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Trade Policy and Poverty Reduction in Brazil

Glenn W. Harrison, Thomas F. Rutherford, David G. Tarr, and Angelo Gurgel

A multiregion computable general equilibrium model is used to evaluate the regional, multilateral, and unilateral trade policy options of Mercosur from the perspective of the welfare of all potential partners in several proposed agreements. The focus for Brazil is on poverty impacts. The results show that the poorest households in Brazil experience gains of 1.5–5.5 percent of their consumption, which are about three to four times the average gains for Brazil. Protection in Brazil favors capital-intensive manufacturing relative to unskilled labor-intensive agriculture and manufacturing. So trade liberalization raises the return to unskilled labor relative to capital and disproportionately helps the poor.

Brazil has several trade policy options. This study evaluates those options from the perspective of the welfare of all potential partners in several proposed trade agreements, looking particularly at the impacts on poor people to determine which trade policy contributes most to poverty reduction in Brazil. The objective is to determine whether there is a tradeoff between aggregate welfare gains to Brazil from trade liberalization and the welfare gains to the poor. The article concludes that there is no tradeoff and explains why.

As part of the Mercosur customs union with Argentina, Paraguay, and Uruguay, Brazil is engaged in negotiations to implement the Free Trade Agreement of the Americas (FTAA). Mercosur is also negotiating a potential free trade

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agreement with the European Union, along with less notable regional arrangements. In addition, Brazil has supported further multilateral negotiations within the World Trade Organization (WTO 2000).

Brazil is concerned that these regional integration initiatives will provide much less market access than agreements that do not constrain the exports of partner countries. Notably, significantly improved access to EU agricultural markets will be very difficult to achieve for the usual EU internal political reasons. As a major agricultural exporter, Brazil believes that the WTO is the best negotiating forum for obtaining freer access to agricultural markets. Moreover, antidumping and stringent rules of origin may limit access to the markets of the main industrial country partners in these regional agreements, especially the Free Trade Agreement of the Americas (FTAA).

Extending the analysis of Harrison and others (2002) on Chile, this study evaluates the value of trade policy options to Brazil if the key industrial country partner in these regional agreements denies access to specific products. For the EU, that means exclusion of preferred access to Mercosur exporters in the most highly protected agricultural products. For the FTAA, that means denial to Brazil of access to the most highly protected products in the United States because of antidumping measures or restrictive rules of origin.

A major policy concern is the link between trade policy changes and poverty in Brazil. Although interest in the topic has increased dramatically in recent years, using general equilibrium modeling with multiple households to examine equity issues dates to pioneering studies by Adelman and Robinson (1978) and Piggot and Whalley (1985). This has typically been done by aggregating households from a household survey into 5–40 households.[1] Recently modelers have focused attention on the impact of trade policy on poverty, and Harrison and others (2003a) have showed that a concern with equity is not equivalent to a concern with poverty.[2]

A second approach is to take price changes from a representative consumer general equilibrium model and feed these into a micro-simulation model of household behavior, such as in Chen and Ravallion (2003) and Bussolo and Lay (2003). This approach allows examination of the diversity of impacts across households: Even if the aggregated poor households gain, many individual poor households could lose. But the approach ignores feedback effects of the quantity changes on the equilibrium outcome in the general equilibrium model and does not reconcile inconsistent information on household income from the national accounts and the household surveys.[3]

The analysis here is in the tradition of the first approach. The model incorporates 20 types of Brazilian households: 10 rural and 10 urban, with

1. For recent applications see the papers for the conference on Poverty and the International Economy (available online at www.worldbank.org/trade).

2. The trade policy change they evaluated resulted in an increase in aggregate real income and greater equity as measured by the Gini coefficient, but the poorest households were worse off.

3. See Cockburn (2001) for an attempt to combine the two approaches.

households further classified by income level. The results show clear and crucial links among trade policy changes, factor intensities at the industry level, economy-wide factor returns, and poverty—the links suggested by the Hekscher-Ohlin and Stolper-Samuelson models. But only because of the attention to detail in the empirical estimation of factor shares are results obtained that can be sensibly used to analyze the poverty dimension of trade policy changes. The results also show the importance of agricultural liberalization for the poor.

The aggregate policy results show that both the FTAA and the EU–Mercosur arrangements are net trade creating for member countries, but excluded countries almost always lose. But multilateral trade liberalization (a 50 percent cut in tariffs and export subsidies) results in estimated gains to the world that are more than four times greater than the returns for either the FTAA or the EU–Mercosur agreement, demonstrating the continuing importance of multilateral negotiations.

A fully implemented agreement with the European Union is about 1.5 times more valuable to Brazil than the FTAA because of access to highly protected EU agricultural markets. But if agriculture is excluded from the agreement, it becomes of very little value to Brazil. Application of antidumping and restrictive rules of origin by the United States against Brazil on the most protected products in the U.S. market similarly reduces the value of the FTAA to Brazil. Nonetheless, the FTAA still has significant value to Brazil because other markets in the Americas and the less protected sectors in the United States are assumed to remain open to Brazilian exporters.

Most of the evaluated trade policy options result in a progressive distribution of the gains, so that the poorest households experience the greatest percentage increase in their incomes. Although Brazil undertook substantial trade liberalization in the 1990s, vestiges of its import-substitution industrialization strategy of the 1960s remain. Trade policy reforms in Brazil tend to shift resources from capital-intensive manufacturing to unskilled labor-intensive agriculture and less capital-intensive manufacturing, increasing the wages of unskilled labor relative to returns to capital and skilled labor. The percentage increase in the incomes of the eight poorest types of households is several times greater than the average percentage increase for the economy as a whole.[4]

Previous work has shown that multilateral agricultural trade liberalization will lead to aggregate gains for agricultural exporting nations. The results here suggest that agricultural trade liberalization, whether multilateral or in a regional arrangement with the European Union, is particularly important for the realization of poverty reduction benefits for agricultural exporters, such as Brazil.

4. These results are consistent with two other analyses of the impact of trade liberalization on the poor in Brazil: Barros and others (2000) and World Bank (2001).

I. A Multiregional Trade Model

A comparative static, constant returns to scale, multiregional, and multisectoral quantitative model is developed to evaluate the impact of trade policy on poverty in Brazil. The model is relatively detailed in the Americas, with 13 countries or regions from that area (table 1). Also included are the European Union 15, Japan, and a residual rest of the world. Of the Mercosur members, Brazil, Argentina, and Uruguay are represented explicitly in the model, whereas Paraguay is represented as part of the rest of South America. The general specification of this model follows the earlier multiregional model of the effects of the Uruguay Round in Harrison and others (1997c) and even more closely their model of trade policy options for Chile (Harrison and others 2002).[5]

Because most of the documentation of the data and model and additional simulations are available in Harrison and others (2003b), only the main features

Table 1. List of Commodities, Regions, and Factors of Production Used in the Model

Commodities		Regions		Factors
PDR	Paddy rice	BRA	Brazil	Capital
GRO	Cereal grains	ARG	Argentina	Unskilled labor
OSD	Oilseeds	URY	Uruguay	Land
AGR	Other agriculture	CHL	Chile	Natural resources
OCR	Other crops	COL	Colombia	Skilled labor
CMT	Bovine meat products	PER	Peru	
OMT	Other meat products	VEN	Venezuela	
MIL	Dairy products	XAP	Rest of Andean pact	
PCR	Processed rice	MEX	Mexico	
SGR	Sugar	XCM	Central America and Caribbean	
OFD	Other food products	XSM	Rest of South America	
ENR	Energy and mining	CAN	Canada	
TEX	Textiles	USA	United States	
WAP	Wearing apparel	E_U	European Union 15	
LEA	Leather products	JPN	Japan	
LUM	Wood products	ROW	Rest of world	
MAN	Other manufacturing			
I_S	Iron and steel			
FMP	Other metal products			
MVH	Motor vehicles and parts			
SER	Services			
CGD	Savings good			
DWE	Dwellings			

5. Harrison and others (1997c, 2002). The model is formulated using the GAMS-MPSGE software developed by Rutherford (1999) and solved using the PATH algorithm of Ferris and Munson (2000). See de Melo and Tarr (1992) for an exposition of the general form of the within-country equations of the model.

are summarized here.[6] Production uses intermediate inputs and primary factors (labor, capital, and land) that are mobile across sectors within a region but immobile internationally. The amount of capital and labor available to any economy is fixed. Output is differentiated between domestic output and exports, but exports are not differentiated by destination country. Except for Brazil, each region has a single representative consumer who maximizes utility, as well as a single government agent. Demand is characterized by a nested Armington structure for each of the 22 sectors (see table 1). The Armington aggregate good is a constant elasticity of substitution (CES) composite of domestic production and aggregate imports, and aggregate imports are a CES aggregate of imports from different regions of origin. This structure allows multistage budgeting. So government revenue remains unchanged in any counterfactual scenario, a tax is imposed to compensate for lost tariff revenue. Each country has a balance of trade constraint, so any change in the value of imports is matched by an equal value change in exports. The model is "real," in the sense that it contains no financial assets. Thus there is only a "real" exchange rate, defined as the price of a country's tradable goods relative to the price of its nontradable goods.

The model does not incorporate increasing returns to scale or endogenous productivity effects of trade policy, despite a number of studies by Brazilian researchers identifying a correlation between the opening of Brazil to external trade in the early 1990s and an increase in productivity in Brazilian manufacturing (Muendler 2001 found a causal relationship). A model that incorporates both of these, such as that developed in Rutherford and Tarr (2002), would be expected to produce much larger gains than this constant returns to scale model, with a resulting further reduction in Brazilian poverty. But because the productivity advances are not likely to be concentrated in the labor-intensive sectors, the relative share of the gains at the household level for the poor may be less progressive than that found here.[7]

Brazilian Households

The most important new feature in this model is the extension to multiple households in Brazil: 10 rural households and 10 urban households, distinguished by income levels (as defined in table 2). The structure of demand for each household is a nested Armington structure, based on CES demand functions, similar to representative households in other regions.

6. These appendixes present details of the model specification, tables with low elasticity results and detailed sectoral results for Brazil, procedures for updating the input-output tables and estimating factor intensities, calculation of the tariff rates in Mercosur, systematic sensitivity analysis, steps for incorporating the household survey information, and some additional references.

7. Most of the trade policy options were also evaluated in separate simulations in a comparative steady-state model. Because the rental rate on capital falls in most of the scenarios, the new equilibrium capital stock does not rise, and the estimated welfare gains to the economy also do not rise.

TABLE 2. Household Types and Characteristics

Rural Households	Mean per Capita Income[a]	Mean Household Income[a]	% of Sample	Representative no. Individuals[b] (millions)	No. Individuals in Survey	Urban Households	Mean per Capital Income	Mean Household Income[a]	% of Sample	Representative no. Individuals[b] (millions)	No. Individuals in Survey	No. Individuals in Survey in Household i[c]	Monthly Household Income (1996 reals)
1	48	129	5.89	6.10	1,090	1	63	135	4.38	4.54	707	1,797	0–206
2	103	259	3.92	4.06	868	2	131	264	5.54	5.74	955	1,823	207–313
3	116	364	2.64	2.73	661	3	155	375	6.14	6.36	1,152	1,813	314–431
4	140	489	2.31	2.39	556	4	196	497	6.78	7.03	1,260	1,816	432–564
5	165	647	1.87	1.94	470	5	239	649	7.34	7.61	1,347	1,817	565–741
6	228	838	1.41	1.46	328	6	286	846	8.74	9.05	1,486	1,814	742–964
7	286	1,074	0.7	0.73	194	7	390	1,123	9.27	9.60	1,624	1,818	965–1,290
8	385	1,528	0.96	0.99	235	8	479	1,561	8.06	8.35	1,582	1,817	1,291–1,889
9	615	2,282	0.32	0.33	103	9	752	2,449	8.99	9.31	1,716	1,819	1,890–3,196
10	2,363	7,864	1.52	1.58	408	10	2,187	6,728	13.22	13.70	2,648	3,056	3,197–66,809
Total			21.54	22.31	4,913	Total			78.46	81.27	14,477	19,390	

[a]Income figures are in 1996 reals.

[b]The number of individuals the stratified sample is estimated to represent.

[c]Rural household 1 plus urban household 1.

Because the CES function is homothetic, changes in the income level of individual consumers will not change the proportions in which they consume commodities. Despite the fact that each individual consumer has homothetic utility functions, relative prices will vary with income levels in the model. This is because the CES demand function parameters calibrated for each household necessarily differ across households, because the initial shares of income spent on different commodities vary by household. This implies that the elasticities of demand with respect to prices and income differ across Brazilian households. Hence if income shifts from household A to household B, aggregate demand will shift toward the commodities consumed more intensely by household B.[8]

General Data and Elasticities

The Global Trade Assistance and Protection 5 (GTAP5) database, described in Dimaranan and McDougall (2002), is used for countries other than Brazil. It includes key protection data (table 3). The 57 sectors in the full GTAP database have been aggregated to 22 sectors, resulting in a model with approximately 2,500 equations. This retains the sectors that are most important to Brazilian trade policy, sectors with high protection in U.S., EU, or Mercosur markets. Aggregating sectors with similar protection levels should not significantly affect the results.[9]

In the scenarios using central elasticities, the lower level elasticity of substitution between imports from different regions, σ_{MM}, is assumed to be 30 and the higher level elasticity between aggregate imports and domestic production, σ_{DM}, to be 15. Although these elasticities are high by the standards of some econometric studies, such as Reinert and Roland-Holst (1992) and Shiells and Reinert (1993), they are supported by the estimates of Reidel (1988) and Athukorala and Reidel (1994). Moreover, elasticities would be expected to increase over time, and this model presumes an adjustment of about 10 years, a long period in the context of these econometric estimates. The higher elasticities are needed in the model to produce results for terms of trade changes that are closer to the results of Chang and Winters (2002).[10]

8. The model was also executed with the linear expenditure system (LES) demand functions at the top level in place of CES for all Brazilian households. Given that the change in real income is not large in the simulations, the welfare results and returns to factors change only negligibly.

9. That is, sectors were aggregated that are not important in trade or that have low rates of protection. Although aggregation may significantly change the results in applied trade policy analysis, this type of aggregation creates quite small aggregation bias. It is acknowledged, however, that services are not treated seriously in this model. Readers interested in the role of services in regional agreements of Brazil may consult Mattoo and others (2002).

10. Larger elasticities in the model result in larger terms of trade effects. The Chang and Winters (2002) results provide support for the higher choice of elasticities, because even the highest elasticities chosen fall short of the terms of trade effects they find for the United States and Japan. The welfare calculations here, however, are broadly consistent with those of Chang and Winters (2002).

TABLE 3. Structure of Protection for All Countries in the Sample (import share trade-weighted average import tariff defined over the set of countries subject to positive tariffs)

	BRA	USA	CAN	MEX	ARG	CHL	COL	PER	VEN	URY	XCM	XAP	XSM	EUR	JPN	ROW
PDR	12	5	[a]	15	12	11	13	22	13	12	25	12	15	65	409	7
GRO	7	1	9	38	7	11	12	12	12	7	9	11	5	44	20	77
OSD	6	18	[a]	3	6	11	11	12	11	6	5	8	4	3	76	52
AGR	10	3	12	17	10	11	17	12	17	10	12	17	7	13	18	24
OCR	8	14	2	12	9	11	12	16	12	9	9	9	7	10	46	20
ENR	4	0	1	7	5	11	9	12	6	5	6	6	4	1	−1	5
CMT	12	5	16	35	12	11	19	15	19	12	15	18	11	95	36	34
OMT	14	4	72	68	14	11	18	20	18	14	20	19	13	61	58	33
MIL	19	42	215	38	19	11	19	19	17	19	24	18	16	90	287	43
PCR	15	5	1	15	15	11	20	20	20	15	36	20	18	86	409	19
SGR	19	53	5	4	19	11	18	12	18	19	20	17	24	76	116	17
OFD	18	8	29	22	18	11	18	15	19	18	16	18	17	28	34	32
TEX	16	11	16	15	16	11	17	16	17	16	16	11	16	10	8	16
WAP	20	13	21	33	20	11	20	20	20	20	24	15	23	12	13	17
LEA	26	13	15	25	26	11	16	18	18	23	15	15	19	8	15	13
LUM	15	2	7	13	13	11	17	12	16	14	15	15	20	3	3	11
MAN	13	2	3	10	13	11	9	12	10	13	9	9	11	4	1	7
I_S	13	3	5	8	12	11	10	12	12	12	6	9	11	3	3	8
FMP	16	4	6	14	16	11	14	12	15	16	10	12	16	4	1	12
MVH	26	2	5	14	26	10	21	12	25	29	13	20	14	5		13

Note: See table 1 for definitions of countries and products.

[a]Imports are from the United States only and are not subject to duties.

Source: Authors' calculations based on GTAP database (Dimaranan and McDougall 2002), updated 1996 input-output table for Brazil, and the 1996 LSMS survey conducted by the Brazilian Institute of Geography and Statistics.

The policy simulations are also performed with lower elasticity values of $\sigma_{MM} = 8$ and $\sigma_{DM} = 4$.[11] Lower elasticities typically lower the welfare gains for the countries that gain from the regional arrangements and reduce the losses for countries excluded from the regional arrangements, but they rarely change the qualitative results in the scenarios examined.[12] Similarly, results at the household level in Brazil are muted with lower elasticities, but the relative gains to the poor in Brazil remain several multiples of the overall gain.

The elasticity of transformation between exports and domestic production is assumed to be 5 for each sector. Elasticities of substitution between primary factors of production is unity. Fixed coefficients are assumed between all intermediates and value added.

Protection Data

All distortions are represented as ad valorem price wedges. Border protection estimates combine tariff protection and the tariff equivalents of nontariff barriers into a single measure of protection referred to as the tariff rate.

Trade in goods within Mercosur was tariff-free by 2000. Members were allowed a list of exceptions to the common external tariff, but the common tariff is being phased in for the exceptions and all members are obligated to fully converge to it by 2006 (WTO 2000, p. 20). Because changes in protection data are crucial to the results and protection rate data are usually available for a more recent period than input-output tables, the protection data used are typically more recent than the data in the input-output tables.[13] Similarly, tariffs on imports of goods between Argentina, Brazil, and Uruguay are assumed to be zero. Because the common external tariff was largely in place in 2003 and is scheduled to be fully implemented by 2006, Argentina, Brazil, and Uruguay are all assumed to apply it.[14]

The North American Free Trade Association (NAFTA), too, is assumed to operate as an effective free trade area, with zero tariffs between Canada, Mexico, and the United States, but with each country maintaining its own external tariff. The model does not incorporate the preferential tariff rates

11. The results for low elasticites reported in Harrison and others (2003a) were erroneously reported as being based on $\sigma_{MM} = 8$ and $\sigma_{DM} = 4$; in fact, they are based on $\sigma_{MM} = 16$ and $\sigma_{DM} = 8$.

12. The impact of unilateral trade liberalization on Argentina is one exception, for reasons explained later.

13. Several sources of protection data were examined, and the data in the GTAP database were assessed as the best. The trade flow data are also from the GTAP database, which is for 1997. Because the input-output table and estimated factor payments in Brazil were updated, a balanced social accounting matrix (SAM) had to be created. An optimization procedure was employed in creating the new SAM that minimizes the sum of the squares of the difference between all the values in the new SAM and the original GTAP database, subject to the constraints of a SAM.

14. The common external tariff is imposed at the tariff-line level but is applied at the level of aggregation of the model. This involves no loss of generality, because if the common external tariff holds at the tariff-line level, then it must hold for more aggregate levels.

found in the many other regional trading arrangements in the Americas that are implemented at various levels of effectiveness.

Table 3 shows the (trade-weighted) average protection rates by product category across all countries. The common external tariff of Mercosur is implemented by imposing the external tariff of Brazil as the external tariff of Argentina and Uruguay. Nonetheless, the trade-weighted average tariff is not precisely equal in all cases for the three countries because of product mix differences across sources of imports.

Brazilian Data for Poverty Analysis

Most of the data for Brazil, which are crucial to effective trade and poverty analysis, were independently constructed for this study. In addition to the protection data, the most important steps were to estimate factor shares in Brazilian industries, update the 1996 input-output table of the Brazilian economy from the 1985 base table in the GTAP database and use the household expenditure survey for Brazil to construct information on household expenditure patterns and sources of income.

The share of value added attributed to capital in input-output tables is notoriously overestimated in agriculture and services and is poorly represented in many manufacturing sectors. The convention of input-output authorities is to take capital's share as the residual from revenue after payments for intermediates, labor, and taxes. Agriculture's lack of official reported wage payments means that input-output authorities often report these sectors as the most capital-intensive sectors in the economy.[15] Similar but less severe problems prevail in services. In manufactures, unprofitable sectors (which often do not export) have a low share of capital in the input-output tables, whereas profitable sectors (which more often export) have a high share of capital. In developing economies the result is that labor-intensive sectors, which may be the most profitable and export-oriented, are likely to be reported as capital-intensive sectors. Harrison and others (2003a) show how this problem can lead to perverse results.

Factor shares in Brazilian industries were thus independently estimated for this study. The reestimation raised labor's share significantly in agriculture and, to a lesser extent, in services. For manufactures there were no significant differences between these estimates and the input-output table. This adjustment is fundamental to the results on the relative impact on the poor.

Household expenditure and income patterns were extracted from the Living Standards Measurement Study (LSMS) survey for Brazil. The survey was designed and conducted by the Brazilian Institute for Geography and

15. Researchers at the International Food Research Institute and the Economic Research Service of the U.S. Department of Agriculture have noted this problem and adjusted for it (see Arndt and others 1998; Thomas and Bautista 1999; Hausner 1999; Burfisher and others 1992).

Statistics. The LSMS survey is a stratified sample, with each household representing a share of the total population in the area sampled. The LSMS focused on the eastern part of Brazil, but it is estimated to represent 103.6 million people in the region (about 63 percent of the total population), 22.3 million of them in rural areas and 81.3 million in urban areas. Although much of the country is not sampled in the survey, experts who have worked with the poverty data in Brazil believe that the poor are proportionally represented or at least are not underrepresented.[16] The Gini coefficient for the entire survey sample is estimated at 0.585.

To aggregate the approximately 5,000 Brazilian households in the survey into 20 households, all households in the sample were first ranked from poorest to richest based on per capita income. Per capita rather than household income was chosen to enable comparisons with the standard per capita poverty measures of the World Bank (World Bank 1990, 2000) and of Ferreira and others (1999) for Brazil. The sample was then divided into deciles, with an equal number of households in each decile (except for the richest decile, which has more households, because they were to receive less emphasis in the analysis). Each decile was then partitioned into two representative households: one rural and one urban. This partition means that the *i*th representative rural household and the *i*th representative urban household have about the same per capita income. Although the *i*th representative rural and the *i*th representative urban household do not have an equal number of households or individuals, the sum of the households they represent is equal, and the sum of individuals they represent is approximately equal. As a result, there are roughly 1,800 individuals in each household group, apart from the richest household group, which has just over 3,000 individuals (see table 2).

The shares of income each household spent on each commodity group and the shares of income each household obtained from capital, rent on land, unskilled wages, and skilled wages were extracted from the LSMS survey. Data on factor incomes were also available from national accounts, so the data from the two sources had to be reconciled before implementing the model.[17] For reasons to be explained, the total payments to factors were taken from the national accounts, and the factor shares of each representative household in the model were adjusted accordingly.[18] This reconciliation minimized aggregate deviations between household factor shares and expenditure shares from the

16. The authors thank Francisco Ferreira, Peter Lanjouw, and Marcelo Neri for helpful conversations on several aspects of assessing poverty in Brazil.

17. A two-stage process in which price changes from a general equilibrium model are fed into a second-stage micro-simulation model can ignore this reconciliation. Of course, inconsistencies then arise if one then wants to allow for feedback from the second stage to the first stage after some policy shock.

18. This rebalancing also required adjusting expenditure shares of households for broad categories of goods, to ensure consistency with the broad patterns of consumer expenditure in the national accounts.

values obtained from the LSMS survey prior to rebalancing, and the shares were weighted by the value of household income and expenditure. The results are reported in table 4 and explained in Harrison and others (2003b, appendix D).

This reconciliation of the two databases significantly increased the share of capital owned by wealthy households, particularly wealthy urban households. Income estimates from LSMS surveys are known to be lower than income estimates from national accounts (see Ravallion 2003; Deaton 2003). Although there are biases in collection of both databases, so that neither source is clearly correct, Deaton (2003) believes that the most likely explanation for the difference is that households fail to respond to the survey, with the probability of nonresponse increasing monotonically with income. It also appears to be the case that the LSMS surveys report a lower share of income for capital than the national accounts do. Vanos (2003) mapped income from the LSMS surveys in 14 countries into factor shares and compared these with the GTAP database. Capital's share from the LSMS surveys was 21 percent of household income, but it was 52 percent of household income based on national account information in the GTAP database. The presumed pattern of nonresponse to the household survey

TABLE 4. Household Income Shares from Factors of Production and Transfers (percent)

Household Type	Skilled Labor	Unskilled labor	Rent from Capital	Rent from Land	Transfers
Rural					
1	6	68	3	1	22
2	8	80	0	0	11
3	11	87	0	2	1
4	8	64	3	2	22
5	11	57	32	0	0
6	22	47	31	0	0
7	9	49	42	0	0
8	15	62	20	3	0
9	18	45	35	1	0
10	7	75	15	3	0
Urban					
1	1	70	0	0	28
2	18	67	1	0	14
3	10	74	3	0	14
4	13	68	8	0	10
5	27	57	16	0	1
6	28	52	19	0	0
7	27	30	42	0	0
8	33	28	39	0	0
9	30	21	49	0	0
10	17	15	69	0	0

Source: Authors' calculations based on the 1996 LSMS survey conducted by the Brazilian Institute of Geography and Statistics.

would also help explain this difference in capital's share, because the rich are likely to have more capital than the poor.[19]

What percentage of the households are poor based on the LSMS data? Poverty lines are defined in several ways. Two well-known measures are $1 a day per person or $2 a day per person at a purchasing power parity exchange rate. From the LSMS data 7.3 percent of the population lives on $1 a day or less and 17.8 percent lives on $2 a day or less. To calculate poverty in Brazil, Ferreira and others (1999) developed a measure of poverty based on a "minimum food basket" in the reference region, metropolitan São Paulo, that would generate the Food and Agriculture Organization–defined minimum intake of 2,288 calories a day. They also developed indices that allow them to define "equivalent" income levels across individual households in different regions of the LSMS. Using purchasing power parity adjustments for 1996, this measure amounts to a poverty line of $1.50 per person per day.[20] Taking the poverty headcounts for each region in Brazil as reported in Ferreira and others (1999, table 3) and sample weights for the individuals in each of the regions of the LSMS in Brazil, their measure implies a national poverty index of 13.03 percent for Brazil using the LSMS.[21]

Based on the Ferreira and others (1999) measure of poverty incidence and the full LSMS database, 82 percent of the households in the poorest two households, urban household 1 and rural household 1, fall below this poverty line. The poorer households are more populous, however, so that this amounts to 13 percent of the individuals in Brazil who are below the poverty line.[22]

II. Results at the Country Level

The model estimates the aggregate change in welfare, measured by Hicksian equivalent variation, in Brazil and the other countries in the model as a result of the trade policy choices hypothetically made by Brazil (tables 5–7). The aggregate estimate of the change in welfare is the weighted sum of the welfare changes for the 20 individual households in the model, reported as a percentage

19. In Brazil, capital's share of factor income from the input-output tables is between 52 percent and 54 percent between 1995 and 1997. Capital's share of factor income is 54 percent in the Brazilian Survey of Industry for 1998 and 76 percent in the Brazilian Census of Agriculture for 1996. Factor shares in production were reestimated to correct for biases in agriculture and services, so that capital's share of income is 50 percent based on the national accounts. But Vanos (2003) estimates capital's share at 22 percent based on the LSMS survey. From our mapping of LSMS data, capital's share is about 10 percent.

20. Specifically, they report a poverty level of 65.07 reals per month. This is divided by 30.417, the average number of days in a month, and then further divided by 1.44 to get the purchasing power parity equivalent in U.S. dollars. This is $1.48656, rounded to $1.50 for ease of recollection.

21. They also report comparable numbers from an alternative survey, known as the PPD, which imply a national poverty index of 24.7 percent using comparable income measures.

22. The average number of people is 5.8 in rural household 1 and 5.0 in urban household 1. This compares with an average of 3.9 for the entire survey.

Table 5. Impact of Mercosur Trade Policy Options on Selected Countries As a Share of Consumption, Central Elasticities (welfare change, percent)

Country or region	FTAA (1)	FTAA with Excluded Products (2)	EU–Mercosur (3)	EU–Mercosur with Excluded Products (4)	FTAA and EU–Mercosur (5)	Unilateral 50% Tariff Cut (6)	Multilateral Tariff Liberalization by 50% (7)	FTAA with no Mercosur Liberalization (8)
Brazil	0.6	0.4	0.9	0.1	1.8	0.4	0.9	0.4
Argentina	−0.2	−0.2	2.3	0.2	2.2	0.2	0.8	0.2
Uruguay	1.7	1.6	43.9	1.2	43.4	1.4	7.8	0.4
Chile	1.1	1.1	−0.2	0.0	0.9	0.1	1.3	0.8
Colombia	1.7	2.0	−0.1	−0.1	1.7	0.0	1.0	1.7
Peru	1.0	1.0	−0.1	0.0	0.9	0.0	1.3	1.0
Venezuela	1.1	1.1	0.0	−0.1	1.1	0.0	0.9	1.1
Rest of Andean pact	1.9	2.0	0.0	0.0	1.9	0.1	2.5	1.8
Mexico	0.3	0.4	0.0	0.0	0.3	0.0	0.5	0.0
Central America and Caribbean	4.3	4.8	0.0	0.0	4.4	0.0	2.1	4.6
Rest of South America	0.8	0.8	−1.2	0.1	0.0	0.3	4.1	0.1
Canada	0.0	0.1	0.0	0.0	0.0	0.0	0.2	0.1
United States	0.0	0.0	0.0	0.0	0.0	0.0	0.1	0.0
European Union 15	−0.1	0.0	0.5	0.1	0.4	0.0	0.8	−0.1
Japan	0.0	0.0	0.0	0.0	0.0	0.0	1.8	0.0
Rest of the world	−0.1	−0.1	0.0	0.0	−0.1	0.0	2.3	−0.2

Note: FTAA with excluded products: FTAA with U.S. antidumping policy denying improved access to its four protected sectors. EU-Mercosur with excluded products: a free trade agreement between Mercosur and the European Union with the seven most protected food and agricultural products in the European Union excluded from the agreement. FTAA and EU-Mercosur: the FTAA combined with a free trade agreement between Mercosur and the European Union. Unilateral 50% tariff cut: a Mercosur-only tariff cut of 50 percent. Multilateral tariff liberalization: all regions reduce tariffs and export subsidies by 50 percent. FTAA with no Mercosur liberalization: the FTAA but Mercosur does not change its external tariff to the rest of the Americas.

Source: Authors' computations based on GTAP database (Dimaranan and McDougall 2002), updated 1996 input-output table for Brazil, and the 1996 LSMS conducted by the Brazilian Institute of Geography and Statistics.

Table 6. Impact of Mercosur Trade Policy Options on Selected Countries, Central Elasticities (welfare gain in billions of 1996 U.S. dollars)

Country	FTAA (1)	FTAA with Excluded Products (2)	EU–Mercosur (3)	EU–Mercosur with excluded products (4)	FTAA and EU–Mercosur (5)	Unilateral 50% Tariff Cut (6)	Multilateral Tariff Liberalization by 50% (7)	FTAA with no Mercosur Liberalization (8)
Brazil	3.1	2.3	5.0	0.5	9.5	1.9	4.6	2.3
Argentina	−0.5	−0.5	5.9	0.5	5.7	0.5	2.0	0.5
Uruguay	0.2	0.2	6.5	0.2	6.4	0.2	1.2	0.1
Chile	0.5	0.6	−0.1	0.0	0.5	0.1	0.7	0.4
Colombia	1.1	1.3	−0.1	−0.1	1.1	0.0	0.6	1.1
Peru	0.4	0.4	0.0	0.0	0.4	0.0	0.6	0.4
Venezuela	0.7	0.7	0.0	0.0	0.7	0.0	0.5	0.6
Rest of Andean pact	0.4	0.4	0.0	0.0	0.4	0.0	0.5	0.3
Mexico	0.9	1.0	0.0	0.0	0.7	0.0	1.2	0.0
Central America and Caribbean	3.4	3.8	0.0	0.0	3.5	0.0	1.7	3.6
Rest of South America	0.1	0.1	−0.1	0.0	0.0	0.0	0.3	0.0
Canada	0.1	0.3	0.0	0.0	−0.1	0.0	0.8	0.2
United States	2.3	2.0	−0.4	−0.4	1.7	0.3	3.0	−0.5
European Union 15	−2.6	−2.2	25.0	5.6	21.2	1.6	39.3	−3.2
Japan	−1.0	−0.9	0.7	0.4	−0.5	0.3	45.7	−1.2
Rest of the world	−4.8	−4.2	−0.2	−0.2	−5.0	1.3	83.6	−5.6
Sum for included countries	12.7	12.4	42.3	6.9	51.6	NA	NA	9.1
Sum for excluded countries	−8.4	−7.2	−0.2	−0.4	−5.5	NA	NA	−9.9
Sum for all countries	4.3	5.2	42.2	6.4	46.1	NA	186.0	−0.9

Note: FTAA with excluded products: FTAA with U.S. antidumping policy denying improved access to its four protected sectors. EU-Mercosur with excluded products: a free trade agreement between Mercosur and the European Union with the seven most protected food and agricultural products in the European Union excluded from the agreement. FTAA and EU-Mercosur: the FTAA combined with a free trade agreement between Mercosur and the European Union. Unilateral 50 percent tariff cut: a Mercosur-only tariff cut of 50 percent. Multilateral tariff liberalization: all regions reduce tariffs and export subsidies by 50 percent. FTAA with no Mercosur liberalization: the FTAA but Mercosur does not change its external tariff to the rest of the Americas.

Source: Authors' computations based on GTAP database (Dimaranan and McDougall 2002), updated 1996 input-output table for Brazil, and the 1996 LSMS conducted by the Brazilian Institute of Geography and Statistics.

TABLE 7. Impact of Trade Policy Options on Macro Variables, Central and Low Elasticities (percentage change)

Macrovariable	Elasticity	FTAA (1)	FTAA with Excluded Products (2)	EU–Mercosur (3)	EU–Mercosur with Excluded Products (4)	FTAA and EU–Mercosur (5)	Unilateral 50% Tariff Cut (6)	Multilateral Tariff Liberalization by 50% (7)	FTAA with no Mercosur Liberalization (8)
Real exchange rate	Central	2.61	2.73	2.25	2.70	3.00	1.97	1.43	−0.2
	Low	1.86	2.01	1.08	1.89	1.98	1.82	1.20	1.9
Change in tariff revenue	Central	0.60	0.56	0.56	0.55	0.69	0.10	0.12	0.0
(% of GDP)	Low	0.52	0.50	0.50	0.48	0.72	0.20	0.24	0.5
Unskilled labor wage rate	Central	2.91	1.87	4.24	2.42	5.81	0.94	3.02	0.0
	Low	1.61	1.05	2.51	1.38	3.64	0.73	2.04	0.7
Skilled labor wage rate	Central	0.97	1.01	1.12	0.60	1.77	0.54	0.31	1.1
	Low	0.66	0.65	0.85	0.44	1.37	0.46	0.48	1.6
Rental rate on capital	Central	−0.13	0.18	−0.47	−0.39	−0.31	−0.08	−0.59	−0.1
	Low	0.17	0.32	0.00	−0.04	0.22	0.10	−0.09	0.2
Rental rate on land	Central	14.21	9.19	25.12	14.84	31.00	5.79	30.00	4.4
	Low	6.31	3.94	13.19	7.38	16.76	3.56	16.27	6.3

Note: FTAA with excluded products: FTAA with U.S. antidumping policy denying improved access to its four protected sectors. EU-Mercosur with excluded products: a free trade agreement between Mercosur and the European Union with the seven most protected food and agricultural products in the European Union excluded from the agreement. FTAA and EU-Mercosur: the FTAA combined with a free trade agreement between Mercosur and the European Union. Unilateral 50 percent tariff cut: a Mercosur-only tariff cut of 50 percent. Multilateral tariff liberalization: all regions reduce tariffs and export subsidies by 50 percent. FTAA with no Mercosur liberalization: the FTAA but Mercosur does not change its external tariff to the rest of the Americas.

Source: Authors' computations based on GTAP database (Dimaranan and McDougall 2002), updated 1996 input-output table for Brazil, and the 1996 LSMS conducted by the Brazilian Institute of Geography and Statistics.

of consumption and in 1996 U.S. dollars. The central elasticity results are presented explicitly, and important differences with low elasticities are also mentioned. Key macrovariables that are important for the interpretation of the household results are presented in table 7.

Regional Arrangements

As part of Mercosur, Brazil is negotiating participation in the FTAA as well as an EU–Mercosur free trade agreement. Brazil will gain an estimated 0.6 percent of personal consumption from the FTAA (about $3 billion; see tables 5 and 6, column 1). The gains to Brazil from a Mercosur–EU agreement are about 1.5 times greater.

Both the FTAA and the EU–Mercosur agreement create very large economic areas, each with one large industrial country partner. These partners have export supply capacities that are large relative to the demand from smaller partners. For a given absolute change in demand resulting from a regional agreement, the larger capacity of these partner countries allows them to supply their smaller partners with relatively elastic supply curves. This prevents the supply price for imports from large partner countries from rising significantly. Finally, large countries offer improved market access, as emphasized in Harrison and others (1997a, 2002). Although in several cases preferential arrangements among small countries have been found to be welfare reducing,[23] for the reasons just mentioned the estimates show that Brazil and most countries in the Americas would gain from an FTAA and that Mercosur countries would gain from a free trade agreement with the European Union.

The one exception to this pattern in the Americas is Argentina, which is estimated to lose slightly from the FTAA. Without FTAA it enjoys preferential access to the markets of the other Mercosur countries. The FTAA provides equivalent access to the other countries in the Americas to the Mercosur market, eroding Argentina's preferential access. The effects of this loss of preferential access plus the trade diversion effects are larger than the trade creation effects.[24]

The combined gains to Argentina, Brazil, and Uruguay are more than 50 percent larger from an EU–Mercosur agreement than from the FTAA (see tables 5 and 6, column 3). The European Union has several agricultural and food products with very high tariffs (see table 2). If Argentina, Brazil, and Uruguay obtain tariff-free access to these markets while the European Union continues to apply these tariffs on other countries, the three countries would receive large terms of trade gains in EU markets. The gains for Uruguay, a relatively small economy, would

23. See Harrison and others (2002) and Bakoup and Tarr (2000). Uruguay also loses from participation in Mercosur.

24. Pereira (1999) and Teixera and others (2002) find the same result for Argentina in the FTAA. Although the gains to Brazil from the FTAA are also eroded because of erosion of preferential access in Argentina, Argentina is a smaller market than the Brazilian market. Thus the erosion of preferential access in the partner's market is more important for Argentina.

be between 6 percent (with low elasticities) and 44 percent (with the central elasticities).[25]

Countries excluded from the agreements typically lose. The European Union, Japan, and the rest of the world all lose from the FTAA, for a combined loss of $8.4 billion (see table 6, column 1). The excluded countries suffer a decline in demand for their exports to the Americas as importers in the Americas shift demand toward suppliers from the Americas. Hence there is both a terms of trade loss on sales that continue and an efficiency loss from having to shift to alternate markets or products. The European Union is estimated to lose $2.6 billion, slightly more than the $2.3 billion the United States is estimated to gain. One exception is Japan under the EU–Mercosur agreement. Japan obtains a small terms of trade improvement in the markets of the rest of the world as countries included in that agreement shift their trade toward each other's markets. The gains to Japan, however, are very small and round to zero at the nearest 0.1 percent of Japan's consumption.

The benefits to Brazil from these two agreements exceed the sum of the benefits for each agreement separately. This is because the combined economic area of the Americas plus the European Union is vast, so Brazil is the less likely to face adverse terms of trade effects as a result of consuming a large share of any exporter's supply. Lost tariff revenues from diverting trade to partner countries that are part of either agreement taken separately are reduced by combining the two agreements. Thus negotiating an agreement with the European Union in addition to the FTAA appears likely to increase the welfare gains to Brazil.[26]

Limitations on Market Access: The Impact of Antidumping, Rules of Origin, and EU Agriculture Exclusions

Although preferential trade arrangements with large industrial countries offer developing economies the promise of increased access to large markets, in practice limitations on improved access significantly reduce the benefits. The European Union has steadfastly refused to grant tariff-free access in its highly protected agricultural products in its association agreements with Central and Eastern European countries, its customs union agreement with Turkey, and its free trade area agreements with various Mediterranean countries (Morocco, Tunisia). Hence it is a priori unlikely to offer such concessions to Mercosur

25. The gains to Uruguay come primarily from the meat sector. Attracted by the tariff umbrella of 95 percent tariffs in the large EU market, Uruguay will dramatically expand meat output and exports to the European Union in the long run. Meat exports are a much more significant share of gross domestic product in Uruguay than they are in Argentina or Brazil. Thus the welfare gain from an improvement in the export price in the European Union in this sector can be expected to result in a larger welfare gain than in Brazil or Argentina. It is likely, however, that such a large expansion of the meat sector would be constrained by "specific factors" in Uruguay that were not modeled.

26. These results are similar to those Harrison and others (2002) found for the "additive regionalism" strategy of Chile, which yielded significantly larger benefits than the agreements taken separately.

when it has refused to offer them to countries for which it might be viewed as having more to gain geopolitically.

As for the FTAA, the United States has strongly resisted efforts to limit the use of antidumping actions as part of the FTAA despite a proposal by Chile to include such a limitation. As the use of tariffs and nontariff barriers has declined, the use of antidumping as a protectionist device has risen significantly in the United States (Finger 1993) (and more recently in the European Union as well; see Messerlin and Reed 1995). Antidumping actions have focused on four sensitive sectors: chemicals, metals, nonelectrical machinery, and electrical equipment. Thus Brazilian authorities have expressed the fear that the benefits of nominally improved access to U.S. markets will be denied by antidumping actions.

Finally, free trade agreements involve rules of origin, requiring that exporters source a share of inputs from within the preferential area. Evidence is accumulating that these rules of origin significantly limit the improved market access of preferential tariff concessions. The Africa Growth and Opportunity Act provides preferential access for African exports to the U.S. markets. Mattoo and others (2002) found that African nonoil exports to the United States would increase by about 50 percent without the stringent rules of origin but by only 10 percent with them. Estevadeordal (2000) found that restrictive rules of origin limited Mexico's improved access to the U.S. market under NAFTA. Brenton and Manchin (2003) argue that EU preferential trade agreements have been ineffective in delivering improved market access, most likely because of the restrictive rules of origin and the costs of proving compliance.

Two simulations illustrate how these limitations of market access by the European Union and the United States can affect the potential gains.

Excluded Agricultural Products in the EU–Mercosur Agreement. This scenario assumes that the European Union fails to provide improved market access to its most highly protected agricultural products in an EU–Mercosur agreement. The EU tariff rates in the database are 65 percent for paddy rice, 44 percent for cereal grains, 86 percent for processed rice, 28 percent for other food products, 95 percent for bovine meat products, 90 percent for dairy products, 61 percent for other meat products, and 76 percent for sugar. These are products in which the Mercosur countries have a comparative advantage, so if the free trade agreement between the European Union and Mercosur excludes these products, the expected benefits would be significantly reduced.

Under the central elasticity results the denial of full market access to these key agricultural products reduces the value of the EU–Mercosur agreement to Brazil from 0.9 percent of consumption to 0.1 percent (see tables 5 and 6, column 4). The estimated gains for Uruguay are also dramatically reduced. The gains to the European Union are also reduced, from 0.5 percent of its consumption to 0.1 percent, reflecting the importance of agriculture liberalization if EU consumers are to reap gains from the agreement.

Antidumping and Rules of Origin in the FTAA. Limitations on access to the U.S. market are more likely to come from restrictive rules of origin and the use of antidumping actions than from explicit exclusion of certain products. Indeed, Brazilian authorities have expressed the fear that the benefits of improved access to U.S. markets will be denied by antidumping actions, as in the steel sector. This scenario estimates the costs to Brazil of continued U.S. protection of its most protected markets even with the FTAA. Protection is 53 percent on sugar, 42 percent on dairy products, 18 percent on oil seeds, and 14 percent on other crops.[27] For these sectors the United States is assumed to employ antidumping duties or stringent rules of origin to neutralize the impact of the FTAA on Brazil's exports (in other words, the U.S. tariff on exports of these products from Brazil does not change).[28]

The impact is to reduce the benefits to Brazil to about two-thirds of the gains it would receive with full market access in the FTAA (see tables 5 and 6, column 2). The reduction is not as severe as with the excluded products in the EU agreement. The large impacts tend to be driven by the tariff peaks, which are not as high in the U.S. market as in the European Union. If the United States fails to provide preferential access to its highly protected products, Brazil can sell these products in other markets in the Americas that also open up to Brazil on a preferential basis as part of the FTAA. With the EU–Mercosur agreement there are no alternate markets in which Brazil has preferential access.

FTAA *with No Change in Mercosur's External Tariffs*. To identify the source of gains, especially at the household level, the impact of the FTAA with no improved access to Mercosur markets is also evaluated. This scenario shows how much of the gains to Brazil come from improved access to the markets of the Americas and how much from lowering Mercosur tariffs, thereby achieving improved resource allocation in Brazil. In this scenario the countries in the Americas outside of Mercosur are assumed to lower their tariffs preferentially to all countries in the Americas (so Brazil obtains improved market access), but the Mercosur countries do not lower their common external tariffs against partner countries in the Americas (so Brazil does not offer any improved market access).

Under this scenario the gains to Brazil are reduced to 0.4 percent of consumption. This shows that improved market access is responsible for about two-thirds of the gain to Brazil from the FTAA and that the remaining one-third of the gain comes from the preferential lowering of the Mercosur tariff (see tables 5 and 6, column 8).

27. The category "other crops" is an aggregate of the following sectors from the full GTAP data set: wheat, vegetables and fruits, fiber-based plants, wool, forestry, fishing, and other crops. Simulations were also performed with wheat as part of grains rather than other crops. Argentina gains more from the EU–Mercosur agreement, but otherwise most of the results change by very small amounts.

28. This is not a full treatment of the potential use of antidumping or rules of origin within the FTAA or of the impact on Brazil. Such treatment would have to account for antidumping duties and stringent rules of origin by the United States against other products and partners in the Americas as well and for the use of antidumping and stringent rules of origin by countries other than the United States.

Tariff Cuts and Uniformity in Mercosur or Multilateral Liberalization—or Both

Simulations were also run for unilateral tariff cuts by Mercosur and for cuts through multilateral trade liberalization under the WTO.

Unilateral Trade Liberalization by 50 Percent and Tariff Uniformity. A 50 percent cut in Mercosur tariffs will result in a welfare increase of about 0.4 percent of Brazilian consumption or about $1.9 billion a year (see tables 5 and 6, column 6). Thus the gains from the FTAA with excluded access to the U.S. market on selected products results in about the same gains as a unilateral tariff cut by Mercosur of 50 percent. With low elasticities, however, the gains are only about 0.2 percent of consumption for Brazil, or $0.4 billion, and the impact on Argentina is negative.[29] The larger terms of trade effects with the lower elasticities account for the lower gains from tariff reduction.

Tariff uniformity (with the same collected tariff revenue) in Mercosur will result in slightly larger welfare gains than a 50 percent cut in tariffs. These results are consistent with earlier results on the benefits of tariff uniformity for Turkey and Chile (Harrison and others 1993, 2002). Similarly, Martinez de Prera (2000) found welfare gains from tariff uniformity in all 13 countries evaluated. Although theory indicates that taxes are more efficient if they are higher on products with relatively low elasticities of demand, evidently tariffs do not typically differ from uniformity in these economies for tax efficiency reasons.[30] On the contrary, the large gains from trade liberalization typically come from reducing tariff peaks, which is effectively accomplished through tariff uniformity. Reducing low tariffs results in proportionately smaller gains and may even result in losses if the importing country possesses monopsony power.

Multilateral Trade Liberalization. Brazilian authorities have also encouraged multilateral trade negotiations and supported the Doha Development Agenda—in part because of a belief that it is the most likely way to achieve agricultural liberalization. In this scenario all countries in the world reduce their tariffs and export subsidies and their taxes by 50 percent.

Brazil gains about 0.9 percent of personal consumption from multilateral trade liberalization in the static model, or about $4.6 billion a year (see tables 5 and 6, column 7). These are larger than the gains from the FTAA and those from an agreement with the European Union that excludes the highly protected

29. Harrison and others (1997c, appendix C) show that the optimal tariff t in any sector of the model is bounded below by $t^* = \{[\sigma_{MM}/(\sigma_{MM} - 1)] - 1\}$. Thus even in the central elasticity case with $\sigma_{MM} = 30$ the optimal tariff is more than 3 percent. But in the low elasticity scenarios, with $\sigma_{MM} = 8$, the optimal tariff is more than 14 percent. With an average Mercosur tariff of 12 percent, the optimum uniform tariff is lower than the existing average tariff in the central elasticity scenarios. The small gains that remain are due to lowering the tariff peaks.

30. The set of elasticities that were chosen, however, makes uniformity beneficial in general. That is, the Ramsey optimal taxation rule suggests that higher taxes should be placed on goods with the lower elasticity of demand. With the virtually homogeneous choice of elasticities here, the Ramsey optimal tariffs are close to uniform.

agricultural and food products. Because these products are likely to be excluded from a Mercosur agreement with the European Union, the results support the strategy of the Brazilian authorities to pursue multilateral liberalization along with the regional options.

The gains to the world from the 50 percent cut in tariffs and export subsidies are estimated at $186 billion with central elasticities (see table 6, column 7) and $87 billion with low elasticities. As Harrison and others (1997b) argue in assessing the Uruguay Round, elasticities play an important role in explaining differences in aggregate gains from multilateral trade liberalization.

Impact on Households and the Poor

The household results follow a similar pattern across all of the policy scenarios (table 8). The poorest household will typically gain several times the aggregate gains for the economy expressed as a percentage of household consumption.[31] Although the impact on household incomes is not strictly progressive, the four poorest urban households and four poorest rural households are among the biggest gainers from the reforms as a percentage of their own household consumption.

What accounts for this robust and encouraging result? Trade protection in Brazil favors capital-intensive manufactures, so liberalization shifts resources toward more unskilled labor-intensive agriculture. Thus the wage rate of unskilled labor increases significantly more that the rent on capital (see table 7).[32] The poorest households earn most of their income from unskilled labor (see table 4), so they gain proportionally more than other households.

Although the impact on sectors depends on the specific agreement, there is a general pattern. Oilseeds, other agriculture (excluding grains and wheat), other crops (which includes fruits and vegetables and wheat), processed food, and leather sectors expand production and exports. These sectors, especially the agricultural sectors,[33] are the most intensive users of unskilled labor in the model. Several manufacturing sectors decline, including motor vehicles, other metal products, and other manufacturing. These declining sectors are among the most capital-intensive in Brazil.

These outcomes reflect relative protection in Brazil, which favors manufacturing at the expense of agriculture and processed food products. Despite substantial trade liberalization, vestiges of Brazil's import-substitution indus-

31. The percentage gains for the poor relative to the aggregate percentage gains are similar for low trade elasticities.

32. The poor typically have not accumulated large stores of real assets or financial assets, so they do not earn significant capital income or income from the rent of land. Nor have the poor typically accumulated much human capital, so they earn a much smaller share of their income from skilled labor than do the middle classes.

33. The EU–Mercosur agreement (without exceptions) induces a much larger increase in agricultural output in Brazil than do the other agreements because of the large increase in preferential access for Mercosur countries in the European Union.

TABLE 8. Impact of Mercosur Trade Policy Options on Brazilian Households as a Share of Consumption, Central Elasticities (welfare change, %)

Household type	FTAA (1)	FTAA with Excluded Products (2)	EU–Mercosur (3)	EU–Mercosur with Excluded Products (4)	FTAA and EU–Mercosur (5)	Unilateral 50% Tariff Cut (6)	Multilateral Tariff Liberalization by 50% (7)	FTAA with no Mercosur Liberalization (8)
Rural								
1	2.5	1.7	4.0	2.1	5.5	1.5	2.9	0.8
2	2.3	1.5	3.9	1.8	5.4	1.2	2.8	1.0
3	2.5	1.5	4.5	1.9	6.2	1.1	3.5	1.3
4	2.5	1.8	3.9	2.2	5.4	1.5	3.1	0.8
5	1.3	0.8	2.3	0.7	3.5	0.6	1.8	0.8
6	1.5	1.0	2.3	0.8	3.6	0.7	1.7	0.8
7	1.3	0.9	2.0	0.7	3.2	0.6	1.6	0.7
8	3.1	2.0	4.8	2.4	6.9	1.2	4.1	1.4
9	0.9	0.4	1.7	0.6	2.6	0.4	1.8	0.7
10	3.7	2.3	6.0	2.8	8.3	1.4	4.9	1.6
Urban								
1	2.5	1.8	3.8	2.1	5.2	1.5	2.7	0.7
2	2.3	1.6	3.8	1.8	5.2	1.3	2.6	0.8
3	2.2	1.4	3.6	1.7	5.0	1.2	2.6	0.9
4	2.0	1.3	3.1	1.5	4.5	1.0	2.4	0.8
5	1.3	0.7	2.4	0.8	3.5	0.7	1.8	0.8
6	1.6	1.0	2.6	1.0	3.9	0.7	1.9	0.8
7	0.4	0.3	0.9	0.0	1.6	0.3	0.7	0.4
8	0.3	0.2	0.7	−0.1	1.4	0.3	0.7	0.4
9	−0.5	−0.4	−0.3	−0.7	−0.1	0.0	0.1	0.2
10	0.0	0.2	−0.2	−0.5	0.5	0.1	0.0	0.2

Note: FTAA with excluded products: FTAA with U.S. antidumping policy denying improved access to its four protected sectors. EU-Mercosur with excluded products: a free trade agreement between Mercosur and the European Union with the seven most protected food and agricultural products in the European Union excluded from the agreement. FTAA and EU-Mercosur: the FTAA combined with a free trade agreement between Mercosur and the European Union. Unilateral 50 percent tariff cut: a Mercosur-only tariff cut of 50 percent. Multilateral tariff liberalization: all regions reduce tariffs and export subsidies by 50 percent. FTAA with no Mercosur liberalization: the FTAA but Mercosur does not change its external tariff to the rest of the Americas.

Source: Authors' computations based on GTAP database (Dimaranan and McDougall 2002), updated 1996 input-output table for Brazil, and the 1996 LSMS conducted by the Brazilian Institute of Geography and Statistics.

trialization protection structure remain. When protection in the economy is reduced, resources shift toward the agriculture and food sectors that had been disadvantaged relative to manufacturing. The expanding sectors tend to be less capital-intensive than the contracting sectors. International trade theory argues that the price of the factor of production used intensively in protected sectors should fall relative to the price of the factor of production in the unprotected sectors following trade liberalization.[34] Thus the wage rate of unskilled labor rises relative to the rent on capital, benefiting the poor. Because the value of land rises even more than the wage rate of unskilled labor, two of the richest rural households are the biggest gainers from the reforms.

To document this interpretation of why the poor can be expected to gain proportionally more than wealthier households, the impact of the FTAA on households was decomposed in table 9. Column 1 reproduces the base results from table 8 for the FTAA. Column 2 presents the results of assuming that all households consume the commodities in the same proportions. Although the gains to the poorest households decline slightly, they remain three to four times greater than the percentage gains for all households together. Thus, disparate consumption shares do not explain why poor households gain more from the trade policy changes.

Column 3 presents the results of the FTAA scenario in which all households are assumed to earn the same shares of income from the wages of unskilled labor and skilled labor and rent on capital and land. Most of the poorest households would obtain only a slightly greater increase in income than the average for all households of 0.6 percent. This confirms that it is the more than proportionate rise in the price of the factors of production important to the income of poor households that explains why poor households gain more from these trade policy options. The factor most important to the poor is the wage rate of unskilled labor (see table 4), which rises fastest among the important household income factors (see table 7).

An additional simulation offers further support for this explanation. For reasons explained earlier, the capital intensity in agriculture sectors is estimated to be significantly less than reported in the Brazilian input-output table. There is a dramatic difference in the results for the estimated welfare gains from the FTAA to Brazilian households when the biased factor shares in the GTAP data are used instead of the corrected data. The poorest rural household is estimated to gain 0.5 percent of its consumption and the poorest urban household 0.4 percent, equal or slightly less than the aggregate average percentage gain (see table 9, column 4). This shows that the corrections to the factor share data are crucial to the results at the household level and supports the interpretation that the shift of

34. This is known as the Stolper-Samuelson theorem. In this model, however, product differentiation mutes strict application of the Stolper-Samuelson results.

TABLE 9. Decomposition of the Impact of the FTAA on Brazilian Households as a Share of Consumption, Central Elasticities (change in welfare, %)

	FTAA	FTAA with Uniform Consumption Shares	FTAA with Uniform Income Shares	FTAA with Factor Shares from Input-Output Table
	(1)	(2)	(3)	(4)
All households	0.6	0.6	0.6	0.5
Rural				
1	2.5	2.0	1.0	0.5
2	2.3	1.8	0.7	0.3
3	2.5	2.0	0.5	0.1
4	2.5	2.1	0.9	1.0
5	1.3	0.8	0.5	0.4
6	1.5	0.7	0.9	0.7
7	1.3	0.6	0.7	0.5
8	3.1	2.8	1.0	2.2
9	0.9	1.2	0.0	0.8
10	3.7	3.2	1.1	2.0
Urban				
1	2.5	2.0	1.0	0.4
2	2.3	1.8	0.9	0.2
3	2.2	1.8	0.7	0.0
4	2.0	1.6	0.7	0.9
5	1.3	1.0	0.3	0.1
6	1.6	1.4	0.6	0.4
7	0.4	0.2	0.3	0.1
8	0.3	0.3	0.2	0.0
9	−0.5	0.0	−0.3	−0.3
10	0.0	0.1	0.9	0.9

Source: Authors' computations based on GTAP database (Dimaranan and McDougall 2002), updated 1996 input-output table for Brazil, and the 1996 LSMS conducted by the Brazilian Institute of Geography and Statistics.

resources toward agriculture is important in increasing the incomes of the poor and reducing poverty.

The results also show that the Mercosur tariff changes are more important to the poor than improved market access (see tables 5 and 8, column 8). In this scenario Mercosur does not change its own tariffs but obtains improved access to the markets of the Americas. The average gains to the economy fall by about one-third compared with the FTAA gains, but the gains to the poorest households fall by two-thirds. This is because Mercosur's tariff changes induce output expansion in the sectors that intensively use unskilled labor, thus increasing unskilled wages relative to other factor prices. Improved market access does not increase the price of unskilled labor relative to capital. With only improved market access, the poor gain, but not progressively as they do with internal liberalization in Mercosur.

Although the trade reforms are significantly propoor, the model implicitly assumes a time horizon long enough to reestablish equilibrium after some policy shock. Thus it is possible that during the transition to a new equilibrium some poor households will be hurt. This is especially likely among households that move out of the declining sectors, such as the more highly protected manufacturing sectors. This emphasizes the need for an effective safety net to assist the poor.

To test the robustness of the results with respect to parameter specification, 500 simulations were conducted of the impact of the FTAA. Key parameter values were drawn randomly from specified probability distributions (see Harrison and others 2003b, appendix F on systematic sensitivity analysis). As a result of the FTAA, the following results will hold with virtual certainty: Brazil will gain at least 0.3 percent of its consumption, FTAA members will gain at least $12 billion a year, excluded countries will lose at least $6.7 billion a year, and the poorest urban and rural households will gain at least 1 percent of their income. The sensitivity results confirm the conclusions drawn from the point estimates of the gainers and losers at the household level and the aggregate country level.

Conclusions

Both the FTAA and an EU–Mercosur agreement would benefit Brazil, but exceptions to the agreements (rules of origin, antidumping, and agricultural exclusions) would significantly diminish the gains. The estimates presented here indicate that Brazil can optimize its choice of trade policies by combining regional arrangements in both the Americas and Europe with multilateral liberalization. If tariff uniformity is added to the regional and multilateral liberalization, further gains could be realized.

Both the FTAA and the EU–Mercosur arrangements are net trade creating for the countries involved, but excluded countries almost always lose. Multilateral trade liberalization results in gains to the world more than four times greater than either of these relatively beneficial regional arrangements, showing the importance of multilateral negotiations.

Most of the trade policy options evaluated result in a progressive distribution of the gains to Brazilian households, with the poorest households experiencing the greatest percentage increase in their incomes. Trade policy changes tend to shift resources from capital-intensive manufacturing toward unskilled labor-intensive agriculture and less capital-intensive manufacturing, inducing an increase in unskilled labor wages relative to the prices of other factors of production. The percentage increase in the incomes of the poorest households is three to four times greater than the average percentage increase in income for the economy as a whole.

A micro-simulation model would likely find that some poor households could lose, especially in the short run. This emphasizes the need for having effective safety net policies in place. But because the sectors that are important to the poor tend to be disfavored by the structure of protection, the medium- to long-run effects of these trade reforms should be positive for the vast majority of the poorest households.

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Trade Liberalization and Industry Wage Structure: Evidence from Brazil

Nina Pavcnik, Andreas Blom, Pinelopi Goldberg, and Norbert Schady

Industry affiliation provides an important channel through which trade liberalization can affect worker earnings and wage inequality between skilled and unskilled workers. This empirical study of the impact of the 1988–94 trade liberalization in Brazil on the industry wage structure suggests that although industry affiliation is an important component of worker earnings, the structure of industry wage premiums is relatively stable over time. There is no statistical association between changes in industry wage premiums and changes in trade policy or between industry-specific skill premiums to university graduates and trade policy. Thus trade liberalization in Brazil did not significantly contribute to increased wage inequality between skilled and unskilled workers through changes in industry wage premiums. The difference between these results and those obtained for other countries (such as Colombia and Mexico) provides fruitful ground for studying the conditions under which trade reforms do not have an adverse effect on industry wage differentials.

Policymakers often promote trade liberalization and openness as a way to increase living standards and welfare in developing economies.[1] From 1988 to 1994, Brazil, like many Latin American economies, followed these policy recommendations. The reforms not only reduced the average tariff level from about 60 percent in 1987 to 15 percent in 1998 but also changed the structure of protection across industries. These drastic tariff reductions were mirrored in increased import penetration in most sectors.

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1. Although the theoretical relationship between free trade and welfare is ambiguous, careful empirical work based on cross-country data by Frankel and Romer (1999) confirms that countries with higher exposure to trade have higher living standards, as measured by per capita GDP.

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Although empirical studies have documented that the trade reforms increased efficiency and growth (Hay 2001; Muendler 2002), the reforms might have also contributed to growing wage inequality. Several studies document growing returns to educated workers in Brazil that coincide with the timing of trade liberalization (Behrman and others 2000; Blom and others 2001; Green and others 2001; Sánchez-Páramo and Schady 2003).[2] Most of this literature concentrates on the effects of trade on the returns to particular worker characteristics (such as skill) in the long run, when labor can move across sectors and industry affiliation does not matter.

This article takes a different approach. It investigates the relationship between trade liberalization and industry wage premiums. Wage premiums represent the portion of worker wages that cannot be explained by characteristics of workers or firms but are attributed to a worker's industry affiliation.

Understanding this relationship is important for several reasons. First, industry affiliation is crucial in predicting the impact of trade reforms on workers' wages in short- and medium-run models of trade and in models with imperfect competition and rent sharing. Studies that do not consider industry affiliation may thus miss an important channel of trade policy effects on wage distribution. These models seem a priori particularly relevant in Latin America, where labor market restrictions that can obstruct labor mobility across sectors are common (Heckman and Pages 2000) and where domestic industries are often shielded from foreign competition, giving rise to market power and industry rents.

Second, the effect of trade policy on industry wage premiums has implications for wage inequality between skilled and unskilled workers. Because different industries employ different proportions of skilled and unskilled workers, changes in industry wage premiums translate into changes in the relative incomes of skilled and unskilled workers. If tariff reductions are proportionately larger in sectors employing unskilled workers, and if these sectors experience a decline in their relative wages as a result of trade liberalization, these unskilled workers will experience a decline in their relative incomes. This effect is distinct from the potential effect of trade liberalization on the economywide skill premium.

Moreover, industry wage premiums might vary across workers with different levels of skill or education. For example, the more educated workers may be more or less mobile in the labor market, have accumulated more sector-specific human capital, or have more bargaining power over industry rents. If wage premiums differ across workers with different levels of education, and if trade liberalization increases industry-specific skill premiums, this could provide an additional channel through which reforms could affect wage inequality. Very

2. Rising skill premiums have been documented in Mexico and many other liberalizing Latin American economies (see Robbins 1996; Cragg and Epelbaum 1996; Harrison and Hanson 1999; Robertson 2000b; Behrman and others 2000; and Attanasio and others 2004).

few studies focus on the relationship between trade policy and industry wage premiums.[3] Those that do have yielded mixed conclusions and except for Goldberg and Pavcnik (forthcoming) do not consider the implications of industry wage premiums for wage inequality between skilled and unskilled workers.

This article empirically addresses the relationship between trade policy and industry wage premiums by combining detailed worker-level information from the Brazilian Monthly Employment Survey (PME) with industry-level data on tariffs, import penetration, and export exposure during 1987–98, which includes the Brazilian trade liberalization episode of 1988–94. The analysis finds no association between trade reforms and industry wage premiums. Although industry affiliation does play a role in determining workers' earnings, accounting for 4–6 percent of the explained variation in log hourly wages, and although industry wage premiums vary widely across industries, the structure of industry wage differentials is very stable and is not affected by the changing structure of trade protection. Moreover, no statistical relationship was found between sector-specific skill premiums (measured by the return to a completed university education) and tariff reductions. Overall, the analysis concludes that trade reform in Brazil did not contribute to wage inequality between skilled and unskilled workers through differential changes in industry wage premiums or through increases in industry-specific skill premiums.

I. Theoretical Background

Trade theory predicts how trade policy might affect industry wage premiums. In short- and medium-run models of trade, where labor is immobile across sectors and industries are perfectly competitive, workers' wages depend on product prices and the marginal product of labor in an industry. The models predict a positive association between industry tariffs and wages, so that declines in industry tariffs lead to proportional declines in industry wages.[4] These predictions are consistent with the popular belief that trade liberalization will make workers in previously protected sectors worse off.

Models with imperfectly competitive product and labor markets provide additional mechanisms through which industry tariffs affect industry wages. For example, in profitable industries unions might be able to bargain over industry rents and secure higher wages. Because trade liberalization likely lowers the profit margins of domestic firms that were previously sheltered from foreign competition

3. Revenga (1997), Gaston and Trefler (1994), Feliciano (2001), Robertson (2000a), Goldberg and Pavcnik (forthcoming), and Arbache and Menezes-Filho (2000) are examples of related work. Arbache and Menezes-Filho (2000) find significant evidence of rent sharing during trade liberalization in Brazilian manufacturing during 1989–95 when they instrument for value added with the effective tariffs.

4. In contrast, the long-run Hecksher-Ohlin model predicts that trade reform should affect only economywide returns to the factors of production but not industry-specific returns, because all factors of productions are mobile across uses.

(Harrison 1994; Levinsohn 1993), lower tariffs are associated with lower industry wages. Grossman (1984) presents a model in which unions extract the rents associated with protection in the form of employment guarantees rather than wages. This channel implies a potentially negative association between tariffs and industry wages.

Finally, trade liberalization might affect industry wages through trade-induced productivity improvements. Although trade theory does not yield clear-cut predictions on whether trade liberalization increases or decreases productivity (Rodrik 1991; Roberts and Tybout 1996; Melitz 2003), empirical work finds strong evidence that declines in tariffs are associated with productivity improvements (Harrison 1994; Krishna and Mitra 1998; Kim 2000; Pavcnik 2002; Fernandes 2001). Hay (2001) and Muendler (2002) estimate that the 1988–94 trade reforms had a significant impact on plant-level productivity in Brazil. As tariffs declined, firms had to become more productive to remain competitive. If the productivity enhancements were partially passed onto workers through higher industry wages, wages would increase in the industries with the largest tariff declines.

Thus although industry affiliation provides an important channel through which trade policy can affect workers' wages, these models do not yield unambiguous predictions about the direction of the expected effect of trade liberalization on wages. The question is one that needs to be resolved empirically.

II. Methodology and Data

A two-stage estimation framework, familiar from the labor literature on industry wages, was used to empirically investigate the effect of trade exposure to wage premiums. In the first stage the log of worker i's wages (w_{ijt}) was regressed on a vector of worker i's characteristics (H_{ijt}), such as education, age, age squared, gender, geographic location; an indicator for whether the person is self-employed; an indicator for whether the person works in the informal sector; and a set of industry indicators (I_{ijt}) reflecting worker i's industry affiliation:

$$(1) \qquad \ln(w_{ijt}) = H_{ijt}\beta_{Ht} + I_{ijt} * wp_{jt} + \epsilon_{ijt}$$

The coefficient on the industry dummy, the wage premium, captures the part of the variation in wages that cannot be explained by workers' characteristics but can be explained by workers' industry affiliation. Following Krueger and Summers (1988), the estimated wage premiums are expressed as deviations from the employment-weighted average wage premium.[5] This normalized wage premium can be interpreted as the proportional difference in wages for a worker in a given industry relative to an average worker in all industries with the same observable characteristics. The normalized wage differentials and their exact standard errors are calculated using the Haisken-DeNew and Schmidt (1997)

5. The sum of the employment-weighted normalized wage premiums is zero.

two-step restricted least squares procedure provided by the authors.[6] The first-stage regressions are estimated separately for each year in the sample, as the subscript t in equation 1 indicates. The second stage pools the industry wage premiums (wp_{jt}) over time and regresses them on trade-related industry characteristics in first differenced form:

$$\Delta wp_{jt} = \Delta T_{jt}\beta_T + D_t\beta_D + u_{jt} \tag{2}$$

The primary variable included in T_{jt}, the vector of trade-related industry characteristics, is tariffs. Other controls in T_{jt} are also considered, such as lagged import penetration, lagged export to output share, and interactions of these variables with exchange rates. The vector D_t consists of a set of year indicators. Because the dependent variable in the second stage is estimated, equation 2 is estimated using weighted least squares, with the inverse of the standard error of the wage premium estimates from the first stage as weights. This procedure gives more weight to industries with smaller variance in industry premiums. To account for general forms of heteroscedasticity and serial correlation in the error term in equation 2, robust (Huber-White) standard errors were computed, clustered by industry.

Labor Force Data

The labor market data from the pme for 1987–1998 cover the six largest metropolitan areas in Brazil: São Paulo, Rio de Janeiro, Porto Alegre, Belo Horizonte, Recife, and Salvador. These areas account for about 31.9 million of the country's 79 million people in the economically active population. Moreover, in 1997 the states in which the six metropolitan areas are located produced 72 percent of Brazilian GNP.[7] The findings are thus representative of the large and modern parts of the Brazilian labor market but do not necessarily carry over to the rural economy. Because the focus is on manufacturing, however, this might not be problematic.

Data were collected on workers affiliated with any of 18 manufacturing and 2 mining sectors and covered employees or self-employed workers ages 15–65 engaged in full-time work (defined as working more than 25 hours a week). The data were used to create several variables capturing worker demographic characteristics, such as wage, age, education, geographical location, employment in the informal sector, self-employment, and industry affiliation.

The wage measure is the hourly wage (one-quarter of the monthly wage times the reported number of hours worked per week), deflated by the monthly national price index. All wages are expressed in September 1997 R$. The

6. Haisken-DeNew and Schmidt (1997) adjust the variance covariance matrix of the normalized industry indicators to yield an exact standard error for the normalized coefficients.

7. Brazilian gnp was 864,112 million reals (R$) and the six states (São Paulo, Rio de Janeiro, Rio Grande do Sul, Minais Gerais, Pernambuco, and Bahia) accounted for R$618,728 million, according to Brazilian Institute of Geography and Statistics accounts of gross regional products in current market prices.

main education indicator is completed years of schooling, computed using an algorithm based on three survey questions on education.[8] Workers are classified as those with no completed level of education, completed elementary education, completed lower secondary education, completed secondary education, and completed tertiary education.[9] Formal and informal sector workers are distinguished by whether they had a signed workcard (*carteira assinada*). Because a signed workcard legally entitles a worker to several rights and benefits, it can be used to identify whether a person works for a formal establishment that complies with labor market regulations. The variable "informal workers" takes a value of one if the worker is employed in the informal sector of the economy.

Trade Exposure Data

Until the 1980s Brazil pursued an import substitution policy to shield domestic firms from foreign competition. High tariffs and a large number of nontariff barriers provided high levels of protection to Brazilian firms and severely impeded access of foreign goods to the Brazilian market. Protection varied widely across industries, with tariffs ranging from more than 100 percent on clothing, the most protected sector, and 82–86 percent on textiles and rubber to almost 16 percent on oil (table 1). This suggests that Brazil strongly protected relatively unskilled, labor-intensive sectors, which conforms to a finding by Harrison and Hanson (1999) for Mexico and Goldberg and Pavcnik (forthcoming) for Colombia.

From 1988 to 1994 Brazil underwent significant, if gradual, trade liberalization. In 1988 and 1989 the average tariff was reduced from about 60 to 39 percent. Kume (2000) and Hay (2001) argue that this initial reduction had no significant bearing on the exposure of domestic industries to increased foreign competition because substantial nontariff barriers remained, including import licenses, special import programs, and administrative barriers. These were eliminated in the second stage of the reforms that started in 1990 as the Collor government sought to improve productivity by exposing domestic firms to increased foreign competition.[10] At the same time average tariffs were further reduced, from 34 percent in 1990 to 11 percent tariff in 1995.

In 1995 the government partially reversed these trade reforms following real appreciation of the real that lowered the competitiveness of the manufacturing sector and widened the current account deficit. Nevertheless, the average tariff climbed only slightly between 1995 and 1998. In addition to the unilateral trade liberalization that took place from 1988 to 1994, Brazil joined Mercosur, a

8. The algorithm follows the standard conversion used elsewhere (see Lam and Schonie 1993; Barros and Ramos 1996).

9. Primary education in Brazil consists of four years of schooling. Secondary education (*ensino medio*) comprises two parts, 5–8 years of schooling and 9–11 years of schooling. Tertiary education includes 12–15 or more years of schooling.

10. Detailed information on nontariff barriers is not available.

TABLE 1. Industry Tariffs and Correlation of Industry Import Penetration and Tariffs for Brazil

	Tariff (%)		Correlation with Import Penetration[a]	
Industry	1986	1998	Current Tariff	Lagged Tarriff
Mineral extraction	20.5	6.4	−.88	−.69
Oil extraction	15.6	0.0	.73	.75
Nonmetalic mineral trasformation	63.7	13.7	−.66	−.73
Metalic products and steel	32.5	11.2	−.44	−.46
Machinery and equipment	47.0	17.2	−.80	−.83
Electrical and electronic equipment	59.8	18.8	−.91	−.91
Transportation vehicles	77.1	32.5	−.65	−.66
Wood and furniture	50.0	14.0	−.51	−.62
Paper, pulp, and cardboard	59.5	14.2	−.61	−.68
Rubber products	82.0	15.0	−.74	−.79
Chemicals	59.9	16.3	−.53	−.52
Petrochemicals	32.5	10.0	−.87	−.95
Pharmaceuticals	72.3	10.7	−.83	−.85
Plastics	36.6	18.1	−.74	−.82
Textiles	85.8	19.0	−.83	−.89
Clothing	102.7	22.8	−.71	−.79
Footwear	74.1	17.9	−.85	−.89
Tobacco	62.5	14.3	−.71	−.74
Foods	60.3	16.0	−.60	−.63
Beverages	80.5	19.0	−.69	−.78

[a]Import penetration refers to imports as a percentage of output plus net imports.

Source: Authors' calculations based on tariff data from Muendler (2002) (http://socrates.berkeley.edu/~muendler), which draws on Kume and others (2000).

regional trading bloc also comprising Argentina, Paraguay, and Uruguay, in 1991. Although the focus here is the impact of the unilateral trade liberalization, Brazil's tariffs on Mercosur imports and its trade within Mercosur are used to check the robustness of the findings.

Brazil's trade liberalization provides an excellent setting to study the relationship between wages and trade. From 1987 to 1998 the average tariff across 20 industrial sectors (table 1) declined from 58.8 percent to 15.4 percent (table 2).[11] The reforms also changed the structure of protection across industries, as different industries experienced different rates of tariff changes, and tariff dispersion declined significantly. The changing structure of protection is reflected in the low year-to-year correlations of industry tariffs from 1987 to 1998. For example, the correlation coefficient between tariffs in 1987, a year preceding the trade reforms,

11. The original tariff data provide the tariff levels for 53 sectors at the level (*nivel*) 80 Brazilian industrial classification. So that the tariff information corresponds to the level of industry aggregation in the labor force data, the data were aggregated here to level 50, and some additional adjustments were made. The reported tariffs are simple averages of more disaggregated data. The tariff series was also constructed using level 80 import penetration as weights, which yielded similar aggregate means; the correlation coefficient between the two series was 0.98. Thus simple average tariffs were used for the study.

TABLE 2. Trade Policy and Trade Exposure 1987–98 (percent)

	Tariffs		Import Penetration[a]		Export Exposure[b]	
Year	Mean	SD	Mean	SD	Mean	SD
1987	58.8	22.8	5.7	8.6	9.7	11.2
1988	50.1	18.3	5.9	8.5	9.5	11.3
1989	39.1	16.4	6.1	8.4	9.4	11.5
1990	34.1	17.0	6.4	8.4	9.2	11.6
1991	25.2	13.3	7.6	8.6	10.9	12.4
1992	19.1	10.3	7.7	8.8	13.4	13.6
1993	14.4	7.2	8.0	8.4	13.0	13.2
1994	12.9	6.2	8.6	8.3	11.5	11.2
1995	10.9	5.7	9.8	8.1	11.0	10.8
1996	12.5	6.6	9.8	8.1	11.4	11.8
1997	12.8	7.0	10.6	8.3	11.7	12.2
1998	15.4	6.5	11.6	7.8	11.2	10.1

Note: The values cover 20 industries, except for 1998, which covers 18 for import penetration and export exposure.

[a]Imports as a percentage of output plus net imports.

[b]Exports as a percentage of output.

Source: Muendler (2002). For tariffs Muendler draws on Kume and others (2000).

and tariffs in 1989 is 0.81. The correlation coefficient between tariffs in 1987 and 1995, the year after the large reforms were completed, drops to 0.6. The vast variation in Brazilian tariffs across industries at a given time and across time provides an excellent setting to study the relationship between trade and wages.

The shifts in Brazil's trading environment are mirrored in the increased import penetration rate (imports as a percentage of output plus net imports) and export exposure (exports as a percentage of output).[12] From 1987 to 1998 average import penetration increased from 5.7 percent to 11.6 percent and average export exposure from 9.7 percent to 11.2 percent. Whereas import penetration almost doubled, it continues to be low compared with a country such as Colombia, which liberalized during the same period. Colombia's manufacturing import penetration rate was about 21 percent in 1984 and exceeded 30 percent after the 1990 tariff reductions (Goldberg and Pavcnik forthcoming). This difference could be attributed to the large size of Brazil relative to Colombia. Moreover, increases in import penetration rates in Brazil varied significantly across sectors. Industries with the largest gains are clothing, transport, textiles, machinery, electronics, and

12. Data on import penetration and export exposure were obtained from Muendler (2002) at online at http://socrates.berkeley.edu/~muendler. The data were adjusted so that the trade exposure information corresponds to the level of industry aggregation in the labor force data. The industry-level trade exposure measures used were weighted by the import penetration of the less disaggregated level 80 industry data. The correlation between the weighted import penetration series and the import penetration series based on simple averages is 0.99. Similarly, the correlation between the weighted export exposure series and the export exposure series based on simple averages is 0.99.

pharmaceuticals—also the industries that experienced large tariff declines. Correlation coefficients for import penetration and tariffs (and lagged tariffs) over time show, unsurprisingly, that imports and tariffs are negatively correlated (oil extraction is an exception), ranging from −0.4 in steel to −0.9 in electrical and electronic equipment. The correlation increases in absolute value for lagged tariffs.

III. Industry Wage Premiums and Trade Policy: Results

Before exploring whether trade liberalization affected industry wage premiums, results are presented for the first-stage regressions of equation 1.

First-Stage Results

The first-stage results show, as in previous work, that several characteristics are associated with higher wages: age, being male, education, being self-employed, and working in the formal sector (table 3). The results also show that workers experience changes in returns to education over time. A noteworthy change is the decline in the wages of workers with secondary education relative to the wages of less skilled workers (no education or completed elementary) only and more skilled workers (complete tertiary education).[13]

Industry affiliation plays a material role in explaining the variation in log hourly earnings. For example, in 1987 worker characteristics and regional indicators alone account for 50 percent of the total variation in log hourly wages. The addition of industry indicators to the regression increases R^2 to 0.52, which suggests that, conditional on other worker characteristics, industry indicators account for 4 percent of the explained variation in log hourly wages in 1987 (see table 3). In general, industry indicators account for 4–6 percent of the explained variation in log hourly wages between 1987 and 1998.

Industry wage premiums vary widely across industries (table 4). The estimates for 1987, for example, range from 0.55 for the petrochemical industry to −0.20 for foods. A worker in 1987 with the same observable characteristics who switched from the textile industry, where the wage premium is −0.079, to the chemical industry, where the wage premium is 0.168, would experience a 25 percent increase in hourly wages. The standard deviations of the industry wage differentials reported at the bottom of table 4 summarize the overall variability of the industry wage premiums. The variation in industry wage differentials in a given year ranges from 13 percent to 16 percent, implying that changing industries has a large impact on worker earnings. The variation is largest in the period 1992 to 1994.

13. Some of this decline is presumably related to the increasing number of workers with a secondary education relative to the number with a primary or a university education (see Sánchez-Páramo and Schady 2003). The increases in the returns to a university education are not confined to workers in manufacturing industries or to urban areas. Blom and others (2001) find similar patterns in the returns to education for workers in traded and nontraded industries. Green and others (2001) also document rising skill premiums using data from National Household Surveys that cover rural and urban areas.

Table 3. First-Stage Regression Results for Worker Characteristics and Industry Indicators, 1987–98

Variable	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
Age	0.067**	0.065**	0.064**	0.063**	0.059**	0.052**	0.054**	0.061**	0.056**	0.059**	0.059**	0.058**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Age squared	−0.001**	−0.001**	−0.001**	−0.001**	−0.001**	−0.001**	−0.001**	−0.001**	−0.001**	−0.001**	−0.001**	−0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	−0.452**	−0.440**	−0.462**	−0.450**	−0.424**	−0.458**	−0.430**	−0.442**	−0.438**	−0.392**	−0.384**	−0.387**
	(0.010)	(0.010)	(0.011)	(0.012)	(0.011)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.018)
Elementary education	0.268**	0.258**	0.252**	0.251**	0.227**	0.220**	0.219**	0.183**	0.190**	0.183**	0.202**	0.187**
	(0.006)	(0.006)	(0.007)	(0.008)	(0.007)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.013)
Lower secondary education	0.572**	0.551**	0.542**	0.523**	0.484**	0.452**	0.442**	0.430**	0.425**	0.421**	0.436**	0.421**
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.015)
Upper secondary education	1.079**	1.047**	1.051**	1.035**	0.951**	0.931**	0.922**	0.933**	0.906**	0.868**	0.867**	0.843**
	(0.008)	(0.008)	(0.009)	(0.010)	(0.009)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.015)
Tertiary education	1.823**	1.862**	1.880**	1.897**	1.831**	1.762**	1.795**	1.806**	1.778**	1.804**	1.766**	1.725**
	(0.010)	(0.011)	(0.012)	(0.013)	(0.012)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.021)
Self-employed	0.091**	0.099**	0.119**	0.148**	0.097**	0.021	0.044**	0.078**	0.137**	0.072**	0.069**	0.074**
	(0.016)	(0.016)	(0.018)	(0.018)	(0.016)	(0.017)	(0.017)	(0.017)	(0.016)	(0.015)	(0.015)	(0.022)
Informal	−0.162**	−0.238**	−0.220**	−0.162**	−0.158**	−0.265**	−0.254**	−0.205**	−0.136**	−0.124**	−0.130**	−0.165**
	(0.010)	(0.010)	(0.012)	(0.012)	(0.011)	(0.012)	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)	(0.015)
R^2	0.52	0.54	0.52	0.52	0.53	0.51	0.51	0.5	0.52	0.53	0.53	0.52
R^2 without industry indicators	0.50	0.52	0.50	0.50	0.51	0.48	0.48	0.47	0.50	0.51	0.50	0.49
Variation attributed to industry indicators	.04	.04	.04	.04	.04	.06	.06	.06	.04	.04	.06	.06
Number of observations	65,455	58,659	48,881	47,983	44,818	38,447	36,720	38,080	37,159	34,933	34,122	16,307

**Significant at the 5 percent level.

Note: Numbers in parentheses are standard errors. All regressions include industry indicators and regional indicators.

Source: Authors' calculations based on data from Brazil's PME.

Table 4. Industry Wage Premiums, 1987–98

Industry	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
Mineral extraction	.238	.216	.115	.109	.142	.189	.166	.164	.037	.178	.269	.146
	(.023)	(.024)	(.027)	(.028)	(.026)	(.029)	(.030)	(.031)	(.032)	(.029)	(.030)	(.042)
Oil extraction	.092	.003	.036	.071	.102	.085	.089	.048	.014	.079	.094	.124
	(.019)	(.020)	(.025)	(.026)	(.026)	(.026)	(.030)	(.030)	(.030)	(.031)	(.029)	(.044)
Nonmetalic mineral trasformation	−.137	−.096	−.083	−.155	−.135	−.090	−.118	−.128	−.115	−.106	−.077	−.135
	(.010)	(.011)	(.012)	(.012)	(.012)	(.013)	(.014)	(.014)	(.014)	(.014)	(.015)	(.021)
Metalic products and steel	.021	.021	.027	.022	.012	.022	.001	.010	.016	−.010	−.009	.001
	(.005)	(.006)	(.006)	(.007)	(.006)	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)	(.010)
Machinery and equipment	.129	.114	.083	.141	.110	.111	.093	.095	.103	.136	.091	.149
	(.008)	(.009)	(.010)	(.011)	(.011)	(.012)	(.013)	(.014)	(.012)	(.013)	(.013)	(.019)
Electrical and electronic equipment	.051	.095	.105	.062	.085	.089	.104	.147	.088	.109	.079	.089
	(.009)	(.010)	(.011)	(.011)	(.011)	(.014)	(.015)	(.015)	(.015)	(.015)	(.015)	(.022)
Transportation vehicles	.085	.133	.125	.098	.139	.227	.231	.215	.202	.198	.170	.183
	(.007)	(.007)	(.008)	(.009)	(.009)	(.010)	(.009)	(.010)	(.009)	(.010)	(.010)	(.014)
Wood and furniture	−.097	−.147	−.114	−.107	−.098	−.141	−.117	−.155	−.087	−.056	−.095	−.078
	(.010)	(.011)	(.012)	(.012)	(.012)	(.013)	(.013)	(.013)	(.012)	(.012)	(.012)	(.017)
Paper, pulp, and cardboard	−.031	−.048	−.019	.013	−.002	−.029	−.025	.029	.041	.030	.062	.070
	(.009)	(.010)	(.011)	(.010)	(.010)	(.011)	(.012)	(.012)	(.011)	(.011)	(.011)	(.016)
Rubber products	.057	.060	−.019	−.021	−.011	.002	.030	.062	.089	−.032	.019	.014
	(.018)	(.018)	(.021)	(.022)	(.019)	(.023)	(.023)	(.023)	(.023)	(.024)	(.025)	(.034)
Chemicals	.168	.172	.155	.200	.174	.178	.136	.168	.111	.088	.131	.085
	(.010)	(.010)	(.011)	(.012)	(.012)	(.014)	(.014)	(.015)	(.015)	(.015)	(.016)	(.025)

(*Continued*)

Table 4. *Continued*

Industry	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
Petrochemicals	.550	.446	.426	.510	.396	.449	.440	.558	.468	.450	.468	.421
	(.016)	(.017)	(.019)	(.021)	(.019)	(.021)	(.024)	(.026)	(.024)	(.024)	(.022)	(.033)
Pharmaceuticals	.012	.015	.034	.053	.094	.018	.041	.046	.079	.089	.090	.162
	(.016)	(.017)	(.020)	(.020)	(.019)	(.022)	(.021)	(.022)	(.022)	(.020)	(.020)	(.030)
Plastics	−.081	−.071	−.082	−.070	−.025	−.086	−.057	−.051	−.092	−.098	−.091	−.101
	(.014)	(.015)	(.016)	(.016)	(.016)	(.018)	(.019)	(.019)	(.017)	(.017)	(.017)	(.025)
Textiles	−.079	−.095	−.037	−.060	−.077	−.089	−.065	−.124	−.117	−.073	−.080	−.120
	(.011)	(.011)	(.013)	(.014)	(.013)	(.015)	(.016)	(.016)	(.016)	(.018)	(.019)	(.029)
Clothing	−.141	−.177	−.133	−.155	−.144	−.196	−.180	−.210	−.146	−.145	−.178	−.159
	(.013)	(.013)	(.015)	(.015)	(.015)	(.017)	(.016)	(.016)	(.015)	(.016)	(.016)	(.024)
Footwear	−.118	−.187	−.165	−.150	−.169	−.194	−.117	−.084	−.131	−.172	−.161	−.193
	(.011)	(.012)	(.013)	(.014)	(.013)	(.016)	(.015)	(.014)	(.013)	(.014)	(.014)	(.021)
Tobacco	.232	.332	.201	.116	.275	.395	.441	.288	.198	.047	.001	.277
	(.041)	(.042)	(.048)	(.051)	(.053)	(.055)	(.058)	(.056)	(.056)	(.065)	(.064)	(.100)
Foods	−.197	−.190	−.210	−.185	−.167	−.199	−.199	−.219	−.190	−.149	−.146	−.177
	(.008)	(.008)	(.009)	(.009)	(.008)	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)	(.013)
Beverages	−.110	−.070	−.122	−.138	−.135	−.132	−.074	−.023	−.026	−.062	−.064	−.060
	(.015)	(.016)	(.018)	(.019)	(.018)	(.020)	(.021)	(.021)	(.020)	(.021)	(.023)	(.032)
SD of industry premiums	.135	.138	.128	.135	.127	.154	.143	.156	.133	.128	.131	.137

Note: Numbers in parentheses are SEs. Industry wage premiums and their standard errors are calculated using the Haisken-DeNew and Schmidt (1997) procedure and are expressed as deviations from the employment-weighted average wage premium.

Source: Authors' calculations based on data from Brazil's PME.

Industry wage premiums tend to be highest in industries that employ a low share of unskilled workers (as measured by the share of workers without a completed university degree), such as the petrochemical industry, tobacco, and chemicals, and lowest in industries that employ a large share of unskilled workers, such food products, textiles, and clothing. The correlation of industry wage premiums with the share of unskilled workers in the industry in 1987 is always highly negative, and the correlation coefficient ranges from −0.89 in 1987 to −0.8 in 1998.[14]

Finally, the first-stage results suggest that the structure of Brazilian industry wages did not change substantially between 1987 and 1998 even though the structure of protection changed substantially. The year-to-year correlations in industry wage premiums are very high, with the correlation coefficient usually exceeding 0.9. This finding is surprising, given results from previous studies on trade liberalization episodes in Mexico (Robertson 2000a) and Colombia (Goldberg and Pavcnik forthcoming). Those studies found low year-to-year correlations of industry wages, suggesting that the trade reforms changed the structure of industry wages. The magnitude of the correlation in Brazil is in line with evidence for the United States, which shows very stable wage premiums across years (year-to-year correlations are always estimated at above 0.9; see Kreuger and Summers 1988 and Gaston and Trefler 1994). This resemblance could be attributed to the fact that despite the large tariff reductions, most Brazilian industries continue to face relatively low import penetration rates, which is also the case for the United States. The stable structure of industry wage premiums suggests that changes in trade policy are unlikely to be associated with changes in industry wage premiums. This relationship is explored in more detail in the next section.

Industry Wage Premiums and Tariffs

Table 5 reports the results for wage premiums and tariffs in the regression framework described in the methodology section. Because the first-stage regression controlled for worker characteristics, the relationship between industry wage premiums and tariffs does not simply reflect industry differences in worker composition that also affect the political economy of protection. Similarly, because the returns to all worker characteristics are allowed to differ from year to year in the first stage, the first-stage coefficients capture changes in the economywide returns to worker characteristics associated with changes in labor supply over time. All second-stage regressions are estimated in first differences and include year indicators. They thus account for unobserved time-invariant,

14. The positive correlation between industry wage premiums and the share of skilled workers in an industry may be related to the fact that in Brazilian unions tend to be concentrated in industries with the highest shares of skilled workers. Arbache (2001) writes that "unionization [in Brazil] is a clear characteristic of managers, skilled production workers, office workers and, in particular, professionals" and shows that unions are able to extract a large union wage premium—about 18 percent. Alternatively, this positive correlation could also reflect positive spillovers between skilled workers.

Table 5. Regression Results for Industry Wage Premiums and Trade Exposure

Variable	1	2	3	4	5	6	7	8	9
Tariff	−.0686	−.0560	−.0483	−.0575	.0141	.0572	−.1594	−.1547	−.1461
	(0.0599)	(0.0543)	(0.0506)	(0.0558)	(0.1159)	(0.1229)	(0.1130)	(0.1099)	(0.1118)
Lagged import penetration[a]		.1508	.1747	.2059		.1884			.1323
		(0.2134)	(0.2403)	(0.2305)		(0.2048)			(0.2136)
Lagged export to exposure[b]		0.2642*	0.2627*	0.2618**		0.2900**			0.2605**
		(0.1298)	(0.1291)	(0.1174)		(0.1317)			(0.1173)
Tariff*lagged import penetration			−.2276						
			(0.6633)						
Lagged imports* exchange rate				−.1154					−.1252
				(0.0972)					(0.1028)
Lagged exports* exchange rate				−.0233					−.0250
				(0.0824)					(0.0844)
Mercosur tariff					−.0856	−.1126			
					(0.1501)	(0.1496)			
Lagged Mercosur imports						.0027			
						(0.0028)			
Lagged Mercosur exports						−0.0026*			
						(0.0015)			
Two-stage least squares	No	No	No	No	No	No	Yes	Yes	Yes
First differences	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	240	240	240	240	240	198[c]	240	240	240

*Significant at the 10 percent level; **significant at the 5 percent level.

Note: Numbers in parentheses are SEs. Reported SEs are robust and clustered by industry. In column 7 tariff changes are instrumented for by presample tariffs and the exchange rate interacted with presample tariffs. In columns 8 and 9 tariff changes are instrumented for by presample tariffs and coffee prices interacted with presample tariffs.

[a]Imports as a percentage of output plus net imports.

[b]Exports as a percentage of output.

[c]The number of observations is lower because Mercosur exports and imports for nonmanufacturing industries are missing.

Source: Authors' calculations based on labor market data from Brazil's PME and trade and trade policy data from Muendler (2002).

industry-specific variables (such as lobbying power) and macroeconomic shocks that could influence wages concurrently with tariffs.

The results in table 5 suggest no relationship between tariffs and industry wage premiums. Although industry wage premiums are an important component of workers' earnings, they do not seem to be associated with trade policy. Because Brazil's tariff changes might overstate the extent of trade liberalization (because of the size of the economy and remaining nontariff barriers), the study next explores whether wage premiums are affected by alternative trade exposure measures.

First, a specification is estimated that includes industry measures of lagged import penetration and lagged export exposure in addition to tariffs (see table 5, column 2).[15] The results suggest that high export exposure is associated with higher industry wages. This result is intuitive because higher industry exports likely increase the demand for workers in that particular industry. However, there is no statistically significant effect of lagged import penetration on wage premiums. To capture the possibility that the effects of tariffs differ across sectors with different degree of import competition (as measured by import penetration), the interaction of tariffs with import penetration is added to the specification (column 3). The insignificant interaction coefficient suggests that import penetration does not affect wage premiums differentially in industries with lower tariffs. Finally, exchange rate fluctuation might also affect wages. Although year effects capture exchange rate fluctuations over time, the effect of exchange rates might vary with the trade exposure of the sector. But when the exchange rate is interacted with lagged trade flows, none of the previous findings is affected (column 4).

This study focuses on the relationship between unilateral trade liberalization and industry wage premiums, but Brazil's trade with Mercosur members is also examined to check the robustness of the findings. In 1991 Mercosur members began to reduce tariffs on internal trade, and by 1995 most intra-Mercosur trade was duty-free (Chang and Winters 2002; Olarreaga and Soloaga 1998). Trade with Mercosur is controlled for in two ways. First, Brazil's tariffs on Mercosur imports are included in the baseline specification in column 1 of table 5. As in Chang and Winters (2002), these tariffs were obtained by applying the negotiated tariff reductions to the most favored nation industry tariff rates.[16] Note that the two tariff rates are strongly positively correlated, at

15. Because trade flows are likely endogenous (they depend on factor costs), the first lags of import and export measures are included in the estimation rather than their current values. To the extent that these variables are serially correlated, this approach might yield biased results, especially in industry fixed effects specifications with relatively small numbers of observations. Nevertheless, the inclusion of these lagged variables does not change the conclusions about the relationship between tariffs and industry wages.

16. The timeline of the negotiated reductions of the internal tariffs relative to the most favored nation rates was as follows: 47 percent after the ratification of the treaty, 54 percent by December 1991, 61 percent by June 1992, 68 percent by December 1992, 75 percent by June 1993, 82 percent by December 1993, 89 percent by June 1994, and 100 percent by December 1994. Although countries could exclude some products from internal free trade, Chang and Winters (2002) suggest that Brazil declared only 27 exceptions, so that most of its Mercosur trade was duty-free.

0.95 during the entire sample period of 1987–98 (likely reflecting the fact that these tariffs were identical until 1991) and at about 0.57 in the Mercosur period, 1992–98.

Two interesting findings emerge. First, the coefficient on the Mercosur tariff is negative and statistically insignificant (column 5). Second, even when Brazil's tariff on Mercosur imports is included, there is no statistical association between most favored nation tariffs and industry wage premiums. In fact, the coefficient on the most favored nation tariff is even closer to zero than the coefficient reported in column 1.

Because this specification would still not capture the potential effect of Mercosur membership on industry wage premiums through increased Brazilian exports to Mercosur partners, a second specification also controls for Brazil's total exports and imports to Argentina and Uruguay.[17] This specification, reported in column 6 of table 5, thus adds Brazil's tariff on Mercosur imports and measures of total lagged exports and imports with Mercosur to the specification in column 2. The only Mercosur-specific variable that is statistically significant is Brazil's exports to Mercosur. Although higher export exposure in an industry continues to be associated with a higher industry wage premium, the negative coefficient on Mercosur exports suggests that an industry's increased exports to Mercosur are associated with a lower industry wage premium conditional on total exports. Again, there is no statistical association between most favored nation tariffs and industry wage premiums even after controlling for Mercosur-specific trade. This analysis was replicated in unreported regressions using only data from 1991 onward, with similar results.

This discussion of industry wage premiums has so far ignored the potential role of labor market institutions, such as minimum wages and union power. These factors are unlikely to affect the findings, however. First consider the minimum wage. It is set nationally and does not vary across industries. As a result, its effects are captured by the year effects in the second-stage regressions and by coefficients on education indicators in the first stage (in the case where the minimum wage is binding only for people with lower earnings). Moreover, any effects that changes in the minimum wage might have had on industry

17. This information is based on bilateral trade with Brazil reported by Argentina and Uruguay in Trade and Production Data complied by M. Olarreaga and A. Nicita available on the World Bank Web site (www.worldbank.org). This information was not available for Paraguay. This lack of data is unlikely to be a big problem because Chang and Winters (2002) suggest that Argentina is Brazil's main trading partner within Mercosur. This is also confirmed in the sample data for this study, which show that average industry imports and exports between Brazil and Argentina are about five to six times larger than those between Brazil and Uruguay. Moreover, these bilateral data focus only on manufacturing industries (and not on mining as well, as is the case in the main data for this study). Finally, Mercosur-specific exports and imports could not be expressed as a share of total output or domestic consumption because industry-level information on output is lacking (the original data on total import penetration and export exposure from Muendler 2002 and sources cited therein do not report trade flows and output separately for detailed industry categories).

wages through compositional channels (for example, because some industries employ more unskilled workers than others) are already controlled for because the first-stage regressions control for industry composition in each year and allow the returns to various levels of education to change from year to year.

Second, although the individual-level data do not provide information on union membership, preventing formal analysis, changes in unionization are unlikely to be driving the industry wage premium results. If changes in union strength vary by industry through time in the same way that changes in tariffs vary by industry, then changes in unionization could affect industry wages independently of tariff changes, potentially biasing the results.[18] But to the extent that union power in each industry has not changed over time in Brazil, first differencing of data would capture the union effects. This may in fact be a realistic assumption. Arbache and Carneiro (2000) report the shares of unionized workers in various manufacturing industries in 1992 and 1995. Their data show that the shares are relatively stable over time.[19] Moreover, no study was found that suggests that changes in union power were industry specific and were correlated with (or led to) changes in tariffs.

Finally, because the structure of protection changed in Brazil during the sample period, it could be argued that unobserved time-varying shocks, which may simultaneously affect tariff changes and sector-specific premiums, drive the results. Thus the analysis also accounts for the potential endogeneity of trade policy changes by instrumenting for changes in trade policy with presample tariffs and with presample tariffs interacted with the exchange rate.

As in Goldberg and Pavcnik (forthcoming), the choice of instruments is guided by the institutional details of Brazilian trade liberalization. Kume (2000) suggests that at the macroeconomic level Brazil changed trade policy in response to exchange rate fluctuations. Moreover, as discussed earlier, some sectors experienced larger tariff reductions than others. Tariffs were widely dispersed across sectors prior to trade reforms. As a result of Brazil's commitment to economywide liberalization, trade reform led to proportionately larger tariff reductions in sectors with historically higher tariff levels. Regression of the tariff decline from 1987 to 1998 on 1986 tariffs yields a coefficient of 0.8 on 1986 tariffs (t-statistic 16.77) and an R^2 of 0.94.

This discussion suggests that the 1986 industry tariff levels and their interaction with exchange rates are highly correlated with industry tariff reductions and may provide good instruments for the tariff changes. Because coffee is a major Brazilian export and coffee prices likely affect the exchange rate, the

18. The situation in which lower tariffs reduce union power, leading to lower wage premiums, is not a concern because in this case unions simply provide a potential mechanism through which tariffs can affect wages.

19. The correlation between industry union membership in 1992 and in 1995 is 0.82. Arbache and Carneiro (2000) use data from pnad. These data are not available for 1991–94, during the trade liberalization of the early 1990s, and the surveys for 1989 and 1990 do not contain information on union status.

interaction of coffee prices (rather than exchange rates) with presample tariffs was also tested as an instrument. The relationship between sector-specific skill premiums and tariffs was estimated in first differences using two-stage least squares. When instrumenting for tariff changes with presample tariffs and their interaction with the exchange rate (see table 5, column 7) and presample tariffs and their interactions with coffee prices (columns 8 and 9),[20] the magnitude of the negative coefficient on tariffs becomes smaller in absolute value, but the coefficients are imprecisely estimated. Thus the results continue to show no statistical relationship between trade policy and industry wage premiums.

Overall, there is no statistically significant evidence that Brazilian trade liberalization affected the industry wage structure and thus wage inequality between skilled and unskilled workers through their industry affiliation. This finding is consistent with the evidence from Mexico (Feliciano 2001), which shows no relationship between industry wages and tariffs, but is inconsistent with the evidence from Colombia (Goldberg and Pavcnik forthcoming) and Mexico (Revenga 1997), which shows that tariff reductions are associated with declines in industry wages.[21]

Industry Wage Premiums for University-Educated Workers

Although the results show no relationship between trade exposure and industry wage premiums, trade policy could still account for part of the increase in the return to university-educated workers if tariff reductions are associated with increases in sector-specific skill premiums. Industry wage premiums could differ for workers with different levels of education for several reasons. For example, more educated workers might be more or less mobile in the labor market. Or workers with different amounts of education might differ in their accumulation of sector-specific skills or their ability to bargain over wages. Revenga (1997) finds in Mexico that the greater the proportion of unskilled workers in an industry, the lower the ability of workers in the industry to capture part of the industry rents. Finally, industry-specific skill premiums might reflect efficiency wages paid to skilled workers to prevent them from shirking if industries face different monitoring costs. Robbins and Minowa (1996), for example, find substantial variation in returns to schooling across industries for manufacturing workers in São Paolo, Brazil, in 1977. They attribute these differences to efficiency wages that firms pay to skilled workers in capital-intensive industries to avoid shirking.

To investigate the relationship between industry-specific skill premiums and trade policy, skill-specific industry wage premiums are computed by employing

20. The first-stage *F*-statistics in these two-stage least square regressions are $F(12,207) = 30.5$, $F(12,207) = 25.5$, and $F(16,203) = 19.3$, respectively.

21. The differences in results in Feliciano (2001) and Revenga (1997) could stem from differences in methodology and from the fact that Feliciano uses worker-level data similar to the data used here, whereas Revenga uses plant-level data.

a modified version of equation 1 that allows industry wage premiums to differ for skilled and unskilled workers:

$$\ln(w_{ijt}) = H_{ijt}\beta_H + I_{ijt} * wp_{jt} + I_{ijt} * S_{ijt} * wp_{Sjt} + \epsilon_{ijt} \tag{3}$$

The variable S_{ijt} is an indicator for whether worker i in industry j is skilled at time t (has a university degree). The coefficient wp_{Sjt} represents the incremental wage premium skilled workers earn in industry j in addition to the base wage premium in industry j, wp_{jt}, which is received by unskilled and skilled workers. The differential impact of trade policy on the industry wages of skilled and unskilled workers is investigated by relating these industry-specific returns to skill to trade policy measures in the second stage of the estimation along the lines discussed in the methodology section.

The first-stage results suggest that industry-specific skill premiums are potentially important (table 6). As in the case of industry wage premiums, the reported coefficients and standard errors are computed using the Haisken-DeNew and Schmidt (1997) procedure, expressed as deviations from the employment-weighted average skill premium. This normalized industry-specific skill premium can be interpreted as the proportional differences in wages through the channel of an industry-specific skill premium for a university-educated worker in a given industry relative to an average university-educated worker in all industries with the same observable characteristics. Thus a negative industry-specific skill premium suggests that the industry has a lower industry-specific skill premium than the average economywide skill premium (and not that skilled workers in this industry earn less than unskilled workers in the industry).

Although the inclusion of industry-specific skill premiums does not increase the explanatory power of the regression by much, the premiums vary widely across industries (see table 6).[22] University-educated workers in the tobacco industry and in oil extraction have the largest skill premiums, whereas university-educated workers in paper and clothing have the smallest. For example, estimates for 1987 suggest that a university-educated worker who switches from the textile to the chemical industry would see an almost 14 percent increase (124−(−0.014)) in wages through the channel of industry-specific skill premiums. The standard deviation of the industry-specific skill premiums ranges between 12.2 and 19.8 percent over 1987–98.

Are these changes in sector-specific skill premiums associated with changes in trade policy? The regression results show no statistical association between tariff changes and changes in industry-specific skill premiums (table 7, column 1). What about other trade exposure measures? The results show that the relationship between tariffs and sector-specific skill premiums is robust to the inclusion of other trade exposure measures (columns 2–4). Although there is no

22. The R^2 in these regressions is basically identical to the R^2 in regressions with industry fixed effects reported at the bottom of table 3 at two decimal points.

TABLE 6. Industry-Specific Skill Premiums for University Graduates

Industry	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
Mineral	.220	.242	.311	.389	.181	−.029	.088	.383	.370	.283	.153	.426
extraction	(.058)	(.057)	(.068)	(.074)	(.084)	(.074)	(.074)	(.099)	(.089)	(.079)	(.082)	(.121)
Oil extraction	.242	.639	.374	.429	.382	.275	.611	.719	.555	.412	.298	.246
	(.068)	(.068)	(.087)	(.081)	(.074)	(.094)	(.108)	(.124)	(.109)	(.105)	(.110)	(.142)
Nonmetalic mineral	.092	.259	.135	.218	.297	.248	.201	.358	.335	.459	.187	−.006
trasformation	(.042)	(.049)	(.063)	(.070)	(.064)	(.064)	(.058)	(.062)	(.063)	(.052)	(.066)	(.090)
Metalic products	.143	.055	.152	.133	.124	.110	.102	.136	.097	.147	.150	.048
and steel	(.023)	(.023)	(.027)	(.028)	(.028)	(.030)	(.030)	(.033)	(.032)	(.031)	(.032)	(.052)
Machinery and	−.032	−.088	−.117	−.172	−.067	−.119	−.035	−.211	−.166	−.108	−.004	−.055
equipment	(.033)	(.031)	(.035)	(.037)	(.035)	(.044)	(.047)	(.051)	(.048)	(.043)	(.043)	(.064)
Electrical and	−.001	.019	.016	−.043	−.110	−.001	−.061	.062	.046	.051	.021	.005
electronic equipment	(.028)	(.028)	(.033)	(.032)	(.032)	(.039)	(.041)	(.042)	(.040)	(.039)	(.040)	(.062)
Transportation	−.089	−.158	−.232	−.134	−.121	−.010	−.027	−.063	−.104	−.205	−.128	.029
vehicles	(.030)	(.030)	(.035)	(.033)	(.034)	(.039)	(.035)	(.042)	(.039)	(.040)	(.037)	(.053)
Wood and	−.145	−.009	.072	.077	−.096	−.525	−.662	−.353	−.367	−.641	−.439	−.061
furniture	(.084)	(.079)	(.081)	(.082)	(.092)	(.112)	(.122)	(.108)	(.085)	(.095)	(.096)	(.116)
Paper, pulp, and	−.322	−.197	−.086	−.223	−.287	−.147	−.104	−.081	−.136	−.149	−.212	−.086
cardboard	(.032)	(.035)	(.037)	(.039)	(.036)	(.038)	(.039)	(.041)	(.036)	(.037)	(.036)	(.050)
Rubber	−.036	−.278	−.182	−.010	.002	−.055	.269	−.067	.026	.047	−.095	.012
products	(.069)	(.072)	(.083)	(.084)	(.078)	(.098)	(.104)	(.115)	(.119)	(.099)	(.098)	(.173)
Chemicals	.124	.013	.028	−.024	.036	−.037	.006	−.094	−.038	.000	.089	.173
	(.027)	(.027)	(.031)	(.029)	(.030)	(.038)	(.040)	(.043)	(.042)	(.039)	(.040)	(.066)

Petrochemicals	−.063	−.086	−.113	−.139	−.056	.042	−.128	−.117	−.021	−.259	−.170	−.219
	(.034)	(.035)	(.043)	(.042)	(.040)	(.046)	(.047)	(.054)	(.052)	(.054)	(.047)	(.067)
Pharmaceuticals	−.120	−.110	−.181	.139	.140	−.153	−.082	−.221	.031	−.080	.041	.009
	(.044)	(.045)	(.054)	(.058)	(.054)	(.067)	(.062)	(.065)	(.064)	(.054)	(.056)	(.080)
Plastics	−.077	.033	−.122	.042	.110	−.065	−.007	.176	.029	.123	.194	−.111
	(.060)	(.067)	(.072)	(.074)	(.079)	(.097)	(.087)	(.094)	(.073)	(.076)	(.067)	(.095)
Textiles	−.014	.085	.196	.156	−.025	.063	−.048	−.030	−.169	.006	.030	.021
	(.050)	(.056)	(.059)	(.067)	(.057)	(.070)	(.074)	(.073)	(.072)	(.085)	(.082)	(.104)
Clothing	−.358	−.441	−.355	−.124	−.299	−.384	−.424	−.599	−.404	−.378	−.248	−.001
	(.072)	(.070)	(.083)	(.083)	(.077)	(.088)	(.095)	(.126)	(.113)	(.095)	(.082)	(.144)
Footwear	−.041	.079	−.352	−.174	−.397	.280	.159	.013	−.107	−.120	.115	.301
	(.089)	(.099)	(.124)	(.112)	(.088)	(.104)	(.184)	(.111)	(.094)	(.109)	(.108)	(.264)
Tobacco	.040	.630	.687	.424	.237	−.054	.164	−.060	−.157	−.465	−.223	.456
	(.133)	(.150)	(.192)	(.182)	(.157)	(.149)	(.193)	(.207)	(.268)	(.204)	(.151)	(.313)
Foods	.082	.173	.215	.262	.238	.121	.071	.024	.104	.140	.038	.050
	(.037)	(.038)	(.043)	(.044)	(.041)	(.047)	(.047)	(.049)	(.050)	(.046)	(.042)	(.059)
Beverages	.258	.255	.046	−.030	.321	.140	.330	.299	.370	.220	.413	−.204
	(.073)	(.073)	(.087)	(.084)	(.073)	(.079)	(.104)	(.090)	(.094)	(.092)	(.085)	(.117)
SD	.139	.163	.157	.156	.163	.122	.138	.175	.156	.198	.147	.098

Note: Numbers in parentheses are SEs. Industry wage premiums and their SEs are calculated using the Haisken-DeNew and Schmidt (1997) procedure. They are all expressed as deviations from the employment weighted average skill premium.

Source: Authors' calculations based on data from Brazil's PME.

Table 7. Regression Results for Industry-Specific Skill Premiums for University Graduates and Trade Exposure

	1	2	3	4	5	6	7	8	9
Tariff	−.1948	−.1334	−.2211	−.1296	−.3130	−.3059	−.0091	−.1667	−.1427
	(0.1678)	(0.1810)	(0.2003)	(0.1833)	(0.3978)	(0.4258)	(0.1663)	(0.1104)	(0.1388)
Lagged import penetration[a]		−.1567	−.3449	−.2708		−.2140			−.2819
		(0.5131)	(0.5667)	(0.5346)		(0.5627)			(0.5456)
Lagged export exposure		1.3691**	1.3740**	1.2357**		1.4292**			1.2342**
		(0.3443)	(0.3509)	(0.4161)		(0.4809)			(0.4137)
Tariff*lagged import penetration			2.0329						
			(1.9014)						
Lagged imports*exchange rate				.2845					.2834
				(0.3351)					(0.3383)
Lagged exports*exchange rate				.4136					.4134
				(0.2934)					(0.2928)
Mercosur tariff					.1221	.1751			
					(0.3258)	(0.3550)			
Lagged Mercosur imports						.0019			
						(0.0054)			
Lagged Mercosur exports						.0039			
						(0.0067)			
Two-stage least squares	No	No	No	No	No	No	Yes	Yes	Yes
First differences	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	240	240	240	240	240	198	240	240	240

*Significant at the 10 percent level; **significant at the 5 percent level.

Note: Numbers in parentheses are SEs. Reported SEs are robust and clustered by industry. In column 7 tariff changes are instrumented for by presample tariffs and the exchange rate interacted with presample tariffs. In columns 8 and 9 tariff changes are instrumented for by presample tariffs and coffee prices interacted with presample tariffs.

[a]Imports as a percentage of output plus net imports.

[b]Exports as a percentage of output.

[c]The number of observations is lower because Mercosur exports and imports for nonmanufacturing industries are missing.

Source: Authors' calculations based on labor market data from Brazil's PME and trade and trade policy data from Muendler (2002).

relationship between sector-specific skill premiums and import penetration, increases in export exposure within an industry are associated with increases in the skill premium in that industry. The findings are also robust to inclusion of Brazil's tariff on imports from Mercosur countries (column 5) and of Brazil's exports to and imports from Argentina and Uruguay (column 6). None of the Mercosur-specific trade measures is statistically significant, and their inclusion does not alter the findings on the relationship between skill-specific wage premiums and tariffs. Finally, instrumenting for tariff changes with presample tariffs and their interaction with the exchange rate (column 7) and presample tariffs and their interaction with coffee prices (columns 7 and 9) again shows a negative, but statistically insignificant relationship between tariff changes and changes in industry-specific skill premiums.[23]

In sum, the study finds no statistically significant evidence that tariff reductions affected worker wages in Brazil through their industry affiliation or that tariff reductions contributed to wage inequality between skilled and unskilled workers through this channel.

IV. Conclusion

The analysis here was motivated in part by the current policy discussion on the benefits and costs of trade reforms. Many people have recently questioned whether the potential benefits of trade liberalization (increased efficiency and welfare) outweigh the potential costs (increased inequality, "race to the bottom" in wages). Several recent studies have proposed the use of labor market policies, such as minimum wages and government social protection programs, to offset the potential increase in inequality associated with trade liberalization (Rama 2001, 2003; Rama and Ravallion 2001).

This study contributes to the policy debate in several ways. First, it is one of only a few studies that focus on trade policy variables (such as tariffs) rather than outcome variables (such as openness) in examining the implications of trade reforms for labor markets. Rodriguez and Rodrik (1999) point out the difficulties in assessing the impact of trade liberalization if trade reforms are measured using outcome variables such as openness, which reflect not only a country's trade policy but also factors such as transport costs, technology, demand, and most important, changes in factor prices.[24] The use of trade policy variables is thus an advantage.

23. The first-stage F-statistics in these two-stage least square regressions are $F(12,207) = 26.8$, $F(12,207) = 22.3$, and $F(16,203) = 17.5$, respectively.

24. One disadvantage of the tariff measures is that changes in tariffs may have little effect if large nontariff barriers remain. Although detailed information on nontariff barriers is not available, trade liberalization also significantly reduced nontariff barriers. Moreover, the findings of this study are essentially unaffected by the inclusion of measures of openness, such as (lagged) import penetration and the export exposure ratio that partially accounts for the effects of nontariff barriers.

Second, opponents of globalization often claim that trade reforms make workers in previously protected sectors poorer and that trade liberalizations leads to a race to the bottom in wages. Some studies report results that are potentially consistent with this claim. For example, Goldberg and Pavcnik (forthcoming) and Revenga (1997) find that tariff reductions are associated with declines in industry wage premiums in Colombia and Mexico. Rama (2001) also finds some evidence of a negative association between openness and wages in the short run in a cross-country study. Rama (2001, 2003) has suggested that trade liberalization could be accompanied by increases in minimum wages to compensate potential losers. The evidence here from Brazil suggests that trade liberalization does not necessarily lead to lower industry wages through the channel of industry wage premiums in the short run. Obviously, trade liberalization could still lower wages through other channels, such as lower returns to education or experience, that are not the focus of this study. Exploring the differences in country characteristics or policies that determine how trade reform affects worker wages through various channels may thus be fruitful ground for future research.

Finally, although no evidence was found that drastic tariff declines worsened inequality through changes in the structure of wage premiums, industry wage premiums were found to vary widely across Brazilian manufacturing sectors, accounting for 4–6 percent of the explained variation in log hourly wages. In addition, industry wage premiums are smallest in sectors with high shares of unskilled workers. This suggests that unskilled workers earn lower wages not only because of the growing economywide skill premium but also because they are disproportionately employed in industries with low wage premiums, a source of inequality that has been undetected in previous studies. This source of inequality, along with the rising skill premium, could be addressed through labor market policies, such as those promoted by Rama (2001) (changes in minimum wages and in social security programs), in addition to improved access to education.

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Lobbying, Counterlobbying, and the Structure of Tariff Protection in Poor and Rich Countries

Olivier Cadot, Jaime de Melo, and Marcelo Olarreaga

A political economy model of protection is used to determine endogenously the intersectoral patterns of protection. Three propositions are derived that are consistent with the stylized patterns of tariff protection in rich and poor countries: Nominal protection rates escalate with the degree of processing, protection is higher on average in poor countries, and rich countries protect agriculture relatively more than they protect manufacturing, whereas poor countries do the reverse. Numerical simulations for archetypal rich and poor economies confirm that the endogenously determined structure of protection is broadly consistent with observed patterns of protection.

Tariff protection in rich and poor countries displays several stylized patterns. Three stand out as particularly robust. First, nominal rates of protection escalate with the degree of processing, which contributes to the widely observed escalation of effective rates of protection with the degree of processing. Second, protection is higher on average in poor countries. Third, rich countries protect agriculture more than they do manufactures, whereas poor countries do the reverse.

Until recently, analysts explained these patterns of protection largely by calling on the theory of second best. They pointed out, for example, that in rich countries farming provides a groomed landscape that benefits the whole population.[1] In poor countries high trade taxes (including taxation of agriculture) are justified by the revenue constraint that because of weak fiscal administration cannot be met by less distortionary instruments. In turn, protection of manufacturing has been justified on infant-industry grounds. In each case, second-best considerations provided efficiency-based arguments justifying protection.

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1. Noneconomic objectives, such as a certain degree of self-sufficiency or an income distribution objective are also often invoked as explanations for the observed pattern of protection. See Corden (1974) for an early treatment.

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While recognizing the validity of these considerations, this article argues that an equally if not more important reason for the observed pattern of protection is rooted in the political economy considerations identified in the new political economy literature.[2] This body of literature views governments not as passive executors of a trade policy to maximize social welfare but as agents interacting with organized interest groups to maximize an objective function in which social welfare is just one argument. This article shows that such an approach can generate endogenously—and perhaps more readily so than the traditional second-best literature—a predicted cross-sectoral pattern of protection that broadly fits the three stylized patterns.

I. Literature Background

Empirically, Anderson (1995) was the first to quantitatively investigate the tariff-protection pattern of agriculture relative to industry in rich and poor countries. Using a Ricardo-Viner model similar to the one developed here, data for archetypal rich and poor economies, and parameter values similar to those used here in, he shows that support to farmers in rich countries raises their incomes substantially while reducing manufacturing incomes only marginally. Conversely, agricultural taxation in poor countries reduces farmers' incomes marginally while benefiting capitalists and workers substantially. These simulations, he concludes, explain the observed pattern of protection of agriculture relative to manufacturing in rich and poor countries. Although highly suggestive, the simulations fall short of endogenizing the level and sectorial pattern of protection.

More recently, in a model nesting the economic (terms of trade) and political (redistribution toward powerful favored groups) arguments, Freund and Djankov (2000) find support for the political economy argument of protection. In a cross-section they find that proxies for political power (past share of income in the hands of the 20 percent richest groups and an index for corruption) and for favoritism (the share of public expenditures on nonpublic goods defined as expenditures other than health, education, and the social safety net) are positively related to the degree of protection. The results are robust to omitted-variable bias (distance and endowments) and to reverse causation (openness to trade, by enhancing competition, could reduce rent-seeking activity and have a negative effect on corruption; see Ades and Di Tella 1999).

Freund and Djankov's finding that favoritism has been an obstacle to trade liberalization is supportive of the approach taken here, and their finding that protection is not correlated with relative country size (the proxy for market

2. Recent empirical work (see Djankov and others 2002) also suggests that distortionary taxation of market entry tends to be associated with indicators of poor governance, themselves largely associated with low income levels. This evidence points toward explanations for observed patterns of taxation that are broadly consistent with the new political economy literature. We are grateful to a referee for attracting our attention to this point.

power) is not surprising because tariff-setting policy is usually not associated with a country's desire to improve its terms of trade. Their approach is also consistent with the common small-country modeling approach taken here and in most of the endogenous protection literature.

This article derives Anderson's results by extending the influence-driven approach to the endogenous determination of tariffs proposed by Grossman and Helpman (1994), itself an extension of the political support approach proposed by Hillman (1982). The Grossman-Helpman approach has the advantage of relating the predicted structure of protection to potentially measurable technology and preference parameters. However, in its original formulation, it predicts that for organized sectors (those with active political lobbying) equilibrium tariff protection increases with domestic output and hence decreases with import penetration. As Rodrik (1995) points out, this prediction is not entirely realistic because it suggests, for instance, that agriculture rather than manufacturing should be protected in poor countries and is at odds with the bulk of existing empirical evidence. As argued later in this article, this apparently counterintuitive implication is not the result of a particular artifact of the Grossman-Helpman model but rather a direct consequence of Hotelling's lemma that is bound to appear in any model of influence activity.[3] If the new political economy's descriptive power is to be taken seriously, therefore, reconciling the model's logic with the empirical evidence is essential.

Several solutions have been proposed to the puzzle, both empirically and theoretically. Empirically, Koujiannou-Goldberg and Maggi (1999) show that when organized and unorganized sectors are treated separately, the estimated relationship between equilibrium trade protection and import penetration is broadly in line with the Grossman-Helpman model's prediction (decreasing for organized sectors and increasing for unorganized ones). Gawande and Bandyopadhyay (2000) have similar results. Maggi and Rodriguez-Clare (1999) show theoretically that when public funds have a distortion cost and trade protection can take the form of either tariffs or quantitative restrictions, trade protection may increase with import protection under plausible conditions in the Grossman-Helpman model.

Although these studies have helped reconcile theory with the observed patterns of protection, the empirical results of Koujiannou-Goldberg and Maggi (1999) and Gawande and Bandyopadhyay (2000) are not without ambiguity,[4] and Maggi and Rodriguez-Clare's (1999) extension comes at the price of substantial complication. They are thus unlikely to be the last word on an issue that is sufficiently important to deserve further exploration.

This article takes a different approach. It keeps the political game untouched but puts flesh on the underlying economy. It introduces factor-market rivalry

3. Hotelling's lemma states that at the profit-maximizing output level the derivative of profits with respect to prices is equal to output.

4. For instance, Koujiannou-Goldberg and Maggi (1999) find stronger results for unorganized sectors than for organized ones, which is problematic for a model that focuses on the effects of lobbying.

and input-output linkages, giving rise to counterlobbying (by organized sectors other than the direct beneficiary of trade protection) and altering the equilibrium pattern of protection in a way that can reduce the gap between prediction and evidence. Indeed, it turns out that this extended model, when applied to archetypal data, yields an endogenous structure of protection that is consistent with the three stylized patterns: Nominal protection escalates with the degree of processing (because of weaker counterlobbying for processed goods), protection is higher on average for poor countries (because of sparse interindustry linkages), and rich countries protect agriculture more than they do manufactures, whereas poor countries do the reverse (because of differences in interindustry linkages and rivalry in factor markets).

In sum, this article extends the Grossman-Helpman model and the simulations by Anderson, providing a simple political economy–based account of observed protection patterns. In doing so, it provides a basis for examining the forces behind trade protection in developing and industrial countries, which is a necessary first step to any successful (and therefore lasting) trade reform.

II. Patterns of Tariff Protection in Developed and Developing Economies

This section provides prima facie evidence for the three stylized facts. Average tariffs by degree of processing for agricultural and industrial products for 37 developing economies and 7 industrial countries (the European Union is counted as one country), the largest available sample, provide evidence for the first two stylized facts (table 1).[5] For agricultural products the average rate of

Table 1. Tariff Escalation in Developing and Industrial Countries, 1997–99 (Unweighted Averages in %)

Stage of production	Developing	Industrial
Agricultural products		
First stage processing	19.0	5.2
Semiprocessed	26.3	5.4
Fully processed	29.6	5.8
Ratio of countries without escalation to sample size	4/37	1/7
Industrial products		
First stage processing	9.5	0.5
Semiprocessed	13.1	4.0
Fully processed	15.2	4.6
Ratio of countries without escalation to sample size	1/37	0/7

Source: WTO 2000 Integrated Data Base CD-ROM and WTO *Trade Policy Reviews*, various issues.

5. The data are from the World Trade Organization (WTO) Integrated Data Base version 4 and the WTO *Trade Policy Reviews*.

protection rises by degree of processing for both the developing country group and the industrial country group. Fully processed agricultural goods have a 55 percent higher tariff on average than goods in their first stage of processing in developing areas and a 12 percent higher tariff in industrial countries.[6] Protection also rises on average with the degree of processing for industrial products. However, fully processed industrial products receive about 55 percent more protection than first-stage processing products in developing economies and 450 percent more in industrial countries.

Because classifying products by degree of processing is subject to error, a more robust estimate of the effect that processing has on tariff structure might compare first-stage and fully processed products. For agriculture products only five countries do not conform to the prediction that tariffs are higher for fully processed goods: China, Republic of Korea, South Africa, Thailand, and Norway). For industrial products, only, Romania does not conform. By this classification the predicted pattern holds in 93 percent of the cases.[7]

On the second stylized fact, that protection is higher in developing economies, a quick look at table 1 suggests that this is the case for both agriculture and industry across all levels of processing.

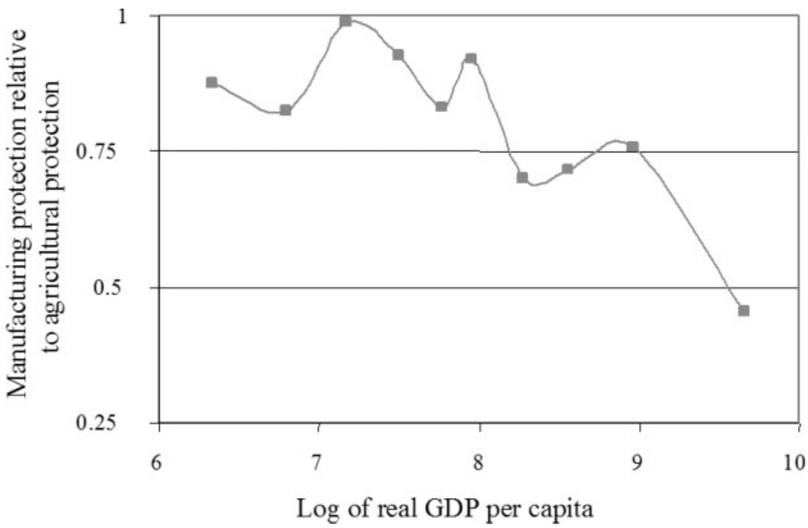
The third stylized fact is that industrial countries tend to protect agriculture more than manufacturing, whereas the opposite is true for developing areas. The ploting of a curve linking the average relationship (by decile) between protection of manufacturing relative to agriculture and gross domestic product (GDP) per capita of a sample of 81 countries shows a negative relation (figure 1).[8] The relative protection variable is t_M/t_A, where t_M and t_A are unweighted average tariffs for manufactures and agriculture for a sample of 81 countries (the largest available sample with data on tariffs and GDP per capita, Y_P). The gain in number of countries covered by using only tariffs rather than all forms of protection, comes at a cost, however. Because these estimates do not include the effects of other price measures (such as export taxes) and nonprice measures (such as nontariff barriers), figure 1 cannot be said to be representative of

6. Note however, that all developed countries use specific tariffs intensively in agriculture and that the WTO's Integrated Data Base does not provide ad valorem equivalents of these tariffs. Including specific tariffs could show a more dramatic picture.

7. One could also object that the pattern of protection in manufactures is the result of bargaining through successive rounds of tariff negotiations rather than determined noncooperatively at the national level, as predicted by the political economy approach developed here. However, because the current observed pattern is largely the result of successive linear across-the-board reductions, it is likely that the current pattern reflects, as a first approximation, patterns established before the multilateral tariff cuts. Ray (1990) discusses how these patterns were established along political economy grounds similar to those explored in this article.

8. The data on agriculture and manufacturing tariffs are compiled from various sources, including WTO, UNCTAD, and the World Bank and are available online at www.worldbank.org/research/trade. The GDP per capita data are from World Bank (2000). Note that the sample is larger than that in table 1 because the disaggregated tariff data necessary to compute average tariffs by levels of processing are not available for some countries.

FIGURE 1. Relative Protection of Manufacturing and Income per Capita, 1995–2000



Source: WTO 2000 CD-ROM *International Trade Statistics* and WTO *Trade Policy Reviews*, various issues for tariff data; World Bank 2000 for GDP per capita.

the relative incentives to agriculture and manufactures as income per capita varies.

The negative relation between protection of manufacturing relative to agriculture and the log of income per capita is confirmed by the results of the following regression, for which $F(1,79) = 9.27$, with the t-statistic in parentheses.

$$t_M/t_A = \underset{(5.77)}{1.68} - \underset{(3.04)}{0.11}\ Y_P \tag{1}$$

The relation, which is significant despite a few outlier observations (including Australia and Turkey), indicates that the relative tariff of agriculture over manufactures increases by 1.1 percent for each 10 percent increase in GDP per capita.

It is likely that the positive relation between the relative protection of agriculture and the level of GDP per capita would have been even more striking had data been available on producer subsidy equivalents for all countries and had more indicative data been available on protection for agriculture in developing countries, along the lines of the Krueger and others (1989) estimates.

III. DETERMINANTS OF THE STRUCTURE OF PROTECTION

The model presented next is an extension of the basic Grossman-Helpman model, with which it shares several elements. First, the political game is the same. Namely, lobbies "bid for protection" through monetary contributions conditioned on the level of tariffs, and the government maximizes a weighted average of social welfare and contributions. Alternatively, the government can be seen as acting as the common agent of the lobbies. The government's objective function is a linear (but not necessarily convex) combination of welfare and monetary contributions from the lobbies.

Second, the underlying economy is similar in most regards. The lobbies represent the interests of specific-capital owners in a Ricardo-Viner model, and individuals have identical quasilinear preferences. The differentiating elements introduced here are that specific-capital ownership is assumed to be sufficiently concentrated that lobbies care only about protecting their industry (they do not internalize the effect of protection on consumer prices), all sectors are assumed to be politically organized (actively lobbying), and all sectors combine specific capital with mobile labor. This symmetry in factor use across sectors relaxes a key assumption in the Grossman-Helpman model—that the existence of a sector operating with labor only under constant returns to scale fixes the wage rate. The interest of relaxing this assumption is that a flexible wage rate introduces intersectoral rivalry in the labor market, with one sector's protection raising the cost of labor for other sectors.

Finally, all goods may enter as input into the production of all other goods, so that one sector's protection raises the costs of other sectors. This creates a second channel of intersectoral rivalry, through input-output linkages.

These new elements create the possibility of counterlobbying: In this model one sector's lobbying for protection meets political opposition from other sectors. The equilibrium pattern of protection results from the net effect of these opposing forces. These extensions generate a pattern of protection that no longer needs to share the unfortunate features of the basic Grossman-Helpman model.

Lobbying in a Specific-Factor Model

Consider a small open economy with $n+1$ tradable sectors, in which good 0 serves as numéraire and export good. All n other goods are import competing. Individuals have different endowments but identical tastes, represented by a utility function:

$$U = c_0 + u(\mathbf{c}), \tag{2}$$

where c_0 is consumption of the numéraire good, and $\mathbf{c}$ is the vector of consumption of the n non-numéraire goods, $u' > 0$ and $u'' < 0$.

All goods produced in the economy are potential inputs in other sectors, and all industries are perfectly competitive. Each sector's technology is Leontief between intermediate consumptions and value added. Thus, value added is nested in the Leontief production function and is created using a specific factor (κ_j) and a mobile factor (ℓ_j) under a general constant returns to scale technology (f^j). Letting a_{ij} be the requirement of good i necessary to produce one unit of good j, and letting x_{ij} be sector j's demand for good i as an intermediate input,

$$y_j = \min \left\{ f^j(\kappa_j, \ell_j);\ x_{0j}/a_{0j};\ \ldots ; x_{nj}/a_{nj} \right\}. \tag{3}$$

The world prices of all goods are fixed and assumed to be unity. The domestic price of good i is thus $p_i = 1 + t_i$, where t_i is the specific tariff (or subsidy if it is negative) applied to it. Let $\mathbf{p} = (p_1, \ldots, p_n)$ be the vector of domestic prices and w the wage rate, and let $\tilde{p}_j = p_j - \Sigma_{i=0}^{n} a_{ij} p_i$ be the net price of good j. Given the technology postulated, industry j's restricted profit function can be written as $\pi_j[\kappa_j;\ \mathbf{p};\ w(\mathbf{p})] = \tilde{p}_j y_j - w\ell_j$. Lobbies representing import-competing sectors (all are assumed to be organized) bid simultaneously for protection with "truthful" contribution schedules $C_j(\mathbf{p}) = \max\{0; \pi_j - b_j\}$ for some nonnegative constant b_j.[9] The form of the contributions reflects the assumption that lobbies are very small in the population. Thus, from their point of view, the effects of protection on consumer surplus and tariff revenue are not commensurate with the direct effect of protection on producer surplus, and they take only that direct effect into account in their lobbying activity.

Faced with such contributions, the government chooses best-response tariffs (domestic prices) maximizing

$$V(\mathbf{p}) = \sum_{j=1}^{n} C_j(\mathbf{p}) + aW(\mathbf{p}) \tag{4}$$

where $W(\mathbf{p})$ is social welfare and a is a constant representing the weight the government attaches to social welfare.[10] Domestic prices satisfy the first-order condition:

$$\partial V(\mathbf{p})/\partial p_i = \sum_{j=1}^{n} (\partial \pi_j/\partial p_i) + a(\partial W/\partial p_i) = \mathcal{P}_i + a\mathcal{E}_i = 0, \tag{5}$$

which picks up the net effect of protection in sector i on economywide rents and measures the incentive for the government to depart from the optimal second-best tariff in sector i. The first term in the equation is the "net political-power component," because it measures the ability of lobby i to make its voice heard above that of opposing lobbies; this term is denoted by $\mathcal{P}_i$. Using Hotelling's lemma, $\partial\pi_j/\partial p_i = (1_{j=i} - a_{ij})\ y_j - \ell_j \partial w/\partial p_i$, where $1_{j=i}$ is an indicator function equal to one when $j = i$ and zero otherwise. Thus, under the assumption that all sectors are politically organized, the effect of a change in p_i on aggregate political contributions (on producer surplus in all sectors) is:

9. The vector of constants b_j determines how the rents from trade protection are shared between the government and lobbies; see Grossman and Helpman (1994).

10. Grossman and Helpman (1996) show that this objective function emerges in a political system in which lobbies use campaign contributions to influence the outcome of the election, while two parties compete for seats in Parliament. However, whether such an objective function is representative of the objective function in low-income countries is more debatable. For a skeptic's view, see Findlay (1991).

$$(6) \qquad \mathcal{P}_i \equiv \sum_j (\partial \pi_j / \partial p_i) = y_i - \sum_j a_{ij} y_j - \ell(\partial w / \partial p_i)$$

where $\ell = \sum_j \ell_j$ is the economy's total labor force.

The first term in $\mathcal{P}_i$ reflects the direct effect of trade protection on the profits of sector *i*—the rent accruing to owners of sector-specific capital in sector *i*. This term is at the origin of the import-penetration controversy. To see this, observe that the larger an industry's domestic output, the larger is (through Hotelling's lemma) the impact of a given tariff increase on its profits. Through the truthfulness assumption, this implies a higher lobbying intensity, which raises the power of the incentive given to the government. Accordingly, in equilibrium the government grants larger protection. But for a given level of demand a larger domestic output reduces the import-penetration ratio, hence the negative association between import-penetration ratios and equilibrium protection. As mentioned, this result is not a modeling artifact but goes to the heart of the influence-activity logic: Whenever lobbying is done by an industry's residual claimants, through Hotelling's lemma a larger domestic output will raise the return to lobbying.

In the Grossman-Helpman model, the story stops here. The only way out of this unfortunate prediction is to note, as Koujiannou-Goldberg and Maggi (1999) do, that the positive relationship between output size and equilibrium protection holds only for organized industries. For unorganized ones the relationship is reversed, at least provided that special interests take consumer effects into account.

Though not contesting this observation, the model here focuses on another effect that also solves the puzzle but generates distinct predictions as by-products. All sectors are assumed to be organized, an assumption that is easily relaxed but at the cost of additional notation. The novelty here comes in the next two terms in equation 6, which reflect the two components of rivalry between lobbies; the stronger the rival lobbies, the lower the level of rent extraction by lobby *i*. The first term $(\sum_j a_{ij} y_j)$ represents the impact of protection in *i* on downstream sectors through input-output linkages. An increase in the tariff on good *i* reduces the net price $\tilde{p}_j$ of all downstream sectors (all sectors *j* such that $a_{ij} > 0$), giving rise to counterlobbying. The last term in equation 6 represents crowding out through the wage rate: As the tariff on good *i* rises, sector *i* expands, bidding up the wage rate, penalizing other sectors, and giving rise to counterlobbying. In general, the political power component, $\mathcal{P}_i$, need not be positive, as general-equilibrium spillovers through input-output and labor-market linkages may more than offset any gains from protection accruing to sector *i*.

The second term $(\partial W / \partial p_i)$ in equation 5 picks up the effect of protection on social welfare and summarizes second-best considerations. It is labeled the "efficiency component" and is denoted by $\mathcal{E}_i$. (The derivations of $\mathcal{P}_i$, $\mathcal{E}_i$ are given in appendix 1.)

Although protection in one sector generates negative downstream spillovers through input-output linkages, it does not generate positive spillovers in upstream (tradable) sectors, because tradables prices (equal to $1 + t_i$ for all i) are unaffected by variations in domestic demand. By contrast, if there were an upstream nontradables sector—a situation not considered formally here or in the simulations for space considerations—its profits would be boosted by protection downstream, because the nontradable's price would be sensitive to variations in domestic demand. Ad hoc coalitions of interests could then form between tradables and nontradables sectors.

Explaining the Three Stylized Facts

Proposition 1 shows that when interindustry linkages are taken into account, the net political power of an industry increases with the amount of that industry's sales to final consumers rather than with its overall output. The first two corollaries help explain the first two stylized facts (tariff escalation and higher levels of protection in poor countries). Proposition 2 shows that sectors that employ a large share of the labor force face stronger counter-lobbying due to factor market rivalry. Corollary 3 helps explain the third stylized fact (higher levels of agricultural than manufacturing protection in rich countries and the reverse in poor countries).

Proposition 1. *The net political power of sector* i *increases with the level of sector* i*'s sales to final users. Moreover, when its free-trade level of output falls short of other sectors' intermediate requirements of good* i *by a sufficiently large amount, sector* i *may get negative protection in equilibrium.*

Proof. The first statement follows directly from the observation that $\mathcal{P}_i$ is an increasing function of $y_i - \sum_j a_{ij}y_j$, which is that part of final domestic consumption covered by domestic output. For the second statement, fix all prices except p_i at their equilibrium level and p_i at its free-trade level ($p_i = 1$). If $y_i < \sum_j a_{ij}y_j$, $\mathcal{P}_i < 0$, provided that the second-best term $\mathcal{E}_i$ is nonpositive or, if positive, not sufficiently large as to offset the political power term $\mathcal{P}_i$, $\partial V/\partial p_i = \mathcal{P}_i + a\mathcal{E}_i < 0$. Under the second-order condition, $\partial V/\partial p_i$ is a decreasing function of p_i; therefore, the equilibrium value of p_i must be lower than 1, which implies negative protection for good i.

Thus, it is the fraction of sector i's output that is sold to final consumers (or exported in the case of an export industry bidding for subsidies) that determines how much protection sector i gets in equilibrium. This is because unlike domestic downstream industries, neither final consumers nor foreign users are organized. Proposition 1 yields a second result as a by-product.

Corollary 1. *The net political power of final-goods industries is greater, ceteris paribus, than that of intermediate-goods industries.*

In a political equilibrium final-goods industries, which in most cases correspond to the fully processed goods industries mentioned earlier, are likely to obtain more protection than intermediate-goods industries unless output levels vary systematically and inversely with the degree of processing and this systematic variation is large enough to undo the negative effect of the term $\sum_j a_{ij}y_j$. Barring this, the overall structure of protection will display tariff escalation.[11] This result accords with the data in table 1 (see also Ray 1990) and helps explain the first stylized fact. It is also in accordance with the more detailed accounts in industry case studies of protection. For example, Moore (1996) notes in his study of protection in the U.S. steel industry that organized steel users joined forces to prevent an extension of the 1984 voluntary restraint agreement on steel imports. Destler and Odell (1987) offer similar arguments on the importance of counterlobbying.

If one is willing to accept that more developed economies have more sophisticated and interlinked production techniques (a lower share of value added to output), then the second stylized fact is also a corollary of proposition 1:

Corollary 2. *Tariffs are higher, ceteris paribus, in countries with sparse interindustry linkages (developing countries).*

To see this, note that $\partial \mathcal{P}_i / \partial a_{ij} < 0$. Thus poor countries tend to have higher levels of protection, because there are few incentives for other sectors to counterlobby increases in protection on intermediate inputs when interindustry linkages are sparse.

Proposition 2. *Incentives for owners of sector-specific capital in sector* i *to lobby for protection decrease with that sector's share of labor in value added.*

Proof. Abstract from interindustry linkages so that $\mathcal{P}_i$ in equation A6 in appendix 1 becomes $\mathcal{P}_i = y_i[1 - \tilde{\varepsilon}_i^w \tilde{\alpha}_i / \lambda_i]$, where $\tilde{\varepsilon}_i^w$ is the elasticity of the wage with respect to the price of good i, λ_i is the share of employment in sector i in the total labor force ($\lambda_i = \ell_i / \ell$), and $\tilde{\alpha}_i = w\ell_i / \tilde{p}_i y_i$ is the share of labor in value added. Given that labor is in fixed supply, $\tilde{\varepsilon}_i^w = (\eta_i \ell_i / \sum_j \eta_j \ell_j)$ where η_i is the real wage elasticity of labor demand in sector i. Furthermore, assuming that η_i is identical across sectors, $\tilde{\varepsilon}_i^w = \lambda_i$. Then, $\mathcal{P}_i = y_i[1 - \tilde{\alpha}_i]$. Finally, $\partial \mathcal{P}_i / \partial \tilde{\alpha}_i < 0$.

Thus the labor rivalry term is increasing in the share of labor in sector i's value added. In a setting with flexible wages incentives to lobby for protection by owners of sector-specific capital are likely to be small in industries where a large share of value added can be attributed to labor.

Finally, if one is willing to accept that the share of labor in value added in agriculture relative to manufacturing is higher in developing economies—as suggested by the calibration of Anderson (1995) and Chenery and Syrquin's (1986) data in the simulations below (table B-2)—then the explanation for the third stylized fact is that:

Corollary 3. *The ratio of agricultural to manufacturing tariffs will be lower, ceteris paribus, in countries where the share of labor in value added is higher in agriculture than in manufacturing (low-income developing countries).*

11. Note that the Leontief technology implies that downstream users of a protected intermediate good are trapped, as they cannot escape the intermediate good's increased domestic price by substituting away from it. With a different technology, input substitution would be an alternative to lobbying against the intermediate good's protection; with a lesser incentive to counterlobby, the tariff-escalation result would be weakened.

According to the data calibration in the next section, in developing areas the share of labor in value added is two times greater in agriculture than in manufacturing. In industrial countries, by contrast, the share of labor in value added is 30 percent larger in manufacturing than in agriculture. According to corollary 3, this could help explain the third stylized fact.

IV. Simulations: Endogenous Protection in Archetypal Rich and Poor Economies

The propositions are ceteris paribus results pertaining only to the political power term in the endogenous-tariff formula, with second-best terms held constant. Relaxing this assumption in simulations can help show how consistent the model's predictions are with the stylized facts presented earlier. Partial equilibrium simulations are conducted for archetypal rich and poor economies with data for three-sector economies with two tradables sectors, agriculture and industry, and one nontradeables sector.[12] The simulations are based on a disaggregation of the economy that includes interindustry flows, a key element of lobby rivalry highlighted by the model. Data sources and parameters representing demand and supply elasticities are described in appendix B and come mainly from Anderson (1995) and Chenery and Syrquin (1986).

On the basis of the data on the two archetypal economies, Anderson calculated the effects on the income of farmers and industrialists of a 10 percent rise in the relative price of industrial products as a result of a tax on agricultural exports. This relative price change reduced farmers' incomes by less than 4 percent while boosting industrialists' real incomes by 40 percent. Results were similar for an increase in the relative price of industrial products that had a small negative effect on farm incomes. By contrast, for the rich economy, a 10 percent increase in agricultural relative prices would boost real farm incomes by 23 percent while lowering industrial incomes by only 3 percent. His simulations suggest that farmers in poor countries who successfully seek price supports or oppose industrial protection would get only one-sixth to one-ninth the benefits of farmers in rich countries who successfully engage in the same activities. Likewise, industrial capitalists in poor countries would have more than 10 times the incentive to seek policies to protect manufacturing and reduce agricultural prices than would industrialists in rich countries.

Anderson conjectures that this adjustment pattern helps explain the difficulties encountered in concluding the Uruguay Round of negotiations, because farm lobbies in industrial countries opposed reductions in farm support. But this observation falls short of fully explaning the observed pattern of protection

12. The words "rich" and "poor" other are used only to indicate the differences in economic structure observed between countries, as in Chenery and Syrquin (1986), for example. This is the choice of names in Anderson (1995), kept here for continuity.

in rich and poor economies. The simulations reported in this article show that Anderson's conjecture emerges endogenously in this model of tariff determination. Moreover, the ability of the Grossman-Helpman model to generate this particular pattern of protection comes specifically from the extensions introduced here.

The simulations are based on a numerical evaluation of equations 5, A-6, and A-5, which are indexed over two tradables sectors (agriculture and manufacturing), though the economy also includes a third, nontradables sector (for consistency with Anderson [1995] and with the Chenery and Syrquin [1986] data). The wage appears as a variable in these equations. This requires adding an equation for determining labor demand for each sector as well as a labor-market constraint ($\sum_i \ell_i = \ell$) determining the equilibrium wage rate.

Thus, the three-sector model used here has seven equations: two determine the tariffs, three determine cost-minimizing labor demand (ℓ_i), and one determines the market-clearing wage rate (w). (Appendix B describes the equations used in these partial-equilibrium simulations.) Because the income–expenditure link is not specified in this system of equations, the model is closed by fixing the price of the nontradables sector.[13] The solution to this model yields tariffs for tradables sectors, labor allocations that clear the labor market, and the value of the wage in terms of the price of the nontradables sector.

This model is best viewed as an approximate local calculation of an equilibrium tariff structure for the selected elasticities of demand and supply (described in table A-2) and an exogenously given preference (given by the parameter a) for the welfare of the representative consumer.[14] For low enough values of the weight attached to welfare (a), the first-order condition of the optimization problem might yield a minimum of the function V, rather than a maximum. In other words, tariffs may be high enough to give rise to negative value-added prices. Such cases are not considered here.

For variations in the weight attached to welfare (a), the model predicts the endogenous tariff rates for the two archetypal economies (table 2). As expected, for both economies the greater the weight politicians attach to welfare, the lower the rate of protection. For sufficiently large weights, tariffs tend to zero. Next, note that the average level of protection is lower in the rich economy. The ratio of value added to output is lower for the rich economy (55 percent) than

13. Eliminating the tradables–nontradables link by omitting the income–expenditure link is justifiable for this illustrative exercise. Simulations here are meant to check only whether the model can reasonably support the stylized facts. Conditions under which this partial equilibrium approach is valid in a general equilibrium model identical to this one are given in Dornbusch (1974).

14. Evaluation of the formula in the text is based on elasticities that are valid only for small changes around the equilibrium. Moreover, some of those elasticities are endogenously determined; for instance, price elasticities of the wage rate depend, at a cost-minimizing equilibrium, on the elasticity of substitution and on the elasticity of labor demand, whose calculation depends on domestic prices, which in turn include a guess about the tariff value. Systematic experimentation with several starting values always yielded the same solution.

TABLE 2. Endogenous Tariff Structure in Rich and Poor Countries (%)

	Rich			Poor		
	$a = 1.5$[a]	$a = 2$	$a = 10$	$a = 1.5$[b]	$a = 2$	$a = 10$
Average agricultural tariff	52.5	27.7	3.3	30.1	23.3	5.9
Average manufacturing tariff	16.8	11.5	1.9	69.7	47.7	9.2
Aggregate tariff	20.9	13.3	2.0	35.6	26.7	6.4

Note: a is the weight politicans attach to the welfare of the representative consumer.

[a]If $a < 1.25$, the solution is not unique.

[b]If $a < 1.25$, the optimization problem yields a minimum.

Source: See appendix B.

for the poor economy (74 percent), so that stronger counterlobbying by downstream users in the rich economy reduces the average rate of protection. But the most striking result is the pattern of protection. In accordance with the stylized facts, the pattern of incentives systematically favors manufacturing in the poor economy and agriculture in the rich economy. In the simulations reported in table 2, agriculture is a net exporting sector in the poor economy and industry is in the rich economy. Thus it is not necessarily true that the endogenous tariff and subsidy structure results in large sectors receiving higher protection because of their larger political weight.

There is no doubt that other factors—limited means of taxation in poor countries and lack of political power by all but a tiny minority of farmers to organize themselves into lobbying activities—are important in explaining the relative pattern of incentives between agriculture and industry in poor and rich economies. Nonetheless, by taking account of intermediate goods and assuming a greater variation in the pattern of elasticities across sectors in poor countries, this model generates a pattern of protection that conforms to the one observed: higher protection and greater variance of tariff rates in poor than in rich economies. But because manufacturing activities are aggregated into a single activity, it is difficult to verify whether the endogenously determined tariff structure captures the escalation in protection by degree of processing. Overall, however, these archetypal representations appear to capture adequately the main differences in the pattern of protection between rich and poor economies. They are broadly consistent with the stylized facts on the pattern of protection in rich and poor countries.

Sensitivity calculations for the rich country archetype help establish the robustness of results to underlying assumptions about parameter values (table 3). For reference, the results from applying the Grossman-Helpman formula are also reported. Not surprisingly, because the Grossman-Helpman model has no intermediate goods, their formula yields a higher level of protection.

Column 1 of table 3 reproduces the result of table 2 for $a = 2$; the other columns report the results of one-by-one variations from this baseline case. Columns 2 and 3 cut the input-output delivery coefficients for manufacturing

TABLE 3. Sensitivity Analysis for the Rich Economy

	Base[a]	$a_{Mj}/2$	$a_{Aj}/2$	$\varepsilon^w_M = 0$	$\varepsilon^w_A = 0$	$\varepsilon^c_{MM}/2$	$\varepsilon^c_{AA}/2$	$\sigma^A = 0.8$ $\sigma^M = 1.2$	G-H[b]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T^A	27.7	39.8	86.2	24.2	56.3	22.2	31.6	38.3	42.4
T^M	11.5	32.5	11.4	25.3	10.8	12.1	11.3	7.2	30.6
$\overline{T}$	13.3	33.3	20.0	25.2	16.0	13.3	13.7	10.6	31.9

[a]Base results are for the rich economy from table 2 for $a = 2$.

[b]Calculated from Grossman and Helpman (1994), Proposition 2, and note 10 under the assumption of no consumer participation in interest groups.

Source: See appendix B.

and agriculture in half. In both cases smaller intermediate sales reduce counterlobbying from other sectors, yielding higher protection for the sector. When the manufacturing sector's input-output delivery coefficients are cut, protection of agriculture also rises. This is because the reduction in the agricultural sector's requirement for manufactured goods raises its equilibrium profits (in spite of the higher price of manufactured goods), leading to stronger agricultural lobbying.

Columns 4 and 5 isolate the general equilibrium effects of the wage rate adjustment by setting the price elasticity of the wage rate to zero. This may be viewed as the most relevant closure empirically, because it could be argued that general equilibrium effects are not central to lobbyists' decisions. Although not reported, the pattern of protection in which agriculture is protected relative to manufacturing holds when both elasticities are simultaneously set to zero. This closure amounts to determining the tariff of a small sector in the sense that changes in that sector's demand for labor have no influence on the equilibrium wage rate. As explained in section II, there is less counterlobbying, and in this highly aggregated model with only three sectors this effect is quantitatively important, resulting in a near doubling of the sector's endogenous tariff.[15]

Columns 6–8 consider second-best effects by varying demand and supply elasticities. A reduction in the own-price elasticity of demand in one sector reduces its elasticity of import demand, in turn reducing the welfare cost of protection in that sector.[16] The same effect is at work in the simulation reported in column 8: A departure from unitary elasticities of substitution in production

15. Though in a different context, this experiment is reminiscent of the thought experiment performed by Mayer (1984) in a direct democracy setting. In a Ricardo-Viner economy he showed that sectors with weaker wage complementarities (by which he meant weaker effects of one sector's tariff on the equilibrium wage rate) are more likely to get support and obtain protection.

16. Ramsey effects also explain why cutting the price elasticity of demand in manufacturing reduces the variance of tariffs, whereas cutting the same elasticity in agriculture raises their variance. In the case of the reduction in the elasticity of demand for manufacturing, the differential in the pattern of elasticities is reduced, whereas in the case of agriculture the opposite occurs. Thus a reduction in the elasticity of demand for manufacturing leads to a more uniform pattern of protection, but the opposite occurs with a reduction in the elasticity of demand for agriculture.

also raises the differential in elasticities of supply. By second-best considerations, this results in a greater dispersion of the tariff structure.

V. Concluding Remarks

This article introduced two extensions to the Grossman-Helpman framework: vertical (input-output) linkages and interindustry rivalry on labor markets. These extensions, though involving minimal additional complication, are potentially useful in several ways.

First, they help overcome a basic contradiction between the logic of influence-activity models and empirical evidence (and intuition). According to the model's logic, a higher level of domestic output raises the return to lobbying. In a common-agency context this leads lobbies to face governments with steeper incentive schedules, leading to more generous trade protection in equilibrium. Intuition and empirical evidence, however, suggest that lobbying intensity and the government's protectionist response should increase with the perceived threat of import competition (with rising import-penetration ratios). The gist of the approach here is to show that equilibrium protection is the result not just of lobbying pressures from direct beneficiaries of protection but also of counterlobbying by negatively affected downstream industries. This argument is distinct from the Grossman-Helpman notion that producer lobbies internalize consumption effects, which requires them to be large enough in the population. This extension of the Grossman-Helpman approach suggests that, empirically, the relationship between domestic output and equilibrium protection may be blurred by potentially larger effects having to do with counterlobbying.

Second, these extensions suggest a pattern of trade protection that displays tariff escalation by degree of processing, an empirical regularity that cannot be accounted for by the basic version of the Grossman-Helpman model, which superimposes a sophisticated political game on an overly stripped-down economy.

Finally, the extended Grossman-Helpman model can generate endogenously a pattern of protection suggested by Anderson (1995), whereby poor countries protect industry relative to agriculture while rich ones do the reverse. Thus the message is that the apparent difficulty of reconciling the logic of the Grossman-Helpman model with stylized facts comes not so much from the political game at the center of their analysis as from the straitjacket of an overly simplistic underlying economy. Once the straitjacket is relaxed, the model proves capable of generating a wealth of plausible implications.

The analysis is perforce essentially positive rather than normative, a characterization that applies to the entire political economy literature. Indeed, the central tenet of this literature is that irrespective of what economists think governments ought to do, economists first need to understand what governments actually do and why they do it, which requires a realistic view of the objectives and constraints of elected political representatives. Normative prescriptions are thus thrown back one step, into recommendations for institutional arrangements capable of mitigating policy capture by special interests.

However, useful if somewhat impressionistic policy considerations can emerge as by-products of this positive analysis. For instance, the emphasis on counterlobbying suggests that in conformity with the general logic of common-agency models, good policies do not necessarily come from politicians maximizing "welfare-friendly" objective functions or operating under tight rules, but possibly from the balance of conflicting special-interest pressures. Conversely, bad policies can result from imbalances in lobbying pressures and from institutional arrangements that weaken pressure from one side aggravate distortions. For instance, duty-drawback schemes can be expected to weaken antiprotectionist pressure from downstream users of import-competing goods, which may well lead to higher tariffs on intermediate products. This idea is pursued theoretically and shown to hold empirically in Cadot and others (2003).

Appendix A. Derivation of the Efficiency and Political Effects

At any point the economy is characterized by its income-expenditure identity, namely

$$E(1, \mathbf{p}, W) \equiv R(1, \mathbf{p}, \mathbf{k}, \ell) + T(\mathbf{p}) \tag{A-1}$$

where 1 is the price of the numéraire, $E(.)$ is the economy's expenditure function, R(.) is its revenue function, **k** is the vector of $n+1$ specific factors, and T(.) is tariff revenue. The homogeneity properties of equation A-1 implies that only relative prices can be determined. Differentiating this identity with respect to p_i and letting E_i stand for the partial derivatives of the expenditure and R_i for revenue functions with respect to p_i gives

$$E_i + E_W(\partial W/\partial p_i) = R_i + (\partial T/\partial p_i). \tag{A-2}$$

Let m_i stand for sector i's import-demand function, c_i and y_i being respectively the domestic consumption and production of good i, and a_{ij} the input-output coefficients. Rearranging equation A-2 and using Shephard's and Hotelling's lemmas, together with the fact that the marginal utility of income is 1 under equation 2, we have

$$\partial W/\partial p_i = -m_i + (\partial T/\partial p_i). \tag{A-3}$$

Choose units so that all international prices are equal to one, and let $t_i = p_i - 1$ be the tariff rate. Tariff revenue can be written as $T(p) = \sum_j t_j m_j(p)$, so that

$$\partial T/\partial p_i = m_i + \sum_j t_j(\partial m_j/\partial p_i).$$

Substituting this into equation A3 gives

$$\partial W/\partial p_i = \sum_j t_j\,(\partial m_j/\partial p_i) = \sum_j t_j\left([\partial c_j/\partial p_i] + \sum_k a_{jk}\,[\partial y_k/\partial p_i] - [\partial y_j/\partial p_i]\right).$$

which confirms that supply of good j is a function of the "net price" of good j, $\tilde{p}_j = p_j - \sum_j a_{kj}p_k$, and of the wage rate. Thus,

$$\partial y_k/\partial p_i = (\partial y_k/\partial \tilde{p}_k)\;(\partial \tilde{p}_k/\partial p_i) + (\partial y_k/\partial w)\;(\partial w/\partial p_i).$$

Collecting terms,

$$\begin{aligned}\partial W/\partial p_i = &\sum_j t_j(c_j/p_i)\varepsilon^c_{ij}\\ &+\sum_j t_j\Bigg[\sum_k a_{jk}[\partial y_k/\partial \tilde{p}_k]\;[\partial \tilde{p}_k/\partial p_i] - [\partial y_j/\partial \tilde{p}_j]\;[\partial \tilde{p}_j/\partial p_i]\\ &+\left(\sum_k a_{jk}\;[\partial y_k/\partial w] - [\partial y_j/\partial w]\right) - (\partial w/\partial p_i)\Bigg].\end{aligned}$$

Let $\tilde{\varepsilon}^s_i = \partial\; log\; y_i/\partial\; log\; \tilde{p}_i\;(>0)$ be the elasticity of supply to the net price of good $i, \mu_i = -\partial\; log\; y_i/\partial\; log\; w\;(>0)$ the wage elasticity of supply in sector i, and $\varepsilon^c_{ij} = \partial\; log\; c_j/\partial\; log\; p_i$ the cross-price elasticity of final demand in sector i. Using the fact that

$$\partial \tilde{p}_k/\partial p_i = \begin{cases} 1 - a_{ii} & \text{for } k = i\\ -a_{ik} & \text{otherwise,}\end{cases}$$

yields

$$\begin{aligned}\sum_k a_{jk}(\partial y_k/\partial \tilde{p}_k)\;(\partial \tilde{p}_k/\partial p_i) &= a_{ji}(1-a_{ii})\;(\varepsilon^s_i y_i/\tilde{p}^i) + \sum_{k\neq i} -a_{jk}a_{ik}(\varepsilon^s_k y_k/\tilde{p}^k)\\ &= a_{ji}(\varepsilon^s_i yi/\tilde{p}^i) - \sum_{k=0}^{n} a_{jk}a_{ik}(\varepsilon^s_k y_k/\tilde{p}^k).\end{aligned}$$

Similar calculations give

$$\sum_j t_j(\partial y_j/\partial \tilde{p}_j)\;(\partial \tilde{p}_j/\partial p_i) = \varepsilon^s_i t_i y_i/\tilde{p}^i - \sum_{j=0}^{n} -a_{ij}(\varepsilon^s_j y_j/\tilde{p}^j),$$

$$\sum_j t_j\left(\sum_k a_{jk}[\partial y_k/\partial w]\right) = -\sum_j\sum_k t_j a_{jk}(\mu_k y_k/w)$$

and

$$\sum_j t_j(\partial y_j/\partial w) = -\sum_j(\mu_j t_j y_j/w).$$

Finally, let $\tilde{\varepsilon}_i^w = \partial log\ w/\partial log\ \tilde{p}_i$ (> 0) be the elasticity of the wage rate to a change in the net price of good i. The expression for the wage-rate adjustment term is given by:

$$\begin{aligned}\partial w/\partial p_i &= \sum_k(\partial w/\partial\tilde{p}_k)\ (\partial\tilde{p}_k/\partial p_i) = \sum_k(\partial w/\partial\tilde{p}_k)\ (1_{k=i} - a_{ik}) \\ &= \tilde{\epsilon}_i^w w/\tilde{p}_i - \sum_k a_{ik}\tilde{\varepsilon}_k^w w/\tilde{p}_k. \qquad \text{(A-4)}\end{aligned}$$

Combining these gives:

$$\begin{aligned}\varepsilon_i = &\sum_j(t_j c_j/p_i)\varepsilon_{ij}^c + \sum_j t_j\left(a_{ji}[\varepsilon_i^s y_i/\tilde{p}^i]\sum_{k=0}^n a_{jk}a_{ik}[\varepsilon_k^s y_k/\tilde{p}^k]\right) \\ &- \left([\varepsilon_i^s t_i y_i/\tilde{p}^i] + \sum_{j=0}^n a_{ij}[\varepsilon_j^s y_j/\tilde{p}^j]\right) \\ &- \left[\left(\sum_j[\mu_j t_j y_i/w]\right) - \left(\sum_j\sum_k t_j a_{jk}\ [\mu_k y_k/w]\right)\right]\left(\tilde{\varepsilon}_i^w w/\tilde{p}i - \sum_k a_{ik}\tilde{\varepsilon}_k^w w/\tilde{p}_k\right).\end{aligned}$$

(A-5)

If $P_i > 0$, the existence of an interior solution with positive protection requires that $E_i < 0$. As in the Grossman-Helpman model, the first term and the following two in parentheses are Ramsey terms minimizing the deadweight loss due to the tariff on good i. They are decreasing in the own-price elasticity of supply (ε_i^s) and in (the absolute value of) the own-price elasticity of demand ($|\varepsilon_{ii}^c|$); that is, $E_i \to -\infty$ (driving down the equilibrium tariff toward zero) when either $\varepsilon_{ii}^c \to -\infty$ or $\varepsilon_i^s \to \infty$. The expression in square brackets is a sum of second-best terms reflecting the presence of positive tariffs on other goods; it is multiplied by a wage-adjustment factor in parentheses. Clearly, if there is no tariff other than i ($t_j = 0$ for all $j \neq i$) there is no second-best argument for a positive t_i on efficiency grounds. But even with positive tariffs on other goods, the second-best argument for a tariff in i vanishes when all general equilibrium linkages picked up by cross-price elasticities of demand, input-output coefficients, and wage-rate adjustment are simultaneously zero, as in Grossman-Helpman.

Using the notation introduced in this appendix, equation 6 can also be rewritten in terms of elasticities and shares. Using equation A4, let $\tilde{\alpha}_i = w\ell_i/\tilde{p}_i y_i$ be the share of labor in sector i's value added, and let $\lambda_i = \ell_i/\ell$ be the share of sector i in total employment,

$$\text{(A-6)} \qquad \mathcal{P}_i = y_i - \sum_j a_{ij}y_j - \left((\tilde{\alpha}_i/\lambda_i)\varepsilon_i^w - \sum_k a_{ik}(\tilde{\alpha}_k/\lambda_k)\tilde{\varepsilon}_k^w\right)y_i.$$

Appendix B. Equations and Calibration of the Simulation Model

The first two equations in the simulation model are the government's first-order condition, given by equations A-6 and A-5, which are indexed over tradables sectors (agriculture and manufacturing). Labor demand is indexed over all activities, and assuming that the nested value-added function is of the CES type, labor demand in sector i (i = agriculture, manufacturing, or nontraded) is given by

$$\ell_i = (\tilde{\alpha}_i\tilde{p}_i/w)^{\sigma i}yi.$$

Finally, the following equation determines the equilibrium wage:

$$\ell = \sum_i \ell_i.$$

Several elasticities in table B-2 are calculated internally from the data. This is the case for μ_i, the wage elasticity of supply used in equation A-5, and η_i, the elasticity of labor demand used in the calibration of $\tilde{\varepsilon}_i^w(\tilde{\varepsilon}_i^w = \eta_i\ell_i/\sum_j \eta_j\ell_j)$, the elasticity of the wage rate to a change in the net price of i used in equations A4 and A5.

Data Sources

Tables B-1 and B-2 give the data used in the simulations. Parameters describing technology and demand and production structures (like ratios of sectoral consumptions and productions) are taken from Anderson (1995, table 1). So are price elasticities of demand and the assumption of a Cobb-Douglas technology (except for sensitivity analysis). Anderson, however, does not provide information on input-output relationships. For the rich economy these were taken from de Melo and Tarr (1992). For the poor economy the coefficients are aggregated

Table B-1. Input-Output Coefficients for Rich and Poor Archetypes

	Agriculture		Manufacturing		Nontradables	
	Rich	Poor	Rich	Poor	Rich	Poor
Agriculture	0.38	0.22	0.04	0.09	0.028	0.03
Manufacturing	0.26	0.04	0.42	0.10	0.26	0.05
Nontradables	0.07	0.05	0.08	0.10	0.11	0.05

Source: See appendix B.

TABLE B-2. Output Composition and Elasticities for Rich and Poor Archetypes

	Agriculture		Manufacturing		Nontradables	
	Rich	Poor	Rich	Poor	Rich	Poor
C	7	35	12	10	31	20
Y	6	61	46	10	68	29
$\tilde{\alpha}_i^{a}$	0.35	0.70	0.50	0.35	0.60	0.60
σ_i	1	1	1	1	1	1
μ_i^{b}	0.54	2.33	1	0.54	1.5	0.43
η_i^{c}	1.54	3.33	2	1.54	2.5	1.43
λ_i^{d}	0.031	0.761	0.379	0.065	0.590	0.175
$\tilde{\varepsilon}_i^{e}$	0.02	0.774	0.258	0.06	0.722	0.166
$\varepsilon^{i,i}$	−0.12	−0.25	−0.52	−0.69	−0.21	−0.49

[a] $\tilde{\alpha}_i = w\ell_i/\tilde{p}_i y_i$.
[b] $\mu_i = \varepsilon_i^s(\sigma_i\tilde{\alpha}_i)/(1-\tilde{\alpha}_i)$.
[c] $\eta_i = \sigma_i/(1-\tilde{\alpha}_i)$.
[d] For $w = 1$.
[e] For $w = 1$ and from expression $\tilde{\varepsilon}_i^w = (\eta_i\ell_i/\sum_j \eta_j\ell_j)$.
Source: See appendix B.

from information used in Chenery and Syrquin (1986, chap. 4) for a typical economy with a \$500 GDP per capita (in 1970 dollars). The main difference in interindustry structure between the rich and poor economies in table B-1 is the higher value-added ratio in the poor economy due partly to the higher value-added ratio in agriculture (see Chenery and Syrquin, figures 3.3 and 3.4). Table B-2 gives the remaining elasticities and shares.

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Social Protection in a Crisis: Argentina's Plan Jefes y Jefas

Emanuela Galasso and Martin Ravallion

The article assesses the impact of Argentina's main social policy response to the severe economic crisis of 2002. The program was intended to provide direct income support for families with dependents and whose head had become unemployed because of the crisis. Counterfactual comparisons are based on a matched subset of applicants not yet receiving program assistance. Panel data spanning the crisis are also used. The program reduced aggregate unemployment, though it attracted as many people into the workforce from inactivity as it did people who otherwise would have been unemployed. Although there was substantial leakage to formally ineligible families and incomplete coverage of those who were eligible, the program did partially compensate many losers from the crisis and reduced extreme poverty.

Income transfer programs are a common social policy response to macroeconomic crises. Stated goals vary, but the common (explicit or implicit) goal is to help protect the living standards of families most adversely affected by the crisis. One of the largest recent programs is Argentina's Plan Jefes y Jefas, introduced in January 2002 as a public safety net response to the severe economic crisis that hit Argentina at the end of 2001. Unemployment and poverty rates reached record levels (World Bank 2003). Jefes aimed to provide direct income support for families with dependents who had lost their main source of earnings due to the crisis. To ensure that the program reached those in greatest need, work requirements were imposed. With support from a World Bank loan (and equivalent counterpart funds from the government), the program expanded rapidly to cover about 2 million households by late 2002.[1]

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1. In 2002 the government of Argentina spent about US$500 million on Jefes, about a quarter of it financed through a World Bank loan. For 2003 the estimate is US$600 million, of which the bank loan will probably cover about half. The loan and counterpart funds cover mainly the payments to beneficiaries. Most costs for supplies and equipment for the workfare projects are covered by the local governments or nongovernmental organizations sponsoring the projects.

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Knowledge of the impacts of such programs has often been limited by a number of factors, including the speed with which crisis programs have to be scaled up and the paucity of appropriate survey data. Critics of the Jefes program have made claims about fraudulent participation, pointing to cases of registered participants who do not appear to satisfy the program's eligibility criteria, or about weaknesses in the implementation and effectiveness of the program's work requirements.[2] At the other extreme, some have argued that the scheme was a big success in reducing poverty and unemployment in the aftermath of the crisis. One assessment claimed that Jefes accounted for the entire reduction in unemployment in the year following the crisis, which happened to roughly equal the increase in Jefes registrations over the same period (INDEC 2002c; World Bank 2003).

Such claims often rest on transparently weak foundations. Anecdotes of abuse attract attention but may not be a sound basis for generalization. Claims about impact (positive and negative) often ignore behavioral responses. For example, it is unlikely that a program such as Jefes would not affect labor force participation choices. It is unlikely that all participants would have otherwise been unemployed. Similarly, the impact on poverty will be clearly overestimated if assessments ignore the forgone earnings of workfare participants, who are unlikely to be entirely idle in the absence of the program. The common failure to take full account of the costs to participants of targeted programs is known to be a serious deficiency of past evaluations (van de Walle 1998).

Several factors make the Jefes program an unusually good case for rigorous study of impacts. Large household surveys were done just before the crisis, in October 2001, and one year later, in October 2002, and the second survey identified Jefes participants. One-third of the October 2001 sample was followed up in the later survey round.

This article uses these survey data and the tools of nonexperimental program evaluation to address the following (related) questions about the Jefes program:

- Who got assistance? Were the program's eligibility criteria enforced?
- How did participants respond to the program, such as through labor supply and household composition? Did participants come solely from the ranks of the unemployed?
- What was the impact on the incomes of participating households? What share of the income loss due to the crisis was recovered through the program?
- What was the distributional impact?
- What was the impact of the program on aggregate unemployment and poverty?

2. See, for example, ERES (2004, annex 1) for a qualitative account. Examples of articles from the press include "Controversia por los planes de trabajo," *La Nacion*, April 1, 2002; "En Santa Fe se venden Planes sociales," *La Nacion*, May 13, 2002; "Escandalo Cordobes por el reparto de subsidios sociales sin control," *Pagina*/12, May 21, 2002; "Denucian que no hay control en la ayuda social," *La Nacion*, August 28, 2002.

In addressing these questions, a key issue is finding a valid comparison group who have similar characteristics to the Jefes participants but did not enroll in the program. This study exploits the fact that because the program was in a period of rapid scaling up, there were many applicants who had not yet received benefits. This group has advantages as the source of a comparison group, though the possibility of selection bias (that current participants are different ex ante to the current applicants) must also be considered. Current participants might have experienced larger income shocks as a result of the crisis and so were the first to join the program. Another possibility is that administrative assignment favored certain groups, possibly working against the program's espoused objectives. Matching methods and longitudinal observations are used to address these concerns, comparing current circumstances for both participants and applicants with a precrisis baseline.

I. The Crisis and the Government's Response through the Jefes Program

Argentina fell into a severe economic crisis at the end of 2001. Widespread concerns about the impending collapse of the convertibility plan (which pegged the Argentine peso to the U.S. dollar) and possible default on external debt led to draconian measures to prevent withdrawals of bank deposits. The final collapse of the convertibility plan and the subsequent sharp devaluation and default on foreign debt, combined with a freeze on deposits, resulted in a large contraction in national output.

The immediate welfare impacts were severe. McKenzie (2004) finds that three-quarters of households surveyed experienced real income declines in 2002, with the majority of them suffering a real income fall of 20 percent or more. Indicators of poverty rose sharply (Fiszbein and others 2002; World Bank 2003). The government's statistics office estimated that the proportion of people living below the poverty line rose from 37 percent just prior to the crisis (October 2001) to 58 percent a year later (World Bank 2003). Unemployment also rose, though McKenzie's (2004) results suggest that this contributed far less to falling living standards than did the shock to real wages.[3] Widespread political and social instability ensued.

As the government's main safety net response to this crisis, Jefes provided a cash transfer of 150 pesos a month to each eligible individual, representing about half of the mean household income per capita in Argentina in 2002. Those formally deemed eligible to participate were unemployed household heads with dependents (children under age 18 or people with disabilities).

3. In particular, McKenzie (2004) finds that about three-quarters of the average fall in total real household income between October 2001 and May 2002 can be attributed to a fall in real wages for workers remaining in the same job, whereas only 10 percent is due to losses from household members exiting their jobs.

Program participation had to be requested through the local municipality or through local offices of the Ministry of Labor.

Jefes replaced the smaller-scale Trabajar workfare program. Trabajar had a tightly enforced work requirement of 30–40 hours a week, with targeting criteria to help ensure that the work was of value to residents of poor communities. Trabajar was found to have been effective in reaching the poorest, both as workers and residents (Ravallion 2000; Jalan and Ravallion 2003). For example, 80 percent of Trabajar workers came from the poorest 20 percent of the population (Jalan and Ravallion 2003).

Because of the magnitude of the crisis, the government's explicit aim for the Jefes program was to reach a broader segment of the population than Trabajar. At its inception Jefes was advertised as a "universal" program, meaning that anyone who was eligible and wanted the transfer could get it. Contrary to its predecessor, Jefes did not have an explicit poverty focus. However, genuine universality for eligible households was clearly not sustainable.

In early 2002 concerns emerged about the budgetary cost of Jefes. There were signs (based largely on anecdotal evidence) that people who were not the most in need were capturing many of the program benefits. Ministry of Labor data based on registration records indicated that over half of Jefes participants were women and probably not heads of households. Administrators were not able to check whether an applicant was really a head of household. There were also anecdotal claims that to cope with the liquidity crisis municipalities and provinces were signing up their employees and that local civil servants were sending their wives (who were not in the workforce) to sign up. Possibly, the program's benefits were spilling over heavily to people who were not much affected by the crisis or who had the personal resources to cope adequately on their own. At the heart of this concern is the fact that verification of unemployment is problematic in Argentina, where over half of employment is in the informal sector. All that the administrators could reasonably verify with confidence was whether an applicant had a formal sector job, and so was registered as such.

Prompted by these concerns, a work requirement was introduced in early 2002, with the aim of helping ensure that the transfers reached those in greatest need.[4] The work requirement was not as demanding as that for the Trabajar program. Participants were required to do a minimum of 20 hours a week of basic community work, training activities, or school attendance. Alternatively, beneficiaries could find employment in a private company and receive a wage subsidy for six months. The municipalities (together with local nongovernmental organizations) were in charge of organizing the work activities, and provincial offices of the Ministry of Labor and municipal and provincial councils were responsible for monitoring the work activities.

4. As a condition for financing the program, the World Bank insisted that the vast majority (90 percent was the target) of Jefes participants had to meet the work requirement.

Because poor people tend to have lower reservation wages, the work requirement is likely to target the poor.[5] But it is not clear how effective the Jefes work requirement was in practice compared with Trabajar because of the weak capacity to organize, supervise, and enforce the work requirement at the local level in such a large program. The program's evolution (the work requirement was something of an afterthought), its rapid scaling up, and the circumstances of the crisis may have made it hard to enforce the work requirement. The work requirement is self-targeting only insofar as participants have to comply with it to obtain the transfer.

The behavioral responses to such a crisis and to such a large public program as Jefes are clearly of interest. Various responses could be expected. Some have argued that all participation in Jefes should be counted as a commensurate reduction in unemployment (INDEC 2003). This clearly ignores possible behavioral responses to the program through other labor supply decisions, either to participate in the workforce or to change the number of hours worked.

Household composition could also change as a response to such a shock, by delaying the formation of new households (Foster and Rosenzweig 2002), or as a response to the public transfers, by changing living arrangements (Duflo 2000). Splitting up households, with parents "sharing" children and applying to the program separately, has been reported anecdotally as a response to the Jefes program.

Behavioral responses are also relevant for assessing impacts on poverty. Following common practice, INDEC (2002b) calculated the program's poverty impact by subtracting the Jefes payment from the incomes of participants. Thus the poverty rate in the absence of the program was calculated from the simulated distribution of net incomes. However, this ignores the fact that participants are unlikely to have remained idle in the absence of the program but would have found some sort of work, possibly doing casual odd jobs. Ignoring participants' forgone incomes clearly leads to overestimation of the poverty reducing impact of the program.

II. Data and Descriptive Results

Data were taken from the October 2001 and 2002 rounds of the Permanent Household Survey (EPH) conducted by Argentina's Statistical Institute (INDEC). The survey collects information on employment, incomes, education, and household demographics in large urban areas and covers about 70 percent of the population. A subset of the sample is linked as a panel, with about a third of the sampled households in 2001 reinterviewed in 2002. For this study a special survey module on Jefes participation was administered in October 2002 to adult household

5. Supportive evidence on this assumption for Argentina can be found in the results of Jalan and Ravallion (2003) on the Trabajar program.

members for whom Jefes was not the main occupation. (The existing survey was deemed adequate for those for whom Jefes was the primary occupation.)

The grossed-up aggregate participation rate in Jefes taken from the EPH was compared with the administrative data on aggregate registrations (see the appendix). The comparison was complicated by the fact that the Jefes program is national in coverage, whereas the EPH sample frame excludes 30 percent of the population. A comparison based on place of residence finds that the grossed-up EPH count of Jefes participants accounts for 91 percent of the administrative aggregate. This leaves a significant discrepancy at the 5 percent level, though just barely: At the upper bound of its 95 percent confidence interval, the survey estimate accounts for 99 percent of registered participants. This suggests that there is unlikely to be any serious undercounting of Jefes participation in the EPH related to its sample frame.

Another question is how Jefes eligibility should be defined in terms of the EPH data. Beneficiaries signed statements certifying that they were unemployed and a head of household. However, the only signal of unemployment status that could be reliably checked by the authorities was whether an individual was participating in the formal labor market. Thus a definition of eligibility was used for this study that is close to what could be enforced by program administrators. A sampled adult was considered eligible if he or she was not employed in the formal labor market and lived in a household with a child (less than 18 years old and belonging to the head or the spouse) or a person with a disability. (Some important differences between this practical eligibility definition and the official theoretical eligibility definition are considered later in the article.)

By this definition about a third of the people receiving the program were not eligible (table 1). About 80 percent of economically active individuals who were eligible (although not necessarily poor) did not get into the program. Applicants

TABLE 1. Errors of Inclusion or Exclusion in the Jefes Program, October 2002

	Ineligible		Eligible		Total	
	Number	Percent	Number	Percent	Number	Percent
Applicants and participants						
Not receiving Jefes	677	14.1	824	17.1	1,500	31.2
Receiving Jefes	994	20.7	2,311	48.1	3,305	68.8
Total	1,671	34.8	3,134	65.2	4,805	100
All economically active adults						
Not receiving Jefes	22,285	71.0	6,763	21.6	29,047	92.6
Receiving Jefes	656	2.1	1,671	5.3	2,327	7.4
Total	22,940	73.1	8,434	26.9	31,374	100

Note: A person is deemed to be "eligible" if he or she lives in a household with dependents (children of the household head younger than 18 or a person with a disability) and is not in the formal labor market, as indicated by receipt of formal job benefits.

Source: Authors' calculations based on data from the October 2002 EPH.

not yet receiving the benefit were more likely to be ineligible than were current recipients.

The average Jefes participant in the sample is female (69 percent of participants, compared with 43 percent for all economically active adults), 36 years old, married, not a head of household (for 57 percent of participants), and has 8 years of schooling (table 2). Jefes participants are less likely to be heads of households than the sample of all economically active adults and more likely to be spouses of heads. The participants tend to come from larger than average households—5.4 people per participating household, compared with 4.2 for all economically active adults—with the difference accounted for by a greater number of children in Jefes households.[6] Jefes households are poorer on average, with a per capita household income of about 30 percent of the average for all economically active adults. Netting out the Jefes transfer payment reduces the per capita household income to 17 percent of the average for all economically active adults. Jefes participants and applicants tend to have similar characteristics, a finding examined more carefully later using a multivariate model.

The households of Jefes participants tend to be poorer on average than the households with eligible heads. Although there is a high incidence of ineligibility among Jefes participants and limited coverage of eligible households, ineligible Jefes participants are less poor than eligible households. A comparison of the empirical cumulative distribution function of per capita household income for eligible and ineligible participating households shows a first-order dominance—no matter what poverty line is used, eligible participants are poorer than ineligible participants (figure 1).[7] Most of the eligibility violations relate to the dependency criterion. Tighter enforcement of this criterion would improve the program's performance in reaching the poor, albeit only slightly.

The precrisis baseline survey for October 2001 shows that 43 percent of Jefes participants as of October 2002 had been employed a year earlier, 38 percent were inactive, and 19 percent were unemployed (table 3). The unemployed participants were more likely to be in the bottom decile of the income distribution.[8] Jefes participants and applicants have similar baseline characteristics in the panel sample.

One possible source of bias in the use of Jefes applicants as a comparison group is that participants may have experienced larger income shocks in the crisis than did the applicants who had not yet joined the program. In that case

6. Note that the extent of multiple participants in the same household is limited: 13 percent of participating individuals live in household with more than one beneficiary, and 7 percent of households have more than one beneficiary.

7. This holds for a broad class of additive poverty measures (Atkinson 1987).

8. More precisely, 26 percent of previously unemployed participants are in the bottom decile of the per capita income distribution in 2001, compared with 16 percent of the previously economically inactive participants and 11 percent of the previously employed participants.

TABLE 2. Descriptive Statistics as of October 2002, Cross-Section

	Jefes Participants		Jefes Applicants		Eligible Heads or Spouses		Economically Active Adults (Age 18–65)	
Characteristic	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Individual demographics								
Male	0.31	0.46	0.39	0.49	0.41	0.49	0.57	0.49
Age	35.8	11.1	37.1	13.5	38.9	10.2	37.9	12.1
Single	0.18	0.38	0.19	0.39	0.02	0.15	0.29	0.45
Married	0.68	0.46	0.64	0.48	0.91	0.28	0.61	0.48
Head	0.43	0.49	0.44	0.49	0.49	0.50	0.49	0.49
Spouse of head	0.34	0.47	0.31	0.46	0.51	0.50	0.21	0.4
Son or daughter of head	0.16	0.36	0.17	0.37	0		0.57	0.42
Years of education	8.07	3.14	8.17	3.29	9.28	3.65	10.77	3.91
Employment status								
Jefes main activity	0.72	0.45						
Doing work requirement (min. 20 hours) if Jefes is main activity	0.83	0.37						
Jefes secondary activity	0.28	0.44						
Doing work requirement (min. 20 hours) if Jefes is secondary activity	0.16	0.36						
Doing work requirement (min. 20 hours)	0.64	0.47						
Employed	0.84	0.36	0.31	0.46	0.52	0.50	0.17	0.38
Unemployed	0.06	0.24	0.36	0.48	0.14	0.34	0.83	0.38
Inactive	0.10	0.29	0.33	0.47	0.35	0.48	–	–

Total hours worked	19.8	14.2	11.6	21.5			32.5	23.7
Total hours worked = 0	0.14	0.34	0.65	0.47			0.19	0.39
Household characteristics								
Household size	5.42	2.42	4.89	2.4	4.83	1.83	4.23	2.05
Number children < 18 years	2.67	1.87	2.08	1.8	2.33	1.51	1.34	1.55
Total household income	420.9	302.1	350.2	323.8	647.3	917.3	985.6	1139.8
Per capita household income	84.1	59.2	77.4	71.87	150.1	226.8	271.8	378.8
Total household income net of Jefes	246	292.6						
Per capita household income net of Jefes	46.8	56.1						
Eligibility criteria for Jefes	1		0		0.12	0.32	0.07	0.26
Household with children of head < 18 years or disabled member	0.80	0.39	0.66	0.47	1		0.53	0.49
Household with any children < 18 or disabled member	0.95	0.22	0.84	0.36	1		0.62	0.48
Individual is formal sector worker	0.02	0.14	0.02	0.16	0			
Household has at least one formal sector worker	0.15	0.35	0.19	0.39	0.28	0.44	0.53	0.49
Eligible individual (2) any children, individual not formal sector worker	0.93	0.25	0.82	0.38	0.80	0.40	0.41	0.49
Eligible individual (1) children of head, individual not formal sector worker	0.69	0.45	0.54	0.49	1		0.26	0.43
Eligible household (household with at least one eligible individual (1)	0.79	0.40	0.64	0.47	1		0.45	0.49
Number of observations	3,092		1,713		13,934		31,374	

Source: Authors' calculations based on data from the October 2002 EPH.

FIGURE 1. Eligibility of Jefes Participants: Cumulative Distributions of Income Postintervention, October 2002

Source: Authors' calculations based on data from the October 2002 EPH.

the measured income losses for applicants during the crisis would underestimate the counterfactual income losses for participants.

The likely extent of mismatch in terms of income shocks can be assessed by comparing income changes under alternative assumptions about the share of forgone income when constructing the counterfactual income of Jefes participants and then calculating the corresponding income shock using the panel data. Comparing the distribution of shocks between Jefes participants and applicants gives a sense of the extent of the bias under alternative hypotheses on the net gains from the program. If the identifying assumption holds, the expected change in income in the absence of the program should be the same for participants and applicants.

The results show that the expected change in income in the absence of the program is balanced across the two groups under the assumption of forgone income of about one-third to one-half, which is consistent with the estimates by Jalan and Ravallion (2003) for the Trabajar program (table 4). However, the tighter work requirements under Trabajar could mean that Jefes forgone incomes are lower. The estimated income changes based on the preferred estimates (explained later) of forgone income of Jefes participants are presented in the last column of table 4.

TABLE 3. Descriptive Statistics as of October 2001, Panel Sample

	Jefes Participants		Jefes Applicants		Eligible Heads or Spouses		Active Adults (Age 18–65)	
Characteristic	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Individual demographics:								
Male	0.29	0.45	0.46	0.5	0.39	0.49	0.59	0.49
Age	35.79	11.17	37.3	13.36	39.64	10.19	38.83	12.15
Single	0.2	0.4	0.24	0.43	0.02	0.15	0.28	0.45
Married	0.69	0.46	0.63	0.48	0.91	0.28	0.62	0.48
Head	0.37	0.48	0.44	0.5	0.47	0.50	0.51	0.50
Spouse of head	0.39	0.49	0.3	0.46	0.53	0.50	0.20	0.40
Son or daughter of head	0.19	0.39	0.19	0.39	0.00	0.00	0.24	0.43
Years of education	8.24	3.2	7.94	3.34	9.05	3.72	10.59	3.94
Employment status								
Employed	0.43	0.5	0.44	0.5	0.46	0.50	0.83	0.37
Unemployed	0.19	0.39	0.19	0.39	0.14	0.34	0.17	0.37
Inactive	0.38	0.48	0.36	0.48	0.41	0.49	0	
Total hours worked	13.9	21.5	14.6	22.7	18.7	26.2	34.31	24.3
Hours worked = 0	0.58	0.49	0.57	0.49	0.55	0.47	0.19	0.39
*Employment status * gender*								
Male * employed	0.66	0.48	0.56	0.5				
Male * unemployed	0.26	0.44	0.28	0.45				
Male * inactive	0.08	0.08	0.15	0.08				
Female * employed	0.36	0.48	0.33	0.36				
Female * unemployed	0.16	0.32	0.11	0.32				
Female * inactive	0.48	0.5	0.55	0.5				
Household characteristics								
Household size	5.58	2.51	5.12	2.61	4.96	1.88	4.35	2.12
Nominal household income	426.4	366.7	427.2	369.1	692.8	998.3	980.2	1130.2
Nominal per capita household income	84.5	81.3	98.4	95	156.5	237.9	279.9	363.2

(*Continued*)

Table 3. Continued

	Jefes Participants		Jefes Applicants		Eligible Heads or Spouses		Active Adults (Age 18–65)	
Characteristic	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Eligibility criteria for Jefes								
Household with children of head < 18 years or handicapped member	0.80	0.40	0.67	0.46	1		0.53	0.49
Household with any children < 18 years or disabled member	0.94	0.22	0.79	0.40			0.61	0.48
Individual is formal sector worker	0.03	0.18	0.06	0.23	0		0.39	0.48
Household with at least one formal sector worker	0.19	0.40	0.25	0.43	0.29	0.46	0.56	0.49
Eligible individual (children of head, individual not formal sector worker)	0.67	0.47	0.52	0.50	1		0.26	0.43
Eligible household (household with at least one eligible individual)	0.78	0.41	0.65	0.47	1		0.44	0.49
Number of observations	1222		679		5,273		11,401	

Source: Authors' calculations based on data from the October 2001 EPH.

TABLE 4. Distribution of Shocks: Actual and Simulated Changes in Real Household Income between October 2001 and October 2002

	Actual		Jefes, Assuming:			
Percentile	Jefes Applicants	Jefes Participants	Zero Forgone Income	One-Third Forgone Income	One-Half Forgone Income	Estimated Forgone Income
Household income						
1	−2,187.9	−1,688.5	−1,838.5	−1,788.5	−1,763.5	−1,759.8
5	−994.0	−1,004.6	−1,154.6	−1,104.6	−1,079.6	−1,127.9
10	−730.9	−647.7	−797.7	−747.7	−722.7	−756.7
25	−410.3	−306.7	−456.7	−406.7	−381.7	−398.2
Median	−168.2	−68.2	−218.2	−168.2	−143.2	−172.1
75	0.0	103.0	−47.0	3.0	28.0	22.1
90	123.3	252.7	102.7	152.7	177.7	169.7
95	280.0	364.5	214.5	264.5	289.5	272.8
99	500.0	685.7	535.7	585.7	610.7	564.1
Mean	−258.9	−151.7	−301.7	−251.7	−226.7	−242.1
St. Dev.	450.6	434.1	434.1	434.1	434.1	437.5
Individual income						
1	−775.2	−825.8	−975.8	−925.8	−900.8	−1,032.2
5	−446.1	−337.9	−487.9	−437.9	−412.9	−469.8
10	−318.2	−198.5	−348.5	−298.5	−273.5	−318.0
25	−139.4	−59.1	−209.1	−159.1	−134.1	−144.8
Median	0.0	140.0	−10.0	40.0	65.0	45.8
75	0.0	150.0	0.0	50.0	75.0	77.4
90	88.5	150.0	0.0	50.0	75.0	90.1
95	150.0	190.0	40.0	90.0	115.0	109.4
99	293.0	300.0	150.0	200.0	225.0	181.8
Mean	−77.3	28.8	−121.2	−71.2	−46.2	−61.2
St. Dev.	213.3	214.6	214.6	214.6	214.6	226.3

Note: The estimated forgone income is based on the single-difference estimates on individual income in October 2002; see discussion in text.

Source: Authors' calculations based on data from the October 2001 and 2002 EPH.

In the precrisis period, 15 percent of participants were in the lowest decile of the income distribution, 40 percent were in the poorest 20 percent, and 90 percent of participants were among the poorest 60 percent, which was about the official poverty rate at the time (table 5).[9] At the same time, unemployed heads of households with dependents were highly concentrated in the bottom 20 percent of the income distribution. Based on the eligibility criteria of the program's official aim (recall that this study uses a weaker definition, closer to what could be implemented in practice), the theoretically eligible population turns out to be quite narrowly defined at just 5 percent of the population at the baseline.

9. The location of Jefes participants and applicants in the national distribution of income (tables 4–6) is unaffected if the tabulations are based on income adjusted for adult equivalents rather than income per capita.

TABLE 5. Initial Location of Participants, Applicants, and Eligible Economically Active Adults in the National Distribution of Household Income by Decile in October 2001, Panel Sample (%)

	Jefes Participants		Jefes Applicants				
Decile	Households	Individuals	Households	Individuals	Eligible Households	Eligible Individuals	Theoretical Eligibility[a]
1	18.47	15.94	14.79	15.30	12.3	14.2	30.7
2	32.99	25.75	27.80	20.95	14.6	16.2	26.9
3	21.25	14.39	23.76	14.90	10.3	10.9	7.8
4	10.56	16.57	9.98	17.26	12.7	12.7	11.8
5	7.99	10.28	7.14	9.41	12.1	11.8	8.5
6	4.07	7.87	6.12	6.85	11.1	10.1	4.7
7	3.44	4.99	7.38	7.07	8.2	7.3	4.1
8	0.17	3.14	1.97	5.30	8.1	7.4	3.7
9	0.59	0.29	0.83	2.59	6.3	5.5	1.7
10	0.46	0.78	0.22	0.37	4.4	4.1	0.1

[a]Household with an unemployed head who has eligible dependents and who is the Jefes participant in the household.

Source: Authors' calculations based on data from the October 2001 EPH.

Only 12 percent of the sample of household heads with dependents (45 percent of heads) were unemployed as of October 2001.

In contrast to the theoretical target population, the practical eligibility criteria are quite broad and only slightly progressive (table 6). If perfectly enforced, these criteria would allow Jefes to reach about 50 percent of the poor at a poverty line of about 100 pesos.

Concentration curves showing the distribution of gains from the program selected using the cross-section samples show that Jefes is clearly not as well targeted as Trabajar (figure 2), consistent with expectations that the work requirement was not as tightly enforced in Jefes. This is true both for an assumption of zero forgone income and for the preferred estimates of forgone income (see table 6). However, spending on Jefes appears to be better targeted than other categories of social spending in Argentina. Gasparini (1999, quoted in World Bank 1999) estimates concentration curves for overall social spending that indicate that the poorest 20 percent of the population receive 22 percent of outlays (30 percent for the subcomponent of social services) and the next poorest 20 percent receive 20 percent (19 percent for social services).[10]

10. Analogously, the concentration curve shows that targeting performance is better for Jefes than for a median transfer program by international standards and by Latin American standards, as indicated by the results of Coady and others (2002). A median social assistance program in Latin America is 60 percent more progressive than a neutral allocation (compared with 25 percent of a median transfer program in developing economies). For comparison with other programs mentioned in Coady and others the benefit shares are 0.20 for the bottom decile, 0.47 for the bottom quintile, and 0.91 for the bottom two quintiles (from table 6, including estimated forgone income).

Table 6. Location of Trabajar and Jefes Participants in the Cross-Sectional Distribution of Income by Household Income by Decile (%)

Decile (Net of Transfer)	With Zero Forgone Income: Trabajar Participants 1997		With Zero Forgone Income: Jefes Participants 2002		Eligible Households	Eligible Individuals	With Estimated Forgone Income: Trabajar Participants 1997	With Estimated Forgone Income: Jefes Participants 2002	
	Households	Individuals	Households	Individuals			Individuals	Households	Individuals
1	58.2	60.1	28.9	29.0	13.5	14.4	48.1	3.6	19.5
2	17.5	18.5	23.2	23.5	11.3	13.6	27.7	41.0	27.4
3	9.9	9.5	18.6	18.6	12.2	13.7	13.5	27.7	20.2
4	6.8	5.8	12.9	13.1	12.6	12.7	7.3	13.9	15.2
5	2.2	1.9	8.9	8.5	11.7	10.8	1.7	8.7	9.1
6	2.5	1.6	5.1	4.9	11.0	10.2	1.7 (Deciles 6–10)	3.3	5.2
7	1.7	1.6	1.5	1.6	8.9	8.2		1.3	2.3
8	0.6	0.5	0.6	0.5	7.2	6.5		0.4	0.9
9	0.4	0.3	0.3	0.3	6.8	5.9		0.1	0.2
10	0.2	0.1	0.1	0.1	4.9	4.2		0.1	0.1

Source: Authors' calculations based on data from the 1997 Social Development Survey (EDS) for Trabajar, with zero forgone income and net gains estimates from Jalan and Ravallion (2003, tables 2 and 5); data from the October 2002 EPH for Jefes.

FIGURE 2. Concentration Curves for the Jefes and Trabajar Programs with No Forgone Income, October 2002

Note: Concentration curves for zero forgone individual income; see table 7.
Source: Authors' calculations based on data from the October 2002 EPH.

III. METHODS OF ASSESSING IMPACTS

Following common practice in the evaluation literature, *impact* is defined as the difference between the outcome indicator with the program and without the program.[11] Also following common practice, the estimate of the counterfactual is based on a matched comparison group of nonparticipants. As with all evaluations the reliability of this method depends crucially on whether the comparison group is sufficiently similar to participants in the absence of the program.

As a first step, individuals who have applied to the program but have not yet received assistance are selected for the comparison group. These applicants have already indicated a preference toward participation in the program (Angrist 1998). So to some extent, unobserved factors influencing participation (such as shocks associated with the crisis) are already revealed by the applicants.

However, latent heterogeneity between participants and applicants that can bias impact estimates cannot be ruled out. As noted, the applicants are less likely to

11. This evaluation focuses on short-run partial equilibrium effects of the program. A referee pointed out the potential underestimation of the impact of the program due to the possibility of feedback effects on the labor market. In a situation of high unemployment and with a cash transfer of only 150 pesos a month (well below the minimum wage), however, it seems unlikely that equilibrium wages were affected much by the program. A possible indirect effect might arise through changes in the search behavior of workers in the labor market: With no time limit on participation and to the extent that the work requirement is not binding for some groups, participants might become dependent on the scheme.

satisfy the eligibility criteria than are the current participants (see table 1). To control for observable heterogeneity, propensity matching techniques were used to construct a counterfactual outcome from the sample of applicants, in which D_i (an indicator of participation in Jefes) equals 1 if individual i participates and 0 otherwise. Following Rosenbaum and Rubin (1983), matching methods are used to estimate the outcome without the program by taking weighted averages over outcomes for individuals who did not participate and that are observationally similar to those of participants in terms of their propensity score, where $P(X_i) = \text{Prob}(D_i = 1|X_i)$ is the probability of participating conditional on observed (predetermined) covariates X_i.

This leaves the problem of selection bias due to unobservable characteristics. To reduce this bias, a subsample of panel households interviewed in the baseline (October 2001) and after the program (October 2002) was used to obtain a double difference (sometimes called difference-in-difference) impact estimator. This eliminates any time-invariant additive selection bias due to unobserved heterogeneity between participants and applicants. Matching in combination with double difference has been found to be effective in eliminating selection bias due to time-invariant omitted effects that might matter to participation (see, for example, Heckman and others 1997). The panel sample allows examination of how impact varies according to differences in baseline characteristics.

Data for October 2002 are available on N participants, indexed $i = 1, \cdots, N$ and C comparators, $j = 1, \cdots C$ in the region of common support, given by the set of propensity scores for which there is positive density for both treatment and comparison groups. Imposing common support means that inferences on the impact of the program can be confined to "comparable people" in terms of their propensity scores.[12] The smaller panel sample contains n and c individuals in the matched treatment and comparison groups. Let Y_{it}^k be the outcome of interest for individual i at time t in state k. There are two possible states for the outcome: $k = 1$ in the presence of the program, and $k = 0$ in its absence; there are two possible dates $t = 0$ (October 2001) and $t = 1$ (one year later, when program participation is observed). The evaluation problem of estimating the impact of any program stems from the impossibility of observing simultaneously both states for the same individual. Because nobody participates at the baseline, D_i is used to denote Jefes participation at $t = 1$. Note that $Y_{i0}^0 = Y_{i0}^1$ for all i.

The matched single-difference estimate of the mean impact is

$$SD = (1/N) \sum_{i=1}^{N} (Y_{i1}^1 - \sum_{j=1}^{C} W_{ij}^{sd} Y_{j1}^0) \tag{1}$$

12. Heckman and others (1997) show that failure to satisfy the common support condition is a major source of bias in nonexperimental evaluations.

where the W_{ij}^{sd} is the weight used in calculating the counterfactual for each participant. Local linear weights are used because they have been found to perform better at the boundaries of the scores, where the extent of the bias is greatest for conventional methods (Heckmanand others 1997). *SD* identifies the impact of the program in expectation if there is no selection bias; the condition for unbiasedness is that (dropping i subscripts): $E(Y_1^0|P(X), D = 1) = E(Y_1^0|P(X), D = 0)$, where the expectation is taken over the distribution of unobservables. The matched double difference is estimated on the matched panel sample and is given by:[13]

$$(2) \qquad DD = (1/n) \sum\nolimits_{i=1}^{n} [Y_{i1}^1 - Y_{i0}^1 - \sum\nolimits_{j=1}^{c} W_{ij}^{dd} (Y_{j1}^0 - Y_{j0}^0)]$$

This yields an unbiased estimate of impact if the selection bias is time invariant and additive: $E(Y_1^0 - Y_0^0|P(X), D = 1) = E(Y_1^0 - Y_0^0|P(X), D = 0)$.

IV. Impacts on Incomes and Employment

Two sets of probits were used for calibrating the propensity scores on the pooled sample of participants and current applicants, one for the October 2002 cross-section (used for the matched single-difference calculations) and one for the panel. Initial occupational status in 2001 (and type of occupation) is included in the estimation of the propensity score in the panel sample. Otherwise, the explanatory variables used are similar.[14]

The first thing to note is that the probits have low explanatory power for participation (table 7). The samples of participants and applicants are clearly quite similar ex ante in terms of observable characteristics. In a check of the sensitivity of the results for the panel sample to the inclusion of baseline household income, the variable was not significant, and its inclusion did not affect the subsequent estimates of the net gains from the program. Given the evident similarity of the Jefes participants and current applicants, it is not surprising to find in the propensity scores for treatment and comparison units for panel and cross-sectional samples a large region of common support in both the single-difference and the double-difference matching (figure 3).

Nonetheless, there are some significant covariates of participation. Jefes participation increases with age and is more likely for women, for households with a larger number of children below the age of 18, and for people who were public employees at the baseline (see table 7). Geographic effects are jointly significant.

13. Note that the set of weights in the single-difference and double-difference matching are not necessarily the same. In the panel sample, X also includes labor market status and occupation at baseline (October 2001).

14. The balancing of covariates in a regression framework as suggested by Smith and Todd (forthcoming) were tested by regressing each variable in the propensity score on a fourth-order polynomial of the propensity score and its interaction with D. We could not reject the null hypothesis that the covariates are balanced.

TABLE 7. Probits for Calibrating Propensity Scores for Jefes Participants and Applicants

	Cross-Section Oct 2002			Panel Oct 2001–Oct 2002	
	Coefficient	*t*-Statistic		Coefficient	*t*-Statistic
Ages 18–24	0.002	0.02	Ages 18–24	0.068	0.53
Ages 25–29	0.191	2.25	Ages 25–29	0.329	2.62
Ages 30–39	0.159	2.12	Ages 30–39	0.094	0.85
Ages 40–49	0.334	4.71	Ages 40–49	0.275	2.52
Male	−0.371	−6.89	Male	−0.544	−5.55
Head	0.012	0.17	Head	−0.022	−0.19
Spouse of head	−0.317	−3.8	Spouse of head	−0.323	−2.45
Single	−0.003	−0.04	Single	−0.041	−0.32
Married	0.144	1.89	Married	0.249	1.94
Incomplete primary	−0.045	−0.51	Incomplete primary	0.003	0.02
Complete primary	0.013	0.16	Complete primary	−0.092	−0.67
Incomplete secondary	0.021	0.27	Incomplete secondary	−0.089	−0.66
Complete secondary	0.002	0.02	Complete secondary	0.041	0.28
House, villa	0.130	1.21	House, villa	−0.089	−0.51
House, apartment	−0.109	−1.6	House, apartment	−0.028	−0.24
1 room house	−0.196	−2.07	Number of rooms	−0.023	−0.82
2 rooms	−0.072	−0.86	Bathroom	0.022	0.2
3 rooms	−0.130	−1.62	Renting house	−0.222	−1.8
4 rooms	−0.117	−1.39	Free renter	−0.404	−3.05
Bathroom	−0.034	−0.33	Walls, masonry	0.002	0.02
Renting house	−0.094	−1.34	Share of members ages 0–5	1.408	3.27
Free renter	−0.117	−1.63	Share of members ages 6–17	1.421	3.6
Walls, masonry	−0.011	−0.15	Share of members ages 18–64	0.468	1.29
Water, drain	0.082	0.89	Household size	0.010	0.64
Water, well	0.151	1.55	Unemployed	0.103	0.9
Water, tube	0.073	0.78	Inactive	−0.115	−1.05
Share of members ages 0–5	1.224	4.91	Public employee	0.533	2.49
Share of members ages 6–17	0.956	4.25	Teacher	0.333	1.25
Share of members ages 18–64	0.185	0.92	Social service	0.251	1.18
Household size	0.006	0.59	Manufacturing	0.087	0.53
Northwest region	−0.373	−4	Construction worker	0.218	1.49
Northeast region	−0.173	−1.79	Domestic worker	−0.145	−1.1
Cuyo region	−0.654	−6.19	Northwest region	−0.344	−2.4
Pampeana region	−0.027	−0.3	Northeast region	−0.185	−1.24
Patagonica region	−0.094	−0.88	Cuyo region	−0.615	−3.76
			Pampeana region	0.134	0.91
			Patagonica region	−0.212	−1.18
Number of observations	4,803		Number of observations	1,899	
Pseudo R^2	0.060		Pseudo R^2	0.0817	

Note: Dependent variable = 1 if individual participated in Jefes in October 2002 and 0 otherwise.

Source: Authors' calculations based on data from the October 2001 and 2002 EPH.

FIGURE 3. Overlapping Support in the Distribution of the Propensity Score for Jefes Participants and Applicants

Note: Histogram of propensity score distribution for Jefes participants (treated) and Jefes applicants (untreated); 28 (2 percent) of the participants are off the common support.

Note: Histogram of propensity score distribution for Jefes participants (treated) and Jefes applicants (untreated); 6 (0.2%) of the participants are off the common support.
Source: Authors' calculations based on data from the October 2002 EPH.

Single difference and double difference can now be calculated as given by equations 1 and 2, using these probits to estimate the propensity scores for matching. Table 8 gives the estimates for the program's impacts on incomes and employment, including both household and individual income gains for the Jefes participants.[15]

The mean impact estimates suggest that participants would have had a larger drop in real income in the absence of the program. The comparison group experienced a mean drop in real income of about 250 pesos per month over the year, whereas Jefes participants experienced a 150 peso decline. This suggests that Jefes acted as a partial safety net and attenuated the drop in income that would otherwise have been experienced. Net gains are on average between a half and two-thirds of the gross wage, depending on whether single-difference or double-difference matching is used. The single-difference method gives lower net gains from the program.

However, there is considerably greater imprecision in the double-difference estimates and in the household level single-difference estimates compared with the individual-based estimates. Indeed, for the double-difference estimate of the impact on household income, the 95 percent confidence interval includes 150, implying that the null hypothesis of zero forgone income cannot be rejected in this case.

A further indication of the high variance in the double-difference estimates is found in the household and individual-level impact estimates underlying the means in table 8. Although naturally there is great imprecision in the individual estimates of impact, studying the distribution of the estimates gives a useful indication of which estimation method is most plausible. Because participation is voluntary, it is plausible that the bulk of the income gains will be found in the interval (0, 150). It cannot be ruled out that some people might have given up a job earning more than 150 pesos a month to join Jefes and therefore have negative net gains (presumably because of differences in the disutility of work), but it seems unlikely. It seems equally unlikely that the net income gain would exceed the gross transfer payment under the program.

By this criterion, all but the individual single-difference estimates are implausible. For the double-difference estimates, 20 percent of the individual income gains are negative, and 60 percent exceed 150 pesos. For 30 percent of the sample, the double-difference estimates of household income gains are negative, whereas 54 percent exceed 150 pesos. For the single-difference estimates, half of the household income gains are negative and 30 percent are greater than 150. However, 83 percent of the individual single-difference estimates are in the interval (0, 150); only 5 percent of individual income gain estimates are negative, and 12 percent are greater than 150. The following discussion thus takes the individual single-difference results as the preferred estimates, although the

15. Real income is adjusted for regional differences in the cost of living. In the panel sample, real income figures are at base October 2002. (The annual inflation rate was 39.4 percent.)

TABLE 8. Average Impact of Jefes Program on Incomes and Employment, October 2002

	Household Income	Individual Income	Individual Employment	Individual Unemployment	Individual Inactivity	Total Hours Worked/Week
Cross-section (October 2002)						
$E(Y_1 \mid D = 1)$	438.3	172.9	0.86	0.04	0.10	20.6
$E(Y_1 \mid D = 0)$	357.1	83.7	0.37	0.30	0.33	11.4
Matched single difference						
$SD = E(Y_1 \mid D = 1) - E(Y_1 \mid D = 0)$	81.19 (16.0)	89.2 (5.27)	0.49 (0.02)	−0.26 (0.02)	−0.23 (0.02)	9.2 (0.8)
95% confidence interval	[63.8, 127.6]	[81.2, 101.9]	[0.45, 0.52]	[−0.29, −0.22]	[−0.27, −0.18]	[8.0, 11.4]
Panel (October 2001–October 2002)						
$E(Y_1 - Y_0 \mid D = 1)$	−147.2	30.2	0.42	−0.15	−0.27	6.4
$E(Y_1 - Y_0 \mid D = 0)$	−250.6	−83.6	−0.03	0.08	−0.04	−2.34
Matched double difference						
$DD = E(Y_1 - Y_0 \mid D = 1) = E(Y_1 - Y_0 \mid D = 0)$	103.41 (32.27)	113.55 (15.08)	0.46 (0.04)	−0.23 (0.04)	−0.23 (0.04)	8.9 (1.5)
95% confidence interval	[67.8, 195.9]	[78.5, 138.4]	[0.32, 0.49]	[−0.27, −0.09]	[−0.30, −0.15]	[5.8, 12.1]

Note: Numbers in parentheses are standard errors, bootstrapped with 100 repetitions. In the panel sample, real income figures are base 2002 (annual inflation rate of 39.4 percent).

Source: Authors' calculations based on data from the October 2001 and 2002 EPH.

double-difference results are also reported when they appear to contain insights that cannot be revealed by estimates based solely on the cross-sectional data.

With the constrained individual single-difference estimates as the most plausible, the mean forgone income is about 50 pesos a month, or a third of the Jefes payment. Although lower than the estimated forgone income of about half the program wage for the Trabajar program in Jalan and Ravallion (2003), this result for Jefes is unsurprising given the general decline in real wages due to the crisis (World Bank 2003); the opportunity cost of participation in workfare would undoubtedly have been lower in the wake of this crisis. Although the null hypothesis of zero forgone income for the double-difference estimate of the impact on household income cannot be rejected, this result is attributable to the considerably more noise in this estimator. The extent of forgone income and displaced hours suggests that the work requirement was having an impact. However, forgone income could also stem from side payments made to intermediaries (*punteros*) to participate. There have been anecdotal reports that such payments are typically around 50 pesos a month.[16] It seems unlikely that a majority of Jefes participants were obtaining access to the program through punteros.[17] So if the typical side payment is 50 pesos, then the estimate of 50 pesos a month in forgone income cannot be explained this way; the more plausible explanation is that there was an opportunity cost to the work requirement.

On average, about half of the participants gained work as a result of the program: Half of these workers were drawn from the ranks of the unemployed (women and men) and half from economic inactivity (mostly women). Moreover, on average Jefes participants increased their hours of work by about nine hours a week. In this respect, single-difference and double-difference results are similar. Overall, the results are suggestive of forgone income in that the net increase in hours worked is about half the Jefes stipulated work requirement of 20 hours a week.[18]

It is clear from these results that Jefes did not just displace unemployment. Indeed, roughly as many participants came from those who would otherwise not have been active in the workforce. This implies that assuming that all Jefes participants would have otherwise been unemployed would grossly overestimate the impact of the program on the rate of unemployment.

16. Such payments to intermediaries need not be interpreted as extortion. As the ethnographic literature suggests, there can be complex reciprocal links between such intermediary brokers and the poor. Auyero (2000) illustrates how the poor and marginalized members of society are drawn into problem-solving networks because of their limited access to formal sources of assistance.

17. In a random sample of Jefes beneficiaries, about 10 percent reported having registered for the program through punteros and 7 percent in similar groups (neighborhood associations or unions of unemployed people, *piqueteros*).

18. Some 78 percent of Jefes participants doing the work requirement reported exactly the legally required number of hours (20). It may be that municipalities, to generate work for a large number of participants, employed them for the minimum number of hours. It is also possible that some participants overreported their number of hours worked to accord with the legal requirements.

Estimates of the program's impact on labor market status can now be used to estimate the impact on the unemployment rate. The preferred single-difference estimates imply that 26 percent of Jefes participants would have been unemployed if not for the program, and 23 percent would have been economically inactive (see table 8). The counterfactual unemployment rates (and activity and employment rates) were also estimated and compared with INDEC's (2002c) estimates, which assumed that all Jefes participants would have been unemployed without the program (table 9).

Allowing for the behavioral responses implied by these results gives an appreciably lower impact on the unemployment rate. Although INDEC's calculation implies a 5.8 percentage point drop in the unemployment rate due to the program, the results here show an impact of 2.5 points. In contrast to the claims by INDEC and others (including World Bank 2003), Jefes was only partially responsible for bringing down the unemployment rate in the aftermath of the crisis. The results here indicate that the unemployment rate would have fallen between May 2002 and October 2002 even without the program.

Further exploration of the double-difference estimates of impacts shows that those attracted out of labor market inactivity were primarily women (table 10). There is no evidence of labor supply responses by other members of the household, other than the change of labor status of the beneficiary (the net gains in

TABLE 9. Impact of the Jefes Program on the Aggregate Unemployment Rate (%)

	Actual			October 2002 without Jefes Program	
	October 2001	May 2002	October 2002	INDEC (2002c) Calculations Assuming that Participants Would Be Otherwise Unemployed	Calculations Based on Estimated Net Gains[a]
Activity rate (share of total)	42.2	41.8	42.8	42.9	42.0
Employment rate (share of total)	34.5	32.8	35.2	32.7	33.5
Unemployment rate (share of economically active individuals)	18.3	21.5	17.8	23.6	20.3

Note: For comparability with previous EPHs, these calculations apply to 28 urban conglomerates (excluding Viedma, Rawson, and San-Nicolas, which were added in October 2002, and new areas added in Greater Buenos Aires). INDEC's definition of activity, employment, and unemployment rates are used.

[a]Estimated net gains on employment, unemployment, and inactivity from table 8, single-difference estimates. Let actual number of employed individuals be E_t, the number of unemployed be U_t, and J be the total number of Jefes participants in October 2002. Then the actual unemployment rate is $U_t/(U_t + E_t)$, and the counterfactual unemployment rate is $(U_t + 0.26J)/(U_t + E_t - 0.23J)$.

Source: Authors' calculations based on data from the October 2001 and 2002 EPH.

TABLE 10. Impact of Jefes Program on Labor Supply and Household Size, October 2002

Panel (October 2001–October 2002)	No. Women in Household			No. Men in Household			Household Size	Number of Children
	Employed	Unemployed	Inactive	Employed	Unemployed	Inactive		
$E(Y_{1t+1} - Y_{0t} \mid D = 1)$	0.39	−0.08	−0.27	0.04	−0.04	0.04	0.05	0.03
$E(Y_{0t+1} - Y_{0t} \mid D = 0)$	0.04	0.07	−0.06	−0.03	0.06	0.01	0.02	−0.01
Matched double difference								
$DD_{t,t+1} = E(Y_{1t+1} - Y_{0t} \mid D = 1) - E(Y_{0t+1} - Y_{0t} \mid D = 0)$	0.35 (0.05)	−0.15 (0.04)	−0.21 (0.05)	0.07 (0.04)	−0.10 (0.04)	0.03 (0.03)	0.03 (0.08)	0.04 (0.07)
95% confidence interval	[0.17, 0.39]	[−0.20, −0.03]	[−0.27, −0.06]	[−0.19, −0.02]	[−0.19, −0.02]	[−0.03, 0.10]	[−0.14, 0.18]	[−0.11, 0.18]

Note: Numbers in parentheses are standard errors, bootstrapped with 100 repetitions.

Source: Authors' calculations based on data from the October 2001 and 2002 EPH.

TABLE 11. Stratification of Net Gains from Jefes Program, Real Income Impacts, October 2002

	Cross-Section Single Difference			Panel Double Difference	
	Household Income Gain	Individual Income Gain	Constrained Individual Income Gain	Household Income Gain	Individual Income Gain
Whole sample	81.18 (6.1)	89.18 (1.5)	104.75 (0.64)	103.41 (12.5)	113.55 (6.3)
Participant is					
Male	32.5 (11.0)	114.6 (3.4)	115.3 (1.0)	67.5 (22.9)	8.2 (15.7)
Female	101.9 (7.3)	78.2 (1.5)	99.4 (0.8)	119.1 (14.9)	159.7 (5.5)
Head	−69.0 (5.6)	100.8 (3.0)	105.9 (1.0)	121.3 (16.6)	24.4 (13.0)
Spouse of head	53.5 (8.1)	81.6 (1.8)	103.1 (1.1)	135.5 (20.2)	176.7 (7.3)
Occupation at baseline					
Employed				11.9 (19.5)	−1.12 (11.2)
Unemployed or inactive				175.8 (15.7)	204.4 (4.5)

Note: Numbers in parentheses are sample standard errors, not bootstrapped.

Source: Authors' calculations based on data from the October 2001 and 2002 EPH.

numbers of employed, unemployed, and economically inactive people mirror the labor supply changes at the beneficiary level).

Nor are there signs that households responded by changing the household composition (for example, by sharing children) to gain access to the program. Household size rose slightly more among participants than among applicants, but the difference is not statistically significant. This is not surprising, because the results suggest that the program's eligibility criteria were not rigorously enforced. Moreover, households might not have been split up in practice, with parents claiming separate households only to gain access to the program.

Stratification of the net gains shows considerable heterogeneity in impact (table 11). Those who were unemployed or economically inactive before the program had no forgone income, so their income gain from the program is the gross wage. Those who were previously employed had high forgone incomes. Spouses of the head and women averaged larger net gains—not surprising because they were more likely to be drawn to the program from unemployment or inactivity than were men, for whom the opportunity cost of Jefes participation was clearly higher. Single-difference impact estimates for individual incomes constrained to be within the interval (0, 150) indicate that forgone income accounts for about one-third of the Jefes payment (see table 11).[19]

Using the preferred single-difference estimates, figure 4 gives the implied impacts on poverty among participants, as indicated by the cumulative density function (CDF) of income per person. The lower curve gives the observed (post-

19. In the constrained estimates, the individual net income from Jefes represents the main source of individual income for about half the sampled participants and the main source of household income for about a quarter of sampled participants.

FIGURE 4. Impacts on Poverty among Jefes Participants: Cumulative Distributions of Income Pre- and Postontervention

Source: Authors' calculations based on data from the October 2002 EPH.

intervention) CDF, and the upper curve gives the CDF implied by the estimates of the impact of Jefes at each sample point.[20] At a poverty line of around 100 pesos a month, the poverty rate among Jefes participants fell from about 82 percent to 70 percent due to the program. At a poverty line of around 50 pesos a month, the poverty rate among participants fell from about 51 percent to 29 percent.

The implied impacts of Jefes on the national poverty rate are shown in table 12 for the government's two official poverty lines. The impacts at the upper and lower poverty lines are negligible for both the official INDEC estimates of the counterfactual poverty incidence, which assume no forgone income, and the estimates using the preferred single-difference estimates of the net income gains. The decline is larger (2 percentage points) for the lower poverty line (indigence).

A further perspective on the ability of Jefes to reduce poverty can be obtained using the panel data to compare the actual joint distribution of income between poor and nonpoor households over time with the estimated counterfactual distribution for Jefes participants (following the methodology in Ravallion

20. This is the same counterfactual distribution used to calculate the counterfactual income shocks for the panel sample, as in the last column of table 4.

TABLE 12. Impact of the Jefes Program on Aggregate Poverty Rates, October 2002 (% below the poverty line)

		Counterfactual, in Absence of the Program	
	Actual, after the Program	INDEC Calculations (Subtracting Jefes Income from Total Household Income)	Calculations Based on Estimated Net Gains on Income[a]
Greater Buenos Aires			
Poverty			
Individuals	54.3	54.7	54.5
Households	42.3	42.6	42.5
Indigence[b]			
Individuals	24.7	27.0	26.2
Households	16.9	18.7	18.0
Total 31 conglomerates			
Poverty			
Individuals	57.5	58.1	57.9
Households	45.7	46.2	46.1
Indigence[b]			
Individuals	27.5	30.5	29.6
Households	19.5	21.9	21.1

Note: Income per adult equivalent is constructed using the adult equivalent scales provided by INDEC. For the analysis, new interview areas were excluded as well as households with partial income responses.

[a]Estimated net gains on income from table 7, single-difference individual estimates.

[b]Indigence is the food component of the poverty line.

Source: INDEC (2002b, 2003).

and others 1995). This distinguishes the extent to which the program prevents people from falling into poverty ("protection") from the extent to which it helps people escape poverty ("promotion").

The actual joint distribution shows that 20 percent of participants were not poor in 2001 but became poor in 2002, whereas only 2 percent of participants who were poor in 2001 escaped poverty by the following year (table 13). Some 71 percent were poor in both periods. Under the counterfactual distribution based on the preferred single-difference individual estimates of counterfactual incomes in October 2002, without Jefes 22 percent of participants who were not poor in 2001 would have become poor in 2002 and only 1 percent would have escaped poverty.

The impacts are greater at the lower poverty line. With Jefes, 30 percent were not indigent in 2001 but became so in 2002. In the counterfactual joint distribution without Jefes, 40 percent would have become indigent in 2002. Again, fewer people would have escaped poverty without the program (5 percent) than with it (8 percent).

The results confirm the social protection nature of the program. Jefes had a small impact in helping the participants escape poverty, but the results show an extra 10 percent of participants would have fallen into extreme poverty in the absence of the program.

TABLE 13. Measures of Protection and Promotion for Jefes Participants

	Nonpoor 2002	Poor 2002	
Official poverty line			
Actual joint distribution			
Nonpoor 2001	0.07	0.20	0.27
Poor 2001	0.02	0.71	0.73
	0.09	0.91	1
Counterfactual joint distribution[a]			
Nonpoor 2001	0.04	0.22	0.27
Poor 2001	0.01	0.72	0.73
	0.06	0.94	1
Official indigence line (food poverty line)			
Actual joint distribution			
Nonpoor 2001	0.39	0.30	0.68
Poor 2001	0.08	0.24	0.32
	0.47	0.53	1
Counterfactual joint distribution[a]			
Nonpoor 2001	0.28	0.40	0.68
Poor 2001	0.05	0.27	0.32
	0.33	0.67	1

Note: Income per adult equivalent is constructed using the adult equivalent scales.

[a]Estimated net gains for 2002 from table 8, single-difference individual estimates.

Source: Authors' calculations based on data from the October 2001 and 2002 EPH and equivalence scales, poverty, and indigence lines from INDEC (2002a,b, 2003).

What role did the work requirement play? Two observations suggest that the work requirement had an impact. In the October 2002 cross-section, 80 percent of sampled participants reported having done work for Jefes.[21] Evidence of forgone income is also consistent with a work requirement.

V. CONCLUSION

The Jefes program provided a basic cash transfer to all households satisfying certain eligibility criteria, and for about 80 percent of participants the transfer payment came with a work requirement. It is clear from the results, however, that the eligibility criteria were not rigorously enforced. About one-third of participants did not meet the eligibility criteria, and about three-fourths of

21. The data do not allow for a consistent definition of the type of activity undertaken by participants. The definition of what represents "work" depends on whether Jefes participation represents the main economic activity of the participants or not. Participants for which Jefes work is the main activity are defined to be doing a *contraprestaction* (complying with the work requirement) if they are working positive hours (among those, about half report working for the public sector, 30 percent for community service, and 8 percent for a private company). Participants for which Jefes is a secondary activity, self-report work as community service (32 percent), participation in training (41 percent), or school attendance (13 percent).

eligible adults were not receiving the program aid. The goal of targeting only unemployed heads of households with dependents was clearly not realized. The results suggest that a large share of participants were women who would not otherwise have been in the labor force. About half of the employment gain from the program came from the ranks of the unemployed and about half from the economically inactive population.

The program reduced Argentina's unemployment rate by an estimated 2.5 percentage points, which is less than half the size of previous estimates that assumed that all Jefes participants would have otherwise been unemployed. Jefes tended to have a positive opportunity cost for participants, consistent with the work requirement being binding for many participants. When forgone incomes are factored in, the program had a small effect on the overall poverty rate and a slightly larger impact on the incidence of extreme poverty. The program allowed an extra 2 percent of the population to afford the food component of Argentina's poverty line. A degree of protection from extreme poverty was also achieved: An estimated 10 percent more of the participants would have fallen below the food poverty line without the program.

It is not clear how much concern there should be about the extent of participation by people who were formally ineligible. There is evidence that unemployment was not the main factor bringing down living standards during the crisis—rather, it was the sharp fall in real wages (McKenzie 2004). In addition, the difficulty of effectively verifying the unemployment status of beneficiaries makes the eligibility requirement based on unemployment status unenforceable.

More effective for propoor targeting were eligibility criteria correlated with structural poverty, such as having dependents or living in households with no members in the formal labor market. Thus targeting performance was relatively good despite weak enforcement of the eligibility criteria. About half of Jefes participants came from the poorest fifth of Argentine families, and all but 10 percent fell below the official poverty line. This is better targeting performance than most social spending in Argentina, though it is not as good as in the Trabajar program. Jefes participants would have suffered an appreciably larger drop in their incomes without the program. Overall, the Jefes program does appear to have contributed to social protection during the crisis, despite the fact that its implementation differed from its design.

Appendix: Comparison with Administrative Records

Comparing the survey aggregates on Jefes participation for the EPH with the administrative records is complicated by the fact that the (urban) sample frame for the survey does not coincide with the (national) coverage of Jefes. This can be dealt with by confining the analysis of the administrative data to areas included in the EPH sample frame. Two ways of doing this were considered. The first used the administrative data only for municipalities included in the EPH sample frame, based on the location of participants' Jefes registration. In other words, only participants registered in the geographical areas where there is an overlap of municipalities with the sample frame of the EPH were considered. The second method restricted the administrative data to those who have their recorded domicile in the EPH sample frame. In other words, only participants whose residence is in the same conglomerate (according to the postal code) as where they receive their payment (*boca de pago*) were considered. This second method deals with cases in which people register in a nearby city in which they are not in fact resident.

The grossed-up estimate of Jefes participation represents about 80 percent of the registered applicants from the administrative data (table A.1). The aggregate from the administrative data is outside the 95 percent confidence interval of the survey-based estimate. When the aggregates are broken down by urban areas, the administrative count is outside the 95 percent confidence interval for 18 areas. These are all cases in which the survey estimate of participation is lower than the administrative data suggest.

TABLE A.1. Comparison of Survey-Based Participation Rates and Administrative Data

	Grossed-up Survey Estimates of No. Participants		Administrative Data	
Conglomerates	Point Estimate	95% Confidence Interval	Registered in Municipalities Covered by EPH	Domiciled in Municipalities Covered by EPH
Tucumán	30,454	[23,451 37,457]	38,829*	29,387
Tierra del Fuego	2,341	[1,494 3,188]	2,694	2,277
Santiago del Estero	11,813	[8,828 14,798]	23,404*	22,066*
Santa Cruz	1,378	[839 1,917]	1,584	1,362
San Luis	3,701	[2,496 4,906]	6,607*	6,361*
San Juan	12,053	[8,988 15,118]	21,131*	16,185*
Salta	23,592	[19,243 27,941]	31,948*	28,412*
Rio Negro	3,049	[2,028 4,070]	2,840	3,706
Neuquen	8,411	[6,469 10,353]	8,831	8,132
Misiones	11,164	[8,302 14,026]	11,997	10,337
Mendoza	20,001	[14,460 25,542]	31,686*	21,828
La Rioja	7,014	[5,671 8,357]	9,768*	8,751*
La Pampa	2,710	[1,832 3,588]	2,956	2,890
Jujuy	15,542	[11,996 19,088]	26,834*	25,718*
Formosa	16,865	[14,037 19,693]	21,513*	20,431*
Gran Parana	6,667	[4,872 8,462]	8,185	7,729
Concordia	5,155	[3,844 6,466]	7,861*	7,643*
Corrientes	16,325	[12,840 19,810]	27,111*	15,936
Rio Cuarto	4,455	[3,093 5,817]	6,503*	5,796
Gran Cordoba	53,380	[40,058 66,702]	48,067	46,317
C.Rivadavia	1,851	[1,049 2,653]	2,988*	2,735*
Rawson	4,310	[3,169 5,451]	4,467	4,018
Chaco	21,709	[16,060 27,358]	36,729*	34,082*

(*Continued*)

TABLE A.1. Continued

	Grossed-up Survey Estimates of No. Participants		Administrative Data	
Conglomerates	Point Estimate	95% Confidence Interval	Registered in Municipalities Covered by EPH	Domiciled in Municipalities Covered by EPH
Catamarca	10,955	[9,046 12,864]	14,879*	15,499*
Gran Rosario	56,920	[43,906 69,934]	79,361*	75,631*
Gran Santa Fe	23,628	[19,255 28,001]	29,513*	28,577*
Villa Consitucion	8,224	[5,276 11,172]	6,353	7,124
Capital Federal	27,008	[10,677 43,339]	55,437*	49,421*
Conurbano	379,009	[311,738 446,280]	418,018	369,349
La Plata	28,593	[21,184 36,002]	25,960	23,885
Bahía Blanca	6,375	[3,502 9,248]	5,244	5,367
Mar del Plata	16,754	[10,788 22,720]	16,789	15,706
Total urban areas	841,406	[767,394 915,418]	1,036,087*	922,658*

*Significantly different from that implied by the survey data.

Source: Calculations (kindly provided by the Ministry of Labor) are based on overlapping the database of liquidacion de beneficiarios and base de personas using data from the EPH and administrative data from the Jefes Program. Standard errors corrected for complex survey design were provided by INDEC.

As would be expected, switching to the residence-based assignment of Jefes participants to urban areas reduces the discrepancy. The tighter matching by residence puts the administrative data close to the upper bound of the 95 percent confidence interval in the aggregate, but it still leaves 14 areas for which the survey gives a significantly lower count.

The results suggest that the survey underrepresents Jefes participation. The source of the discrepancy is unclear. It could be respondent ignorance of Jefes participation or a desire to hide participation because of formal ineligibility. Alternatively, it might reflect overcounting in the administrative data. This could arise if there is some expropriation of the Jefes transfers for other purposes. However, once one allows for the residence-based assignment of participants, the discrepancy does not appear large enough to warrant serious concern about sampling bias in the EPH.

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On the Unequal Inequality of Poor Communities

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Communities differ in important ways in their needs, capacities, and circumstances. Because central governments are not able to discern these differences fully, they seek to achieve their policy objectives by relying on decentralized mechanisms that use local information. Household and individual characteristics within communities can also vary substantially. A growing body of theoretical literature suggests that inequality within communities can influence policy outcomes in ways that are either harmful or helpful, depending on the circumstances. Until recently, empirical investigations into the impact of inequality have been held back by a lack of systematic evidence on community-level inequality. This study uses household survey and population census data to estimate per capita consumption inequality within communities in three developing economies. It finds that communities vary markedly in their degree of inequality. It also shows that there should be no presumption that inequality is less severe in poor communities. The kind of community-level inequality estimates generated here can be used in designing and evaluating decentralized antipoverty programs.

Governments commonly implement decentralized antipoverty programs that are designed to distribute assets or cash to individuals or households. Usually, the central government distributes antipoverty funds to communities, which then decide how to allocate the funds. One example is social fund projects, a type of community-based development initiative in which poor communities identify projects, apply for funding, and design, implement, and manage their projects (Mansuri and Rao 2004).[1] These initiatives intend to improve poverty

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1. Mansuri and Rao (2004) distinguish community-based development from community-driven development, popularized by the World Bank, which refers to projects in which communities have direct control over key decisions as well as management of investment funds. *Community-based development* can be thought of as a broader term that accommodates but is not restricted to the World Bank's community-driven development concept.

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targeting and project implementation by using local information and inviting local participation. In practice, however, these potential benefits of local involvement may be outweighed by the possibility of resources being captured by local elites.[2] In a review of the community-based development approach, Mansuri and Rao (2003) argue that although potential gains are large, there are also important risks inherent in the basic precepts of the approach.

Uncertainty about the ultimate impact of such programs implies that a blanket application of a given approach in all communities may not be appropriate. Again, Mansuri and Rao (2004) caution against the wholesale scaling up of best practices identified in a few pilot settings, because the success of such pilot projects might depend crucially on local conditions that are not found elsewhere. Still, large projects such as a countrywide cash transfer or social fund program cannot take into account the full range of local characteristics that could possibly affect project performance. Hence, policymakers must confront the challenge of designing schemes that take critical local information into account but are not prohibitively costly to implement.

Governments have traditionally dealt with this problem by categorizing communities by easily observable characteristics and adapting schemes for each group. Lacking local-level data on poverty, government programs may draw on proxy indicators—believed to be correlated with local poverty conditions—to determine the eligibility of communities for various projects. But despite emerging theoretical analysis and empirical evidence that local inequality may also affect local development outcomes, such information has rarely made its way into program design. One reason is that estimates of local inequality have not been widely available until recently.[3] Another is that inequality may not be considered of primary importance when the target of an intervention is a small, poor community in a developing economy. The natural assumption is that where livelihoods are at the subsistence level there is little likelihood that well-being would vary much across households and individuals.

This article addresses both these issues. Applying a newly developed methodology, it estimates local-level welfare outcomes using the detailed information available from household surveys and the large-scale representation of the population census for Ecuador, Madagascar, and Mozambique. These techniques can be used to derive meaningful estimates of income or expenditure inequality for small areas for many countries, using readily available data. The article examines the importance of local-level inequality by decomposing national inequality in each country into a within-community and between-community component. This decomposition exercise produces a summary sta-

2. A vivid illustration of elite capture problems in practice and a theoretical treatment of this issue are provided in Platteau and Gaspart (2003).

3. McKenzie (2003) provides a recent attempt to proxy local inequality on the basis of easily observed correlates of household income.

tistic that masks significant heterogeneity in inequality across communities. The article provides additional evidence that this heterogeneity in inequality is evident even among poor rural communities. It demonstrates that information on local inequality can help program implementers further categorize communities after conditioning on local poverty and type of area.

I. How Can Local Inequality Affect Welfare Outcomes?

Mansuri and Rao (2004) present a comprehensive overview of the theoretical and empirical literature on the relationship between local inequality and development outcomes. Two critical issues emerge. How does inequality within a community influence the targeting impact of a particular project? How does local inequality affect collective action within communities?

Recent theoretical analysis suggests that inequality may affect targeting outcomes of social fund projects or antipoverty transfer schemes by reducing the relative power of the intended beneficiaries (Galasso and Ravallion forthcoming; Bardhan and Mookherjee 1999). In such cases, the advantage of such decentralized approaches to make use of better community-level information about priorities and the characteristics of residents could be offset by the possibility that the local governing body is controlled by elites, who may have different objectives than the poor within their communities.

Although the predictions from this theoretical work are ambiguous, limited empirical evidence shows that both the pros and the cons of decentralized decisionmaking are at work in various countries. Alderman (2002) finds that communities in Albania were able to improve targeting by using information unavailable to the central government. By contrast, Galasso and Ravallion (forthcoming) find that high levels of local inequality (as measured by landholding) were associated with worse targeting performance under the Food for Education program in villages in Bangladesh.

A detailed case study of the small north Indian village of Palanpur from the late 1950s through the early 1990s shows how local elites appropriated public resources and opportunities that were to be made available to the whole community (Drèze and others 1998). The study documents the introduction of 18 types of government-provided programs into the village, including a public works road-building program, free schooling, free basic health care, old-age pensions, a fair-price shop, and a farmer cooperative. The sobering diagnosis is that most of these programs were nonfunctional, particularly programs that had a redistributive component. Drèze and others argue that a key explanation for this dispiriting record is that village institutions were dominated by privileged groups and that only programs that enjoyed their backing were allowed to succeed. Drèze and others (1998, p. 211) see "little prospect of major improvement in the orientation and achievements of government intervention without a significant change in the balance of political power, both at the state and at the local level."[4] There is also a rich body of literature on the relationship between inequality and collective action, with implications for the provision of public goods, management of common pool resources, and group participation (Olson 1973; Balland and Platteau 1999, 2001, 2003; Dayton-Johnson and Bardhan 2002). This literature points to the possibility that some inequality may be necessary to mobilize the collective action needed for group provision of a public good. If a community is large and homogeneous, no single individual could make any significant difference in the provision of the public good, and so all would want to free-ride, resulting in no provision of the good.

Again, the theoretical relationship among inequality, participation, and collective action is complex. Most of the empirical evidence, however, seems to point to a negative or U-shaped relationship, with increased inequality leading, at least initially, to a decline in collective action (Dayton-Johnson 2000; Dayton-Johnson and Bardhan 2002; Khwaja 2002; Alesina and La Ferrara 2000; La Ferrara 2002).

The growing literature on the relationship between local inequality and development outcomes thus suggests several ways that local inequality could influence development efforts of the kind described in this article. The empirical literature, though still far from complete, suggests that on balance inequality is likely to hamper local development efforts. Incorporating information on inequality into the design of development efforts might therefore be necessary.

II. Data and Methodology

This study drew on data from both household surveys and population censuses in Ecuador, Madagascar, and Mozambique (see appendix table A.1 for details on data sources and coverage).

The construction of comprehensive "geographic profiles" of inequality across localities has been constrained by the limitations of conventional distributional data. Detailed household surveys, which include reasonable measures of income or consumption, are samples and thus are rarely representative or of sufficient size at low levels of disaggregation to yield statistically reliable estimates. Census data (or large sample surveys) of sufficient size to allow disaggregation either have no information about income or consumption or measure these variables poorly (see Alderman and others 2003).

This article uses a recently developed statistical procedure to combine data sources in a way that takes advantage of the detailed information available in

4. The review by Drèze and colleagues (1998) does not cover any specific community-based development projects in Palanpur. It is possible that performance of such projects might have been different. The review does indicate, however, that any notion that the villagers in Palanpur all have the same objectives, interests, and influence would be sorely mistaken. That villagers differed in economic well-being was clearly discernible in the study: income inequality within Palanpur was on the same order of magnitude as measures of inequality for India as a whole (Lanjouw and Stern 1998).

household sample surveys and the comprehensive coverage of censuses to estimate inequality at a level of disaggregation previously unattainable. The methodology is developed in detail by Elbers and others (2002, 2003a), and applications are described elsewhere (see Demombynes and others 2004; Elbers and others forthcoming; Mistiaen and others 2002), so only a brief description is provided here.[5]

A model of log per capita household expenditures, y, is estimated using the sample survey data, restricting the explanatory variables to those common to both the survey and the census or to those in a tertiary data set that can be linked to both of these data sets.[6] Then, the expected level of an indicator of poverty or inequality, W, is estimated given the census-based observable characteristics of the population of interest using parameter estimates from the first stage model of y. The same approach could be used with other household measures of well-being, such as assets, income, or employment.

The first-stage estimation is carried out using the household sample survey. The first concern is to develop an accurate empirical model of household consumption. Consider the following model:

$$\text{(1)} \qquad \ln\ y_{ch} = E[\ln\ y_{ch}|x_{ch}{}^{T}] + u_{ch} \approx x_{ch}{}^{T}\check{\boldsymbol{a}} + \eta_c + \varepsilon_{ch}$$

where household h is located in sample cluster c, and η and ε are uncorrelated with each other and with observables. This specification allows for an intracluster correlation in the disturbances. For any given disturbance variance, $\sigma_{ch}{}^2$, the greater the fraction due to the common component η_c, the less the benefit from aggregating over more households. Welfare estimates become less precise. Furthermore, failing to account for spatial correlation in the disturbances could bias the inequality estimates.

A Hausman test (described in Deaton 1997) is used to determine whether to estimate with household weights. The $\bar{R}^2$ is generally high, ranging from 0.45 and 0.77 in Ecuador to 0.29 to 0.63 in Madagascar and 0.27 to 0.55 in Mozambique. (For details see Elbers and others 2002, 2003a; Mistiaen and others 2002; Simler and Nhate 2002.)

Next, the variance of the idiosyncratic part of the disturbance, $\sigma_{\varepsilon,ch}{}^2$, is modeled. To model heteroscedasticity in the household-specific part of the residual, 5–20 variables, $\mathbf{z}_{ch}$, are chosen that best explain variation in $e_{ch}{}^2$ of all potential explanatory variables, their squares, and interactions.[7]

5. Elbers and others (2003a) is an earlier version of the present study and does not explore the potential relevance of local inequality to policy or the association between poverty and degree of inequality in communities.

6. As described in Elbers and others (2002, 2003a), a separate model is estimated for each stratum, rather than forcing the models and the parameter estimates to be the same for the whole country.

7. The number of explanatory variables is limited to avoid overfitting, and a bounded logistic functional form is used.

Finally, the distribution of h and e is determined using the cluster residuals $\bar{\eta}_c$ and standardized household residuals. Normal or *t* distributions are used with varying degrees of freedom, or the actual standardized residual distribution is used when a semiparametric approach is taken. The estimated variance-covariance matrix is used to obtain final generalized least squares estimates of the first-stage consumption model.

Welfare estimates are obtained from 100 simulations in each of the three countries. For each simulation a vector of simulated parameters is drawn from a multivariate normal distribution with variance-covariance matrix estimated in the survey-based consumption and heteroscedasticity regressions. In addition, disturbance terms at the cluster and household level are drawn from their standardized parametric or semiparametric distributions. These parameter and disturbance draws are then applied to the census-level regressors to predict per capita consumption. For the next simulation, a new set of parameters and disturbances is drawn and a new value of per capita consumption is calculated for each household. For a given locality a separate measure of inequality is calculated for each vector of simulated per capita consumption. The average across the 100 simulations yields the estimate of inequality for the locality, and the standard deviation of the inequality measures yields the estimate of the standard error.

III. Estimates of Local Inequality in Three Countries

This section presents the census-based estimates of inequality produced using the methodology described and compares them with estimates from household surveys at the level at which those surveys are representative.[8] If the methodology is applied with proper attention to data comparability, first-stage regression models, and the error structures used in simulating the inequality measures, then stratum-level estimates, virtually by construction, should correspond closely with those in the household survey. As such, these comparisons cannot be considered a test of the methodology. Clearly, however, if the census-based estimates are wildly different from the corresponding survey comparators, there would be little basis for proceeding further.

Estimated per capita consumption for each country from the household survey was compared with the census data at the stratum level (for which the household survey is representative). The results show that in nearly every case the hypothesis that estimates of average per capita consumption are the same across the two data sources (at the 95 percent confidence level) cannot be rejected (table 1). With few exceptions, point estimates match closely. Note that the standard errors of the per capita consumption estimates are often smaller for estimates based on census data than for those based on household survey data. Although the census estimates are predicted with error due mainly to the imprecision of the first-stage regressions, they are free of sampling error, making them more precise than estimates from the household survey.

8. For a similar analysis, focusing specifically on poverty, see Demombynes and others (2004).

Table 1. Comparison of Survey- and Census-Based Average per Capita Consumption Estimates at the Stratum Level in Ecuador, Madagascar, and Mozambique

Ecuador (Sucres per Capita)			Madagascar (Francs per Capita)			Mozambique (Meticais per Capita)		
Stratum	Survey	Census	Stratum	Survey	Census	Stratum	Survey	Census
Quito	126,098	125,702	Antananarivo	513,818	576,470	Niassa	4,660	5,512
	(11,344)	(8,026)	Urban	(48,455)	(23,944)		(355)	(484)
Sierra	121,797	122,415	Fianarantsoa	360,635	372,438	Cabo Delgado	6,392	6,586
Urban	(8,425)	(4,642)	Urban	(42,613)	(21,878)		(416)	(433)
Sierra	66,531	63,666	Toamasina	445,514	417,823	Nampula	5,315	5,547
Rural	(4,067)	(2,213)	Urban	(73,099)	(15,406)		(287)	(279)
Guayaquil	89,601	77,432	Mahajanga	613,867	580,775	Zambezia	5,090	5,316
	(5,597)	(2,508)	Urban	(74,092)	(31,025)		(208)	(274)
Costa	86,956	90,209	Toliara	343,111	321,602	Tete	3,848	4,404
Urban	(3,603)	(2,391)	Urban	(76,621)	(32,193)		(267)	(176)
Costa	57,619	61,618	Antsiranana	504,841	693,161	Manica	6,299	6,334
Rural	(4,477)	(2,894)	Urban	(46,148)	(93,437)		(741)	(527)
Oriente	110,064	174,529	Antananarivo	312,553	324,814	Sofala	3,218	4,497
Urban	(9,078)	(56,115)	Rural	(23,174)	(14,378)		(191)	(379)
Oriente	4,7072	59,549	Fianarantsoa	319,870	251,312	Inhambane	4,215	4,177
Rural	(4,420)	(3,051)	Rural	(45,215)	(18,091)		(359)	(134)
			Toamasina	275,943	279,239	Gaza	6,024	6,521
			Rural	(22,832)	(15,838)		(356)	(355)
			Mahajanga	325,872	321,398	Maputo Province	5,844	8,559
			Rural	(30,209)	(19,385)		(613)	(745)
			Toliara	233,801	259,537	Maputo City	8,321	11,442
			Rural	(22,174)	(16,222)		(701)	(4,956)
			Antsiranana	486,781	442,431			
			Rural	(91,181)	(54,869)			

Note: Numbers in parentheses are standard errors. All household survey estimates are computed using weights that are the product of household survey weights and household size. The census-based estimates are weighted by household size.

Source: Authors' calculations based on household survey and census data.

Comparing stratum-level estimates of inequality is less straightforward. Inequality measures tend to be sensitive to the tails in the distribution of expenditure. Since far-off portions of the tails are typically not observed in the survey (because of its small sample size), survey estimates of inequality will often be below the true level of inequality. Perhaps more important, nonresponse may be an issue in household surveys, and to the extent that nonresponse is more prevalent among rich households, the resulting selection bias will lead to further downward bias of survey-based estimates (see Mistiaen and Ravallion 2003). To the extent that a census suffers less from such problems of observation, and assuming that the expenditure model is correct, the expenditure of rich households will be better represented in the census-based estimates of inequality. These considerations lead to expectations of higher inequality estimates from census-based imputation.

Reflecting the complex sample design of the household survey for the survey-based estimates and the imputation procedure for the census-based estimates, standard errors are presented for all estimates of Gini coefficients (table 2). For Ecuador and Mozambique, the census-based estimates of consumption inequality tend to be higher than the survey-based estimates, although not generally to such an extent that one can reject the hypothesis that they are the same.[9] For some provinces in Mozambique, such as Sofala, Maputo Province, and Maputo City, the estimates from the census are not only higher than those in the survey but are also imprecisely estimated.[10]

In Madagascar the standard errors on the survey estimates of inequality are quite high. This serves as a reminder that although stratum-level estimates of welfare in household surveys are often referred to as representative, the sample size in these strata can be small, so the accompanying welfare estimates are not always very precise. Nonetheless, it is encouraging that the point estimates of the Gini coefficient from the survey and the census data in Madagascar are often quite close.

Elbers and others (2002, 2003a) demonstrate that standard errors on census-based estimates are inversely correlated with the size of the target population. Thus, although estimates of inequality may look good at the stratum level, they could become quite imprecise for smaller localities.

9. These issues are subjects of current research. If anything, the true difference between census-based and survey-based inequality estimates is expected to be even larger, because extreme draws of the error terms were ignored in the simulations underlying the poverty maps. Again, this might lead to under-representation of high-expenditure cases. To the extent that extreme draws of the error terms were not culled severely enough, census-based average consumption estimates would also be expected to exceed their survey-based counterparts. Mean per capita consumption, unlike the median, is directly affected by tails of the consumption distribution. A quick scrutiny of the consumption and inequality estimates for Mozambique suggests that trimming was possibly too light and that as a consequence both mean and inequality estimates are higher in the census than in the survey.

10. There is no evidence that the census-based estimates become even noisier at lower levels of aggregation in Mozambique.

Table 2. Comparison of Survey- and Census-Based Inequality Estimates (Gini Coefficients) at the Stratum Level in Ecaudor, Madagascar, and Mozambique

Ecuador			Madagascar			Mozambique		
Stratum	Survey	Census	Stratum	Survey	Census	Stratum	Survey	Census
Quito	0.490 (0.023)	0.465 (0.012)	Antananarivo Urban	0.492 (0.027)	0.469 (0.012)	Niassa	0.355 (0.020)	0.402 (0.025)
Sierra Urban	0.436 (0.020)	0.434 (0.011)	Fianarantsoa Urban	0.430 (0.038)	0.426 (0.015)	Cabo Delgado	0.370 (0.025)	0.413 (0.021)
Sierra Rural	0.393 (0.034)	0.457 (0.013)	Toamasina Urban	0.434 (0.042)	0.402 (0.015)	Nampula	0.391 (0.026)	0.400 (0.020)
Guayaquil	0.378 (0.014)	0.416 (0.011)	Mahajanga Urban	0.371 (0.027)	0.392 (0.016)	Zambezia	0.324 (0.017)	0.366 (0.012)
Costa Urban	0.359 (0.015)	0.382 (0.011)	Toliara Urban	0.514 (0.052)	0.504 (0.030)	Tete	0.346 (0.019)	0.394 (0.018)
Costa Rural	0.346 (0.036)	0.400 (0.015)	Antsiranana Urban	0.362 (0.025)	0.433 (0.039)	Manica	0.413 (0.036)	0.449 (0.020)
Oriente Urban	0.398 (0.035)	0.563 (0.104)	Antananarivo Rural	0.376 (0.023)	0.404 (0.015)	Sofala	0.405 (0.031)	0.529 (0.032)
Oriente Rural	0.431 (0.034)	0.478 (0.014)	Fianarantsoa Rural	0.470 (0.050)	0.437 (0.018)	Inhambane	0.382 (0.037)	0.398 (0.012)
			Toamasina Rural	0.352 (0.036)	0.362 (0.017)	Gaza	0.380 (0.024)	0.421 (0.023)
			Mahajanga Rural	0.320 (0.026)	0.306 (0.015)	Maputo Province	0.424 (0.029)	0.518 (0.029)
			Toliara Rural	0.383 (0.029)	0.377 (0.017)	Maputo City	0.444 (0.033)	0.560 (0.108)
			Antsiranana Rural	0.518 (0.110)	0.453 (0.048)			

Note: Numbers in parentheses are standard errors. All household survey estimates are computed using weights that are the product of household survey weights and household size. The census-based estimates are weighted by household size.

Source: Authors' calculations based on household survey and census data.

This study produced estimates of inequality at the third administrative level (the *firaisana* in Madagascar, the *parrqouia* in rural Ecuador, the administrative post in Mozambique). Does this imply that at such fine levels of disaggregation, these inequality estimates are too noisy to be useful? Elbers and others (forthcoming) document that standard errors correspond to about 5–15 percent of point estimates of inequality for these localities (see also later discussion)—the same range that is generally judged to be acceptable at the stratum level in household surveys. Elbers and others also show that the explanatory power of simple, descriptive ordinary least square regressions of inequality at the smallest administrative level on a set of simple community characteristics is quite high in these three countries (with an R^2 ranging between 0.57 and 0.78 in urban areas and between 0.38 and 0.57 in rural areas). They find that community-level inequality is typically higher in communities with large shares of the elderly, whereas in rural (but not urban) areas it is generally also positively correlated with total population.[11] If the inequality estimates produced with this methodology were just noise, such correlations and explanatory power would not be expected.[12]

From the evidence presented here, it can be concluded that the applied estimation technique can yield meaningful estimates of inequality for small areas. The next section turns to inequality decompositions by administrative units and the heterogeneity of inequality across communities.

IV. Decomposing Inequality by Geographic Subgroups

Decomposition of inequality by geographic subgroups in both developed and developing economies has a long tradition. The policy implications may be quite different when national inequality is attributable largely to differences in mean incomes across localities (between-group inequality) than when national inequality is basically an expression of heterogeneity that already exists at the local level (within-group inequality). To decompose inequality using the general entropy class of inequality measures, a class of measures that is particularly well suited for this exercise:[13]

11. A conditional correlation with estimated per capita consumption is also observed in rural areas, although evidence of a Kuznet's inverted U-curve is not particularly strong. Elbers and others (forthcoming) suggest that although the inequality measures included in these regressions have been estimated, this does not invalidate their use for these purposes (although they do advocate correcting standard errors for model error when estimated variables are included as regressors).

12. Demombynes and Özler (forthcoming) find evidence of a strong association between local inequality estimates produced on the basis of this methodology in South Africa and official crime statistics collected at the local level.

13. Following Bourguignon (1979), Shorrocks (1980), and Cowell (1980). Cowell (2000) provides a useful recent survey of methods of inequality measurement, including a discussion of the various approaches to subgroup decomposition. Sen and Foster (1997) and Kanbur (2000) discuss some of the difficulties in interpreting results from such decompositions.

$$
\begin{aligned}
GE_c &= [1/c(c-1)]\Sigma_i f_i[(y_i/\mu)^c - 1] \text{ for } c \neq 0, 1 \\
&= -\Sigma_i f_i \log(y_i/\grave{\imath}) \text{ for } c = 0 \\
&= \Sigma_i f_i(y_i/\grave{\imath}) \log(y_i/\grave{\imath}) \text{ for } c = 1
\end{aligned} \tag{2}
$$

where f_i is the population share of household i, y_i is per capita consumption of household i, μ is average per capita consumption, and c is a parameter to be selected by the user.[14] This class of inequality measures can be decomposed into between- and within-group components along the following lines:

$$
\begin{aligned}
GE_c &= [1/c(c-1)][\Sigma_j g_j(\mu_j/\mu)^c - 1] + \Sigma_j GE_j g_j(\mu_j/\mu)^c \text{ for } c \neq 0, 1 \\
GE_c &= [\Sigma_j g_j(\mu/\mu_j)^c - 1] + \Sigma_j GE_j g_j \text{ for } c = 0 \\
GE_c &= [\Sigma_j g_j(\mu_j/\mu) \log(\mu_j/\mu)] + \Sigma_j GE_j g_j(\mu_j/\mu) \text{ for } c = 1
\end{aligned} \tag{3}
$$

where j refers to subgroups, g_j refers to the population share of group j, and GE_j refers to inequality in group j. The between-group component of inequality is captured by the first term on the right. It can be interpreted as measuring the level of inequality in the population if everyone within the group had the same (the group-average) consumption level, μ_j. The second term on the right reflects within-group inequality, or the overall inequality level if there were no differences in mean consumption across groups but each group had its actual within-group inequality, GE_j. Ratios of the respective components with the overall inequality level provide a measure of the percentage contribution of between-group and within-group inequality to total inequality.

At one extreme, when inequality is measured at the national level, all inequality is by definition within groups. At the other extreme, when each individual household is taken as a separate group, the within-group contribution to overall inequality is zero and all inequality is between groups. But where does the between-group component start to outweigh the within-group component? Is it reasonable to suppose that at a sufficiently low level of disaggregation, such as the village or community, inequality within groups is small, and most of overall inequality is due to differences between groups?

The highest between-group inequality at the community level (measured as the mean log deviation, or $GE[0]$)[15] is observed in Ecuador, at about 41 percent (table 3). The share of inequality that can be attributed to mean expenditure differences between communities is much smaller in the other two countries—

14. Lower values of c are associated with greater sensitivity to inequality among the poor, and higher values of c place more weight to inequality among the rich. A c value of 1 yields the well known Theil entropy measure, a value of 0 provides the Theil L or mean log deviation, and a value of 2 is ordinally equivalent to the squared coefficient of variation.

15. Results remain virtually identical for other values of c.

TABLE 3. Decomposition of Inequality between and within Communities in Ecuador, Madagascar, and Mozambique

Level of Decomposition	Number of Subgroups	Within-Group Inequality (%)	Between-Group Inequality (%)
Ecuador			
All communities	1,579	58.8	41.2
Urban	664	76.7	23.3
Rural	915	85.9	14.1
Madagascar			
All communities	1,248	74.6	25.4
Urban	131	76.7	23.2
Rural	1,117	81.9	18.1
Mozambique			
All communities	424	78.0	22.0

Note: Communities are defined at the third administrative level (1,000–10,000 households): the *zona* (urban) and *parroquia* (rural) in Ecuador, the *firiasana* (commune) in Madagascar, and administrative posts in Mozambique.

Source: Authors' computations based on household survey and census data.

25 percent in Madagascar and 22 percent in Mozambique.[16] There is also evidence, particularly for Ecuador, that the observed between-group inequality is due mainly to differences between urban and rural communities. When attention is focused solely on rural communities in Ecuador, the between-group component of inequality falls to less than 15 percent of total inequality. In Madagascar the share of between-group inequality in rural areas is significantly lower (18 percent) than the combined share for rural and urban areas. In all three countries overall inequality is attributable mostly to inequality within communities, even when the community is defined as the lowest level of central government administrative unit.[17]

16. Elbers and others (2003a) show that the between-group share of inequality at higher levels of aggregation (first and second administrative levels) is below 10 percent in all countries other than urban Madagascar.

17. Inequality estimates produced on the basis of the methodology described in the article are averages calculated over a number of simulations (100 in our case). It is possible that a decomposition of inequality carried out after this averaging procedure has occurred overstates the within-group component of inequality because differences in inequality across communities have been smoothed out. To check this, the decomposition was carried out for each of the 100 simulations and then averaged across the decomposition results. The between-group component of inequality increased by at most 1–2 percent, and the qualitative results were unchanged. There is no other reason to suspect that the methodology for estimating local-level inequality is associated with any built-in tendency to overstate within-group inequality. One way to test this is to carry out the imputation exercise described here for a data set that also contains directly collected information on welfare and then to compare decomposition results on the basis of imputed welfare with those on the basis of observed welfare. Elbers and others (2003b) show for Brazil that a decomposition of inequality based on imputed consumption reaches virtually identical conclusions as a decomposition based on observed income.

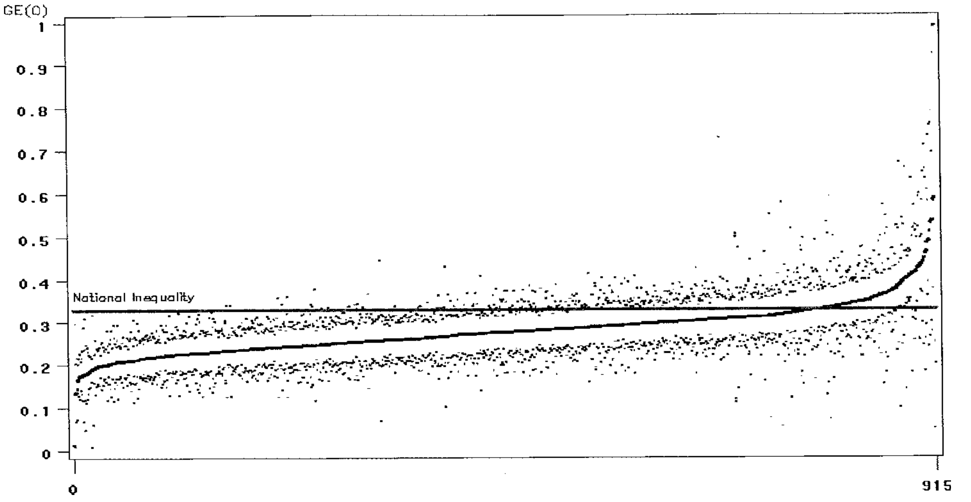
Interpretations of decompositions like these are not completely straightforward, however. For example, the decomposition results reported (documenting a large within-group component of inequality) do not imply that local inequality levels are uniformly high or even that the majority of communities exhibit high levels of inequality. Rather, the decomposition provides a summary statistic, suggesting that on average within-group inequality is not particularly low at the third administrative level. In other words, it is possible that a country has both highly equal and highly unequal communities.

A simple example can illustrate this. Consider a population of eight individuals for whom the vector of consumption values is (1, 1, 2, 2, 4, 4, 5, 5). This population could be divided into two communities of (1, 2, 4, 5) and (1, 2, 4, 5) or two of (1, 1, 5, 5) and (2, 2, 4, 4), both cases having the same average consumption. The between-group inequality component from a decomposition exercise such as that carried out is always zero (and the within-group share is thus 100 percent in both cases). However, in the first case inequality in the two communities is exactly equal to national inequality, whereas in the second case one community has a higher and the other a lower level of inequality than the national level. Thus, finding a high within-group share of inequality in a decomposition exercise of a large number of communities is consistent with great heterogeneity in inequality levels across those same communities.

The obvious question to ask, then, is whether communities vary widely in their degree of inequality. This can be answered by plotting community-level inequality estimates and comparing them with overall inequality. Communities in rural Ecuador are ranked from most equal to most unequal, and 95 percent confidence intervals on each community-level estimate are included as scatterplots (figure 1). Although the within-group share from the decomposition is as high as 86 percent, the summary statistic masks considerable variation in inequality levels across *parroquia*. A large majority of *parroquia*-level point estimates are well below the national level. Even allowing for the imprecision around the *parroquia*-level estimates (which are typically 5–15 percent of the point estimate), equality is unambiguously greater in a large proportion of *parroquias* than at the national level. Another sizable proportion is not obviously less or more unequal than the country as a whole, and a smaller proportion is considerably more unequal.[18] In urban Ecuador the proportion of *zonas* that have lower inequality than the national inequality rate is even higher than in rural areas (figure 2). The precision of point estimates is somewhat higher in urban areas of Ecuador than in rural areas; accordingly, more *zonas* lie

18. Note that there are more communities with inequality below the national level than above the national level because between-group inequality, although relatively small, is not absent. Differences in average per capita consumption ensure that at least some of total inequality is attributable to differences between groups. If there were no within-group inequality, or if all communities had the same level of within-group inequality, overall inequality would be greater than or equal to inequality in each of the individual communities.

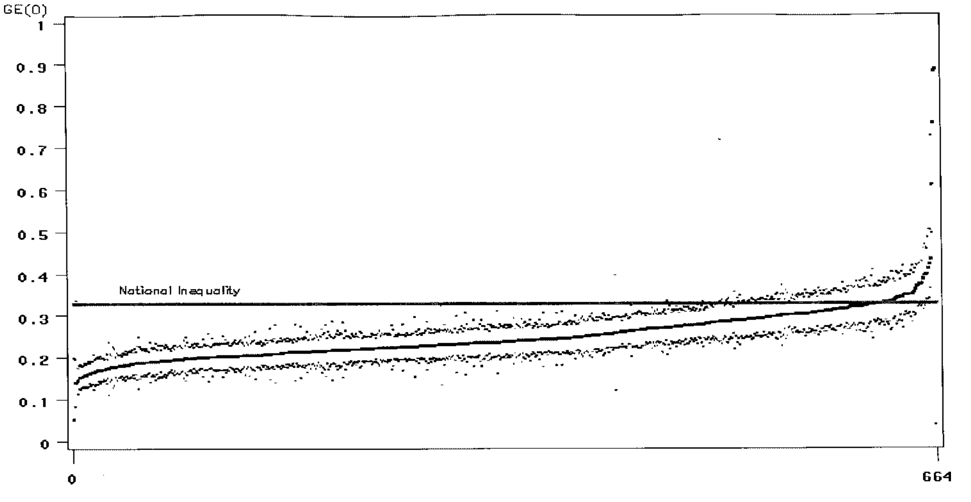
FIGURE 1. Distribution of *Parroquia*-Level *GE*(0) Inequality across *Parroquias* in Rural Ecuador



Note: The 915 *parroquias* are ranked from most equal to most unequal, and the average number of households per *parroquia* is 1,050. The scatterplot indicates the 95 percent confidence interval.

Source: Authors' calculations based on the 1994 National Survey of Living Conditions (ECV) and the 1990 population census.

FIGURE 2. Distribution of *Zona*-Level *GE*(0) Inequality across *Zonas* in Urban Ecuador



Note: The 664 *zonas* are ranked from most equal to most unequal. The average number of households per *zona* is 1,325. The scatterplot indicates the 95 percent confidence interval.

Source: Authors' calculations based on the 1994 ECV and the 1990 population census.

unambiguously below the national inequality level. The pattern is similar in rural and urban Madagascar and in Mozambique (not shown).

In all three countries there is a large group of communities with lower inequality than inequality in the country as a whole, another large group for which inequality is not significantly different from inequality in the country as a whole, and a small third group of communities with inequality higher than the national level.

V. Are Poor Communities "More Equal" Than Others?

Although most of the inequality in Ecuador, Madagascar, and Mozambique is attributable to inequality within communities, there is considerable heterogeneity in inequality across communities within each country. This section looks at whether inequality is less marked if the focus is on poor communities. Community-based development programs are often targeted primarily to poor communities. If the communities have low levels of inequality, it may be less important that policymakers incorporate information on inequality into the design and implementation of community-based development projects. That turns out not to be the case for the countries examined in this study.

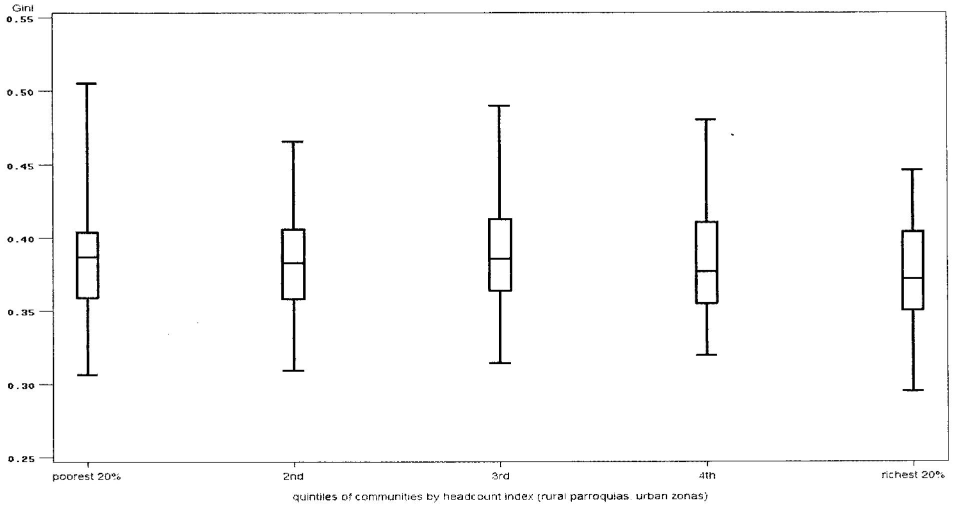
The Gini index at the community level by quintiles of the imputed headcount index (see Demombynes and others 2004)[19] ranges from 0.299 to 0.501 in Ecuador (figure 3), 0.231 to 0.466 in Madagascar (figure 4), and 0.261 to 0.534 in Mozambique (figure 5).[20] In all three countries median inequality in the poorest quintile is no lower than in any of the richer quintiles. Furthermore, the range of inequality levels across communities is among the widest in the poorest quintile—even when only rural communities are considered. Thus, inequality in a typical poor community in any of these three countries—even in rural areas—is at least as great as in other communities, and the range of inequality among poor communities is no narrower than for the country as a whole.

At the beginning of this article, reference was made to the literature suggesting that inequality at the local level has a bearing on the political economy of communities and in this way can affect the performance of community-based development initiatives. Another way of thinking about the implications of the finding is to consider how local inequality would undermine the effectiveness of policies to alleviate poverty through fine geographic targeting of transfers. In a parallel study Elbers and others (2004) illustrate that with relatively high

19. It is possible that high levels of inequality would be observed in high-poverty areas simply because these two measures of welfare are highly correlated. However, the results presented in this section are the same when communities are ranked by their mean consumption levels instead of the headcount index.

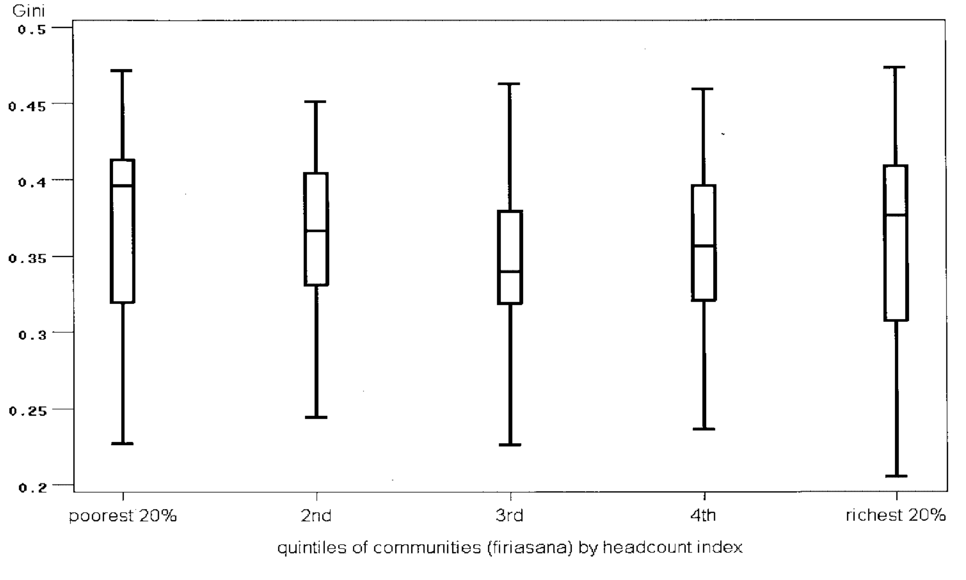
20. These reported ranges exclude the top and bottom 1 percent of communities (in terms of the Gini index) in each country.

FIGURE 3. Unequal Inequality of Communities in Ecuador



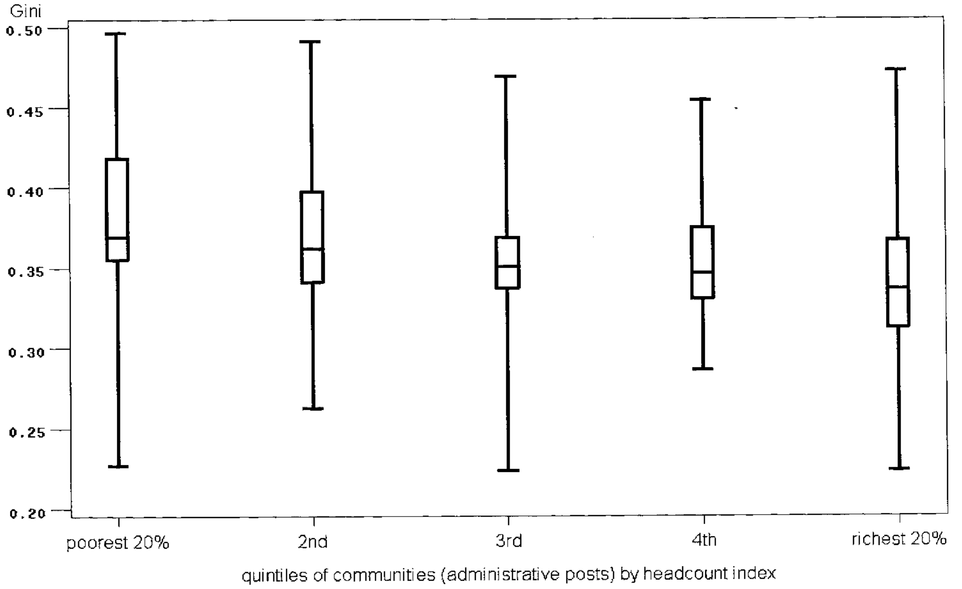
Note: The quintiles are based on the headcount poverty index of all communities (rural and urban), and the box-whisker percentiles shown are the median, 25/75 and 1/99.
Source: Authors' calculations based on the 1994 ECV and the 1990 population census.

FIGURE 4. Unequal Inequality of Communities in Madagascar



Note: The quintiles are based on the headcount poverty index of all communities (rural and urban), and the box-whisker percentiles shown are the median, 25/75 and 1/99.
Source: Authors' calculations based on the 1993/94 National Household Survey and the 1993 population census.

FIGURE 5. Unequal Inequality of Communities in Mozambique



Note: The quintiles are based on the headcount poverty index of all communities (rural and urban), and the box-whisker percentiles shown are the median, 25/75 and 1/99.

Source: Authors' calculations based on the 1996/97 National Household Survey on Living Conditions (IAF96) and the 1997 population census.

inequality in poor communities, even fine geographic targeting of communities would result in a considerable amount of mistargeting. An attempt to quantify such mistargeting reveals that in Ecuador targeting at the community level (and implicitly treating everyone within the commune as equally poor) would achieve at best only half the poverty reduction that would be attainable if the consumption level of each individual household had been observable.

VI. POLICY IMPLICATIONS

There has been a massive increase in resources devoted to community-based development programs in the past 10 years. The review by Mansuri and Rao (2004) suggests that funding for such projects rose from around \$325 million in 1996 to \$2 billion in 2003. Although the main goal is to achieve better outcomes by involving local communities in the decisionmaking process and management of projects, governments also need some basic indicators for targeting communities and tailoring basic features of these projects to different types of communities. So far, governments have commonly used type of area (urban or rural) and proxy information on poverty at the community level for such purposes.

This article proposes measures of inequality at the community level as possible additional indicators to inform the design of decentralized antipoverty programs and community-based development projects. Recent theory and limited empirical evidence suggest that inequality may be related to outcomes at the community level. Inequality at the community level may lead to the capture of the intended benefits by the local elite, or inequality may be highly correlated with another (not easily observed) factor that leads to capture by the elite. Collective action and the provision of public goods may also be correlated with the level of inequality within communities.

A recently developed small area estimation technique can provide estimates of inequality at the local level. In the three countries examined here, although most of the consumption inequality on average is attributable to inequality within communities, local inequality varies widely across communities. Furthermore, inequality is highly heterogeneous even in the poorest communities. Not only is inequality as high in a typical poor community as it is in other communities, but the range of inequality levels is at least as wide in poor communities as it is in richer communities. This finding remains true even when attention is restricted to rural areas.

These findings suggest that even after controlling for the type of area and the poverty levels of communities, local inequality measures can provide additional information that can enhance desired outcomes. For example, for transfer programs that expect local communities to identify poor beneficiaries, eligible communities could be categorized broadly as low, middle, and high inequality. Random audits and means-tested targeting by the central government (as, for example, in Mexico's Progresa program) could then be considered to improve propoor targeting in the middle- and high-inequality communities.

Clearly, a first priority is to undertake more systematic research into the relationship between local inequality and various development outcomes. Critical questions are the manner and extent to which current development processes and practices interact with local inequality.[21] Better estimates of consumption inequality at the local level using the techniques described here and related approaches promise important new insights. Microlevel estimation of welfare based on the methodology has been completed or is under way in some 25 developing economies. These estimates can be combined with detailed information on the operation of antipoverty programs and community-based development projects in these countries, with an eye toward uncovering systematic relationships, positive or negative.

21. An even more basic question concerns the relationship between community-level consumption or income inequality and the welfare inequality that is of ultimate interest. Dasgupta and Kanbur (2001) show how the presence of community-specific public goods could imply that the distribution of nominal income provides a very misleading picture of real inequalities and tensions in society, both between and within communities.

Appendix

Table A.1. Data Summary

Instrument	Ecuador	Madagascar	Mozambique
Household survey			
Year	1994	1993/94	1996/97
Source	National Survey of Living Conditions	National Household Survey	National Household Survey on Living Conditions
Sample size	4,500 households	4,508 households	8,250 households
References	Hentschel and Lanjouw (1996); Hentschel and others (2000)	Mistiaen and others (2002)	Simler and Nhate (2002)
Population census			
Year	1990	1993	1997
Coverage	About 10 million individuals in 2 million households	About 11.9 million individuals in 2.4 million households	About 16 million individuals in 3.6 million households

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Ghost Doctors: Absenteeism in Rural Bangladeshi Health Facilities

Nazmul Chaudhury and Jeffrey S. Hammer

Unannounced visits were made to health clinics in Bangladesh to determine what proportion of medical professionals were at their assigned post. Averaged over all job categories and types of facility, the absentee rate was 35 percent. The absentee rate for physicians was 40 percent at the larger clinics and 74 percent at the smaller subcenters with a single physician. Whether the medical provider lives near the health facility, the opportunity cost of the provider's time, road access, and rural electrification are highly correlated with the rate and pattern of absenteeism.

People frequently express dissatisfaction with the performance and quality of public services.[1] Service quality may be poor because not enough money is allocated or because the money is not spent effectively. This article quantifies one way in which public money may not be spent effectively—or at least not as originally intended. It reports on a study that conducted unannounced visits to health clinics in Bangladesh to determine what proportion of medical professionals were present at their assigned post.

Absenteeism of public servants has long been discussed as an impediment to effective delivery of public services. Glewwe and others (1999) found that teachers in one area of Kenya were absent from school 28 percent of the time and in school but absent from their classrooms an additional 12 percent of the time. A much publicized report on primary education in several states of India found absentee rates of 33 percent among head teachers and absentee rates so high among all teachers that teaching was occurring in less than half the schools visited (PROBE Team 1999). The report also notes gross misconduct among teachers who do show up for work, but emphasized absenteeism as the major

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1. See Filmer and others (2000, 2002) for ways that public spending on health may fall short of expectations.

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problem. Recent work in the state of Udaipur also found very high rates of absence from health centers (Banerjee and others 2004a,b).

In Bangladesh, too, absenteeism of workers in health clinics has frequently been identified as a problem reducing the effectiveness of public spending in health (Sen 1997; Begum and Sen 1997). Nowhere, however, have systematic efforts been made to quantify the extent of this problem on a nationally representative scale.[2] This gap was the main motivation for the current study.

Special concern arises over the staffing of facilities in rural areas because of both the benefits expected from public provision of health care in rural areas and the particular difficulties in achieving such provision. Although health care is a private good, being both excludable and rivalrous, public provision of health services can be justified on grounds that vary with the nature of the service. Considerations of equity and of correcting externalities associated with communicable disease (itself correlated with poverty; see Bonilla-Chacin and Hammer 1999) argue for extending services to rural areas where the incidence of poverty is much greater than in urban areas. In a country with as small an urban population as Bangladesh, the majority of poor people live in rural areas.

Two problems in particular are common to providing public services in rural areas. One is that many posts do not get filled because no one is willing to accept certain placements. The other is that even when the post is filled, the provider often fails to show up. Both situations compromise the ability to serve the poor, but with different welfare and budgetary implications. Unfilled posts reflect an absence of public medical care, but they do not absorb budget resources for salaries, though upkeep of the facility is still required. Absent personnel, on the other hand, still receive salaries.[3] A large proportion of expenditures in health and education are absorbed by wages. If public servants are not on the job, the expenditures embodied in salaries do not reach the intended beneficiaries. In the case of absenteeism, the welfare implication is unclear because services may be delivered by the absent personnel in their role as private providers. Assessing the welfare implications of public subsidies for the private provision of services is beyond the scope of this article. The analysis herein is purely positive, and no value judgments are made concerning the results.

Why is staffing rural clinics so difficult? One hypothesis is that most medical practitioners in developing economies are urban-born and reared, are highly educated compared with the population as a whole, and have skills that are

2. Various surveys of health facilities (Thomas and others 1996 report on one in Côte d'Ivoire) and schools (Schleicher and others 1995) have highlighted the problem of provider absenteeism. However, these surveys were not specifically designed to address this problem (the questionnaires were administered only to headmasters, without independent verification of information on individual teachers).

3. See Reinikka and Svensson (2001) for an example from Uganda. In other contexts leakages of central government expenditures for public services have been documented, with monies never reaching their intended recipients.

highly marketable. If they have children, they are likely to want the same advantages for them. These considerations are most applicable to physicians but are likely to be true of nurses and paramedics, if to a lesser extent. Chomitz and others (1998) find that in Indonesia inducing physicians to live in remote areas requires paying them a multiple of their current salary.

Because medical skills are marketable and greatly in demand, there is usually a ready opportunity to make money as a private provider outside (and sometimes inside) the public clinic, whether legal or not. So in addition to the problem of getting practitioners to serve in rural areas, there is the problem of getting them to forgo their private earning potential to provide services in the public facility. Many of the results reported here are consistent with stories that medical personnel have substantial discretion over where and when they discharge their public responsibilities—favoring rich areas and responding to earning ability in the private market.

Although there is a great deal more to learn about the reasons for incomplete attention to work responsibilities, this article starts by trying to get a general estimate of the extent of the problem. The survey conducted for this study can get at some of the correlates of staff absence, but the primary purpose is to focus on the overall magnitude of the phenomenon.

I. Methods

In rural areas the government of Bangladesh provides health services through a three-tier system. The 376 *upazila* (subdistrict) health complexes deliver inpatient services. They are managed by physicians[4] and staffed by nurses[5] (four years of training), medical officers/paramedics (minimum of three years of training), family welfare visitors and senior family welfare visitors[6] (minimum of 18 months of training), pharmacists,[7] and lab technicians. Some 1,000 upgraded union health and family welfare centers and rural health dispensaries are staffed by one physician, paramedics, and family welfare visitors. The government plans to increase the number of these facilities by posting more physicians and improving facilities. Finally, there are about 3,000 union health and family welfare centers managed by paramedics and family welfare visitors. Both types of union family welfare centers provide outpatient care.

4. *Upazila* health complexes have been staffed by medical doctors; it is rare for nonallopathic physicians (for example, homeopathic, *unani*) to be assigned to public health facilities. This policy is being revised, however, to bring nonallopathic physicians into the public health system.

5. Nurses are posted only at *upazila* health complexes (and hospitals).

6. Family welfare visitors are supposed to be involved primarily with reproductive health issues and public health programs, such as vaccination campaigns.

7. These pharmacists are not pharmacology school graduates, but rather have received about 18 months of training and earned a certificate.

The survey sample was made up of 150 health facilities. Sixty rural *upazilas* were selected at random from among the 376 in the country. The sole health complex in each *upazila* was included in the sample, and one union family welfare center was chosen randomly from each of the 60 *upazilas*. Finally, 30 upgraded union family welfare centers from the same areas were sampled, also at random but only from *upazilas* having at least one such facility. This strategy balanced the need for wide coverage to achieve national representation with the need to keep costs down—choosing facilities at different levels of the health system from the same subdistricts reduced the required travel for interviewers.[8]

The official opening and closing times of the health facilities are 9 AM and 3:30 PM. Each sample facility was visited by a team of trained investigators, who recorded the availability of doctors and paramedics at the facility once at approximately 9:30 AM and once no later than 2:30 PM. Between those times the team collected information on the facility and its providers. Facilities were not notified in advance of the visit. All facilities were visited between mid-March and mid-April 2002. Although there is no way to be sure that the results would be the same in other seasons, there were no major festivals in the period surveyed that might lead to higher than usual rates of absence, and it was not the monsoon season (which is in June and July). In any case, official rules of attendance do not differ by season.

Besides noting the presence or absence of medical practitioners, interviewers recorded information on key characteristics of the physicians, nurses, paramedics, family welfare visitors, lab technicians, and pharmacists. For practitioners who were present, information was collected on age, gender, education, professional training, location of residence, length of service, and duration of posting. For physicians who were absent in both the morning and the afternoon, information came from a variety of sources, including the statistical officer, *upazila* health complex administrators,[9] and other medical staff. Statistical officers in *upazila* health complexes usually maintain an updated profile of all medical staff (age, gender, years in service, duration of posting, residence). The statistical officer was present during visits to all 60 *upazila* health centers. When the only physician was absent in upgraded union family welfare centers, information on the physician was provided by the paramedic. Facility-specific information was also collected (for example, distance to *upazila* headquaters). Besides practitioner and facility information, secondary data were also collected on *upazila* characteristics (for example, percentage of households in the *upazila* with electricity).

8. There was a preliminary stratification of the subdistricts based on the presence of nongovernmental organizations, which is not discussed here and which did not affect any of the statistical results because very few areas were not covered by both government and nongovernmental organization facilities.

9. Union health and family planning officer (the chief administrator) and resident medical officer are the two highest ranking doctors at *upazila* health complexes.

TABLE 1. Vacancy Rates in Sampled Health Facilities in Bangladesh (%)

	Vacancy Rate (%)	
Division, Profession, and Facility Type	All Staff	Physicians Only
Total	26.2	
Division		
Barisal	30.3	
Chittagong	32.6	
Dhaka	20.1	
Khulna	25.2	
Rajshahi	26.7	
Sylhet	37.7	
Profession		
Physician	41.0	
Nurse	11.1	
Paramedic	17.6	
Senior family welfare visitor	20.4	
Family welfare visitor	4.9	
Pharmacist	46.0	
Type of facility		
Upazila health complex	23.7	41
Upgraded union family welfare center	24.0	44

Source: Public health facility surveys, March–April 2002.

II. DESCRIPTIVE RESULTS

This section looks at the basic descriptive results to identify problems of vacancy and absenteeism.

Which Posts Are Vacant?

Nationwide, the average number of vacancies over all types of providers is high, at 26 percent (table 1). Even without considering the problem of attendance, then, it already appears that providing public sector services is severely hampered by a lack of people willing to take such positions in the first place. The high average percentage of vacant posts is made worse by the wide variation across various dimensions. Regionally, the share of recent posts is generally higher in the poorer parts of the country. In the richer regions of Dhaka, Khulna, and Chittagong, 25 percent of posts for all types of providers are vacant, compared with 31 percent in the poorer regions of Syhet, Rajshahi, and Barisal.[10] The differential increases from 37 percent for the richer regions and 50 percent for the poorer when only physician vacancies are considered.

10. This split by income is based on commonly held notions of relative standards of living. Bangladesh does not have credible measures of income at subnational levels.

There is also substantial variation across types of medical practitioners. By far the greatest proportion of vacant posts are for physicians, pharmacists, and senior family welfare visitors. With the notable exception of paramedics and nurses, this pattern is consistent with the relatively high opportunity cost for being in public service and the related scarcity of such workers for public service.[11] Gender variation between professions may also play a part, a consideration explored later.

There are no substantial differences in number of vacant posts across different levels of facility, however. Both upgraded union family welfare centers and upazila health complexes have vacancy rates of 40–45 percent for doctors. The consequences of vacant positions in the two types of facilities are substantially different, however. A full complement of physicians in a *upazila* health complex is 7–9, whereas there is only one physician posted per upgraded union family health center. Although other available staff can fill in for vacant positions in the larger facilities, a physician vacancy in the upgraded union family welfare center means that there is no public physician for that village.

Who Shows Up for Work?

Practitioners in sanctioned and filled posts were considered absent all day when they were not at the facility during either of the two visits by the survey teams, absent a half day when they were at the facility during either the morning or the afternoon visit, and present when they were there at both times. Average absentee rates for any group of practitioners reflect the average of the values of 1, 0.5, and 0 for each practitioner in the group. So, an absentee rate of 50 percent could mean that all providers are absent for a half day or that half the providers are absent all day.

The reasons for absences were not explored at any depth, but others at the center were asked whether the absent person was on "deputation" to another post or to training. This category amounted to 2.7 percent of filled posts. Because the background variables of people on deputation more closely match those of other absent people than those of people who were present, people on deputation were considered absent in calculating the absentee rates.

Several clear patterns emerge for staff absences (table 2). For the entire sample of practitioners, the absentee rate is 35 percent. But variation around this figure is dramatic. Professionals are far more likely to be at work during the official morning hours than in the afternoon. This is not surprising: It is common knowledge that practitioners of all types use the afternoons to see private patients. A second clear result is that physicians have the highest rates of absenteeism—again consistent with the overall view that the opportunity cost of a practitioner's time is an important determinant of public service performance.

11. Until recently it was rare for nurses to be engaged in private practice in Bangladesh. However, with the emergence of private retirement homes, there is a growing demand for nurses in the private sector.

TABLE 2. Absentee Rates in Sampled Public Health Facilities in Bangladesh (%)

Variable	All Providers	Physicians Only
Total	35	42.2
Profession		na
Physicians	42.2	na
Nurses	27.3	na
Paramedics	25.0	na
Senior family welfare visitors	30.0	na
Family welfare visitors	32.0	na
Pharmacists	28.0	na
Type of facility		
Upazila health complex	34.4	40.4
Upgraded union family welfare center	37.4	74.0
Union family welfare center	35.3	na
Time of day		
Morning	15.5	22.7
Afternoon	54.4	61.7
Administrative division		
Dhaka	41.7	45.2
Chittagong	33.2	40.3
Khulna	26.5	31.8
Rajshahi	34.4	45.5
Sylhet	29.3	41.7
Barisal	29.0	40.0
Gender		
Male	34.5	40.6
Female	31.9	41.7
Place of residence		
Same as facility	28.0	33.0
Other	39.0	47.8

Note: na is not applicable. All differences between values of the variables are significant at the 5% level except for gender. As examples, the *F*-test between means of the different professions and the *t*-test between morning and afternoon show significant differences.

Source: Authors' calculations based on public health facility surveys, March–April 2002.

Curiously, although physicians are supposed to be at the clinic during the hours that the survey teams visited but were often absent, family welfare visitors are usually supposed to be visiting families as part of their outreach duties, but were rarely absent from the clinic. This gives some empirical credence to the general perception that even family health workers, who are recruited from the local community, are often reluctant to deliver services to poor households.

In contrast to vacancy rates, which are higher in poorer areas than in richer areas, there are no significant differences in absentee rates in poorer and richer areas. When absentee and vacancy rates are combined, however, the effective number of public service physicians in the poorer regions is much less than government norms would imply. With an overall official coverage rate of 20 physicians per 100,000 people based on sanctioned positions, the 41 percent vacancy rate from the sample implies a de facto coverage rate of 12 physicians per 100,000 people. Factoring in the absentee rate of 42 percent from the sample drops the de facto coverage rate to 8.4 physicians per 100,000 people.

A finding of potentially great policy significance is the difference in attendance rates between *upazila* health complexes and the upgraded union family welfare centers. The government recently proposed increasing the number of upgraded facilities by assigning a doctor to each of the ordinary union family welfare centers that normally do not have one. The absentee rate of physicians is 40 percent at the *upazila* health complex level but jumps to an astounding 74 percent in the upgraded union family welfare centers.[12] For both types of facilities the physician is expected to be on site during the clinic's hours of operation. The position does not include outside responsibility for home or community visits, for example, which could make the impact of this result ambiguous. The difficulty of keeping physicians in attendance in the relatively remote rural areas served by the upgraded union family welfare centers should lead policymakers to reexamine the proposed change.

Another strong finding is that practitioners who live within the facility compound or in the village where the facility is located are more likely to be at work at some time during the day than practitioners who live elsewhere. This finding is even clearer when the results are broken down into more specific categories and when separate absence rates are reported for half days and whole days (see table 2). For example, among physicians in *upazila* health complexes, 36 percent of those who live outside the town in which the facility is located did not show up for work at all during the day of the survey team visits, compared with 13 percent of those who live in the same town. Similarly, only 22 percent of those living in a different town were present at the facility all day, compared with 43 percent of those living in the same town. Although commuting time and costs would be expected to be a barrier to attendance, the magnitude of the difference is striking.

Finally, absentee rates differ across gender but in a way counter to expectations. Female physicians were absent 41.7 percent of the time and male physicians 40.6 percent of the time, though the difference was not statistically significant in a simple *t*-test of difference in means. This can be of policy importance because some women in rural Bangladesh are not allowed by their families to be treated by male doctors. Differential attendance makes the already skewed availability of medical services away from rural women that much worse. The full story of why female physicians are absent more than male physicians requires the closer examination that follows.

12. This absence rate was 80 percent during a second round of visits several months later, indicating that this was not a seasonal or idiosyncratic phenomenon.

TABLE 3. Matching Variables to Factors Influencing Costs and Benefits

	Opportunity Costs	Accessibility of Work	Sanctions for Absence	Internal Motivation
Profession (physician versus other)	✓		✓ (for professionals other than physicians)	✓
Length of time in profession	✓			✓
Duration of current posting	✓			✓
Gender	✓			✓
Lives in area		✓		✓
Type of facility	✓		✓	
Road within 1 kilometer of facility		✓		
Interaction of road and place of residence		✓		
Electricity in village			✓	
Literacy rate in village			✓	
Regional dummy variables	✓		✓	

Source: Authors' calculations based on public health facility surveys, March–April 2002.

III. EXPLORATORY MULTIVARIATE ANALYSIS

The main purpose of this research was to establish the magnitude of the problem of absenteeism. Because the survey is a single cross-section,[13] many questions related to identification and the direction of causality cannot be answered. However, a multivariate analysis was conducted to examine some basic partial correlations, to see what kinds of patterns emerge in the data. These results can establish a benchmark for replications of this type of study. Further studies can be designed with more attention to the determinants of practitioner behavior.

The variables used in the analysis were limited to those that could be easily collected and that were relevant to practitioners who were absent for the entire day at the time of the visits. Underlying these variables is the assumption that people will be present at their jobs if that is the best use of their time—they balance the costs and benefits of showing up for work each day. Because public service providers are paid on salary, there is no monetary incentive to go to work in the morning. Other factors that could influence the decision to go to work and for how long are the opportunity cost of practitioners' time, the actual costs (time, money, effort) of getting to work on any particular day, sanctions that can be expected for not showing up for work, and practitioners' sense of responsibility toward their job or the community they serve. The proxies used for these variables are listed in table 3

13. This study led to a much larger one done for the *World Development Report 2004: Making Services Work for Poor People* (World Bank 2004) that made repeated visits to both health facilities and primary schools in several countries, including Bangladesh and every major state in India. This research is currently under way, but preliminary results are available from the authors.

and matched to the underlying variables they represent. (For further discussion of this mapping of variables to concepts see Chaudhury and Hammer 2003.)

The analysis used a multinomial logit estimation procedure based on a random utility model of choice. The choices are among these alternatives: to be present at the assigned post for the whole day, to be present for a half day, and to be absent all day. This was not modeled with the three categories being ordered because the option of being present for a half day is not intermediate between the other two options in any relevant sense. As will be evident, the choice to be present part-time involves different considerations than those of being present full-time or completely absent.

Regression Results

The regressions were estimated (table 4), and the results were used to predict the probabilities of being absent for different combinations of independent vari-

TABLE 4. Multinomial Logit Regression of Absenteeism

	Physician Sample		Nonphysician Sample	
	Absent All Day	Absent Half Day	Absent All Day	Absent Half Day
Length of service	−0.050 (2.09)**	−0.006 (0.31)	−0.034 (0.82)	0.003 (0.16)
Duration of posting	0.057 (0.59)	0.048 (0.69)	−0.009 (0.17)	0.010 (0.43)
Female	−0.397 (0.81)	−0.402 (1.00)	0.563 (1.18)	0.188 (0.85)
Lives in area	−19.368 (51.69)***	−1.071 (0.53)	−2.423 (2.70)***	−0.660 (1.29)
Road $\leq$ 1 km	−0.274 (0.20)	−3.744 (2.58)***	−0.879 (1.43)	−1.273 (2.95)***
Lives in area and road $\leq$ 1 km	18.372 (.)	0.886 (0.43)	0.990 (0.93)	0.642 (1.13)
Upazila health complex	−0.870 (1.03)	3.540 (2.48)**	−2.634 (3.32)***	0.592 (2.25)**
Percent of households with electricity	−0.020 (1.10)	−0.017 (1.80)**	0.012 (1.07)	−0.007 (0.76)
Literacy rate	−0.002 (0.10)	−0.014 (0.85)	0.019 (0.71)	−0.002 (0.12)
Dhaka	0.097 (0.14)	2.088 (2.86)***	1.750 (1.54)	1.019 (2.13)**
Chittagong	−0.151 (0.20)	0.717 (0.95)	1.322 (1.11)	0.419 (0.85)
Khulna	−1.310 (1.45)	0.999 (1.31)	0.196 (0.15)	0.230 (0.45)
Sylhet	0.280 (0.26)	1.008 (1.01)	1.126 (0.72)	−0.585 (0.74)
Rajshahi	−0.037 (0.05)	1.355 (1.85)	0.736 (0.63)	0.158 (0.34)
Constant	1.680 (1.03)	−0.199 (0.13)	−1.691 (0.93)	−0.029 (0.03)
Observations	321	321	397	397
Pseudo R^2 (joint estimation)	0.10			0.10
Log likelihood	−298.4			−317.9
H_0: χ^2 (10) division effects = 0 {joint estimation}	22.30***		14.9	

*Significant at 10 percent level; **significant at 5 percent level; ***significant at 1 percent level.

Note: Numbers in parentheses are absolute value of z statistics. Reference choice category is "Present all day." Left out division is Barisal.

Source: Author's calculations based on public health facility surveys, March-April 2002.

TABLE 5. Average Predicted Probabilities from Multinomial Logit Regression of Absenteeism: Physician Sample (%)

Variable	Absent All Day	Absent Half Day	Present All Day
9 years experience	24.9	35.6	39.5
21 years experience	17.0	37.6	45.4
Female	19.0	32.9	48.1
Male	22.2	38.2	39.6
Lives in area	12.8	37.3	49.9
Doesn't live in area	29.4	35.5	35.1
Road ≤ 1 km	24.3	31.5	44.2
Road ≤ 1 km	10.0	80.2	9.8
Lives in area and road ≤ 1 km	17.3	32.3	50.4
Doesn't live in area and road ≤ 1 km	31.8	30.5	37.7
Lives in area and road ≤ 1 km	0.0	77.3	22.7
Doesn't live in area and road ≤ 1 km	13.0	78.9	8.1
Upazila health complex	13.6	50.3	36.1
Upgraded union family welfare center	42.3	5.7	52.0
Dhaka	19.7	50.3	30.0
Chittagong	24.3	25.7	50.0
Khulna	9.7	35.0	55.3
Sylhet	29.9	28.2	41.9
Rajshahi	22.6	36.6	40.8
Barisal	29.9	15.2	54.9

Source: Author's calculations based on public health facility surveys, March–April 2002.

ables, other variables being kept at their mean values (tables 5 and 6). This was done because the magnitude of the regression coefficients in a multinomial probit regression are difficult to interpret directly. Variables with statistically significant results are discussed next.

PROFESSION. Results of separate regressions for the subsample of physicians and the subsample of other practitioners are generally consistent with the differences in means for overall absenteeism presented earlier (see table 2). The impact of variables included in the regression are much stronger for the physician subsample than for the other practitioners subsample in both magnitude of effect and statistical significance. This may simply reflect the heterogeneity of professions within the other practitioners subsample.

TYPE OF FACILITY. Workers in the higher-level *upazila* health complexes are much more likely than workers in other facilities to be absent for a half day. The probability of half-day absence rates for physicians are 50.3 percent in the *upazila* health complexes but just 5.7 percent in upgraded union family welfare centers. For other practitioners, the probabilities are 46.7 percent in *upazila* health complexes and 28.4 percent in upgraded union family welfare centers. Working in a *upazila* health complex is more prestigious than working in a lower-level facility and so can contribute to higher outside earnings from

TABLE 6. Average Predicted Probabilities from Multinomial Logit Regression of Absenteeism: Nonphysician Sample (%)

Variable	Absent All Day	Absent Half Day	Present All Day
9 years experience	6.0	40.4	53.6
21 years experience	4.3	42.0	53.7
Female	6.3	43.4	50.3
Male	4.3	40.1	55.6
Lives in area	2.2	41.3	56.5
Doesn't live in area	8.5	40.5	51.0
Road $\leq$ 1 km	5.0	38.6	56.4
Road $\leq$ 1 km	6.0	65.5	28.5
Lives in area and road $\leq$ 1 km	2.5	39.3	58.2
Doesn't live in area and road $\leq$ 1 km	8.0	37.7	54.3
Lives in area and road $\leq$ 1 km	1.7	54.1	44.2
Doesn't live in area and road $\leq$ 1 km	9.4	63.5	27.1
Upazila health complex	1.1	46.7	52.2
Upgraded union family welfare center	14.8	28.4	56.8
Dhaka	6.9	53.4	39.7
Chittagong	6.1	40.1	53.8
Khulna	2.6	37.4	60.0
Sylhet	6.8	20.4	72.8
Rajshahi	4.2	35.1	60.7
Barisal	2.3	32.4	65.3

Source: Author's calculations based on public health facility surveys, March–April 2002.

self-referrals (requiring half-day absences), particularly for physicians.[14] But the larger facilities also have more people to monitor absenteeism. It is much more unusual for nonphysician practitioners to be absent all day than half a day, which makes sense if being absent all day is considered a worse offense than being absent for half a day.

EXPERIENCE. Years of experience but not tenure at the current post was significant and only in the physician regression. The number of years of experience is correlated with being present all day (and negatively and significantly correlated with being absent all day or half a day). This seems surprising because more experienced physicians may have more established private practices. But experienced people who are still working in these kinds of facilities tend to be either less successful or particularly dedicated to providing primary care. These are not prestigious jobs for physicians (even the *upazila* health complex positions), and experienced physicians prefer to practice in well-equipped hospitals or administrative posts in urban areas.

VILLAGE ELECTRIFICATION. Electrification and village literacy rates (which never appear significant) are the only measures of income of the village, crude as they

14. See Chawla (1996) for a discussion of self-referral from public to private practice in India.

are. Electrification tends to increase the probability of all-day attendance for physicians (significantly reducing half-day absences and reducing, but not significantly, full-day absences). If electricity is measuring wealth, it may reflect more pressure from the community. Working conditions should not be directly affected since facilities have their own generators.

ACCESSIBILITY: PLACE OF RESIDENCE AND ROADS. As in the bivariate analysis, living in the village is strongly correlated with being present at official posts for at least part of the day. The correlation is much stronger for physicians than for other practitioners and stronger for full-day than for half-day absence. Physicians can have a private practice in the same village in which the public facility in which they serve is located, so living nearby is not necessarily an impediment to outside earnings. The effect, at least for physicians, is so strong that the decision to live nearby is examined later in greater detail.

Having a road within a kilometer of the facility is highly correlated with being present for a half day, and the correlation is much stronger for physicians than for others. Lack of a road is correlated with the highest rate of half-day absence for all providers, a finding consistent with needing a lot of time to get to work. Roads are also correlated with whether providers live near the facility (discussed later).

The interaction effects between roads and place of residence yield some interesting results. Although the *t*-statistics for the interaction variables in each equation are individually insignificant, their joint effect on the pair of equations (whole- and half-day absences) is significant at the 5 percent level. The sign is consistently positive, indicating that the presence of a convenient road reduces the correlation of living outside of town with absenteeism.

When there is no road, the difference in attendance rates between practitioners other than physicians who live in town and those who do not is 17.1 percentage points (44.2–27.1 percent). With a road, the difference falls to 3.9 percentage points (58.2–54.3 percent), suggesting that ease of access is a partial substitute for proximity of residence. For physicians the story is a little different: Physicians who live in the village where they work are never absent all day when the road is far away (see table 6).

REGIONAL EFFECTS. Regional variations do not appear very important except that working in Dhaka increases the probability of being absent for half a day but not a whole day. Dhaka is the most lucrative market and would exert the most pull for private practice. Why only for a half day rather than all day could be because of opportunities for self-referral or because of the proximity of supervisors, increasing the probability of being discovered away from one's post.[15]

15. Readers familiar with Bangladesh may find this discussion refreshingly naive because there isn't really much shame or probability of formal sanctions associated with being absent, but we mention it as a possibility anyway.

DOGS THAT DIDN'T BARK. Some variables that were expected to be highly correlated with absenteeism were not. Gender was one. In other contexts, women are often found to be less corrupt and otherwise more rule abiding (Dollar and others 2001; Swamy and others 2001). Any effect may be attenuated by place of residence (examined later). Literacy in the village also had no effect—it is not a good measure of public pressure, public pressure may have no influence, or it may be too strongly correlated with electrification. Finally, duration of tenure at the current posting is unrelated, either because it has opposing effects—long residence may increase private opportunities but also increase sense of responsibility to the community—or simply because it is unrelated.

Correlates with Place of Residence

In taking a closer look at who chooses to live near their assigned facility (because living nearby is closely correlated with absenteeism), the cross-section structure of the survey is an impediment because this decision may be jointly determined with the expectation of how often one plans to be at the public service job. In a sense, there are two types of decisions about how to allocate time. One is made more or less annually—where to live—and one is made daily—conditional on where you live, how do you allocate your time during the day? For now, the decision to live in an area will be considered to have been made prior to the decision to go to work each day, and the simultaneous effects that expectations about the second might have on the first will be ignored.

The main results, perhaps not surprisingly, are related to characteristics of the town or village (table 7). The proportion of households with electricity is a strong encouragement for practitioners to live in the village. This may be due to electricity being a proxy for wealth, or it may be due to the direct benefits of living with access to electricity.

A road being more than a kilometer away from the facility (and the village) is associated with a lower likelihood that the practitioner lives in the village, as would be expected of an indicator of the quality of life. Roads, therefore, are not only related to absenteeism directly but also through their correlation with residential location.

Two variables distinguish physicians from the other providers. First, whether the facility is an *upazila* health complex influences physicians but not other practitioners. It appears that physicians will not live in villages far from *upazila* headquarters, whereas other practitioners are just as likely to live by a union family welfare center as by a larger facility. Second, the probability that a female physician will live in the town or village of her assigned health care facility is very low. An educated woman in Bangladesh will almost certainly be married (all but 4 of the 42 female physicians in the sample were married) to an educated man with a career, and they will likely live near his job.[16] This result explains

16. If both the husband and wife are physicians, then priority is supposed to be given for assignment to the same health facility. Information on spouses was not collected, however.

TABLE 7. Probit Regression of Choice of Residence (marginal probabilities)

	Physician Sample	Nonphysician Sample
Length of service	0.005 (1.17)	−0.005 (1.10)
Duration of posting	−0.001 (0.03)	0.008 (1.36)
Female	−0.168 (1.90)*	0.001 (0.02)
Road $\leq$ 1 km	0.217 (1.32)	0.100 (1.36)
Upazila health complex	0.390 (2.08)**	−0.023 (0.38)
Percentage of households with electricity	0.007 (2.48)**	0.001 (0.50)
Literacy rate	0.005 (1.21)	−0.000 (0.10)
Dhaka	−0.194 (1.43)	0.373 (3.41)***
Chittagong	−0.046 (0.32)	0.383 (3.50)***
Khulna	−0.099 (0.66)	0.155 (1.24)
Sylhet	0.142 (0.65)	0.392 (2.81)***
Rajshahi	−0.281 (2.11)**	0.280 (2.48)**
Number of observations	321	397
Pseudo R^2 (joint estimation)	0.10	0.06
Log likelihood	−199.3	−259.5
H_0: χ^2 (10) division effects = 0	16.56***	22.7***

*Significant at 10 percent level; **significant at 5 percent level; ***significant at 1 percent level.

Note: Numbers in parentheses are absolute value of *z*-statistics. Dependent variable takes a value of 1 if provider lives in the area and 0 otherwise. Left out division is Barisal.

Source: Author's calculations based on public health facility surveys, March–April 2002.

the unexpected relationship between low attendance and being female. Conditional on residential location, there is no difference between men and women as far as attendance is concerned (though this, too, was somewhat unexpected).

What Difference Does Absenteeism Make?

Does the absence of medical practitioners from their public posts have measurable consequences on other outcomes one might care about? It cannot be claimed that the observed absentee rates necessarily imply adverse welfare effects relative to full attendance. Being on the public payroll may be necessary to induce professionals to locate outside of metropolitan areas. Although their services are not provided free in the public facilities, they reach areas that would otherwise not be served. The real world does not play by the rules, but it might be that the rules are unrealistic and unduly restrictive. The data collected for this study do not include any information from households that would help assess the overall availability of medical care by geographic area. Nor is it possible to assess the full welfare implications of the results empirically.[17]

However, such high rates of absenteeism give rise to a strong presumption that something is wrong. The survey collected information that will permit examination

17. Current work by Banerjee and colleagues (2001) in the Udaipur district of Rajasthan state in India is designed to better address welfare implications.

TABLE 8. Correlations between Absenteeism and Facility Characteristics

	Absenteeism	Working Toilet	Visual Privacy	Auditory Privacy	Water in Examination Area	Adequate Lighting	No. Patients per Week
Absenteeism	1.000						
Working toilet	−0.204	1.000					
Visual privacy	−0.068	0.239	1.000				
Auditory privacy	−0.071	0.252	0.873	1.000			
Water in examination area	−0.210	0.297	0.095	0.106	1.000		
Adequate lighting	−0.233	0.178	0.016	0.035	0.342	1.000	
No. patients per week	−0.128	0.264	0.191	0.164	0.510	0.269	1.000

Note: Number of observations is 144; the facility is the unit of observation. Correlations greater than 0.162 in absolute value are significant at the 5 percent level.

Source: Authors' calculations based on public health facility surveys, March–April 2002.

of the correlation between the pattern of absences and two sets of indicators of facility performance. One comes from observations on a set of clinic characteristics that were made by the interviewers. These include subjective judgments about the cleanliness of the facility, the degree of visual and auditory privacy, and the adequacy of lighting, as well as more objective measures, such as whether the piped water and toilets were functional. A simple hypothesis is that professionals who regularly attend their assigned post will look after these aspects of the facility, generally keeping it in good repair. The direction of causality could, of course, go the other way. Another set of indicators relates to the use of the facility by clients. The absence of professionals, particularly of physicians, has often been noted by clients as a cause of low utilization of facilities. Interviewers were instructed to look at the intake sheet of the facility over the past week and note the number of people visiting the facility.

Piecing out the various causal pathways that each of these variables might have with others is very difficult with cross-section data and the limited instruments available.[18] Simple correlation coefficients between each of the various characteristics of facilities and their average absentee rate show that all of these variables move together (table 8). Low absenteeism is correlated with generally well-functioning facilities, particularly as measured by the more objective criteria (rather than interviewers' subjective judgments) and greater utilization. Interestingly, the direct correlation of absenteeism and number of patients is weak.

18. An attempt to disentangle the various possible causal relations is presented in Chaudhury and Hammer (2003). We think that the case can be made that absenteeism is more likely to lead to poor maintenance than that poor maintenance lowers attendance. Furthermore, we believe that better maintenance leads to more visits by patients. Skeptics, however, are entitled to remain so.

All of these indicators are also highly correlated with the level of the facility. The larger *upazila* health complexes are all better staffed, better attended, and better maintained than the lower-level facilities. This is consistent with the argument in Filmer and others (2002) that managing small primary health centers poses a much greater challenge than running larger facilities, because physicians especially are more likely to attend and manage themselves.

V. Conclusion

The aim of this study was to assess the commonly held belief that staff attendance in public health clinics is low. Common opinion appears to be correct. Absenteeism is very high and can reach 74 percent or higher in small rural posts.

The results cannot take us much farther than this. They cannot be used to assess the welfare consequences or to identify causal relations, let alone policy-related interventions. Data from surprise visits are not collected frequently and by their nature are limited in determining the reasons for attendance patterns. It is hard to get more detailed information on absent workers without large increases in costs.

The results do, however, suggest important areas for future work. First, they highlight the fact that public employees are active decisionmakers and that services do not get provided simply by fiat. The motivations of workers, the opportunity cost of their time, and the conditions under which they work (or get to work) are all likely to influence their performance on the job. Sometimes these conditions can be changed through policy measures. Sometimes they have to be taken as given constraints. Research on the behavior of service providers is in its early stages.[19] The World Bank (2004) highlights the importance of understanding provider behavior. It was intended to stimulate further research. As part of that report, a major research project was initiated to study absence rates of medical providers and teachers in primary schools in several countries (Chaudhury and others 2004a,b).

In Bangladesh, the policy direction that the government has chosen—upgrading union family welfare centers to include a posting for a physician—appears to run up against the reluctance of physicians to serve in such posts. Whether the posts can be made attractive enough to increase the very low attendance rates observed in this study, whether the government is willing to accept these rates in the hope that more qualified physicians can be induced to live in rural areas as private physicians (a hypothesis that should be rigorously tested), or whether the policy should be reconsidered and different means found to help the rural poor are important decisions made starker by the results of this study.

Second, both theoretical and empirical work are necessary to answer questions about the welfare implications of the results. For example, where are the

19. See Leonard (forthcoming) and references therein for examples of such work.

private practitioners when they are away from their public post? Are they in rural areas near their post, or are they serving the richer market in urban areas? How much does their absence reduce medical services for the rural poor, and how much does it translate into greater travel time and costs that patients must bear? For both positive analysis and policy discussions, the supply side of service provision should be a high priority for future work.

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Small-Scale Industry, Environmental Regulation, and Poverty: The Case of Brazil

Rajshri Jayaraman and Peter F. Lanjouw

Governments and international development agencies have intensified efforts to promote small-scale enterprises as an engine of propoor growth. In Brazil, however, small-scale industries may also be responsible for the bulk of air pollution emissions. Although employees of polluting small-scale industries in Brazil are not disproportionately poor, simulations suggest that stringent environmental regulation resulting in widespread closures of pollution-intensive small-scale industries would result in a nonnegligible increase in poverty among employees of these firms. The results suggest that the enthusiasm for small-scale enterprises needs to be tempered by awareness of the potential environmental costs imposed by this sector.

Small-scale enterprises have generated a surge of interest among policymakers and development agencies in recent years. The World Bank and the International Finance Corporation (IFC) have been particularly active in promoting small-scale enterprises, setting up a separate department for them in 2000 and allotting $1.5 billion toward their development in 2002.

This emphasis is not unwarranted. Small-scale enterprises are the dominant employers in much of the developing world. In Ecuador, for instance, 99 percent of firms have 50 or fewer employees (Lanjouw 1997). In northeast and southeast Brazil, 71 percent of workers are employed in firms with fewer than 20 employees. Despite forming the economic bedrock of most low-income countries, small-scale enterprises often operate in difficult business environments and weak institutional settings, and with little access to physical and human capital (IFC and World Bank 2002). Improving the investment climate faced by small-scale enterprises is therefore increasingly viewed as pivotal to promoting economic growth in low-income countries.

There is also a long-standing belief that "small-scale enterprises in developing countries play a major role in providing income opportunities among the urban

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poor" (World Bank 1978, p. 9; see also IFC and World Bank 2002; ILO 1991). Poor people typically have a limited endowment of the human capital thought to be necessary for employment in the public sector or in more technology-intensive, large-scale enterprises. Small-scale enterprises more commonly employ unskilled labor.

Improving the investment climate for small-scale enterprises is therefore widely promoted as a means of reaping the double dividend of propoor growth. However, in the rush to embrace small-scale enterprises, there has been little discussion of the potential environmental costs imposed by one subset of such enterprises, small-scale industries. This article examines this aspect of small-scale industries.

The manufacturing sector typically accounts for a small share of total small-scale enterprise employment. In Brazil, for instance, only 9.4 percent of employees in firms with 20 or fewer employees work in the manufacturing sector. Although manufacturing processes tend to use technologies—fuel burning, in particular—that are major contributors to air pollution, different industrial sectors have different emission intensities because of different production technologies. Some industrial processes are simply "dirtier" than others. Paper processing is more emission-intensive than furniture making. Pollution also varies according to firm size, a fact attributable to two types of scale economies. One is public economies of scale: The government may be better able to regulate or monitor pollution from large firms than from less visible small firms. Another is private economies of scale: Larger firms have access to different technologies, associated with different pollution intensities, than smaller firms do.

There is no consensus, however, about whether small-scale industries are more pollution-intensive than large-scale industries. Some argue that small-scale industries may be more environmentally sustainable because of such factors as informal community pressure and regulation (see Schumacher 1989; Blackman and Bannister 1996; Pargal and Wheeler 1996). But there are also contrary views. Beckerman (1995) and Branden (1993) argue that small industries may be more harmful to the environment. As Branden (1993, pp. 4–11) notes, small-scale industries "often pollute more per unit of output than large firms operating in the same sector." This has variously been attributed to their failure to employ more efficient, updated technology; the difficulty of monitoring their compliance with regulations; their inability to safely dispose of the waste they produce; and their limited awareness of the potentially harmful effects of their activities.

Should policymakers be concerned about the environmental costs associated with measures to promote small-scale industries? Are they right in believing that the poor are overrepresented in this sector? Is their concern that regulation aimed at attenuating environmental costs would come at the price of exacerbating poverty?

This article broaches each of these questions in the context of air pollution in the manufacturing sector of Brazil. The findings, based on applying air pollution per worker coefficients by industry and firm size from a Mexican emissions study to data from a Brazilian household survey on employment and size of workplace, suggest that small-scale industries contribute a nonnegligible amount to aggregate air pollution in Brazil. Policymakers need to be alert to this environmental consequence of rapid growth in small-scale industry activity. The results also show that the poor are not overrepresented in polluting small-scale industries. Even so, simulations indicate that the most draconian regulation, aimed at eliminating the environmental costs associated with pollution-intensive small-scale industry activity, could exacerbate poverty in Brazil.

I. Data Sources

The analysis draws on two data sources: the 1996/97 Living Standards Survey (PPV) for Brazil and the National Information System on Pollution from Fixed Sources (SNIFF) database on air pollution emissions in Mexico. The PPV, fielded in northeast and southeast Brazil by the Brazilian Institute of Geography and Statistics, is a cross-sectional survey of 4,940 households comprising 14,409 individuals and covering 10 major geographical regions.[1] The PPV is an extremely rich data set. It follows the World Bank's Living Standards Measurement Study format in collecting information on a wide range of household characteristics, including demographic characteristics, education levels, asset ownership, occupation, sector and size of workplace, and incomes and consumption levels. It details 41 sectors of employment, each broken down into subsectors, and describes the size of the firm in which respondents are employed—important for identifying small-scale industries.

Because emissions data by firm sector and size are unavailable for Brazil, emissions data for Mexico are used to simulate emissions for Brazil. Both countries have similar technologies and relative input prices in manufacturing (Dasgupta and others 2002). The SNIFF database, maintained by the National Environment Institute in Mexico's Environment Ministry, records industrial sector, number of employees, and emissions of conventional air pollutants (such as particulates, sulfur dioxide, nitrogen oxides, and carbon monoxide) for around 6,000 plants. Dasgupta and others (1998) used the SNIFF database to calculate annual airborne suspended particulate emissions in metric tons per employee by firm scale and sector of production. This article applies their calculations to the PPV data to determine the pollution intensity of small- (1–20 workers), medium- (21–100 workers), and large-scale (101+ workers) firms in 27 industrial sectors.[2]

1. The 10 regions are: Fortaleza, Recife, Salvador, other northeast urban areas, northeast rural areas, Belo Horizonte, Rio de Janeiro, São Paolo, other southeast urban areas, and southeast rural areas.

2. For further details on methodology on calculating pollution coefficients, see Dasgupta and others (1998, 2002).

Some 64 percent of the economically active population in Brazil are employed in workplaces with 10 or fewer employees, and 71 percent are employed in workplaces with fewer than 20 workers. Less than half the population in the industrial sector (41 percent) is employed in firms of 10 or fewer workers, and only about half (52 percent) work in firms of 20 or fewer. Thus, although small-scale employment dominates in Brazil on the whole, in the industrial sector it is roughly on par with larger-scale employment.

The poverty line for this study was constructed from the PPV data set by Ferreira and others (2003). It is based on the cost of a minimum food basket (yielding 2,288 calories per person per day) plus the cost of some basic nonfood requirements. To correct for the considerable regional price variation in a country as large as Brazil, Ferreira and others also calculate a spatial price index based on the food items covered in the PPV. Both the poverty line (131.97 reals per person per month) and all consumption expenditure measures are expressed in 1996 São Paulo prices.

II. Do Small-Scale Industries Pollute More Than Larger Industries?

Before exploring how environmental regulation of small-scale industries would affect poverty, it is worth seeing whether small-scale industries pose enough of a threat to the environment to warrant regulation. After all, policymakers look to small-scale industries to promote propoor growth, and small-scale industries may be more costly to monitor than larger-scale firms. It would be difficult to justify regulation if small-scale industries contribute only marginally to overall pollution loads.

Applying the Mexican SNIFF data on annual particulate emissions in metric tons per employee by firm size calculated by Dasgupta and others (1998) to the Brazilian PPV data shows the contribution of small-scale industries to total air pollution among 27 industrial sectors in Brazil (table 1). Small-scale industries contribute a nonnegligible share of total pollution loads, as measured by particulate matter small enough to be inhaled (at 10 micrograms per cubic meter, PM_{10}). In 10 industrial subsectors, small-scale industries account for more than three-quarters of overall sectoral pollution, and in 3 they account for more than half. On aggregate, small-scale industries account for 62 percent of industrial air pollution in Brazil but for only 50 percent of industrial employment.

In general, the evidence points to an environmental protection rationale for regulating small-scale industries. How such regulation would affect the poor is not clear, however.

This study is limited to small-scale industries. Although there may be concerns about environmental stress stemming from agricultural practices and from small-scale services, these are likely to center on common property resource problems (such as overgrazing, deforestation, and water pollution arising from pesticide use) or urban congestion (see World Commission on Environment and

TABLE 1. Industry Contributions to Employment and Air Pollution, by Industry Size and Sector

	Employment (number)				Annual particulate emissions (metric tons)							Ratio of total pollution to number of poor employed		
					Total				Per employee					
Sector	Small	Medium	Large	Total	Small	Medium	Large	Total	Small	Medium	Large	Small	Medium	Large
Apparel	634,922	97,165	95,979	828,066	4,444.45	97.17	–	4,541.62	0.007	0.001	0.000	0.019	0.002	0.000
Furniture	172,931	26,283	14,314	213,528	1,556.38	26.28	14.31	1,596.98	0.009	0.001	0.001	0.021	0.001	0.008
Professional equipment	25,850	15,600	14,840	56,290	542.85	31.20	–	574.05	0.021	0.002	0.000	0.038		
Wood products	188,682	18,720	60,328	267,730	99,058.05	992.16	5,308.86	105,359.07	0.525	0.053	0.088	1.116	0.079	0.090
Other manufacturing	120,946	18,508	39,842	179,296	1,209.46	55.52	39.84	1,304.83	0.010	0.003	0.001	0.037	0.007	
Rubber	29,745	15,187	9,281	54,213	2,498.58	151.87	92.81	2,743.26	0.084	0.010	0.010	0.259	0.012	0.080
Leather products	43,832	8,744	20,929	73,505	1,183.46	87.44	167.43	1,438.34	0.027	0.010	0.008	0.080	0.010	0.174
Other foods	472,591	155,675	48,228	676,494	117,202.57	33,158.78	530.51	150,891.85	0.248	0.213	0.011	0.693	1.489	0.083
Metal products	258,079	115,586	168,548	542,213	10,839.32	1,271.45	1,854.03	13,964.79	0.042	0.011	0.011	0.102	0.277	0.047
China and pottery	173,399	59,055	75,382	307,836	1,907.39	413.39	150.76	2,471.54	0.011	0.007	0.002	0.016	0.012	0.014
Other nonmetallic	135,245	40,469	72,032	247,746	12,307.30	5,625.19	288.13	18,220.61	0.091	0.139	0.004	0.423	2.666	0.032
Beverages	13,177	10,815	83,633	107,625	7,089.23	205.49	3,847.12	11,141.83	0.538	0.019	0.046		0.088	0.366
Textiles	225,099	10,304	108,837	344,240	2,250.99	175.17	1,523.72	3,949.88	0.010	0.017	0.014	0.036		0.042
Other chemicals	71,921	24,125	56,824	152,870	1,150.74	554.88	852.36	2,557.97	0.016	0.023	0.015	0.087	0.044	0.368
Iron and steel	33,536	25,376	134,887	193,799	8,316.93	3,121.25	8,362.99	19,801.17	0.248	0.123	0.062	0.933	0.231	0.516
Printing	55,569	65,734	77,346	198,649	55.57	131.47	–	187.04	0.001	0.002	0.000	0.006	0.015	0.000
Paper	12,472	7,501	17,390	37,363	336.74	367.55	921.67	1,625.96	0.027	0.049	0.053	0.048	0.243	0.755
Nonferrous	26,476	19,305	20,387	66,168	582.47	772.20	1,794.06	3,148.73	0.022	0.040	0.088	0.479	0.238	0.628
Transport equipment	37,691	104,917	226,213	368,821	263.84	314.75	904.85	1,483.44	0.007	0.003	0.004	0.020		0.015
Machinery	57,279	25,088	167,986	250,353	4,925.99	526.85	24,693.94	30,146.78	0.086	0.021	0.147	0.289		5.320
Electrical apparatus	40,925	22,155	114,590	177,670	409.25	199.40	2,635.57	3,244.22	0.010	0.009	0.023	0.024	0.096	0.166
Glass	14,695	20,287	14,453	49,435	73.48	628.90	491.40	1,193.77	0.005	0.031	0.034	0.005		
Basic foods	124,598	68,836	205,930	399,364	2,242.76	7,090.11	46,540.18	55,873.05	0.018	0.103	0.226	0.038	2.705	0.951
Industrial chemicals	3,659	4,953	98,693	107,305	219.54	341.76	13,718.33	14,279.62	0.060	0.069	0.139	0.119	0.142	0.416
Petroleum refining	–	–	9,797	9,797	–	–	244.93	244.93	0.087	0.040	0.025			0.025
Footwear	126,891	23,085	61,526	211,502		23.09	–	23.09		0.001	0.000	0.000	0.001	0.000
Tobacco products	–	–	8,992	8,992			62.94	62.94			0.007			0.007
Total	3,100,210	1,003,473	2,027,187	6,130,870	280,667.33	56,363.27	115,040.75	452,071.35				0.052	0.030	0.029

Note: Small = 1 to 20 employees, medium = 21 to 100, large = 101+.

Source: Dasgupta and others (1998); 1996/97 PPV for Brazil.

Development 1987). These are important environmental issues meriting further study. However, agriculture and small-scale services are unlikely targets of air pollution regulation, which tends to be directed toward processes that are fuel-combustion intensive.

III. Small-Scale Industries, Poverty, and Pollution

The evidence on particulate emissions suggests that governments concerned with environmental pollution have reason to pay attention to small-scale industries. However, governments may be hesitant to intervene if they believe that such regulation would affect the poor disproportionately.

With a few exceptions (textiles, leather products, and beverages), the 13 sectors in which small-scale industries are responsible for the bulk of PM_{10} pollution also have higher ratios of pollution to the number of poor people employed than the corresponding large-scale firms (see table 1). This suggests that environmental regulation of the worst offenders among small-scale industries may be an effective strategy for reducing emissions while simultaneously minimizing the damage to the poor.

The consequences of regulation for poverty clearly depend on which small-scale industries are targeted. A government wanting to reduce air pollution would presumably concentrate on the small-scale industries with the highest burden of air pollution. But what this means in practice may not be clear. This study considers several scenarios for targeting small-scale industries for regulation.

- Scenario 1: Small-scale industries contributing 50 percent or more of their industrial sector's PM_{10} emissions (these firms account for 58 percent of total industrial air pollution).
- Scenario 2: Small-scale industries producing higher PM_{10} emissions per employee than large-scale industries in their industrial sector (these firms account for 60 percent of industrial air pollution in their sectors).
- Scenario 3: Small-scale industries producing higher total PM_{10} emissions than large-scale industries in their industrial sector (these firms account for 58 percent of industrial air pollution in their sectors).

Government concern with air pollution is not restricted to its environmental costs. A large body of evidence from North America and Western Europe indicates that air pollution from combustion processes is responsible for numerous health problems, ranging from eye irritation to death. Human health damage in urban areas is roughly proportional to the product of PM_{10} concentration times city population (Pandey and others forthcoming). Polluting small-scale industries in cities where this product is high would be natural candidates for regulation.

A recent World Bank–World Health Organization (WHO) study measured annual concentrations of PM_{10} for 180 Brazilian cities with populations greater than 100,000.[3] Although the PPV identifies only 10 geographic regions, 6 of these that are identified as cities correspond to the cities in the World Bank's WHO study that are among the worst offenders in the country.[4] The product of PM_{10} times city population concentration is the highest in the country in São Paolo and second highest in Rio de Janeiro, followed by Belo Horizonte (4th), Salvador (6th), Fortaleza (7th), and Recife (17th). Thus, two additional scenarios consider what would happen to poverty if regulation were restricted to small-scale industries in these cities:

- Scenario 4: Small-scale industries in six cities with high air pollution as measured by population times PM_{10} concentration: São Paolo, Rio de Janeiro, Belo Horizonte, Slavador, Fortaleza, and Recife (these firms account for 24 percent of total industrial air pollution).
- Scenario 5: Small-scale industries that produce more than 1,000 metric tons of PM_{10} emissions in six cities with high air pollution as measured by population times PM_{10} concentration (these firms account for 19 percent of total industrial air pollution).

Two questions are examined: Does employment of poor people in the small-scale industries targeted under the five scenarios differ systematically from that in other sectors of the economy? What contribution do these firms make to overall poverty among workers? Answering these questions will help verify whether the poor in Brazil are indeed, as often supposed, overrepresented in pollution-intensive industries.

The incidence of poverty among employees of small-scale industries targeted according to pure emissions criteria (scenarios 1–3) is around 37–38 percent (table 2). That poverty differs little across these groups reflects the fact that the sectors targeted under these three scenarios do not vary dramatically. However, small-scale industries targeted under the urban pollution scenarios 4 and 5 have significantly lower poverty rates. Just 25 percent of these workers are poor. Workers in small-scale industries targeted under scenarios 1–3 have significantly higher poverty rates than workers in large-scale industries. Because these three scenarios would target more than 80 percent of workers employed in small-scale industries, this higher poverty rate may explain the common perception that the poor are concentrated in these industrial sectors. However, poverty among workers employed in small-scale industries targeted under the narrower urban pollution scenarios 4 and 5 (targeting fewer than 40 percent of workers employed in small-scale industries) is not significantly different than poverty among employees of large-scale industries. Poverty rates of workers targeted

3. These data were made available by David Wheeler, lead economist in the infrastructure/environment team of the World Bank's Development Research Group.

4. The Brazilian authorities chose not to distribute information regarding the specific location of each household to protect the confidentiality of informants.

TABLE 2. Poverty and Employment by Targeting Criteria for Five Regulatory Scenarios (%)

	Scenario 1			Scenario 2			Scenario 3			Scenario 4			Scenario 5		
	Bulk of sectoral pollution[a]			High emissions per employee relative to large-scale indsutries[b]			High total emissions relative to large-scale industries[c]			Located in urban area with high health damage[d]			Highly polluting SSI located in urban area with high health damage[e]		
Sector	Poverty incidence	Contribution to poverty	Contribution to employment	Poverty incidence	Contribution to poverty	Contribution to employment	Poverty incidence	Contribution to poverty	Contribution to employment	Poverty incidence	Contribution to poverty	Contribution to employment	Poverty incidence	Contribution to poverty	Contribution to employment
Targeted small-scale industries	38.56	4.61	5.29	38.05	4.53	5.26	37.35	4.72	5.59	26.22	1.53	2.59	26.55	0.51	0.85
	(4.67)	(0.55)		(4.53)	(0.54)		(4.49)	(0.55)		(4.08)	(0.37)		(9.03)	(0.13)	
Nontargeted small-scale industries	34.46	1.01	1.29	35.80	1.09	1.35	38.93	0.90	1.02	44.92	4.08	4.02	39.23	5.10	5.75
	(6.39)	(0.27)		(6.37)	(0.28)		(7.2)	(0.26)		(4.49)	(0.57)		(3.79)	(0.59)	
Agriculture[f]	80.00	39.40	21.79	80.00	39.40	21.79	80.00	39.40	21.79	80.00	39.40	21.79	80.00	39.40	21.79
	(2.08)	(3.16)		(2.08)	(3.16)		(2.08)	(3.16)		(2.08)	(3.16)		(2.08)	(3.16)	
Mining	18.13	0.18	0.43	18.13	0.18	0.43	18.13	0.18	0.43	18.13	0.18	0.43	18.13	0.18	0.43
	(7.96)	(0.09)		(7.96)	(0.09)		(7.96)	(0.09)		(7.96)	(0.09)		(7.96)	(0.09)	
Small-scale services	36.50	31.41	38.06	36.50	31.41	38.06	36.50	31.41	38.06	36.50	31.41	38.06	36.50	31.41	38.06
	(3.17)	(2.00)		(3.17)	(2.00)		(3.17)	(2.00)		(3.17)	(2.00)		(3.17)	(2.00)	
Large-scale industries	25.14	3.55	6.24	25.14	3.55	6.24	25.14	3.55	6.24	25.14	3.55	6.24	25.14	3.55	6.24
	(3.32)	(0.59)		(3.32)	(0.59)		(3.32)	(0.59)		(3.32)	(0.59)		(3.32)	(0.59)	
Large-scale services	22.89	9.71	18.77	22.89	9.71	18.77	22.89	9.71	18.77	22.89	9.71	18.77	22.89	9.71	18.77
	(2.74)	(0.85)		(2.74)	(0.85)		(2.74)	(0.85)		(2.74)	(0.85)		(2.74)	(0.85)	
Ill defined/undeclared	66.29	2.04	1.36	66.29	2.04	1.36	66.29	2.04	1.36	66.29	2.04	1.36	66.29	2.04	1.36
	(8.86)	(0.39)		(8.86)	(0.39)		(8.86)	(0.39)		(8.86)	(0.39)		(8.86)	(0.39)	
Construction	52.94	8.09	6.76	52.94	8.09	6.76	52.94	8.09	6.76	52.94	8.09	6.76	52.94	8.09	6.76
	(4.05)	(0.78)		(4.05)	(0.78)		(4.05)	(0.78)		(4.05)	(0.78)		(4.05)	(0.78)	
Subtotal Nontargeted small-scale industries	44.55	95.39	94.71	44.57	95.47	94.70	44.64	95.28	94.41	44.71	98.47	97.41	44.38	99.49	99.15
	(3.42)	(0.55)		(3.41)	(0.54)		(3.42)	(0.55)		(3.33)	(0.37)		(3.37)	(0.13)	
Total	44.24	1.00	1.00	44.24	1.00	1.00	44.24	1.00	1.00	44.24	1.00	1.00	44.24	1.00	1.00
	(3.42)			(3.42)			(3.42)			(3.42)			(3.42)		

Note: Numbers in parentheses are SE.

[a] Small-scale industries contributing 50 percent or more of their industrial sector's air pollution (PM_{10}).

[b] Small-scale industries producing higher PM_{10} emissions per employee than large-scale industries in their sector.

[c] Small-scale industries producing higher total PM_{10} emissions than large-scale industries in their sector.

[d] Small-scale industries in six cities with high air pollution (population times PM_{10} concentration): São Paolo, Rio de Janeiro, Belo Horizonte, Salvador, Fortaleza, and Recife.

[e] Small-scale industries that produce more than 1,000 metric tons of PM_{10} emissions in six cities with high air pollution (population times PM_{10} concentration).

[f] Includes fishing, livestock production, and forestry.

Source: Authors' calculations based on 1996/97 PPV for Brazil and the SNIFF database on air pollution.

under all five scenarios are markedly lower than that among all workers combined (44 percent), although this difference is not statistically significant.

The data do not confirm the assertion that poor people are disproportionately represented in polluting small-scale industries. Under scenarios 1–3, poor people in targeted small-scale industries account for 4.6–4.7 percent of all poor people while these industries represent 5.3–5.6 percent of the working population. This is in marked contrast to large-scale industries, which constitute 6.24 percent of employment and 3.55 percent of poor people. Poor people in targeted small-scale industries under scenarios 4 and 5 are even more underrepresented: These industries' contribution to poverty is just over half their contribution to employment.

In Brazil poverty is concentrated in small-scale services and agriculture. Poverty rates are not significantly different in small-scale services than they are in small-scale industry as a whole. However, nearly 40 percent of the workforce is employed in this sector, making its contribution to overall poverty extremely high (at 31 percent). In agriculture, the problem is even more severe: 80 percent of workers fall below the poverty line, and they constitute more than 39 percent of the number of poor workers. Moreover, unlike workers in small-scale services, workers in agriculture are overrepresented among the poor because they account for just under 22 percent of employment.

For the original question motivating this study of how poor people would fare if regulation focused on small- rather than large-scale industries, the data suggest that the toll on the poor would be substantially higher if regulation were based on broadly defined emissions criteria. If small-scale industries were more narrowly targeted on the basis of location, the data suggest that there would be little impact on poverty as a result of targeting small- rather than large-scale firms.

IV. Environmental Regulation of Small-Scale Industries and Poverty

The analysis so far suggests that employees of pollution-intensive small-scale industries are not doing too badly relative to the average worker. They are not disproportionately poor, their poverty rates are not significantly different from that of the working population at large, and they do considerably better than employees in some of the other major sectors (especially agriculture). However, these statistics do not by themselves give a firm grasp of how regulation of pollution-intensive small-scale industry would affect poverty. This requires understanding how well employees in this sector are equipped to deal with employment shakeouts ensuing from regulation.

Are Employees of Pollution-Intensive Small-Scale Industries Vulnerable to Poverty?

Because vulnerability to poverty extends beyond workers to their families, the analysis needs to include the working-age population at large. Overall, the data indicate that most poor people are black or mulatto, have little education, and live in urban areas, although households in rural areas are poorer on average.

Do individuals employed in pollution-intensive small-scale industry share these characteristics? Of the 12,892 individuals in the sample, 455 are targeted under scenario 1, 455 under scenario 2, 480 under scenario 3, 365 under scenario 4, and 102 under scenario 5. The results for the probability that an individual ages 15–80 is employed in the sectors targeted under each of the five scenarios, controlling for a variety of individual and household characteristics, are quite consistent (table 3). The results show an inverted U-shaped relationship between the probability of working in a polluting small-scale industry and the dependency ratio (ratio of nonworkers in a household to total number of household members). A Kolmogorov-Smirnov equality of distributions test reveals, however, that the families of individuals employed in the targeted small-scale industries do not differ significantly in size from those of employees in other sectors.

Mulattos appear less likely to work in polluting small-scale industries in the six major cities, but otherwise race appears to have no connection with the probability of working in these small-scale industries. Age generally matters, with the probability of working in polluting small-scale industries rising up to ages 35–40 and declining thereafter. With the exception of scenario 1, women are significantly less likely than men to work in these small-scale industries. Educational attainment is also important: Individuals with some primary and secondary schooling are significantly more likely to work in the polluting small-scale sector than those with no education.

Because targeting in scenarios 4 and 5 is based on geographic criteria, regional dummy variables are not included in these two models. However, regional dummy variables play an important role in indicating employment in industries targeted solely by emissions criteria. In particular, all other things equal, individuals residing in Fortaleza have a higher probability of working in polluting small-scale industries than do residents of São Paolo, whereas those in Recife, Salvador, and Rio de Janeiro and those in rural areas have a lower probability. Finally, the larger the proportion of municipal residents working in pollution-intensive small-scale industry, the more likely it is that any given individual in the municipality will be employed in a pollution-intensive small-scale industry.[5] The role of this variable will become apparent shortly.

Overall, the profile of individuals employed in small-scale industries that are potential targets of regulation does not appear to closely resemble the profile of poor people. As mentioned, poverty tends to be associated with having low levels of education, residing in rural and small urban areas, and being black or mulatto. The probits for working in pollution-intensive small-scale industry

5. Endogeneity is avoided here by omitting the individual in question when calculating this ratio.

TABLE 3. Probit Model on Employment in Targeted Small-Scale Industry (for population ages 15–80)

	Scenario 1[a]			Scenario 2[b]			Scenario 3[c]			Scenario 4[d]			Scenario 5[e]		
	Coefficient	Marginal effects	Probability value	Coefficient	Marginal effects	Probability value	Coefficient	Marginal effects	Probability value	Coefficient	Marginal effects	Probability value	Coefficient	Marginal effects	Probability value
Dependency ratio	0.450	0.027	0.188	0.406	0.350	0.245	0.442	0.342	0.197	0.445	0.379	0.241	0.634	0.569	0.265
Dependency ratio squared	−1.328	−0.080	0.001	−1.166	−0.381	0.002	−1.331	−0.381	0.000	−1.233	−0.414	0.003	−1.057	−0.625	0.091
Household head*	0.117	0.007	0.163	0.121	0.084	0.148	0.125	0.082	0.128	0.064	0.091	0.481	0.182	0.143	0.202
Black*	−0.113	−0.006	0.431	−0.113	−0.142	0.427	−0.151	−0.144	0.294	−0.170	−0.149	0.255	−0.173	−0.165	0.297
Mulatto*	−0.008	0.000	0.912	−0.022	−0.074	0.761	−0.032	−0.073	0.664	−0.161	−0.064	0.012	−0.534	−0.088	0.000
Amerindian*	0.072	0.005	0.871	0.010	0.433	0.981	0.038	0.440	0.930	0.199	0.459	0.664			
Age	0.032	0.002	0.007	0.035	0.012	0.003	0.033	0.011	0.004	0.025	0.012	0.042	0.003	0.018	0.862
Age squared	−0.0004	0.000	0.002	−0.0005	0.000	0.000	−0.0004	0.000	0.001	−0.0003	0.000	0.034	−0.0001	0.000	0.546
Female*	−0.088	−0.005	0.195	−0.215	−0.068	0.002	−0.102	−0.067	0.129	−0.216	−0.073	0.003	−0.389	−0.127	0.002
Single*	−0.008	0.000	0.918	−0.007	−0.077	0.930	−0.012	−0.077	0.876	0.070	0.086	0.415	0.193	0.134	0.148
Incomplete primary education*	0.423	0.026	0.000	0.381	0.107	0.000	0.384	0.103	0.000	0.465	0.143	0.001	0.237	0.190	0.213
Completed primary education*	0.391	0.032	0.004	0.362	0.140	0.010	0.358	0.136	0.009	0.573	0.159	0.000	0.252	0.212	0.234
Incomplete secondary education*	0.491	0.045	0.001	0.434	0.154	0.005	0.468	0.147	0.002	0.597	0.180	0.001	0.229	0.255	0.369
Completed secondary education*	0.192	0.013	0.139	0.162	0.137	0.240	0.194	0.131	0.140	0.336	0.164	0.041	0.043	0.245	0.861
Some university education*	−0.167	−0.009	0.271	−0.108	−0.155	0.483	−0.177	−0.151	0.240	0.210	0.178	0.238	−0.373	−0.248	0.132
Completed professional course*	0.821	0.105	0.132	0.791	0.558	0.156	0.754	0.554	0.174						
Immigrant*	−0.096	−0.006	0.227	−0.049	0.079	0.530	−0.088	−0.079	0.266	−0.084	−0.082	0.306	−0.167	−0.119	0.162
Fortaleza*	0.169	0.012	0.088	0.157	0.097	0.107	0.171	0.097	0.079						
Recife*	−0.307	−0.014	0.014	−0.284	−0.120	0.017	−0.272	−0.120	0.023						

(Continued)

TABLE 3. Continued

	Scenario 1[a]			Scenario 2[b]			Scenario 3[c]			Scenario 4[d]			Scenario 5[e]		
	Coefficient	Marginal effects	Probability value	Coefficient	Marginal effects	Probability value	Coefficient	Marginal effects	Probability value	Coefficient	Marginal effects	Probability value	Coefficient	Marginal effects	Probability value
Salvador*	−0.241	−0.012	0.045	−0.253	−0.118	0.033	−0.213	−0.117	0.069						
Urban northeast*	−0.110	−0.006	0.332	−0.134	−0.111	0.229	−0.105	−0.112	0.348						
Rural northeast*	−0.307	−0.015	0.017	−0.270	−0.128	0.035	−0.278	−0.128	0.030						
Belo horizonte*	0.123	0.008	0.234	0.078	0.101	0.438	0.135	0.102	0.187						
Rio de Janeiro*	−0.163	−0.009	0.123	−0.163	−0.104	0.117	−0.136	−0.102	0.182						
Urban southeast*	−0.036	−0.002	0.708	−0.061	−0.095	0.518	−0.026	−0.095	0.780						
Rural southeast*	−0.155	−0.008	0.149	−0.290	−0.113	0.010	−0.163	−0.107	0.128						
Ratio of polluting small-scale industry workers in municipilaty	2.722	0.164	0.002	2.919	0.888	0.001	2.556	0.866	0.003	13.439	0.776	0.000	10.347	1.997	0.000
Constant	−2.441	0.303	0.000	−2.444	−0.310	0.000	−2.397	−0.303	0.000	−3.106	−0.335	0.000	−2.484	−0.497	0.000
Log likelihood			−1829.7017			−1818.7848			−1904.576			−951.97			−400.86282
Pseudo R^2			0.0691			0.0683			0.0668			0.1617			0.1251

*Marginal effects indicate discrete changes from 0 to 1 for dummy variables.

Note: 12,892 observations. Completed professional course dummy variable perfectly predicts success in scenarios 4 and 5 and so were dropped. Amerindian predicts failure perfectly in scenario 5, so 37 observations were dropped.

[a]Small-scale industries contributing 50 percent or more of their industrial sector's air pollution (PM_{10}).

[b]Small-scale industries producing higher PM_{10} emissions per employee than large-scale industries in their sector.

[c]Small-scale industries producing higher total PM_{10} emissions than large-scale industries in their sector.

[d]Small-scale industries in six cities with high air pollution (population times PM_{10} concentration): São Paolo, Rio de Janeiro, Belo Horizonte, Salvador, Fortaleza, and Recife.

[e]Small-scale industries that produce more than 1,000 metric tons of PM_{10} emissions in six cities with high air pollution (population times PM_{10} concentration).*Source:* Authors' calculations based on 1996/97 PPV for Brazil and the SNIFF database on air pollution.

turn out insignificant coefficients for the dummy variable for being black, and mulattos are less likely to work in these industries under the geographic targeting scenario. Models 1–3 also produce insignificant coefficients for the two small urban area dummy variables. The probit regressions further indicate that people with some schooling are more likely to work in polluting small-scale industries than people with no schooling (although the best-educated people in the population are not significantly more likely to be found in pollution-intensive small-scale industries).

A Thought Experiment: Five Scenarios for Regulating Small-Scale Industries

How would poverty change following environmental regulation of pollution-intensive small-scale industry? Ideally, the answer to this question would require panel data and observation of what happened to employment and poverty in a country that introduced pollution regulation. This option is not available because the data are cross-sectional and regulation is hypothetical. Instead, the question is approached by asking what consumption level a person employed in a pollution-intensive small-scale enterprise might hope to attain outside that sector, given the person's personal attributes and endowments.

Two assumptions are made. First, regulation consists of closing down the polluting small-scale enterprises under each of the five scenarios, so that all employees are out of work. This assumption serves a dual purpose. One, it skirts the issue of differential impacts of environmental regulation depending on the choice of instrument. If environmental regulation is so draconian as to cause a firm to fire all its employees, then it does not really matter whether regulation took the form of market- or incentive-based interventions (including taxes or markets in pollution rights) or command and control methods (technology or performance standards, for instance). Two, in the absence of any firm-level data (on production process or costs, for instance) this assumption seems preferable to making arbitrary assumptions about differential impacts of regulation on employment or wages depending on firm sector or size (the only pertinent firm-level data available for this study).

The second assumption is that returns to education, unemployment rates, and so on are unaltered in the remaining sectors—a partial equilibrium analysis. In effect, the consumption of individuals who are not subject to regulation is assumed to be unaffected by the regulation, and the workers who become redundant as a consequence of the regulation are assumed to enjoy consumption levels analogous to those of workers in untargeted firms with similar characteristics. Although the relatively small contribution of the pollution-intensive small-scale sectors to total employment makes this assumption less a concern than it would otherwise be, it is nonetheless far from ideal. The alternative would be to create a general equilibrium framework. However, given the degree of structure that would have to be assumed in such a model, it is not clear how much more realistic it would be. Furthermore, these two assumptions counterbalance each other to some extent in that the first would tend to overestimate the impact of regulation, whereas the second would underestimate it.[6]

Three simulations are conducted for each of the five regulation scenarios, estimating consumption expenditures if redundant workers join the pool of informal sector workers, if they join the pool of informal sector workers or the unemployed, and if they join the general pool of workers ages 15–80.[7]

The first two simulations attempt to capture the idea that finding another job will be neither costless nor instantaneous. In Brazil, where labor unions are powerful at the firm level (Amadeo and Camargo 1993 estimate a 30 percent union density) and labor markets are segregated by sector (Carneiro 1998), it seems particularly unrealistic to assume that individuals who lose their jobs in the aftermath of regulation could readily find employment in the formal sector. Although this paints a somewhat pessimistic picture (larger, formal sector firms might be expected to replace the output of regulated small-scale firms) this pessimism is countered by the optimistic assumption that returns to labor in the informal sector are unchanged despite the positive shock to labor supply in this sector following job losses by small-scale industry employees (which might otherwise be expected to dampen wages in this sector.)

A more neutral picture is offered by the third simulation, which bases the estimated consumption of redundant workers on average consumption of the remainder of the working-age population (unemployed and in formal or informal sector employment). The resulting estimate of poverty is not obviously biased in any direction, particularly given the small number of redundant workers.

The Exercise and Econometric Issues

The object of the thought experiment is to understand how poverty among those currently employed in pollution-intensive small-scale industry would evolve as a result of redundancy following the introduction of draconian regulation. Doing that requires an understanding of how employees of polluting small-scale industries would fare if they were to join the pool of informal sector employees and the unemployed. One way of doing this would be to estimate an ordinary least squares (OLS) regression of expenditure on the personal characteristics of the pool of informal sector employees and the unemployed and then use these estimates to predict the expenditures of employees of polluting small-scale industries following regulation.

6. Research from the European Union suggests that the overall employment effect following environmental regulation is most often positive, although a few studies note a neutral or slightly negative employment effect (ILO 1990, p. 42). Therefore, the implicit assumption that the net employment effect is unchanged seems reasonable.

7. Participation in the formal sector is defined as having a license for the self-employed and as holding a work card (*carteira assinada*) for employees. Where this information was unavailable, it is defined as contributing to social welfare (*instituto de previdência*).

However, such an exercise is likely to yield biased estimates. In particular, the unobserved characteristics of employees in the informal sector or of the unemployed may differ systematically from those of employees of polluting small-scale industries. For instance, informal sector employees may lack the connections necessary to acquire more secure employment, and the unemployed might have high reservation wages. Without data on such personal characteristics, therefore, the conditional expectation of the error term in an OLS regression is likely to be nonzero, yielding inconsistent estimators.[8]

To conduct the thought experiment, estimates from the following equation are applied to employees of polluting small-scale industries:

$$E(y_{i2}|x_i, y_{i1} = 1) = x'_{i2}\beta_2 + E(u_{i2}|u_{i1} > -x'_{i1}\beta_1) \tag{1}$$

where x_{i1} is a vector of individual i's personal characteristics, y_{i2} denotes the log of i's per capita expenditure, $y_{i1} = x'_{i1}\beta_1 + u_{i1} = 1$ if i is either unemployed or employed in the informal sector (but not in polluting small-scale industries), and u_i is the error term. Assuming normality of the error terms and integrating equation 1 reduces to:

$$E(y_{i2}|x_i, y_{i1} = 1) = x'_{i2}\beta_2 + \tau\lambda(x'_{i1}[\beta_1/\sigma_1]) \tag{2}$$

The last term in equation 2 is essentially a bias correction term, where $\lambda(z) = \phi(z)/\Phi(z)$ is the inverse Mill's ratio.

The estimation proceeds in three steps. First, Heckman's (1976) two-step method is used in estimating equation 2. Step 1 estimates $\alpha_1 = \beta_1/\sigma_1$ with a probit model. Step 2 regresses y_{i2} on x_i and $\lambda(x'_{i1}\hat{\alpha}_1)$ by least squares for the reference population (the pool of individuals that redundant employees of small-scale polluting industries are expected to join).

Second, the estimates for β_2 are used to predict consumption of the small-scale enterprise employees, were they to join one of the three pools: employees of the informal sector, informal sector employees and the unemployed, and the general working population ages 15–80. Next, residuals (a different one for each household) are drawn from the assumed normal distribution and added to the predicted consumption measures. This exercise is repeated 100 times, and the summary poverty measures are calculated after each draw. The Foster-Greer-Thorbecke (FGT) class of poverty measures is used, with parameter values 0, 1, and 2. A mean over the 100 respective poverty measures provides an estimated poverty rate following the switching out of the employees of the pollution-intensive small-scale enterprises into the hypothetical pools. This yields a measure of predicted poverty in the reference group following regulation, which can then be compared with current (observed) poverty levels.

8. The remainder of the section draws largely from Jakubson (1998).

There is one further econometric issue to consider before estimation. Heckman's two-step method yields consistent estimates for α_1, τ, β_2, and σ_{22}. Although in theory this serves to identify the remaining elements of the parameter variance matrix (σ_2, σ_{12}, and σ_1) even when $x_{i1} = x_{12}$, such identification relies on the functional form (essentially the curvature of the λ function) for identification. This is not an attractive means of identification because there is no theoretical basis for preferring one functional form over another. It is preferable to use an exclusion variable as a means of identification—a variable that affects an individual's employment status but not the expenditure outcome.

The identifying variable used here is the ratio of (other) individuals working in pollution-intensive small-scale industry to the total number of individuals residing in a particular municipality. This ratio is expected to have a negative effect on the chances of observing a worker in the non–pollution-intensive small-scale sector, but there is no reason to expect that it would affect the expenditure levels of workers. The value of this ratio will differ for every household because it is calculated excluding the respective household from both the numerator and denominator of the ratio.

Results

Rather than detail the results from Heckman's two-step method, which largely mirror those presented in table 3, two key points are worth highlighting. First, the probit coefficient on the ratio of individuals working in pollution-intensive small-scale industries within a given municipality is negative and highly significant, suggesting that this exclusion has bite.

Second, the presence of a sample selection problem is confirmed in two ways. First, the coefficient on λ is significant, so the null hypothesis that there is no sample selection problem is rejected. Second, there are a few large differences in coefficients when the expenditure equation is estimated using standard OLS, particularly for the primary education and geographic location coefficients. This suggests that unobservables play an important role in explaining the sector of employment (and unemployment) and thereby incomes.

For these reasons coefficients and residuals from the Heckman two-step method are used to predict expenditures for employees of polluting small-scale industries under all five scenarios. Results of the final stage of the simulation—comparison of poverty before and after the regulation—are reported in table 4, which provides poverty rate calculations of the incidence (*FGT*), poverty gap (*FGT1*), and squared poverty gap (*FGT2*) for individuals ages 15–80 currently employed in pollution-intensive small-scale industries. The base case refers to observed poverty. The informal case refers to predicted poverty if employees of pollution-intensive small-scale industries lose their jobs and find employment in the informal sector subsequent to regulation. The informal and unemployed case adds the currently unemployed to informal sector employees to see what would happen to poverty were redundant workers to join this larger pool. Finally, the general population case looks at what would happen to poverty

TABLE 4. Poverty among Current Employees of Targeted Small-Scale Industries before and after Regulation

	Poverty Incidence			
	(FGT)	(FGT1)	(FGT2)	Number of poor
Scenario 1[a]				
455 observations				
Base case	0.377	0.128	0.062	907,198
Estimated case				
Informal sector	0.476	0.195	0.106	1,144,159
Informal and unemployed	0.472	0.308	0.238	1,134,947
General population	0.449	0.289	0.220	1,080,376
Scenario 2[b]				
455 observations				
Base case	0.374	0.130	0.064	890,745
Estimated case				
Informal sector	0.487	0.319	0.246	1,162,498
Informal and unemployed	0.482	0.314	0.242	1,148,226
General population	0.447	0.286	0.217	1,064,800
Scenario 3[c]				
480 observations				
Base case	0.368	0.125	0.061	929,742
Estimated case				
Informal sector	0.489	0.323	0.250	1,236,802
Informal and unemployed	0.482	0.315	0.243	1,218,401
General population	0.443	0.286	0.219	1,119,576
Scenario 4[d]				
365 Observations				
Base case	0.255	0.084	0.037	300,260
Estimated case				
Informal sector	0.477	0.312	0.241	561,756
Informal and unemployed	0.467	0.306	0.236	550,959
General population	0.437	0.282	0.215	514,844
Scenario 5[e]				
102 Observations				
Base case	0.254	0.095	0.046	99,971
Estimated case				
Informal sector	0.492	0.324	0.251	193,575
Informal and unemployed	0.489	0.322	0.248	192,559
General population	0.449	0.289	0.220	176,745

[a]Small-scale industries contributing 50 percent or more of their industrial sector's air pollution (PM_{10}).

[b]Small-scale industries producing higher PM_{10} emissions per employee than large-scale industries in their sector.

[c]Small-scale industries producing higher total PM_{10} emissions than large-scale industries in their sector.

[d]Small-scale industries in six cities with high air pollution (population times PM_{10} concentration): São Paolo, Rio de Janeiro, Belo Horizonte, Slavador, Fortaleza, and Recife.

[e]Small-scale industries that produce more than 1,000 metric tons of PM_{10} emissions in six cities with high air pollution (population times PM_{10} concentration).

Source: Authors' calculations based on 1996/97 PPV for Brazil and the SNIFF database on air pollution were redundant workers in targeted small-scale industries to join the general working population ages 15–80.

Scenarios 1–3 describe base case and estimated poverty rates if small-scale industries are targeted on the basis of their contribution to pollution. Although different groups of individuals are targeted under each scenario, neither the base case nor the estimated poverty rates are significantly different across groups. Base case poverty lies between 37.7 and 36.8 percent among these three target groups. Were the targeted small-scale industries to be shut down and redundant workers to join the pool of informal sector workers, poverty among the targeted groups would increase by between 26 percent and 33 percent, raising poverty rates to between 47.6 percent and 48.9 percent—corresponding to between 230,000 and 290,000 more people in poverty. If redundant workers joined the group of unemployed and informal sector employees, poverty would increase by slightly less (between 25 percent and 31 percent). Although this may seem counterintuitive, in Brazil most of the unemployed are former formal sector workers, who alone are eligible for unemployment benefits, which are often a percentage of previous earnings. As a result, the unemployed may well be better off than many who are employed but working in the informal sector. Finally, the increase in poverty is significantly lower under the more optimistic scenario in which redundant workers join the general working age population. Even then, however, poverty rises by between 19 percent and 20 percent, or an additional 173,000–180,000 people.

At just over 25 percent, base case poverty is much lower in scenarios 4 and 5, which target small-scale industries by geographic location. Increases in poverty are much more dramatic, however. When regulation shuts down all small-scale industries in São Paolo, Rio de Janeiro, Belo Horizonte, Salvador, Fortaleza, and Recife, poverty increases by 87 percent when redundant workers join the informal sector, by 83 percent when they join the pool of informal sector workers and the unemployed, and by 72 percent when they join the general working age population. The increase in poverty is 6, 9, and 5 percentage points higher in each case when the target group in these cities is restricted to small-scale industries producing more than 1,000 metric tons of particulate emissions. Narrowing the target population to the most polluting small-scale industries results in at least 300,000 fewer individuals in poverty following regulation. Indeed, basing targeting on geographic criteria rather than broader aggregate pollution criteria reduces both the number of individuals targeted and the estimated number of poor people.

In addition to a rise in the incidence of poverty, there is a marked deepening of poverty, irrespective of the assumed pool into which redundant employees fall, especially under scenarios 4 and 5. The increase in the poverty gap (*FGT1*) and squared poverty gap (*FGT2*) vary dramatically depending on the target group and the assumptions regarding which pool redundant workers join. The increase in the poverty gap ranges from 52 percent (when redundant workers from small-scale industries that contribute to at least 50 percent of overall sectoral pollution join the informal sector) to 270 percent (when redundant workers from small-scale industries in the six major cities join the informal sector). The corresponding increases for the poverty gap squared are 69 percent and 543 percent.

V. Conclusion and Policy Implications

The analysis in this article indicates that the contribution of small-scale industries to pollution in Brazil is far from marginal. Such firms can be viewed as disproportionately large polluters in the sense that they contribute more to overall industrial air pollution than they do to industrial employment. Indeed, in the data used here, small-scale industries are responsible for the bulk of industrial pollution in Brazil. There is thus a clear environmental rationale for looking closely at pollution control of small-scale industries. Such regulation may have an additional welfare benefit to the extent that firms brought under environmental regulation also impose pollution-related health costs on their employees and on nearby populations. However, the environmental and health benefits from regulation would have to be weighed against the potential for an accompanying increase in the incidence of poverty.

The simulations suggest that shutting down the small-scale industries that produce the most air pollution (as described in scenarios 1–3) would reduce particulate emissions by more than 260,000 metric tons annually, amounting to a 58 percent reduction in aggregate air pollution.[9] The target group in such a scenario would, however, cover the bulk of small-scale employees, and closure of these firms would result in an estimated 200,000 additional people in poverty—a more than 25 percent increase in the poverty rate for the current employees of the targeted small-scale industries.

If regulation is targeted instead to small-scale industries in major cities, the simulated increase in the number of people in poverty following closure of such firms is smaller. However, there is a dramatic increase in the poverty rate and a smaller drop in particulate emissions of only 100,000 metric tons. Narrowing the target group further to the most polluting small-scale industries in major cities also increases the poverty rate substantially, although the number of newly poor people is relatively low: roughly one-half to one-third of that under the other four scenarios. This comes at a cost of an even lower reduction in particulate emissions of roughly 90,000 metric tons.

The simulations thus suggest that in terms of both environmental benefits and distributional costs, draconian regulation of small-scale industries could be

9. This is an unreasonably optimistic outcome. In reality, if pollution-intensive small-scale industries were shut down as a result of draconian regulation, large-scale firms operating in the same sectors would expand their production to accommodate aggregate demand. This expansion of production would be associated with an increase of pollution (albeit possibly less than the reduction accompanying the departure of the pollution-intensive small-scale industries).

expected to have quantitatively important consequences. The estimated poverty impacts warrant reflection. The results show that on balance employees of polluting small-scale industries are substantially better equipped than most others to cope with the potential repercussions of regulation, such as finding a new job should they lose their current one. Although they are not the best educated, they enjoy relatively high levels of education, they are more likely to live in areas with greater employment opportunities, and they are more likely to belong to more advantaged racial groups. If this group fares so badly when regulation pushes them out of employment, more disadvantaged groups—such as agricultural or construction workers—could suffer even more if they were to be subjected to similar environmental regulation. Any regulation aimed at such groups—such as measures to contain deforestation or to limit urban growth—should be approached with sensitivity to the potential consequences for the poor.

In thinking about the potential environmental benefits from regulation of small-scale industries, it is important to emphasize that numerous reasons remain to focus as well on large-scale industries. In the face of international pressure and growing private sector understanding that environmental degradation is in many instances reaching unsustainable proportions, governments face a choice of which industries to regulate rather than whether to regulate. Although small-scale industries may well be responsible for more air pollution than large-scale industries, promoting small-scale industries while regulating large-scale industries may be a preferable means of reaping propoor, pro–environmental growth. The incidence of poverty tends to be substantially lower in large-scale industries than in small-scale industries and among workers at large, and the poor are significantly underrepresented in this group. The rationale for regulating large- rather than small-scale industries is bolstered when the higher costs of monitoring small-scale industries is also considered. Finally, large firms are probably also less likely to impose health costs on the poorest members of society for reasons of technology and location.

The picture painted in this study looks stark. But the scenarios described are extremely tentative and possibly unrealistically grim. First, the pollution coefficients for Mexico may not be appropriate for Brazil. Second, it is most unlikely that regulation would be so draconian as to force polluting small-scale industries to shut down altogether. Indeed, empirical evidence on the net employment effect of the environmental regulation seen in practice is mixed (ILO 1990). Third, the fact that employees of polluting small-scale industries are reasonably well endowed in human capital means that they are likely to be reasonably well equipped to find jobs, possibly even in the formal sector. Assuming full-scale redundancy and minimal opportunities may therefore be regarded as a worst-case scenario.

However, it is also clear from the greater responsiveness to the simulations of the distribution-sensitive poverty measures that within the pollution-intensive small-scale sector there are many workers who are well below the poverty line.

In the event of less draconian regulation, these workers would likely be the first to be made redundant (because of less bargaining power or fewer and weaker networks within the firms). If so, these workers would also be particularly poorly placed for finding formal sector jobs.

The main message from this study is a cautionary one. Attention to small-scale enterprises as a means of promoting propoor growth must be traded off against the potentially non-negligible environmental costs imposed by small-scale industries. At the same time, policymakers should be wary of environmental regulation of small-scale industries because of the potential consequences for poverty. The tradeoffs might be avoided if efforts to improve the business climate for small-scale enterprises focused on the small-scale services sector. This sector, also important to the poor, does not impose the same types of environmental or health costs that small-scale industries do.

There has been virtually no critical debate over the current popularity of promoting small-scale enterprises as a means of stimulating propoor growth. There are clearly many more questions that remain unanswered. What is the real contribution to overall air pollution by small-scale industries in Brazil? What about other types of pollution? What would happen if regulation were less draconian? What kind of regulatory instruments are available? Should efforts to promote the small-scale sector be focused on services rather than industry? What other tradeoffs are there besides an environmental one? This article is one attempt to begin the discussion and to encourage research.

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