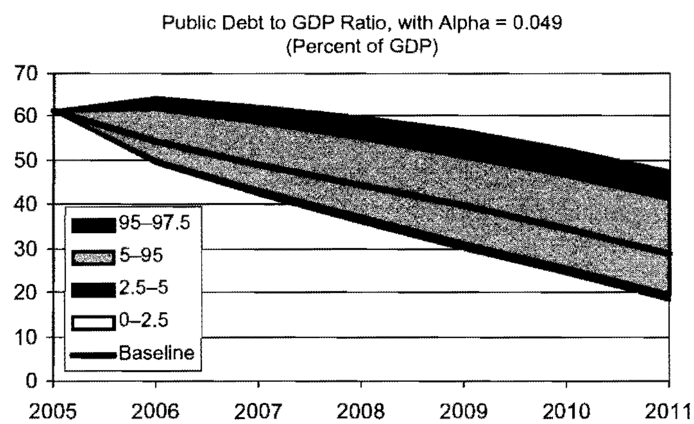
***Significant at the 1 percent level.

Source: Authors' analysis based on data from the International Monetary Fund (IMF), International Financial Statistics database and IMF staff estimates.

If the estimated value for feedback coefficient alpha is introduced to the stochastic simulations, the distribution of debt stocks narrows considerably (figure 4). Instead of fanning out to a 95 percent range of almost 35 percentage points, as in figure 3, the same range narrows considerably once the empirically obtained feedback rule is incorporated. A Value at Risk analysis would thus come up with better numbers. Of course, a regime shift might also mean that the feedback coefficient obtained from past data might lose relevance for analysis of the future.

The high feedback coefficient reflects Turkey's strong adjustment effort after the crisis. Budina, van Wijnbergen, and Li (2008) find a lower coefficient of 4.9 percent in a sample of resource-rich countries, close to the 4.3 percent found

FIGURE 5. Fan Chart for Public Debt to GDP Ratio, with Alpha = 0.049

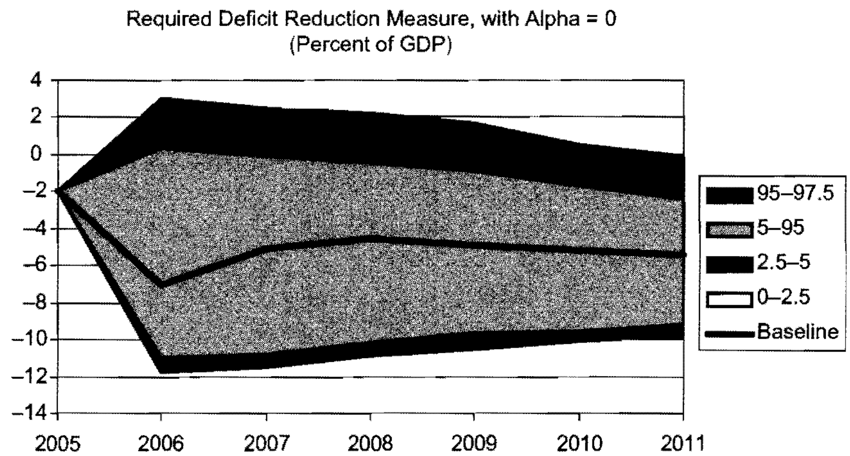


Source: Authors' analysis based on data from the International Monetary Fund (IMF) *World Economic Outlook* (various years) and IMF staff estimates.

by Celasun, Debrun, and Ostry (2006). Redoing the simulation with that lower value of alpha results, not surprisingly, in less variance reduction (figure 5).

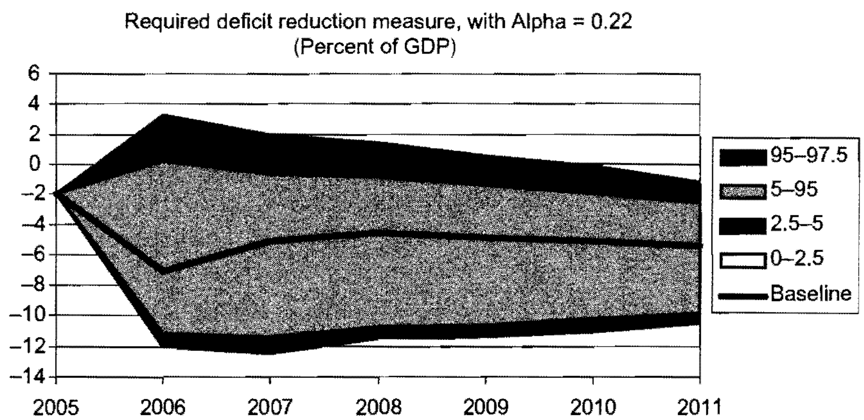
The same Monte Carlo runs can also show the probability distribution of the required deficit reduction measure—the additional fiscal effort needed to stabilize the public debt to GDP ratio in any given year at the value it has in that year with respect to GDP. Figures 6 and 7 show the results, again for a zero alpha (figure 6) and with a feedback rule (figure 7). Figure 6 indicates that if Turkey maintains its sizable primary surplus of 6.5 percent of GNP, there is

FIGURE 6. Fan Chart for Required Deficit Reduction Measure, with Alpha = 0



Source: Authors' analysis based on data from the International Monetary Fund (IMF) *World Economic Outlook* (various years) and IMF staff estimates.

FIGURE 7. Fan Chart for Required Deficit Reduction Measure, with Alpha = 0.22



Source: Authors' analysis based on data from the International Monetary Fund (IMF) *World Economic Outlook* (various years) and IMF staff estimates.

a 97.5 percent probability that fiscal policy will be at least consistent with stable debt ratios and targeted inflation rates in 2011. So there is only a 2.5 percent probability that additional fiscal adjustment may be needed in 2011 to ensure consistency with the adopted inflation targets and to stabilize the debt to GNP ratio at its 2011 level.

Introducing a positive alpha reduces *rdr* at the top of the distribution (now the 97.5 percent boundary is almost −2 percent, indicating overadjustment). Interestingly, no such narrowing occurs at the bottom end of the distribution; the lower end corresponds to higher debt stocks than in the alpha = 0 scenario, so *rdr* does not fall.

V. Conclusions

This article reviewed various quantitative approaches to fiscal sustainability analysis with the objective of combining them into a user-friendly tool that reflects modern developments. Fiscal sustainability is defined as reaching stable debt-output ratios at unchanged policies. This is in most cases ($r > n$) a sufficient though not necessary condition for solvency.

The analysis built on a simplified version of the steady-state consistency approach introduced by Anand and van Wijnbergen (1988), combining it with dynamic simulations (as in Burnside 2005) to be able to analyze debt dynamics. Two methods were incorporated to deal with uncertainty: user-defined stress tests, to assess vulnerability to specific shocks in individual variables and analyze the robustness of strategies under extreme events, and stochastic (Monte Carlo) simulations to derive the complete probability distribution of future debt stocks (though they cannot focus on extreme events). This makes it possible to take a Value at Risk approach to fiscal sustainability analysis. The tool is more policy oriented than most approaches by going beyond distributions of debt stocks to the evaluation of the full future distribution of the fiscal adjustment required to stabilize debt-output ratios (*rdr*). The fiscal sustainability tool incorporates an endogenous debt feedback rule for the primary surplus, a fiscal policy reaction function.

The authors have not taken the step toward full-fledged macroeconomic modeling as the underlying basis for the stochastic simulations, instead relying on a reduced form Value at Risk approach to extract distribution parameters. This enables wide applicability to many countries in data-sparse environments. Alternative, possibly more sophisticated econometric techniques and non-normal distributional assumptions ("fat tails") can easily be incorporated.

The second part of the article presented a fiscal sustainability analysis for Turkey using the new tool. Applying the tool involved:

- Using public debt dynamics to create a baseline projection of future trends in public debt to GDP ratio, with existing macroeconomic projections.

- Conducting sensitivity tests to the baseline scenario, to debt dynamics and to the consistency measure *rdr*.
- Performing Monte Carlo simulations using distributional assumptions obtained from a Value at Risk estimation to derive the distribution over time of debt-output ratios and the required deficit reduction measure *rdr*.
- Checking the sensitivity of the public debt and *rdr* probability distributions to the adoption of an empirically verified fiscal policy reaction function.

The results suggest that if the current fiscal adjustment persists, with primary surpluses of about 6 percent of GDP, there will be a rapid decline in public debt (as a share of GDP) over the projection period. Moreover, stochastic simulations suggest that if the current fiscal strategy is to be maintained, there is still considerable leeway: a 95 percent chance that the public debt ratio will be below 50 percent at the end of the projection period and a 50 percent chance that the public debt ratio will be below 29 percent of GDP at the end of the projection period.

But risks to fiscal sustainability remain. There is concern that the quality and durability of the fiscal adjustment may not be sustainable. In particular, while the overall primary surplus is impressive by international standards, substantial pension deficits crowd out the rest of public expenditure and make it harder for Turkey to sustain this primary surplus. Pension reform may be necessary to allay this fear. A crisis scenario, conducted as a deterministic stress test, shows that the sustainability conclusion is not robust to faltering fiscal reforms, rising interest rates, and a resulting sudden stop of capital inflows. But the Monte Carlo analysis does show that if the fiscal adjustment stays on track, debt stocks are unlikely to derail.

The application to Turkey demonstrates the flexibility and easy applicability of the fiscal sustainability analysis tool presented here. Be aware, however, that all conclusions remain probability statements, with substantial judgmental elements. Despite increasing sophistication of the tools, fiscal sustainability analysis remains as much art as science.

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Globalization and the Gender Wage Gap

Remco H. Oostendorp

There are several theoretical reasons why globalization will have a narrowing as well as a widening effect on the gender wage gap, but little is known about the actual impact, except for some country studies. This study contributes to the literature in three respects. First, it is a large cross-country study of the impact of globalization on the gender wage gap. Second, it employs the rarely used ILO October Inquiry database, which is the most far-ranging survey of wages around the world. Third, it focuses on the within-occupation gender wage gap, an alternative to the commonly used raw and residual wage gaps as a measure of the gender wage gap. This study finds that the occupational gender wage gap tends to decrease with increasing economic development, at least in richer countries, and to decrease with trade and foreign direct investment (FDI) in richer countries, but finds little evidence that trade and FDI also reduce the occupational gender wage gap in poorer countries. JEL Codes: F16, F21, J16, J31, J44, O15

Many studies have analyzed whether globalization has important distributional impacts, across poor and rich countries, across urban and rural regions, and across low- and high-skill workers. Much less attention has been paid to whether globalization affects male and female workers differently. Recent empirical studies from 61 countries indicate that the gender wage gap is still large, amounting to 23 percent in developed countries and 27 percent in developing economies (World Bank 2001, pp. 55–57). Only about one-fifth of this gender gap in earnings can be explained by observed differences in worker and job characteristics, and therefore it is an important question whether globalization will reduce or increase this still significant gap.

The literature suggests several reasons why globalization would have a narrowing effect on the gender wage gap. First, according to neoclassical theory,

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globalization will lead to increasing competitive pressures, making it more costly for individuals and firms to discriminate (Becker 1971). Second, expanding trade will boost job opportunities, increasing the number of women being absorbed in export-oriented industries (Cagatay and Berik 1990; Wood 1991; Joekes 1995; Anker 1998; Standing 1999; Ozler 2000). If the greater demand for female labor increases women's relative wages, the gender wage gap will decline.[1] Third, insofar as trade spurs economic growth with increased investments in infrastructure and improved availability and quality of public services, gender disparities in human capital would tend to fall, and with them the gender wage gap (World Bank 2001).

However, globalization may also worsen the gender wage gap. First, standard trade theory predicts that trade will adversely affect the compensation paid to the relatively scarce factors of production in the economy. If female workers in developed economies have lower average skills than male workers do, then the wages of female workers will fall more than those of male workers as trade with developing economies increases. This skill effect would increase the gender wage gap. The opposite is true for developing economies—their gender wage gap should decrease with increases in trade. Second, globalization through increasing competition may weaken the bargaining power of workers, especially of female workers if they are disproportionately employed in sectors increasingly competing on the basis of "cheap" labor (Seguino 2005). Third, there are complicated linkages between the traded sectors and other sectors in the market economy, as well as between the market economy and the unpaid household economy, where women are the main workers (Fontana and Wood 2000). For instance, if trade leads to more occupational segregation or less leisure time for female workers, women may be less motivated to pursue a life-long career, thereby increasing the gender wage gap.

Only a few country studies have looked at the impact of trade and foreign direct investment (FDI) on the gender wage gap, and most suggest that more trade and FDI reduce the gap (Fontana and Wood 2000; García-Cuéllar 2000; Artecona and Cunningham 2002; Berik, van der Meulen Rodgers, and Zveglich 2004; Black and Brainerd 2004).[2] The country studies look at the impact of trade and FDI on the overall gender wage gap (raw wage gap) or the unexplained gender wage gap (residual wage gap). This study contributes to this literature in three respects. First, it is a large cross-country study of the impact of globalization on the gender wage gap.[3] Second, it employs the rarely used ILO October Inquiry database, the most far-ranging global survey of

1. However, if the increased employment for women is mostly low wage and low skill, the total gender wage gap could increase (Hunt 2002; Blau and Kahn 2003; Olivetti and Petrongolo 2006).

2. Insofar as globalization affects market structure, some country studies suggesting that deregulation, market power, and transition affect the gender wage gap are also relevant (Brainerd 2000; Black and Strahan 2001; Hellerstein, Neumark, and Troske 2002).

3. The move from country- or industry-specific analysis to cross-country analysis is useful because the impact may vary across country characteristics, such as level of economic development, and because the impact may extend beyond an individual industry, due to externalities and spillovers.

wages with information on the gender wage gap in 161 narrowly defined occupations in more than 80 countries for 1983–99 (International Labour Organization various years). And third, it focuses on the within-occupation gender wage gap (occupational gender wage gap), which is analytically and empirically related to the commonly used raw and residual wage gaps.

Section I introduces the ILO October Inquiry database and discusses the occupational gender wage gap as an alternative indicator of gender wage inequity. The occupational gender wage gap is analytically and empirically related to the commonly used raw wage gap and the residual wage gap. Section II discusses the extent to which theories about the impact of globalization on the gender wage gap are also relevant for the occupational gender wage gap and includes a descriptive analysis of the occupational gender wage gap around the world, particularly its relationship with economic development, trade, and FDI. Section III uses a regression framework to examine whether globalization reduces the occupational wage gap.

I. The Ilo October Inquiry and the Occupational Gender Wage Gap as an Indicator of Gender Wage Inequity

The data for this study were derived from the ILO October Inquiry, which collects information on pay (wages, earnings, and hours of work) across detailed occupations at the four-digit International Standard Classification of Occupations (ISCO88) level. The scope of the ILO October Inquiry has been increasing since its inception in 1924, both in country coverage and in number of occupations included. In gender breakdown, the 1983–99 data are the most extensive, providing information on pay for men and women across 161 occupations and 83 countries. However, the number of occupations varies across years for each country, and most countries do not report for each year. The supplemental appendix provides a detailed description of the database (available at http://wber.oxfordjournals.org/).

With the ILO October Inquiry data, it is possible to look up the occupational gender wage gap—the female–male wage difference within an occupation for a given country and year. The ILO October Inquiry does not contain information on employment within occupations, and the data cannot be used to measure the average gender wage gap across workers or to estimate the impact of globalization on female and male employment.[4]

Before analyzing the occupational gender wage gap, the occupational wage gap must be shown to be a useful indicator of gender wage inequity, alongside

4. It has been suggested that sectoral employment weights from other data sources could be used to derive an aggregate gender wage gap. However, for many countries, there are not enough observations with female–male wage data across (even broad) sectors to get a good measure of the overall gender wage gap.

the commonly used indicators of the raw wage gap and the residual wage gap (Blinder 1973; Oaxaca 1973).

First, the occupational gender wage gap can be interpreted as an independent measure of the relative female wage position that abstracts from occupational segregation. Note that the raw wage gap and the occupational wage gap are related through the following identity:

$$(1) \qquad (\overline{\ln W^m} - \overline{\ln W^f}) = \sum_{j=1}^{J} P_j^f (\overline{\ln W_j^m} - \overline{\ln W_j^f}) + \sum_{j=1}^{J} (P_j^m - P_j^f)\, \overline{\ln W_j^m},$$

where $\overline{\ln W^m}, \overline{\ln W^f}$ are the average log wages of men and women; P_j^m and P_j^f denote the occupational distribution of men and women, with $j = 1, \ldots, J$ occupations and $\sum_{j=1}^{J} P_j^m = \sum_{j=1}^{J} P_j^f = 1$; and $\overline{\ln W_j^m}, \overline{\ln W_j^f}$ are the average log wages of men and women within occupation j. Equation (1) indicates that the raw wage gap is equal to the average occupational wage gap (first term on the right side) and an interoccupational component, which represents the part of the raw wage gap that is due to differences in the distribution of men and women across occupations (second term on the right side). Clearly, unless men and women are similarly distributed across occupations (no occupational segregation) or the gender differences in the occupational distribution, $(P_j^m - P_j^f)$, and the wage distribution, $\overline{\ln W_j^m}$, are uncorrelated, the raw and occupational wage gap will differ.

In practice, however, there is a strong correlation between both gender gaps. From the Luxembourg Employment Study and the Living Standards Measurement Study, the raw and occupational wage gaps were constructed for 19 countries (10 developed countries from the Luxembourg Employment Study and 9 developing economies from the Living Standards Measurement Study). The analysis was limited to the most recent survey (Luxembourg Employment Study) or the largest (the Living Standards Measurement Study) that was nationally representative, free of charge, and included occupational information at the two-digit (or higher) level (ISCO88). Gaps were calculated for hourly wages for working people between the ages of 15 and 65. Occupations for which there were fewer than 25 observations for either men or women were excluded.

The overall correlation between the raw wage gap and the occupational wage gap is high, at 0.79. The correlation between the average gender wage gap and the unweighted occupational gender wage gap, $(\frac{1}{J}\sum_{j=1}^{J} (\overline{\ln W_j^m} - \overline{\ln W_j^f}))$, is even higher, at 0.86. This is important, because the ILO October Inquiry lacks information on employment, and the analysis is based on unweighted data.

Second, the occupational wage gap can be viewed as a proxy for gender wage discrimination. The raw wage gap typically measures the gender wage differential for all employed workers or for broad occupational categories. Because of gender differences in human capital, this measure will tend to overstate the actual gender wage gap if these differences were controlled for. Thus many studies have looked at the so-called residual wage gap, which is the female–male wage differential that remains if gender differences in human capital are removed (typically through regression analysis).

In cases where female and male workers in narrowly defined occupations have similar skills, the occupational wage gap provides a direct measure of the residual gender wage gap. The critical issue is then whether the gender gap in skills within occupations is relatively narrow. Although it is straightforward to calculate the gender gap in skills within occupations, there is not one measure of skills but several, such as education, age, work experience, and tenure, and the relative importance of these different gaps is not clear a priori. The Oaxaca–Blinder decomposition of gender wage gaps can be used to calculate first how much of the raw wage gap can be explained by observed skills. Next, one can calculate how much of the raw wage gap can be explained by skills if one also controls for occupation in the Mincerian wage regression by including occupational dummy variables. The results of the Oaxaca–Blinder decomposition for the sample of countries from the Luxembourg Employment Study and the Living Standards Measurement Study are reported in table 1.

In line with the literature, human capital differences can explain only a small part of the total raw wage gap. A sizable residual gender wage gap remains. Important, however, is that when the Mincerian regression also controls for occupational heterogeneity, the explanatory power of human capital differences is significantly reduced (from 0.053 to 0.036 for developed countries and from 0.064 to 0.032 for developing economies). This implies that for given occupations, human capital differences explain little of the gender wage gap.

Even if human capital differences do still explain (a small) part of the raw wage gap, this does not imply that the occupational wage gap is an inferior proxy for gender wage discrimination compared with the residual gender wage gap. First, the occupational information in the Luxembourg Employment Study and the Living Standards Measurement Study is available mainly at the two-digit level, while the ILO October Inquiry data are at the four-digit level, where within-occupation human capital differences should play an even smaller role. Second, the residual gender wage gap controls only for observable human capital differentials, while the occupational wage gap also corrects for unobservable human capital differences to some extent. Third, the occupational wage gap does not rely on a regression model to eliminate the impact of human capital differences.

For these reasons, the occupational wage gap could be a superior proxy for gender wage discrimination compared with the residual gender wage gap.

TABLE 1. Oaxaca–Blinder Decomposition without and with Occupation Dummy Variables

		Explained by skills				Explained by skills	
		Occupation dummy variables				Occupation dummy variables	
Developed country	Raw wage gap	No	Yes	Developing economy	Raw wage gap	No	Yes
Belgium	0.16	0.02	0.02	Albania	−0.01	−0.04	−0.01
Canada	0.21	0.02	0.02	Bosnia and Herzegovina	0.12	0.00	0.00
France	0.19	0.01	0.01	Brazil	−0.09	−0.21	−0.17
Germany	0.34	0.07	0.03	Bulgaria	0.11	−0.03	−0.02
Ireland	0.31	0.09	0.07	Kosovo	0.17	−0.03	−0.02
Luxembourg	0.37	0.13	0.05	Nicaragua	−0.45	−0.17	−0.04
Netherlands	0.25	0.09	0.07	Panama	0.36	0.02	0.03
Spain	0.23	−0.01	0.01	Peru	−0.02	−0.06	0.00
Switzerland	0.33	0.08	0.07	Vietnam	0.13	−0.02	0.00
United States	0.16	−0.01	−0.01				
Average[a]	0.255	0.053	0.036		0.162	0.064	0.032

Note: The following skill measures were included in the Mincerian regressions: dummy variables for levels of education completed, age (squared), potential work experience (if information available, defined as age minus age when obtained highest level of education or training), and tenure (squared). The male wage structure is used to estimate the Mincerian returns to skills.

[a]Average of absolute values.

Source: Author's analysis based on the Luxembourg Employment Survey and the Living Standards Measurement Study.

Thus, both the occupational and residual wage gaps can be viewed as useful proxies for gender wage discrimination.

That being the case, it becomes interesting to look at the empirical properties of both measures. The first desirable property is that they move in the same direction with respect to the raw wage gap. The second desirable property is that both proxies are highly correlated. Both properties hold for the sample of 19 countries (figure 1).[5] The horizontal axis shows the difference between the raw and residual wage gaps. The vertical axis shows the difference between the raw and occupational wage gaps. It is clear for almost all countries that the residual and occupational wage gaps move in the same direction—they lie in either the first or third quadrant.[6] And there is a strong correlation between the residual and occupational wage gaps (the correlation coefficient is 0.83).

II. A Descriptive Analysis of the Occupational Gender Wage Gap Around the World

Does the occupational gender wage gap become larger or smaller with economic development? Figure 2 shows the occupational gender wage gap for 63 countries by the level of economic development measured as the logarithm of GDP per capita (in constant 1995 US dollars).[7]

The overall average occupational gender wage gap is 0.11 across all countries in the data set.[8] There is a nonlinear and inverted U-shaped cross-section relation between the occupational gender wage gap and the level of economic development.[9] This suggests that the expected negative relation

5. The figure reports the unweighted occupational gender wage gap—the results for the weighted occupational gender wage gap are virtually the same.

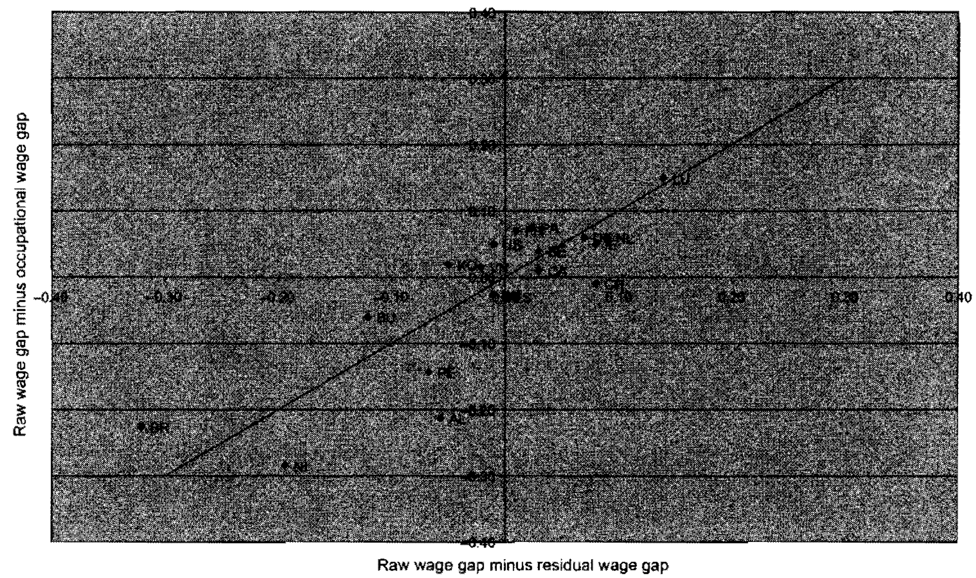
6. It is also interesting to note that for countries in the first quadrant, women tend to be less educated than men, and for countries in the third quadrant, women tend to be more educated than men.

7. The occupational gender wage gap is measured as one minus the average ratio of the reported female and male wage for a given country across occupations and years (at least two occupational gaps). See table S.6 in the supplemental appendix for more information on the underlying data.

8. The reported occupational gender wage gap is below zero for some countries, such as Bangladesh, Democratic Republic of Congo, and Togo, suggesting that women's occupational wages are higher than men's in these countries. This is possible, for instance, if female workers have more education than male workers in these countries, even within narrowly defined occupations (see section I on the importance of human capital differentials for understanding occupational gender wage gaps). More important, however, the reported gender wage gaps are measured with error, and especially in the poorest countries, there is a wide variation in reported occupational gender wage gaps. The countries with negative occupational wage gaps are not excluded, however, as this would create a sample selection (truncation) bias in the estimates.

9. The cross-section relationship was estimated with an OLS regression, with dummy variables for the outlier countries of Cyprus, Japan, and Republic of Korea [estimated coefficients, with standard errors in parentheses, for linear and quadratic GDP per capita variables: 0.09 (0.08) and −0.0047 (0.005); p-value of F-test on joint significance 0.08]. The dummy variables were included because the cross-section estimates were strongly affected if these high wage gap countries were excluded (see footnote 21).

FIGURE 1. Residual and Occupational Wage Gaps



Note: Country key: AL: Albania; BH: Bosnia and Herzegovina; BE: Belgium; BR: Brazil; BU: Bulgaria; CA: Canada; CH: Switzerland; DE: Germany; ES: Spain; FR: France; IE: Ireland; KO: Kosovo; LU: Luxembourg; NI: Nicaragua; NL: The Netherlands; PA: Panama; PE: Peru; US: United States; VN: Vietnam.

Source: Author's analysis based on data from the Luxembourg Employment Survey and the Living Standards Measurement Study.

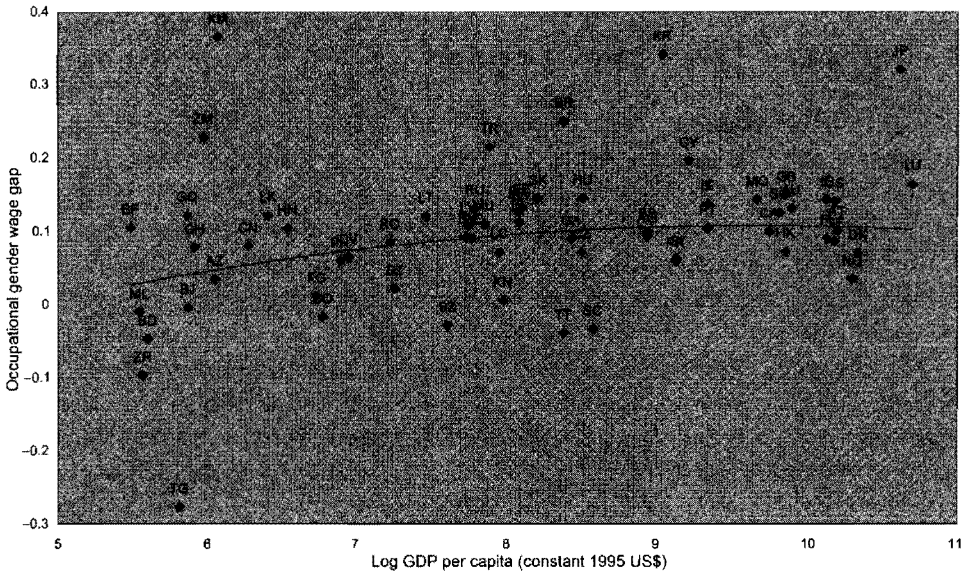
between the occupational gender wage gap and the level of economic development holds only for richer countries but not for poorer countries.[10] Boserup (1970) noted three decades ago that development has to reach a certain threshold before gender gaps close with further economic growth, and Dollar and Gatti (1999) found more recently a convex relation between per capita incomes and the proportion of women in parliament and gender equality in secondary school attainment, with minor or nonexistent correlation as countries move from very low income to lower middle income.

Countries report different occupations, and this may affect the estimated relation between the occupational gender wage gap and the level of development in figure 2. However, if the occupational gender wage gap is adjusted for cross-country differences in occupations reported, there is still a nonlinear relationship.[11]

10. The reported (between-country) regression will be biased if countries report varying sets of occupations and there are correlated unobserved occupation fixed effects [α_o in equation (2)]. This can explain the too high turning point in figure 2 (at log GDP per capita equal to 9.6) compared with the turning point in a regression including occupational fixed effects (at log GDP per capita equal to 9.1; see table 3, column 1).

11. Specifically, a regression was run of the occupational gender wage gap on occupation dummy variables and country by year dummy variables. The adjustment was done by subtracting the part of the occupational gender wage gap that could be explained by the occupation dummy variables.

FIGURE 2. Occupational Gender Wage Gap versus Log GDP per Capita, by Country



Notes: For each country, the year is included for which most occupational gender wage gaps were reported (minimum two occupational wage gaps). Country key: AG: Antigua and Barbuda; AT: Austria; AU: Australia; AZ: Azerbaijan; BB: Barbados; BD: Bangladesh; BF: Burkina Faso; BJ: Benin; BO: Bolivia; BR: Brazil; BY: Belarus; BZ: Belize; CA: Canada; CN: China; CV: Cape Verde; CY: Cyprus; CZ: Czech Republic; DK: Denmark; EE: Estonia; FI: Finland; GA: Gabon; GB: United Kingdom; GH: Ghana; GQ: Guinea Equatorial; HK: Hong Kong, China; HN: Honduras; HU: Hungary; IE: Ireland; IS: Iceland; JP: Japan; KG: Kyrgyzstan; KM: Comoros; KN: St Kitts and Nevis; KR: Republic of Korea; LC: St Lucia; LK: Sri Lanka; LT: Lithuania; LU: Luxembourg; LV: Latvia; ML: Mali; MO: Macau; MU: Mauritius; MX: Mexico; NO: Norway; PE: Peru; PH: Philippines; PL: Poland; PR: Puerto Rico; PT: Portugal; RO: Romania; RU: Russian Federation; SC: Seychelles; SE: Sweden; SG: Singapore; SI: Slovenia; SK: Slovakia; SZ: Swaziland; TG: Togo; TR: Turkey; TT: Trinidad and Tobago; US: United States; ZM: Zambia; ZR: Democratic Republic of Congo.

Source: Author's analysis based on data from ILO October Inquiry and World Bank World Development Indicators Database.

Also, if the analysis is limited to the country-year pairs reporting at least 5 of the 20 most reported occupations, an inverted U-shape relation is found between the level of economic development and the gender gap.

This descriptive analysis suggests that if there is any relation between the occupational gender wage gap and the level of development, it may have an inverted U-shape. However, the analysis above does not take into account other country differences that may affect the occupational gender wage gap and that are correlated with the level of development, such as trade and FDI. Nor does it allow for possible endogeneity bias. The regression analysis in section III will control explicitly for these possible biases.

One can control for time-invariant country characteristics, however, by looking at within-country changes in the occupational gender wage gap. The sample was separated into two groups: the top third of countries that have seen the fastest growth in GDP per capita between the 1980s and 1990s, and the bottom third slowest growing countries. The average change in the occupational gender wage gap for the slow GDP growth group is +0.04 (median change +0.02) between the 1980s and 1990s (table 2). The corresponding change for the fast growth group is −0.02 (median change −0.01). Thus, the fast growth group experienced a narrowing of the occupational gender wage gap, while the slow growth group experienced a widening. Also, six of eight countries in the slow growth group experienced an increase in the occupational gender wage gap, while six of eight countries in the fast growth group experienced a decrease.

The introduction mentioned several theories about the impact of globalization on the gender gap. These theories often have implications for the gender wage gap across occupations or skill levels but not for the gender wage gap within occupations or skill levels. For instance, standard trade theory predicts that the compensation paid to the relatively scarce factors of production will fall, implying that both male and female wages will fall in occupations intensive in scarce factors. Similarly, any trade-induced fall in gender disparities in human capital will probably lead to more employed women in the higher skill

TABLE 2. Change in and Number of Countries with Increase and Decrease in Occupational Gender Wage Gap between the 1980s and 1990s, by Growth in GDP per Capita, Trade, and FDI

	Change		Number of countries with	
Growth measure and country group	Mean	Median	Decrease	Increase
GDP per capita				
Bottom third	0.04	0.02	2	6
Top third	−0.02	−0.01	6	2
Trade				
Bottom third	0.04	0.02	3	5
Top third	−0.05	−0.02	5	2
FDI				
Bottom third	−0.01	−0.02	6	2
Top third	0.02	0.01	0	7

Note: Country groups: GDP per capita growth, bottom third: Australia, Denmark, Finland, Gabon, Honduras, Iceland, Peru, and Sweden; top third: Cyprus, Hong Kong (China), Republic of Korea, St Lucia, Sri Lanka, Mauritius, Portugal, and Singapore. Trade growth, bottom third: Cyprus, Gabon, Iceland, Japan, Republic of Korea, Peru, Sweden, and Singapore; top third: Bolivia, Finland, Hong Kong, Honduras, Sri Lanka, Mauritius, and United States. FDI growth, bottom third: Australia, Bolivia, Cyprus, Gabon, Iceland, Seychelles, Singapore, and United States; top third: Austria, Denmark, Finland, Japan, Norway, Peru, and Sweden.

Source: Author's analysis based on data from ILO October Inquiry and World Bank World Development Indicators Database.

occupations, but not necessarily to a lower gender wage gap within occupations.

However, globalization is expected to narrow the gender wage gap within occupations. First, trade will lead to more competition and therefore less discrimination. Second, increases in trade will drive up the relative demand for female labor because women are disproportionately represented in export-oriented sectors, at least in developing economies. Thus, prima facie, a negative relation between globalization and the occupational gender wage gap is expected.

Globalization can be measured along different dimensions and is measured here by trade as a percentage of GDP (in current prices) and by FDI net inflows as a percentage of GDP. The cross-country relation between these measures of globalization and the occupational gender wage gap is negative (figures not shown). Similar results are found if trade is measured as a percentage of GDP in constant local currency units, the measure used by Dollar and Kraay (2004). Hence, cross-country analysis suggests that trade and FDI inflows lower the occupational gender wage gap.

However, instead of looking at the cross-sectional pattern, one can also compare countries with low and high trade growth. Countries in the bottom third of countries for increase in trade (as a percentage of GDP in current prices) between the 1980s and 1990s are in the low trade group, and countries in the top third are in the high trade growth group. The average change in the occupational gender wage gap is +0.04 (median +0.02) for the low trade growth group and −0.05 (median −0.02) for the high trade growth group (table 2). Five of seven countries among the high trade growth group have seen a decrease in the occupational gender wage gap, as against three of eight among the low trade growth group. This supports the cross-section finding that trade lowers the occupational gender wage gap.

However, the opposite pattern is found for FDI net inflows as a percentage of GDP. The mean and median occupational gender wage gap rose for the group of countries with the largest increase in FDI net inflows, but fell for the group of countries with the smallest increases in FDI net inflows (table 2). The regression analysis discussed in the following section, however, suggests that the positive cross-section relation between FDI and the occupational gender wage gap is primarily a result of reverse causality.

III. Does Globalization Reduce the Occupational Gender Wage Gap? A Regression Analysis

It is clear that the descriptive analysis above may suffer from occupational heterogeneity (specification bias), feedback effects from the gender gap on trade and FDI (simultaneity bias), and the omission of factors that may have caused the changes in the occupational gender wage gap (omitted variable bias).

The following more in-depth regression analysis of the impact of globalization on the gender gap takes these potential biases into account.

The impact of globalization on the gender gap may vary across occupations. First, globalization may be expected to have the greatest impact on occupations with the largest gap (and potential for reduction). Second, occupations differ in worker and sector characteristics and may therefore be affected differently.

An important distinction is between high- and low-skill occupations in combination with the distinction between poorer and richer countries.[12] If the gender gap is reduced primarily through sector expansion (with increasing relative demand for female labor), trade would be expected to have a negative (narrowing) impact on the low-skill gender gap in poorer countries and the high-skill gender gap in richer countries. Conversely, if the gender gap is reduced primarily through sector contraction (with increasing competition from imports), then a large impact would be expected on the high-skill gender gap in the poorer countries and the low-skill occupations in the richer countries. Thus, trade may have different impacts on the gender gap depending on the income (or average skill) level of the country and the skill type of the occupation.[13] Therefore, the following regression model is estimated for low and lower middle income countries and high and higher middle income countries:[14,15]

$$
\begin{aligned}
occ_{cot} = {} & \beta_1^j D_o^{LS} GDP_{ct} + \beta_2^j D_o^{HS} GDP_{ct} + \beta_3^j D_o^{LS} GLOB_{ct} \\
& + \beta_4^j D_o^{HS} GLOB_{ct} + \beta_5^j COMM_{ct} + \alpha_o^j + \alpha_t^j + \varepsilon_{cot}^j,
\end{aligned} \tag{2}
$$

where c is country, o is occupation, t is year, $j \in$ {low/lower middle income countries, high/higher middle income countries}, occ is the occupational gender wage gap, D_o^{LS} and D_o^{HS} are dummy variables for low- and high-skill occupations, GDP is GDP per capita in constant 1995 US dollars, $GLOB$ is a measure of globalization, $COMM$ is a dummy variable for communist countries, α_o and α_t are occupation and year fixed effects, and ε is an error

12. The author is grateful to Aart Kraay for pointing this out.

13. It could be argued that the effect of globalization might be observed more strongly in occupations in traded than in nontraded sectors (at least in the short-run). The results for virtually all regression models showed no difference in impact, suggesting that the results should be interpreted as long run, in the sense that occupational labor markets between traded and nontraded sectors are integrated.

14. Following the suggestion of an anonymous referee, an interaction term was also included for country-income level and skill type, as statistically different coefficients were found for the GDP per capita variable across these skill types.

15. A Chow test confirms that the coefficients vary significantly across these two groups of countries. The high and higher middle income countries tend to be high education (skill) countries and the low and lower middle income countries to be low education countries. Although the sample is relatively small for the low and lower middle income countries, an equal split of the sample would make the relatively poor countries highly heterogeneous in human capital (measured as the number of years of education for the population ages 25 years and older).

term. The inclusion of year dummy variables in the regression subsumes any time pattern revealing the cross-sectional relation between globalization and the occupational gender wage gap. Occupation dummy variables are included to control for possible occupation-specific differences in occupational gender wage gaps.[16] Because communist countries have shown a particular commitment to gender equality in the labor market with, for instance, relatively high minimum wages and generous maternity leave and day care benefits (Brainerd 2000), a dummy variable for communist countries is included to capture these institutional features.[17] Aggregate trade (in current and constant prices) and FDI net inflows (in current prices) as a percentage of GDP are used as measures for globalization. Because independent information is lacking on the skill or education levels in each occupation, high-skill occupations are defined as falling within the top half of the occupational wage distribution within a country and low-skill as falling within the bottom half.[18] The regressions have different numbers of observations because of differences in data availability of the trade and FDI variables.

Moulton (1990) shows that there may be a serious downward bias in estimated standard errors if attempts are made to measure the effect of aggregate variables on micro units while assuming independent disturbances. In the analysis above, the dependent variable varies over three dimensions (country, occupation, year), while *GDP* and *GLOB* vary over only two dimensions (country, year). Moulton suggests that if disturbances are correlated within country-year groupings, then even small levels of correlation can cause the standard errors from ordinary least squares (OLS) to be seriously biased downward. Shore-Sheppard (1996) shows that this problem may also arise in an instrumental variables (IV) analysis if the instruments are measured at a higher level of aggregation than the dependent and explanatory variables. This is indeed the case in the IV analysis below, where instruments are used that vary over only one dimension (country). Therefore, the error term in equation (2) is assumed to have the following random components:

$$\varepsilon^j_{cot} = c^j_c + c^j_{ct} + c^j_{co} + u^j_{cot} \tag{3}$$

where c^j_c captures the correlation within country groupings, c^j_{ct} the correlation within country-year groupings, c^j_{co} the correlation within country-occupation groupings, and u^j_{cot} is an idiosyncratic error term. The first component (c^j_c) is included because the instrument varies only across countries (following Shore-Sheppard), while the second component (c^j_{ct}) is included because the

16. The Hausman test for random occupation effects was strongly significant (rejected) for the high and higher middle income countries (p-value < 0.00001) and insignificant for the low and lower middle income countries. This result may be due to the small sample size, and therefore occupation fixed effects are preferred in all regressions.

17. Omission of the communist dummy variable does not affect the results.

18. See supplemental appendix for more details on this measure of skill.

explanatory variables *GDP* and *GLOB* vary only across country-year groupings (following Moulton). The term c^j_{co} is also included because correlation within country-occupation groupings is also expected (if the gender wage gap is high for an occupation in a given country and year, it is likely to be high in another year as well). The standard errors were calculated with a three-step procedure.[19] First, the regressions were estimated with OLS or IV. Second, the residuals were used to estimate the variance–covariance matrix of ε^j_{cot} $(\hat{\Omega})$. Third, the standard errors of the OLS and IV regressions were calculated according to the formulas (Moulton 1990; Shore-Sheppard 1996):[20]

$$\text{OLS:} \quad (X'X)^{-1}X'\hat{\Omega}X(X'X)^{-1} \tag{4}$$

$$\begin{aligned}\text{IV:}\ &[(X'Z)(Z'Z)^{-1}(Z'X)]^{-1}(X'Z)(Z'Z)^{-1}Z'\hat{\Omega}Z(Z'Z)^{-1}(Z'X)\\ &[(X'Z)(Z'Z)^{-1}(Z'X)]^{-1},\end{aligned} \tag{5}$$

where X and Z are matrices of explanatory and instrumental variables, respectively.

Table 3 (panel A, columns 3, 4, 7, and 8) reports the OLS estimates of equation (2).[21] The relation between the occupational gender wage gap and the GDP per capita variable for the whole sample, including a quadratic term, is reported in column 1. Although the quadratic term is just insignificant at 10 percent, there appears to be a nonlinear relationship, as was evidenced in figure 2.[22] However, occupations have been reported by varying sets of countries, creating a possible selection bias.[23] Therefore, the regression in column 1 was also re-estimated for a subset of occupations reported for a consistent set of countries. Because none of the occupations has been reported by all countries simultaneously, a subsample of 10 countries that reported most

19. Generalized method of moments (GMM) estimates based on an optimal weighting matrix [reflecting the variance–covariance structure of the error term in equation (2)] would in principle be more efficient, but the estimates turned out to be very similar. The commonly used OLS and IV estimates were therefore applied, but corrected for the nonhomoskedastic nature of the error term.

20. The standard errors of the first-stage IV regressions were similarly corrected.

21. Azerbaijan; Hong Kong, China; Luxembourg; and Singapore were omitted in all regressions, because they are untypical in either trading volume or FDI net inflows. Dummy variables for Cyprus, Japan, and the Republic of Korea were included because the cross-section estimates were strongly affected if these high wage gap countries were excluded. With these dummy variables, the results are robust to the exclusion of any country in the sample.

22. The turning point is log GDP per capita of 9.1, with 18 economies on the declining part of the relationship: Austria, Australia, Canada, Cyprus, Denmark, Finland, Ireland, Iceland, Japan, Republic of Korea, Macau (China), Norway, Puerto Rico, Portugal, Sweden, Slovenia, United Kingdom, and United States.

23. The author appreciates the thorough discussion of this issue with one of the referees. He notes, however, that possible sample selection bias is arguably reduced by the inclusion of occupational dummy variables in the regression (identifying the impact of GDP per capita on country-differences in the gender wage gap for a *given* occupation rather than for the average reported occupation).

TABLE 3. Ordinary Least Square and Instrumental Variables Regression Estimates of the Effect of per Capita Income, Trade, Foreign Direct Investment Net Inflows on the Occupational Gender Wage Gap, by Occupational Skill Level and Country Income

	1	2	3	4	5	6	7	8	9	10
	All		Low/lower middle income countries				High/higher middle income countries			
Variable	OLS	OLS	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Panel A. Ordinary and two-stage least squares										
Log GDP per capita (*10^{-2})	7.29	11.9								
	(0.09)	(0.20)								
* Low skill			0.60***	0.64***	−0.59*	0.18	**−2.42*** **	−0.71***	**−3.18*** **	−12.9***
			(0.66)	(0.60)	(0.69)	(0.95)	(0.00)	(0.31)	(0.01)	(0.05)
* High skill			2.22***	1.72***	0.96*	0.19	**−2.06*** **	−0.39***	**−2.39*** **	−12.5***
			(0.10)	(0.16)	(0.50)	(0.92)	(0.00)	(0.58)	(0.01)	(0.07)
(Log GDP per capita)^2 (*10^{-2})	−0.40	−0.67								
	(0.12)	(0.21)								
Trade (percent GDP, current prices) (*10^{-3})										
* Low skill			0.01		0.15		−0.76***		**−1.50*** **	
			(0.97)		(0.75)		(0.00)		(0.00)	
* High skill			−0.23		0.29		−0.72***		**−2.11*** **	
			(0.42)		(0.51)		(0.00)		(0.00)	
FDI net inflows (percent GDP) (*10^{-2})										
* Low skill				**−0.58**		−1.11		**−0.44**		**−14.3*** **
				(0.09)		(0.77)		(0.11)		(0.09)
* High skill				0.11		3.09		−0.14		**−14.5*** **
				(0.74)		(0.39)		(0.60)		(0.01)
Year dummy variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation dummy variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummy variables for Cyprus, Japan, Republic of Korea	Yes	Yes[a]	na	na	na	na	Yes	Yes	Yes	Yes
Dummy variable for communist country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.40	0.53	0.24	0.24			0.56	0.56		
Number of observations	9,600	1,103	2,184	2,182	1,704	1,702	7,398	7,161	7,004	6,877

(Continued)

TABLE 3. Continued

Panel B. First stage for regressions	(5) Trade* Low skill	Trade* High skill	(6) FDI* Low skill	FDI* High skill	(9) Trade* Low skill	Trade* High skill	(10) FDI* Low skill	FDI* High skill
Log GDP per capita								
* Low skill	**6.16*****	0.35***	**0.55*****	−0.02***	**−3.96*****	**−7.76*****	**−0.34*****	**−0.54*****
	(0.00)	(0.78)	(0.01)	(0.93)	(0.00)	(0.00)	(0.01)	(0.00)
* High skill	**1.61*****	**4.96*****	0.11***	**0.39*****	**−7.40*****	**−4.35*****	**−0.44*****	**−0.46*****
	(0.18)	(0.00)	(0.59)	(0.03)	(0.00)	(0.00)	(0.00)	(0.00)
Geographic trade								
* Low skill	**1.59*****	**0.11*****	0.01***	**0.02****	**1.47*****	0.02***	**0.01*****	−0.001***
	(0.00)	(0.03)	(0.40)	(0.01)	(0.00)	(0.75)	(0.01)	(0.85)
* High skill	**0.11*****	**1.68*****	**0.02*****	**0.01****	−0.001***	**1.46*****	−0.002***	**0.02*****
	(0.04)	(0.00)	(0.01)	(0.08)	(0.99)	(0.00)	(0.65)	(0.00)
Tariff rate (*10^{-2})	−2.04	−2.98	0.61	−0.53	−10.6	−2.05	−0.46	−0.59
	(0.64)	(0.52)	(0.40)	(0.40)	(0.19)	(0.79)	(0.55)	(0.31)
Year dummy variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation dummy variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummy variables for Cyprus, Japan, Republic of Korea	Na	na	na	na	Yes	Yes	Yes	Yes
Dummy variable for communist country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F on excluded instruments	8.21	9.99	3.50	2.91	31.2	33.2	20.8	35.9
Hansen's J-statistic (p-value)	0.22		0.35		0.20		0.58	
Adjusted R^2	0.92	0.92	0.65	0.69	0.92	0.92	0.49	0.49
Number of observations	1,704		1,702		7,004		6,877	

na is not applicable.

Note: Numbers in parentheses are p values. Numbers in bold are coefficients that are individually significant at a 10 percent level or less. Standard errors are corrected (see text).

*Coefficients for the variables for which both interaction terms (for low- and high-skill occupations) are jointly significant at 10 percent,**5 percent, or ***1 percent.

[a]Dummy variable for Republic of Korea only.

Source: Author's analysis based on data sources discussed in the text.

wage gaps across the occupations was selected, and the occupations reported by each them were identified (21 occupations).[24] The subsample includes countries across the entire GDP per capita spectrum and many low- as well as high-skill occupations.[25] Estimates for this subsample are reported in column 2. The coefficients are less precisely estimated—because of the much smaller sample—but comparable to those in column 1 (not statistically different taking into account the sampling error) and imply a similar turning point.[26] Thus the observed nonlinear relationship in column 1 appears not to be driven by selection bias.

The nonlinearity is also evident in the OLS estimates of equation (2), with a significant positive impact of GDP per capita on the gender wage gap in poorer countries and a negative impact in richer countries. A nonlinear relation suggests a gender-equivalent of the Kuznets curve. However, IV analysis (discussed below) suggests that the positive relation between GDP per capita and the gender wage gap observed for poorer countries is not be robust, leaving a negative relation for the richer countries.

A negative impact of trade on the gender wage gap is found for low- and high-skill occupations in the richer countries.[27] This result confirms the finding in table 2. For the poorer countries, there are no significant results for the impact of trade. However, there is a negative impact of FDI net inflows on the gender wage gap for low-skill occupations in poorer and richer countries.

In general, caution should be exercised in inferring causality from a cross-section regression, given the possibility of simultaneity bias. The gender wage gap may affect trade and FDI net inflows in turn (reverse causality), for instance, because a high gender wage gap may reflect low female wages and potential cost-savings if exports are "female-led" (Rodrik 2000; Seguino 2000; Kucera 2002; Busse and Spielmann 2006). Also, a high gender wage gap may reflect discrimination and inefficiency in an imperfectly competitive environment, affecting the incentives for trade and foreign investment.

It is therefore important to consider instruments for the trade and FDI variables. In principle, trade policy variables such as tariffs and nontariff barriers are good instruments except that trade outcomes and trade policy variables are extremely weakly correlated (Dollar and Kraay 2004). Alternatives are the Frankel and Romer (1999) measures of the geographic component of countries' trade. Here, both tariff rates and the Frankel–Romer instruments are used and

24. Other subsamples were also tried, but they generated similar results.

25. The following countries (log GDP per capita in parentheses) are included in the subsample: China (6.3), Romania (7.2), Latvia (7.7), Estonia (8.1), Poland (8.1), Republic of Korea (9.0), Portugal (9.3), Austria (9.9), Finland (10.1), and Sweden (10.2). The selected occupations include laborers, garment cutters, building painters, sales people, mathematics teachers, chemical engineers, and general physicians, among others.

26. The implied turning point is at log GDP per capita of 8.9.

27. The table reports the results for trade in current prices. The results for trade in local currency units are virtually the same.

interacted with the skill level of the occupation as instruments for the trade and FDI measures.[28] The resulting IV estimates are reported in table 3, panel A, columns 5, 6, 9, and 10, and the corresponding first-stage regression results are reported in panel B. The Hansen *J*-statistic does not reject the joint null hypothesis that the instruments are valid (uncorrelated with the error term) and that the excluded instruments are correctly excluded from the estimated equation at the 5 percent significance level.[29] The *F*-statistics on the excluded instruments suggest that there is no problem with weak instruments (creating a bias toward the OLS estimates) apart, possibly, from the regression with FDI net inflows for poorer countries (regression 6).

Now the positive relation between GDP per capita and the gender wage gap in poorer countries is no longer robust. It becomes insignificant, with its sign depending on the specification. For the richer countries, there is still a negative relation between GDP per capita and the gender wage gap—economic development tends to reduce the gender wage gap.[30] This finding is consistent with Boserup's idea (1970) and Dollar and Gatti's finding (1999) that development has to reach a certain threshold before gender gaps close with further economic growth.

A significantly negative impact of trade on the gender wage gap is still found for low- and high-skill occupations in the richer countries.[31] For the FDI variable, there is now a significantly negative impact for both low- and high-skill occupations in the richer countries. Also, there is still a negative impact for low-skill occupations in poorer countries, but the coefficient is no longer significant.

The estimated coefficients for the trade and FDI variables are generally larger when instrumented. This result may reflect the attenuation bias because of measurement error as well as simultaneity bias. In the richer countries, a 100 percentage point increase in trade (as a percent of GDP) is estimated to lower the gender gap by approximately 15–21 percentage points. The impact of FDI on the gender gap is also significant—a 1 percentage point increase in FDI (as a percent of GDP) would lower the gender gap in richer countries by 14 percentage points.[32] Given that the average occupational gender gap is 8 percentage points, trade and FDI can potentially reduce the occupational gender wage gap substantially.

28. Tariff rates are unweighted averages for all goods in ad valorem rates, applied rates, or most favored nation rates, depending on which data are available for a longer period.

29. The optimal GMM estimator is used for computing the Hansen *J*-statistic (Cameron and Trivedi 2005, p. 181).

30. Although the coefficients for the GDP variables differ considerably across columns 9 and 10, the 95 percent confidence intervals are overlapping.

31. The results for trade in local currency units are virtually the same.

32. Average trade is 93 percent of GDP, while average FDI net inflows are 2.4 percent of GDP for the sample of countries.

These estimates may still suffer from omitted variable bias if the occupational gender wage gap is affected by factors other than economic development, trade, and FDI net inflows. Therefore, it was also investigated whether wage-setting institutions, intracountry trade, or occupational segregation and inequality could explain the observed relation between globalization and the gender wage gap. They did not—the observed relations did not change.[33] Also, after including a measure of female net supply, following the methodology of Blau and Kahn (2003), the coefficients for the GDP per capita, trade, and FDI variables are barely affected.

IV. Conclusion

This article undertakes one of the first truly global studies of the effect of globalization on the gender wage gap. The study is based on the most far-ranging cross-country survey of wages available, the ILO October Inquiry, permitting the gender wage gap to be measured within narrowly defined occupations. The occupational wage gap is an interesting indicator of gender inequality, as an independent measure of the relative female wage position that abstracts from occupational segregation, and also as an independent proxy for gender wage discrimination next to the residual wage gap.

The results of the study show that the occupational gender wage gap appears to decrease with increasing economic development, at least for richer countries. Also, the occupational gender wage gap tends to decrease with trade and FDI in richer countries, but no clear effect is found for poorer countries.

The findings for richer countries conform to the theoretical expectation that trade has a narrowing impact on the gender wage gap within occupations, through either a reduction in discrimination or an increase in the relative demand for female labor. If either of these explanations dominated, the impact would be expected to differ across skill level (see section III). However, there is no noticeable difference in impact across the skill level of occupation, suggesting that neither of these two mechanisms dominates.

Therefore, the evidence that trade and FDI reduce the occupational gender wage gap applies primarily to richer countries. For poorer countries, there is little evidence of this effect, in line with Boserup's (1970) conjecture that development has to reach a certain threshold before gender gaps close with further economic growth. At the same time, the possibility remains that it could be simply attenuation bias due to measurement error—poorer countries rely relatively more on nonsurvey-based data sources compared with richer countries (see supplemental appendix)—or the relatively small sample size for the poorer countries. Also, the gap is already relatively low in the lower middle income countries and particularly the low-income countries (supplemental appendix, table S.6), and therefore there is less scope for a reduction in the first place.

33. Results are reported in Oostendorp (2004).

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