ARE THE POOR PROTECTED FROM BUDGET CUTS?
EVIDENCE FOR ARGENTINA

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Macroeconomic adjustment programs often emphasize the need to protect social spending from cuts, and to protect pro-poor spending in particular. But does this happen in practice during fiscal contractions? The paper presents evidence for Argentina. Using aggregate time series data the paper first finds that social spending was not protected historically, although more "pro-poor" social spending was no more vulnerable. Turning next to new data for an externally-financed workfare scheme introduced in response to a macro crisis, the paper finds that this program was far better targeted than other social spending. However, it appears that the program still had to assure that a small but relatively well-protected share of its benefits went to the non-poor. This appears to be a political economy constraint.

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I. Introduction

It is now common for macroeconomic adjustment programs to call for a pro-poor shift in the *composition* of public spending — in combination with

an overall fiscal contraction, as usually called for to assure macro stability.[1] Donors have been particularly keen to support new public anti-poverty programs and "social funds" that claim to target extra assistance to the poor at times of crisis and adjustment.

The case for such action to protect pro-poor social spending rests on the answers to a number of questions: Do cuts tend to fall more heavily on the social services that matter most to the poor? When cuts are borne by the poor, do they gain similarly from expansions? Do add-on "crisis programs" help the poor? What happens when such programs are also cut?

The literature has offered little guidance on the answers. One theoretical argument that has been made is that targeting spending to the poor can undermine political support for the taxation needed to finance that spending; the poor might even end up worse off (Gelbach and Pritchett, 1997; De Donder and Hindriks, 1998).[2] However, broad political support for greater targeting is possible when there is an exogenously imposed spending cut, which brings tax savings to the non-poor (Ravallion, 1999a). Even when the poor have no power over how cuts are implemented, it is theoretically possible that they will be protected from cuts without further intervention. The outcome depends on the preferences of those in power, notably the extent to which they gain directly from public spending on the poor, and (less obviously) how quickly the marginal utility of their spending on the poor declines relative to the marginal utility of spending on themselves (Ravallion, 1999a). Nor is it clear that the poor will be powerless even when they are a minority. They may be able to form a small but influential special interest group, represented by Non-Governmental Organizations, or they may be able to form a coalition with non-poor sub-groups who see it as in their interests to not have the burden of cuts fall on the poor. Even when the poor are a relatively powerless minority,

[1] See for example the World Bank's recent adjustment loan to Argentina (World Bank, 1998). Also see the discussion in Lustig (2000).

[2] Also see the discussions in Besley and Kanbur (1993), Sen (1995) and van de Walle (1998).

the incidence of cuts is unclear on *a priori* grounds given the external costs of poverty to non-poor people (Ravallion, 2000a).

Argentina provides an interesting case study for investigating these issues empirically. The country has undergone a number of sharp fiscal contractions over the last two decades. And good data are now available for studying the impacts of aggregate fiscal contractions on the composition of spending. The paper studies two very different sources of data. Firstly, the following section uses aggregate times series data on public spending allocations to see how the composition of spending changes with aggregate contraction and expansion. Secondly, the paper turns to a new data set (constructed for the purpose of this paper) on one of the programs explicitly introduced by the Government of Argentina (with support from the World Bank) to deal with the effects of a macro crisis on the poor. Section III describes the program and how its performance in reaching the poor is to be measured, while section IV tests how the program's performance in reaching poor areas was influenced by spending cuts. Section V offers some conclusions.

II. Social Spending in Argentina during Fiscal Expansions and Contractions

While methods of measurement differ, it is widely agreed that in the mid-1990s less than 30% of the population is poor by Argentinean standards (World Bank, 1999). The level of "social spending" averaged 56% of total government spending in the period 1980-97 (Government of Argentina, 1999). Less than half of this went to "social services" (education, health, water and sewerage, housing and urban development, social assistance, and labor programs); the remainder can be labeled "social insurance" (pensions, public health insurance, unemployment insurance). Spending on social services is believed to be pro-poor, in that the poorest x% of households receive more than x% of spending, but this is not so for social insurance (Gasparini, 1999; Llach and Montoya, 1999). Table 1 reproduces recent estimates of the incidence of public spending on social insurance as distinct from the social services. The results confirm

that social service spending is more pro poor than social insurance. Access to social insurance in Argentina typically requires that one has a job in the formal segment of the labor market, which is less than half of the workforce, and relatively few of the poor (World Bank, 1999).

Table 1. Incidence of Social Spending and Taxes in Argentina 1996

Shares of spending and taxes attributed to quintiles of households ranked by income per person:

	1 poorest	2	3	4	5	Total
Social services	29.8	18.8	21.7	16.8	13.0	100
Social insurance	9.90	20.6	19.5	23.6	26.5	100
Total social spending	21.8	19.5	20.8	19.5	18.4	100
Taxes	7.10	10.7	14.9	20.1	47.2	100
Income shares	4.00	8.40	13.2	21.2	53.2	100

Source: World Bank (1999), quoting Gasparini (1999); estimates for urban Argentina in 1996.

There is evidence that spending on social services in Argentina has responded more to changes in national income than has social insurance. Wodon and Hicks (2000) study the effects of changes in GDP on targeted spending on social services (about 4% of total government spending). They find that the ratio of targeted public spending to the number of poor had a positive elasticity (of about three) to GDP; in recessions, there were more poor people, and less was spent on them.

The political regime is also likely to matter. For a sample of Latin American countries (including Argentina), Brown and Hunter (1999) find that democracies are more likely to protect social spending in a recession, but that authoritarian regimes are more inclined to expand social spending when the

crisis is over.[3] Argentina has been a democracy since 1983. The data series I will use start in 1980, and so it is reasonable to ignore the change in regime.

In this setting it is of interest to examine how changes in the government's total budget affected the level and composition of social spending. Did budget cuts have similar effects to budget expansions? Were the categories of spending that are known to matter to the poor more protected than other types of spending?

Figure 1. Total Public Spending and Social Spending in Argentina 1980-97 (changes in logs)
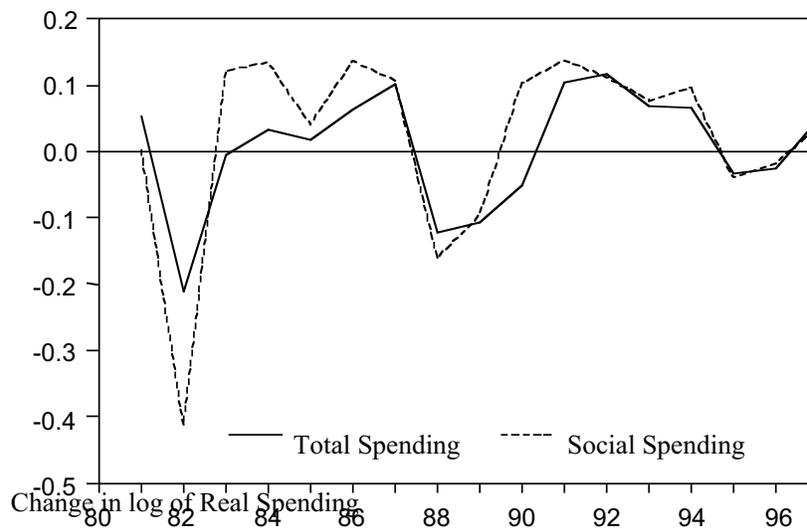
Figure 1 offers a direct test for whether social spending has been protected from budget cuts. The figure plots the time series of changes in the log of total public spending ("Gasto Público Consolidado") and the log of social spending

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[3] This study does not, however, control for the level of total spending, so it is unclear whether the identified effects operate through the composition of spending or its aggregate level; "non-social spending" may well behave the same way as social spending.

(both in 1997 prices) as compiled by Government of Argentina (1999) and covering the period 1980-97.[4] Unlike most other compilations of public spending data, this one includes all levels of government. In this and other respects, considerable care appears to have gone into constructing the data.

There is clearly little sign that social spending was protected from cuts. Indeed, there is sizable co-movement, with indications that (if anything) social spending was more volatile than other types of spending. One can see quite large proportionate declines in social spending in every year in which total spending falls. On the other hand, one often sees smaller (and sometimes negative) changes in social spending when total spending rises.

Let G_t^S denote social spending at date t, and G_t total spending, one can test whether the elasticity to an increase in total spending differs from that for a decrease using the following regression:

$$\Delta \ln G_t^S = \alpha + [\gamma_1 \delta_t + \gamma_2 (1 - \delta_t)]\Delta \ln G_t + \varepsilon_t \tag{1}$$

where α is a time trend, γ_1 is the elasticity when total spending increases, γ_2 is the elasticity when it falls, and $\delta_t = I(\Delta \ln G_t)$ takes the value unity when $\Delta \ln G_t > 0$ and zero otherwise.

The data in Figure 1 yield an estimate of 0.14 for γ_1. This is not significantly different from zero (the standard error is 0.37). On the other hand, the estimated elasticity to a decrease in total spending (γ_2) is 2.14, which is significantly greater than one (the standard error is 0.26). Social spending responds elastically to aggregate cuts, but the responsive to fiscal expansions is not statistically significant.[5] The constant term of 0.086, which is significant (t-ratio = 2.83), indicating a sizable independent trend increase in the share

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[4] Both time series are highly serially correlated; indeed, augmented Dickey-Fuller tests do not reject the unit root hypothesis for either variable at even the 10% levels. So the following analysis will focus on changes from year to year rather than levels.

[5] The average elasticity (constraining γ_1 and γ_2 to be equal) is 1.366, with a standard error of 0.21; however, the restriction that $\gamma_1 = \gamma_2$ performs poorly (t = 3.16).

of social spending. By contrast, non-social spending was well protected; the elasticity when total spending fell was 0.09, and not significantly different from zero (a standard error of 0.22); on the other hand, the elasticity to an increase was 1.68 (standard error of 0.41).

While these elasticities are of descriptive interest, their causal interpretation requires that we believe that changes in total spending are uncorrelated with the error term ε_t in (1). To test for the causal effect on social spending, I assume that the elasticity to higher spending is in fact zero (on the grounds that the OLS results are so strong that it is difficult to believe they are not robust in this respect). Under this assumption, I use lagged values of both social spending and other spending as instruments for cuts in total spending. (The instruments were jointly significant in the first-stage regression.) The resulting 2SLS estimate for the elasticity of social spending to a cut in the total budget is 2.28 with a standard error of 0.27. Again, not only is social spending not protected, its elasticity to cuts exceeds one, implying a fall in the share of social spending during fiscal contractions.

This protection of "non-social" spending does not however mean that the non-poor shift the cuts to the "powerless poor". Social spending in Argentina includes types of spending that matter more to the non-poor than the poor, such as social insurance. Also, there may be pro-poor changes in the composition of social spending, dampening the marginal impact on the poor.[6]

To see how the composition of social spending changes with cuts, Table 2 reports estimates of equation (1) for various categories of social spending. The same pattern is evident in almost all spending components; social spending responds elastically to cuts in the total budget, but does not respond significantly to budget increases. The only exception is housing and urban spending, which does not respond differently to an increase in total spending versus a decrease, and the coefficient is not significantly different from zero.

The elasticity to budget cuts is very similar for social services as social

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[6] There is evidence (for India) that spending composition is not homogeneous in the level of spending (Lanjouw and Ravallion, 1999).

Table 2. Elasticities of Social Spending to Total Public Spending in Argentina

| Spending category | Sub-categories | Share of total spending (%) | Elasticity to a change in total public spending | | 2SLS estimate of the elasticity for a decrease in spending |
			Increase in total spending	Decrease in total spending	
Social insurance		32.38	0.070	2.050*	2.240*
			(0.396)	(0.368)	(0.340)
	Social care (incl. pensions)	21.24	-0.129	2.243*	2.449*
			(0.533)	(0.569)	(0.509)
	Health	8.36	0.321	1.698	1.773
			(0.561)	(0.376)	(0.438)
	Work (incl. unemploy. comp.)	1.88	0.704	2.904*	3.637
			(2.068)	(0.937)	(1.691)
Social services		23.30	0.246	2.255*	2.343*
			(0.543)	(0.332)	(0.389)
Social services (excl. housing and urban)		21.43	0.386	2.327*	2.589*
			(0.481)	(0.320)	(0.367)
Sector classification of social services					
	Education	11.47	0.270	2.283*	2.328*
			(0.591)	(0.398)	(0.435)
	Health	4.83	0.740	2.098*	2.678*
			(0.578)	(0.333)	(0.443)
	Housing and urban	1.87	0.444	0.444	0.551
			(0.524)	(0.524)	(1.158)
	Social assistance (incl. family allowances)	1.76	0.377	2.992*	3.650*
			(0.912)	(0.611)	(0.797)

Table 2. (Continued) Elasticities of Social Spending to Total Public Spending in Argentina

Spending category	Sub-categories	Share of total spending (%)	Elasticity to a change in total public spending		2SLS estimate of the elasticity for a decrease in spending
			Increase in total spending	Decrease in total spending	
	Employment programs	0.15	1.698 (1.239)	2.740* (0.591)	4.515* (1.355)
Targeted/universal classification of social services					
	Targeted	4.09	-0.488 (0.876)	2.200* (0.554)	1.587 (0.726)
	Targeted (excl. housing and urban)	2.22	0.220 (0.834)	3.009* (0.568)	3.422* (0.695)
	Universal	19.21	0.398 (0.542)	2.267* (0.343)	2.505 (0.402)
Total		55.68	0.138 (0.368)	2.140* (0.260)	2.277* (0.272)

Notes: Regressions of the change in the log of each spending category on the change in the log of total public spending, with intercepts, estimated on annual data for 1980-97. 2SLS estimator uses lagged total spending and lagged social spending as the instruments; the dummy variable for whether total spending has decreased is used as its own instrument. The F-test for the first stage regression was 4.43, significant at the 3% level. White standard errors in parentheses; * indicates significantly different from one at the 5% level.

insurance.[7] The table also gives the 2SLS estimate of the elasticity to budget cuts. The most notable difference is that spending on employment programs becomes highly elastic to cuts; these include the Trabajar Programs we will study in depth in the next section.

This uniformity in the elasticities to budget cuts between social insurance and social services is inconsistent with the idea that cuts will simply be passed onto the categories of spending that matter most to the poor. As we have seen from Table 1, middle (and upper) income groups are likely to benefit relatively more from pensions and (formal sector) unemployment compensation than the poorest quintile. The non-poor might also be expected to resist cuts to these categories of spending given that formal social insurance spending is heavily pre-committed, and hence harder to cut. Yet we find that the proportionate cuts are just as great for social insurance as social services; the absolute cuts are in fact higher for social insurance, given that it accounts for a higher share of the budget.

A possible explanation for this result is that the benefits to the non-poor from social services are tied to consumption by the poor. It is hard to cut spending on schools without also hurting the non-poor. Inability to finely target many social services thus helps protect the poor from differentially higher cuts, even though the non-poor benefit proportionately less from this type of spending than from social insurance.

This begs the question: did more targeted categories of social services receive heavier cuts? Government of Argentina (1999) provides a classification of social service spending according to whether it is "targeted" or "universal". The targeted programs are housing and urban programs, social assistance and employment programs; on average, these account for 17.7% of spending on social services. Table 2 also gives the estimates of γ_1 and γ_2 classified this way. The elasticity to total spending cuts is not any higher for the targeted

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[7] The difference between social insurance and social services in the elasticities is not significant (t = 0.30); nor can one reject the null that the parameters are jointly the same (F = 0.28).

components of social services; indeed, if anything, the elasticity is higher for universal social services.

However, this conclusion is sensitive to the classification of "targeted" spending. Table 2 also gives separate estimates for two of the components of targeted social service spending, namely social assistance and employment. Both have high elasticities to a fall in total spending. If one excludes housing and urban from the targeted component we also find a relatively high elasticity. So these results do offer some support for the conclusion that targeted social spending is more vulnerable to fiscal contraction.

III. Tracking Expansion and Contraction in an Anti-Poverty Program

The above results do not suggest that social spending provided a good safety net for Argentina's poor at times of fiscal adjustment, given how exposed that spending was to aggregate cuts, and how little of it went to the poor at normal times. This provides a strong motivation for looking to alternative programs that might better reach the poor in a crisis. We shall now study one such program in depth. The program was picked because of the unusually rich data available, and the fact that these data cover a period in which the program both expanded and contracted. We will examine how well the program performed in reaching the poor in a crisis, and see how its performance changed with both aggregate expansion and contraction, exploiting the fact that this happened differently in different provinces.

A. The Trabajar Programs

The Government of Argentina introduced the Trabajar Program in 1996, in the wake of a sharp rise in unemployment, and evidence that this was hurting the poor more than others. In May 1997 the unemployment rate for the poorest decile of households (ranked by household income per capita) in Greater Buenos Aires was 40% versus 17% on average. The Trabajar Programs also followed a period of declining social spending (Figure 1).
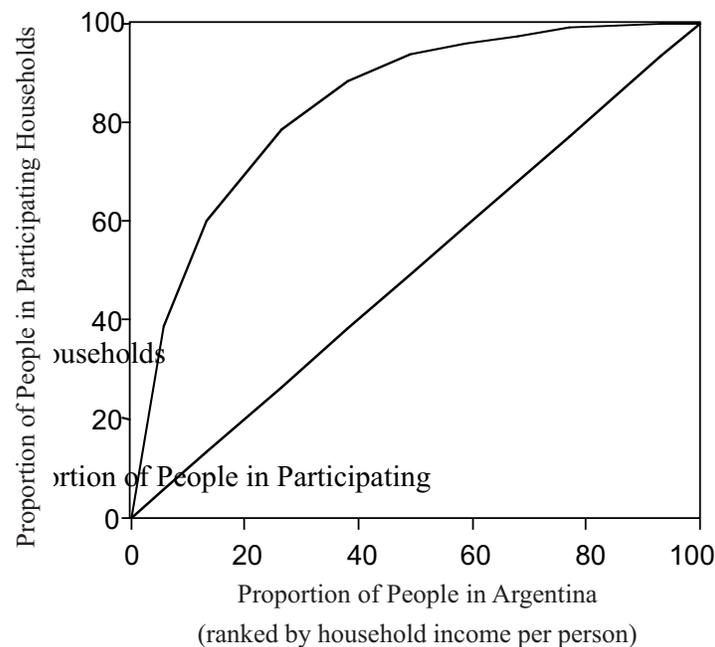
The program's aim was to reduce poverty by providing relatively low wage work on community projects in poor areas. The central government pays for the wage cost, and local or provincial governments cover the non-wage costs. Within provincial budget allocations, proposals for sub-projects compete for central funding according to a points system. Three versions of the program have been tried since then, Trabajar I, II and III. In terms of design, Trabajar II (TII) and III are more similar to each other than either are to Trabajar I (TI). There were substantial design changes between Trabajar I and II. The inter-provincial allocation of spending was reformed, moving away from a largely political process to an explicit formula based on the estimated number of poor unemployed workers in each province. TII also put greater emphasis on creating assets of value to poor communities. Poverty measures were included in the center's budget allocation rules and in the selection criteria for sub-projects. The poverty focus was also made clearer to provincial administrators. TIII was very similar to its predecessor in design. The main difference was that greater emphasis was placed on the quality of sub-projects, to assure that the assets created were of value to the communities. The World Bank has supported TII and TIII by loans (disbursed against the wage payments), and through technical support on program design, monitoring and evaluation. All results quoted for TIII in this paper relate to the first 16 months of its operation, up to November 1999.

From the point of view of this paper, an important difference between the three versions of the program is in the level of funding. In Trabajar I, disbursements by the center (covering wages for participating workers) averaged $77 million per annum; for TII this rose to $160 million per annum, and it then fell to $98 million per annum under TIII. As we will see, there were also differences in levels of funding between sub-periods.

Survey-based impact evaluation methods have been used to assess the gains to participating workers and their families from TII and TIII. Propensity-score matching methods were used to construct a comparison group to surveyed Trabajar participants from an identical national sample survey implemented at the same time. Income gains were then estimated by comparing

incomes of the Trabajar participants with the matched comparison group. The results have indicated that Trabajar jobs are well targeted to the poor; Figure 2 gives the concentration curve for worker participation in the program. This was estimated by locating the families of a random sample of 3,500 Trabajar workers within the national distribution (based on a sample of 22,000 families).[8] For example, 76% of people living in the households of participating workers had a household income per capita that placed them amongst the poorest 20% of Argentineans nationally.

Figure 2. Concentration Curve of Participation in the Trabajar II Program

Proportion of People in Participating Households

Proportion of People in Argentina
(ranked by household income per person)

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[8] Identical surveys were used for the program participants and the national sample, and the surveys were implemented at approximately the same time; for details see Jalan and Ravallion (1999).

How does this incidence of income gains compare to other social spending in Argentina? Table 3 gives the concentration curves for the Trabajar program and both aggregate social service spending and social insurance, all on a household basis (to assure that the Trabajar concentration curve is comparable with the numbers in Table 1). Since there is very little variation in the Trabajar wage rate, the concentration curve based on participation is also the benefit incidence curve for gross wage payments.[9] It can be seen from Table 3 that the direct income gains from the program were far better targeted than social insurance and social services as a whole. The program's targeting also appears to be better than any other targeted programs in Argentina. Amongst the programs for which incidence calculations are given in World Bank (1999, Table 3.7), the next best performance was for programs directed at pregnant mothers and children, for which 70% of the benefits went to the poorest quintile of households, which was itself an unusually good performance compared to other programs.

Of course, targeting performance is only one factor in assessing the performance of such programs in reducing poverty (Ravallion, 1999b). A program such as Trabajar is designed to help in one dimension of poverty, while programs directed at health and nutrition of the poor help in quite different dimensions; both types of programs can have important roles. Nor does targeting performance tell us anything about net income gains. Using propensity score matching methods, Jalan and Ravallion (1999) estimate the net income gains from the Trabajar program, allowing for foregone income (mainly from part-time "odd-jobs"). The net income gains to participating workers represented 50% of the gross wage gains on average (Jalan and Ravallion, 1999). (Factoring in foregone income mainly affects the concentration curve below the 20th percentile.)

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[9] Again this does not net out foregone income, though nor do the standard benefit incidence calculations in Table 1 take account of behavioral responses. However, as noted above, factoring in foregone income mainly affects the concentration curve below the 20th percentile (Jalan and Ravallion, 1999).

Table 3. Selected Points on the Concentration Curves

Poorest x% of households ranked by income per person; x =	Proportion of Trabajar households with an income per person that places them in the poorest x % nationally	Cumulative share of benefits from social services	Cumulative share of benefits from social insurance
20	76	30	10
40	92	49	31
60	97	70	50
80	99	87	74

Notes: For comparability with Table 1 the figures for Trabajar participants are households not people (Figure 2 is people not households). Sources: As for Table 1, except for Trabajar participation which is from Jalan and Ravallion (1999).

Such calculations relate solely to the benefits from the work provided by the scheme. There are also indirect benefits from the assets created. While non-poor people are unlikely to find the Trabajar wage attractive, they would no doubt like to have the scheme producing things of value in their communities. (There is negligible cost recovery.) How well did the program perform in assuring that the work was provided in poor areas? How did this change when the program expanded and contracted?

One can monitor the extent to which the program reached poor areas, by tracking the geographic distribution of disbursements and comparing this to the poverty map of Argentina. By doing so within a period of budget expansion then contraction, and comparing the results across provinces, we will be able to test for budget effects on this aspect of the programs' poor-area targeting performance. The following section outlines the method.

B. Assessing Poor-Area Targeting Performance

Each provincial government's optimal allocation to a household is unobserved, but it is assumed to depend on the household's level of welfare. That may in turn depend on where the household lives, but I assume that the poverty rate in the area where it lives does not matter to a household's allocation independently of its own level of welfare. In other words, there is no "poor-area bias" in that a poor person living in a poor local-government area expects to get the same amount from the program as an equally poor person living in a rich area of the same province. (The allocations need not be identical, but only equal in expectation; random deviations are allowed.) The same holds for the non-poor. This assumption can be thought of as a form of horizontal equity within provinces (Ravallion, 2000b).

Let us consider how to measure each province's performance, making this assumption of horizontal equity in expectation within the province. The central government allocates a total budget of G per capita across M provinces such that G_j per capita is received by province j. After that, each province decides how much should go to the poor versus the non-poor. The chosen allocation by province j is G_j^n per capita for the non-poor and G_j^p for the poor. Province j comprises M_j local government areas, called "departments". The per capita allocations to department i (=1,.., M_j) within province j can be written as:

$$G_{ij}^n = G_j^n + \varepsilon_{ij}^n \quad \text{and} \quad G_{ij}^p = G_j^p + \varepsilon_{ij}^p \tag{2}$$

where the ε's are departmental deviations from province means.

Total disbursements to the poor and non-poor must exhaust the budget. This creates an accounting identity linking total program expenditure per capita to the poverty rate in a department. Let G_{ij} denote program spending in the i'th department of the j'th province, and let the corresponding poverty rate be H_{ij} — the "headcount index", given by the proportion of the population that is poor (for which the overall poverty rate in the province is H_j). Then:

$$G_{ij} = H_{ij}G_{ij}^p + (1 - H_{ij})G_{ij}^n \tag{3}$$

Using equation (2) we can re-write (3) in the form of a simple linear regression across all departments in province j:

$$G_{ij} - G_j = T_j(H_{ij} - H_j) + \nu_{ij} \tag{4}$$

where

$$\nu_{ij} = \varepsilon_{ij}^n + (\varepsilon_{ij}^p - \varepsilon_{ij}^n)H_{ij} \tag{5}$$

and $T_j = G_j^p - G_j^n$ is the absolute difference between the average allocation to the poor and that to the non-poor in province j. If T_j is negative then the program favors the non-poor in absolute terms; if T_j is positive, then the program favors the poor, and the higher the targeting differential, the more provincial spending favors the poor.

How can the targeting differential be estimated? Under the horizontal equity assumption, the error term in (5) has zero mean for any given province and is uncorrelated with H_{ij} since the ε's are zero-mean errors within any given province and are uncorrelated with H_{ij} (and its squared value). Thus H_{ij} is exogenous in (4) and so one can estimate T_j from an OLS regression of G_{ij} on H_{ij} across all departments within a given province.[10]

Provincial performance in reaching poor areas can thus be measured by the regression coefficient of spending per capita on the poverty rate, estimated across all departments in each province. Call this the "targeting differential" (TD) for province j. This is estimated by:

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[10] Equation (5) indicates that the error term will not be homoskedastic. Standard errors of the targeting differential were corrected for heteroscedasticity.

$$T_j = \frac{\sum_{i=1}^{M_j}(G_{ij} - G_j)(H_{ij} - H_j)}{\sum_{i=1}^{M_j}(H_{ij} - H_j)^2} \tag{6}$$

One can similarly define a national inter-departmental targeting differential, by calculating (6) over all departments nationally (ignoring province boundaries).

The targeting differential can be interpreted as a measure of absolute progressivity, namely the difference between per capita spending on the poor and that on the non-poor. A TD of zero indicates that there is no difference in Trabajar spending on the poor versus non-poor. A positive TD means that the program favors poor areas; a negative coefficient means it favors non-poor areas. Poverty is measured by the proportion of the population deemed to have unmet basic needs (UBN), based on the 1991 census.

The overall targeting differentials across all 510 departments were $41, $110 and $76 per capita for TI, TII and TIII respectively; all three are significant at the 1% level. To help interpret these numbers, compare the poorest department, namely Figueroa (in Santiago Del Estero province) where the incidence of unmet basic needs is 75.5%, with the least poor department, namely Chacabuco (in Chaco province) where the poverty measure is 3.3%. The expected difference in spending was $30 under TI, $79 under TII, and $55 under TIII.

So the expansion to the program between TI and TII was associated with a more pro-poor allocation of funds geographically, while the contraction between TII and TIII came with a less pro-poor allocation. Next we will see if this aggregate correlation is borne out when we compare provinces over times.

IV. Program Spending and Poor-Area Targeting across Provinces

With the extra degrees of freedom made possible by exploiting the changes

in the inter-provincial allocation of spending, it is possible to test for statistically significant effects of fiscal expansion and contraction on the program's targeting performance. The better information system for TII and TIII allows a breakdown of the aggregates into sub-periods by province. Intervals of five months were chosen. The working paper version of this paper gives the detailed breakdown of the aggregate targeting differentials by these intervals, as well as program spending per capita for each five-month period (Ravallion, 2000a).[11]

To assess the effect of the cuts on targeting performance, one can regress the province and period-specific targeting differentials on program spending per capita across provinces, pooling all five-month periods and all provinces. The targeting differential will, however, vary across provinces according to other factors, such as the strength of provincial concern for the poor, how poor the province is as a whole (Ravallion, 1999c), the history of the provincial efforts at targeting the poor, and the capabilities of local managers. It is not implausible that some or all of these variables will also be correlated with program spending. So their omission will yield a biased estimate of the effect of cuts on targeting performance. However, this problem can be dealt with by treating these differences in provincial targeting performance as provincial fixed effects when estimating the impact of program spending.

Given these considerations, the test for the effect of changes in program disbursements on targeting performance takes the form of a regression of the province and date-specific targeting differential on aggregate spending per capita in the province and a set of province-specific effects. The regression is thus:

$$T_{jt} = \cdot + , G_{jt} + \acute{A}_j + \ddot{I}_{jt} \qquad (j = 1, .., 22; \ t = 1, 2, 3,..) \tag{7}$$

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[11] This is an extended version of the data used in Ravallion (1999). The latter paper only used data for TII. Adding TIII more than doubles the number of degrees of freedom in the data.

where T_{jt} is the targeting differential for province j at date t, G_{jt} is spending by province j at date t, η_j is the province-specific effect and μ_{jt} is an innovation error, representing random, idiosyncratic, differences in targeting performance uncorrelated with spending. As discussed above, the aggregate spending allocation G_{jt} is allowed to be endogenous in that it is correlated with the province effect η_j. It is assumed that $cov(G_{jt}, \mu_{jt}) = 0$. This would not hold if program spending was adjusted according to targeting performance. However, this would have been difficult given the timing of data availability. In a meeting with the program's central manager and staff it was confirmed that program spending across provinces had not been adjusted according to indicators of performance in reaching poor areas within provinces.

This regression can be used to estimate a counter-factual targeting differential, which controls for differences over time in program spending. In particular, define the *budget-neutral targeting differential* as the value of TD if program spending did not vary over time within provinces, and was given by the mean spending of TII and TIII. This is identified by simply re-writing equation (10) as:

$$T_{jt} = ,(G_{jt} - G_j) + T_j^* + \ddot{I}_{jt} \tag{8}$$

Thus T_j^* is the expected value of the budget-neutral targeting differential. By regressing T_{jt} on spending expressed as a deviation from the overall (five month) mean spending per capita for TII and TIII, and a complete set of province dummy variables, one can then estimate T_j^* by the regression coefficient on the j'th dummy variable. For example, the coefficient on the province dummy variable for Cordoba in TII can be interpreted as the estimated targeting differential for that province under Trabajar II if it had its mean budget allocation across TII and TIII.

Table 4 gives the results, both for the combined sample and split between TII and TIII. When the regressions for TII and TIII are combined, allowing all coefficients to differ between TII and TIII, a joint test convincingly rejects the null hypothesis that the budget-neutral TDs are the same for the two

programs. I also tested whether the estimated value of $\hat{a}$ was different when spending increased versus decreased; there was no significant difference (the coefficient on the interaction effect between $G_{jt} - G_j$ and $I(G_{jt} - G_j)$, where I is the indicator function, had a t-ratio of –0.38). There is no difference in the absolute value of the effects of spending cuts versus increases.

Table 4. Budget Effects on Poor-Area Targeting of Argentina's Trabajar Programs

Variable	Full sample		Trabajar II		Trabajar III	
	coefficient	t-ratio	coefficient	t-ratio	coeffcient	t-ratio
Program spending (deviation from time mean TII+TIII)	3.13	4.81	3.55	5.32	10.39	4.44

Budget-neutral Targeting Differentials

Buenos Aires	-5.62	-2.50	-8.35	-2.38	-3.78	-0.43
Catamarca	49.48	3.34	20.38	2.12	93.31	9.54
Chaco	10.07	2.13	6.73	0.60	31.11	3.02
Chubut	31.53	3.92	29.89	2.89	39.99	4.46
Cordoba	144.60	10.25	131.35	6.94	161.51	18.35
Corrientes	24.68	4.64	19.16	2.51	41.25	4.38
Entre Rios	15.27	3.12	16.29	1.96	22.68	2.50
Formosa	10.38	1.82	6.54	0.51	26.74	2.81
Jujuy	61.23	4.59	46.80	8.58	92.46	9.28
La Pampa	6.37	1.36	11.15	1.16	8.01	0.89
La Rioja	3.97	0.43	-1.82	-0.09	26.64	2.62
Mendoza	29.98	4.17	34.67	2.50	31.64	3.54
Misiones	-2.10	-0.29	-15.69	-1.68	23.62	2.47
Neuquen	-8.07	-1.55	-6.32	-0.66	6.79	0.68

Table 4. (Continued) Budget Effects on Poor-Area Targeting of Argentina's Trabajar Programs

Variable	Full sample		Trabajar II		Trabajar III	
	coefficient	t-ratio	coefficient	t-ratio	coeffcient	t-ratio
Rio Negro	52.33	4.28	59.11	2.60	54.82	5.97
Salta	67.30	10.81	64.22	6.20	86.70	8.63
San Juan	50.50	6.73	63.15	8.69	48.72	5.23
San Luis	37.08	6.11	30.34	3.55	61.68	5.94
Santa Cruz	9.33	1.21	4.62	0.30	26.50	2.81
Santa Fe	18.52	2.95	30.05	4.23	16.54	1.79
Santiago Del Estero	22.53	3.97	20.09	2.06	43.67	4.12
Tucuman	46.22	5.23	60.32	4.63	46.90	4.76
no. observations	132		66		66	
R-squared	0.778		0.813		0.903	
S.E. of regression	0.265		0.209		0.176	
Mean dep. variable	0.307		0.328		0.276	
F-statistic	17.38		8.493		8.568	

Note: The dependent variable is the targeting differential given by the regression coefficient of Trabajar spending per capita at department level for each province and time period on the incidence of unmet basic needs per capita. The observation period for each of TII and TIII was divided into three five month-intervals (one six month interval for TIII, converted into a five month equivalent); a statistical addendum with details is available from the author. The t-ratios are based on White standard errors.

The regression coefficient of the targeting differential on program spending is 3.13 for the combined samples. So a $10 cut in spending reduced the targeting differential by $3.13 on average. For TII, the regression coefficient of the targeting differential on program spending is 3.55. For TIII, the estimated regression coefficient rises to 10.22. So not only has targeting performance

deteriorated in the change from TII to TIII, but the effect of changes in program spending on targeting performance has increased under TIII.

The budget neutral TD for TII is positive in 18 of the 22 provinces, and significantly so (at the 5% level or better) in 14 of those; there is one province (Buenos Aires) in which the budget neutral TD is significantly negative. Under TIII, the province effects are now positive in all except one province, and are statistically significant in 18 provinces.

There is a high correlation between the budget-neutral TD's for TII and TIII ($r = 0.88$). However, it is notable that the budget-neutral TD's are generally *higher* for TIII. The weaker targeting performance of TIII largely vanishes if one controls for the difference in budget allocation. Indeed, the targeting performance of TIII would generally be better than that of TII if both had the same disbursement rate over time for each province. Thus the decline in targeting performance can be attributed entirely to the decline in spending.

The theoretical model in Ravallion (2000a) offers some clues as to why we observe a deterioration in targeting performance with cuts, and an improvement with program expansion. A long-standing concern about any program such as Trabajar is that poor municipalities have a harder time raising the cofinancing required for the sub-projects. A provincial government that wants to influence which municipalities participate can readily do so through its ability to propose and cofinance projects. In some provinces, it is clear that the provincial government is active in proposing projects in the capital city so as to placate vocal well-organized groups. The workers involved may well be just as poor as those in poorer municipalities outside the capital city. However, to assure maximum impact on poverty it is still preferable for the assets created by the program to be in poor areas.

The political economy of the program's operation in most provinces entailed that the cuts were borne heavily by poor areas. The cofinancing requirements allow considerable provincial discretion in the geographic allocation of program spending. Discussions with a number of the provincial project managers and staff suggested that it was politically difficult in a number of provinces to assure that the cuts came only from non-poor areas. This

reflected (in part) the fact that the program was already favoring poor areas, and so there was little slack for cutting heavily elsewhere while still leaving sufficiently broad participation.

Given these pervasive local political-economy constraints, we can begin to understand why lower disbursements resulted in worse performance in reaching poor areas. When a program such as this is cut, there is little obvious saving, via project financing, to non-poor areas. The program has negligible cost-recovery from non-poor areas, even for sub-projects in those areas. Low cost-recovery (at the margin) of program benefits in non-poor areas leaves the poor more exposed to cuts. Also it is not implausible that marginal benefits to the non-poor were quite high; the initially high degree of targeting implied low allocations to non-poor areas and so probably high marginal benefits. The fact that the program provided work to poor neighbors in non-poor areas presumably also entailed indirect benefits to the non-poor. Under these conditions, sub-projects in non-poor areas would have to be protected from cuts to avoid a welfare loss to the non-poor.

One can argue that all this helped assure this program's success in helping the poor in the crisis. While the program was clearly well targeted (to both poor workers and poor communities), it was almost certainly not a political equilibrium to assure that only poor areas participated. The other side of the coin to good targeting, was that the (relatively modest) spending on the non-poor had to be protected from cuts.

V. Conclusions

Aggregate budget cuts in Argentina during the 1980s and '90s typically resulted in proportionately greater cuts in social spending; it was "non-social" spending that was protected. However, the proportionate cuts were about the same for types of social spending that matter more to the poor as for those that tend to favor the non-poor. The absolute cuts were in fact greater for "social insurance" that matters more to the non-poor.

However, spending on targeted social assistance and employment programs

was more vulnerable to aggregate spending cuts than more universal social services. While social spending as a whole was clearly exposed to fiscal contraction, this was somewhat less true of pro-poor spending on things that benefited the non-poor too. Fine targeting may thus be a mixed blessing for the poor; a higher mean may come with greater vulnerability to cuts — and quite possibly the cuts will come at times when help is most needed. There is a strong case for action to protect pro-poor social spending at such times.

The paper studied one program that attempted to do so, namely the Trabajar Program. This was introduced to help compensate poor unemployed workers and their families for the effects of a macroeconomic shock. The design features of the program — providing low wage work targeted to poor communities — helped assure that the program was far better at reaching the poor than the pre-existing components of social spending in Argentina.

The program was clearly subject to the same constraints in the political economy that influenced the incidence of past fiscal contractions in Argentina. The program expanded into poor areas when the budget increased, but it retreated from poor areas when the program was cut. It was the program's disbursements to non-poor areas that were protected. Given the low wage rate offered, the direct benefits from the work are still very likely to have favored the poor, even after the cuts. So the design features of the program undoubtedly helped protect the poor from cuts.

In conclusion, the time series data for Argentina suggest that action to support pro-poor sending at times of aggregate fiscal contraction is warranted. Social spending in general, and targeted social spending in particular, took a heavy hit at times of fiscal austerity. The add-on program studied here was able to achieve far more pro-poor targeting than pre-existing social spending. The new program was clearly not immune to the same underlying forces in the political economy that help protect spending on the non-poor from aggregate fiscal contractions. But the program helped the families of poor unemployed workers at a time of need; given the pattern of past public spending, it appears unlikely they would have received such help otherwise.

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