Who is Protected? On the Incidence of Fiscal Adjustment

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Abstract: Standard policy advice at times of fiscal adjustment is to protect public spending on the poor. However, there is remarkably little theory or evidence to draw on in assessing the case for such policies. To help fill this gap, the paper begins with a theoretical model of the incidence of fiscal expansions and contractions, identifying conditions under which the poor will be exposed to cuts without a policy change. The paper then studies various social programs in Argentina, Bangladesh and India, focusing on how targeting performance varied with aggregate outlays. The results suggest that it tends to be program spending on the non-poor that is protected from budget cuts. Drawing on these results, recommendations are made for more effective safety nets in developing countries.

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[1]   These are the views of the author, and should not be attributed to the World Bank or any affiliated organization.

Introduction

With heightened sensitivity to impacts on the poor, macroeconomic adjustment programs often call for a pro-poor shift in the composition of public spending, in combination with overall fiscal contraction. Donors have been particularly keen to support new public anti-poverty programs and "social funds" that aim to target extra assistance to the poor at times of aggregate fiscal austerity.

To assess the impact of such programs we need to know the counter-factual incidence of a fiscal contraction. In the absence of intervention, do cuts tend to fall more heavily on the poor? Do add-on programs really help the poor? What happens when such programs are also cut? What are the implications for assessing the impact of add-on programs?

In principle at least, it has long been recognized that political economy plays an important role in determining the incidence of budget cuts required to assure macroeconomic stability. For example, in an early discussion of the distributional impact of stabilization programs, Johnson and Salop (1980) argued that the distribution of political power was key to determining how the burden of adjustment was shared across income groups. It has also been recognized in the literature that finer targeting of pubic spending can be a mixed blessing for the poor. The main argument is that finer targeting undermines political support for the required taxation (Besley and Kanbur, 1993; Sen, 1995; Gelbach and Pritchett, 1997; De Donder and Hindriks, 1998; van de Walle, 1998).

In settings in which the majority of voters are not poor, it is often asserted that the poor will be obliged to bear a disproportionate share of a budget cut on the grounds that they are the least powerful. However, there are some problems with this argument:

- If the poor have little or no power, and power is all that matters to the allocation of public spending, then the same reasoning would suggest that the poor gained little from public spending before the cuts — in which case they can have little to lose from cuts.

- The non-poor may value spending on the currently poor. This might be due to altruism, negative externalities of poverty, or other spillover effects, such as arising from the public good nature of some types of public spending including social spending which offers insurance in risky environments. But then the non-poor will want to protect spending on the poor from cuts, and will do so without further intervention.

- It has been argued that the non-poor can substitute easily between publicly-provided goods and market goods and so protect their welfare, while the poor are more reliant on publicly

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provided goods and services.[2] Then marginal social gains from protecting spending on the poor will be larger than for the non-poor. To the extent that the political equilibrium embodies such differences, spending on the poor will automatically be protected without further intervention.

- The aggregate fiscal contraction may come with a change in the balance of power. Depending on how an aggregate income shock is distributed, the resulting fiscal contraction may come with higher or lower relative power of the poor, with corresponding shifts in the composition of spending.

This paper studies how the performance of social programs in reaching the poor varies with aggregate outlays. The theoretical model in the following section suggests that the outcome is unclear on *a priori* grounds even when the poor are a powerless minority. So it is an empirical question. One possible empirical approach is to examine data on the composition of public spending, and to see how composition is affected by aggregate contractions. For example, Ravallion (2002) uses a time series of public spending data for Argentina to show that "social spending" in Argentina has not been protected in the past from aggregate cuts. Indeed, during the large fiscal contractions that were required to restore macroeconomic stability in the 1980s, social spending took more than a proportionate hit, as can be seen in Figure 1, which plots the proportionate changes in social spending on proportionate changes in total spending (both measured by first differences in logs).[3]

While this type of evidence provides important clues, "social spending" in Argentina (as elsewhere) is a heterogeneous category, and certainly cannot be equated with "spending on the poor." It includes types of spending such as pensions, formal unemployment insurance and higher education that tend to favor the non-poor, as well as (probably) more pro-poor spending on basic education and health and certain social assistance and active labor market programs.[4] And even within the latter categories, there are likely to be both poor and non-poor beneficiaries. Breaking down social spending can be revealing; for example, using the same data source as Figure 1, there is no sign that the categories of social spending in Argentina that are thought to matter more to the poor were more

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[2]   This is consistent with cross-country evidence on the effects of differences in public spending on health care on aggregate health outcomes, allowing those effects to differ systematically between the poor and non-poor (Bidani and Ravallion, 1997).

[3]   The time series data for 1980-97 suggest that social spending in Argentina has responded elastically to cuts in total spending (with an elasticity of about two) but that the elasticity of social spending to increases in total spending is not significantly different from zero (Ravallion, 2002).

[4]   Evidence consistent with these claims from survey-based incidence studies for Argentina in the 1990s can be found in Gasparini (1999) and Llach and Montoya (1999). Also see World Bank (1999).

protected (Ravallion, 2002). However, a deeper understanding of the incidence of fiscal contractions calls for a more micro-based approach in which there is a clearer mapping of spending to beneficiaries.

This paper draws instead on micro-based empirical studies of various social programs in India, Bangladesh and Argentina. The first two studies discussed below use cross-sectional comparisons of how incidence varies with aggregate spending, while the third case study uses longitudinal data. While the discussion will draw heavily on existing empirical studies, the comparison reveals similarities with respect to how fiscal incidence changes as programs expand and contract. The paper's final section tries to draw some lessons for institutional reforms that should be able to offer more reliable protection to the poor.

Are budget cuts simply passed onto the "powerless poor"?

Consider the following model. Public spending on a transfer payment or excludable good is allocated between equal numbers of poor (who receive G^p) and the non-poor (G^n). A natural assumption in this context is that the non-poor finance the spending out of their own income, Y^n, so that the non-poor have all the incentive from the revenue side to cut spending when income falls. However, one can readily allow some of the tax to be borne by the poor, by interpreting G^p (>0) as spending on the poor net of any taxes or user charges levied on them.

In this model, there are two possible reasons why some public spending goes to the poor. Firstly, the non-poor may gain from spending on the poor. Non-poor people might view G^p as insurance, to the extent that they face a positive probability of becoming poor. Or G^p may yield an external benefit to the non-poor, such as when they gain as employers from having a healthier and better-schooled workforce.

The second possible reason why there is spending on the poor is that they have political power, meaning that the allocation gives positive weight to the welfare of the poor. This can be interpreted in various ways. One possibility is to assume that (however it is achieved in practice) the outcome of public decision making is Pareto efficient, in that it maximizes some positively weighted sum of all utilities. Or one might interpret the weights are "capture coefficients" in a model of electoral competition in which there are differences between the poor and non-poor in voter information and ability to lobby (Grossman and Helpman, 1996; Bardhan and Mookherjee, 2000). An alternative interpretation is that the poor may revolt unless some reservation utility is assured.

Maintaining political stability is sometimes identified as a justification for protecting the poor at times of macroeconomic adjustment.

Not much will hinge on these differences in the reasons why some of the spending goes to the poor prior to the cuts. The model will have a parameter that reflects the weight given to the welfare of the poor in the allocation of public spending. While I will write the model as if there is no utility gain to the non-poor from spending on the poor, this is a matter of interpretation.

A natural assumption in this context is that both poor and non-poor prefer lower inter-temporal variability in spending at a given mean. This is virtually equivalent to assuming declining marginal utility of public spending for each group. For the non-poor, this can simply arise from the income effect of the taxation required to finance the spending; the non-poor may or may not have positive and declining marginal utility from public spending at given income net of taxes.

Utility of the non-poor is $U^n(Y^n - G, G^n)$ where $G = G^n + G^p$. The function U^n is taken to be additively separable, though I will note implications of relaxing this assumption. The function is strictly increasing and concave in after-tax income, $Y^n - G$, and (at given $Y^n - G$) it is weakly increasing and at least weakly concave in G^n (i.e., $U_Y^n > 0$, $U_{YY}^n < 0$, $U_G^n \geq 0$, and $U_{GG}^n \leq 0$ in obvious notation). The non-poor will then prefer less variable G^n, allowing for effects on net income. (Notice that this holds even if $U_{GG}^n = 0$, given that $U_{YY}^n < 0$.) Utility of the poor is $U^p(Y^p, G^p)$, which is increasing and strictly concave in both arguments and also additive separable.

One interpretation of these utility functions is as follows. For both groups, imagine that there are two possible states of nature, with known probabilities. The non-poor are taxed in the high-income state, which then yields utility $V(Y^n - G)$, while the non-poor rely on government transfers in the low-income state, giving utility $V^n(G^n)$ (subsuming income in the low income state in the function V^n). For the poor, utility in their high-income state is $V(Y^p)$ while in their low-income state it is $V^p(G^p)$. The utility functions U^n and U^p can then be interpreted as the expected utility of each group embedding the two probability distributions across the states of nature:

$$U^n(Y^n - G, G^n) = (1 - \pi^n)V(Y^n - G) + \pi^n V^n(G^n) \tag{1.1}$$

$$U^p(Y^p, G^p) = (1 - \pi^p)V(Y^p) + \pi^p V^p(G^p) \tag{1.2}$$

where π^n and π^p are the probabilities of the non-poor and poor falling into their low-income state of nature. (Different probability distributions entail that the derived utility functions vary, even if the underlying "primal" utility functions do not vary.) I will return to this interpretation.

The allocation of spending maximizes:

$$U^n(Y^n - G, G^n) + \lambda(Y^n, Y^p)U^p(Y^p, G^p) \tag{2}$$

where $\lambda(Y^n, Y^p)$ is a non-negative number giving the relative power of the poor over public spending. (Alternatively, in an insurance model, λ would be interpreted as the odds ratio of the non-poor becoming poor; or λ might measure the external gain to the non-poor from spending on the poor.) The relative power of the poor is allowed to depend on the distribution of income. On *a priori* grounds the most plausible assumption would appear to be that higher inequality means that the poor have less power over fiscal decision-making.[5]

We can write the solutions to this problem in the generic forms:

$$G^n = G^n[Y^n, \lambda(Y^n, Y^p)] \tag{3.1}$$

$$G^p = G^p[Y^n, \lambda(Y^n, Y^p)] \tag{3.2}$$

(Without separability, the solutions also depend on the income of the poor, at given λ.) Aggregate spending is:

$$G = G^n + G^p = G[Y^n, \lambda(Y^n, Y^p)] \tag{4}$$

Now imagine that the non-poor receive a negative income shock, which calls for a cut in G to restore equilibrium. In analyzing the implications, it is convenient to use (4) to eliminate Y^n from (3.1) and (3.2), and so write the following equations for how the spending allocation will vary with total spending in equilibrium (allowing for effects through λ and subsuming Y^p):

$$G^n = \Pi^n(G) \tag{5.1}$$

$$G^p = \Pi^p(G) \tag{5.2}$$

In a neighborhood of the optimum, it is readily verified that the derivatives of (5.1) and (5.2) w.r.t. G are given by:

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[5]   There is some (indirect) evidence to support that view in the finding of Galasso and Ravallion (2001) that the within-village performance of an anti-poverty program in Bangladesh in reaching the poor tended to deteriorate the more unequal the village.

$$\Pi_G^n = \frac{\lambda U_{GG}^p + \lambda_G U_G^p}{U_{GG}^n + \lambda U_{GG}^p} \tag{6.1}$$

$$\Pi_G^p = \frac{U_{GG}^n - \lambda_G U_G^p}{U_{GG}^n + \lambda U_{GG}^p} \tag{6.2}$$

where $\lambda_G \equiv \lambda_n / G_Y$ (in obvious notation). Equations (6.1) and (6.2) tell us the incidence of the change in aggregate spending.

Do the poor bear more of an aggregate cut than the non-poor? A natural measure of targeting performance is the "targeting differential" (T), given by the difference in per capita allocations to the poor and non-poor, i.e., $T(G) \equiv \Pi^p(G) - \Pi^n(G)$.[6] From (6.1) and (6.2) we have:

$$T_G = \frac{U_{GG}^n}{U_{GG}^n + \lambda U_{GG}^p} - \frac{(2\lambda_G U_G^p + \lambda U_{GG}^p)}{U_{GG}^n + \lambda U_{GG}^p} \tag{7}$$

This can be either positive or negative. The first term on the RHS of (7) can be called the "utility effect" since it arises from declining marginal utility of spending for the non-poor. This effect tends to make targeting performance deteriorate during an aggregate fiscal contraction. With contraction, the marginal utility of spending rises for the non-poor, and so the equilibrium switches in their favor, at the expense of the poor. The second term on the RHS of (7) — the "power effect" — works in the opposite direction when the power of the poor is positive and is enhanced by the contraction.

Some special cases are instructive. First consider the case in which $\lambda = 0$ implying that $G^n(Y^n, 0) = G$ and $G^p(Y^n, 0) = 0$. Nothing will be spent on the poor, and so they will lose nothing from a fiscal contraction, which will be borne entirely by the non-poor. (In the expected utility interpretation, this will also be the case if the poor are fully protected from the low-income state ($\pi^p = 0$), for then there will be no reason to spend anything on the poor.) Suppose instead that λ is a positive constant i.e., the fiscal contraction has no effect on the relative power of the poor ($\lambda_G = 0$). It is then evident from (6.2) that the poor will be fully protected from a fiscal contraction as long as the non-poor do not have diminishing marginal utility of spending, i.e., $U_{GG}^n = 0$. In this case, no further action to protect the poor during adjustment periods is needed. By the same token,

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[6]   See Ravallion (2000), which discusses the properties of this measure of targeting performance further. The case studies discussed later in this paper will rely heavily on this measure.

the poor will not gain anything from "trickle down", here interpretable as higher public spending stemming from an income gain to the non-poor. Even a very small weight on the poor could be consistent with this outcome. The above model is sufficiently general to allow an arbitrarily small positive share of spending going to the poor. By contrast, if U^p is nearly linear in G^p (i.e., U^p_{GG} close to zero) then the poor will bear virtually all of a retrenchment, and all of an increment to spending will go to the poor.

Now consider the case in which the relative power of the poor depends negatively on the extent of income inequality; $\lambda_G < 0$. Then we have the possibility that the poor gain from the fiscal contraction, through its effect on their relative power over spending allocations. This is sure to be the case if the non-poor do not experience diminishing marginal utility of spending, for then $\Pi^p_G = -\lambda_G U^p_G / (\lambda U^p_{GG})^{-1} < 0$.

The upshot of the above observations is that the incidence of a fiscal contraction is ambiguous even in this simple model. The need for specific actions to protect the poor cannot be pre-judged and must be deemed an empirical question. In the cases studies we will see whether the "utility effect" dominates the "power effect."

Primary schooling and anti-poverty programs in India

One source of evidence on the incidence of changes in aggregate outlays is by examining the differences in incidence between geographic areas with different levels of total spending. This section will review such evidence for various social programs in India.

For this purpose, let us define the marginal odds of participation (MOP) as the increment to the program participation rate of a given expenditure quintile (say) associated with a change in aggregate participation in that program. With appropriate survey data one can readily calculate the average participation rates for a given program for each quintile and each geographic area ("region" hereafter) identified in the survey (subject to sample design). One can then see how the participation rate for a given quintile varies across regions according to the level of public spending on the program in the state to which each region belongs. To estimate the MOP by program and expenditure

quintile one can regress the quintile-specific participation rates across regions on the average state participation rates (all quintiles) for each program.[7]

Following this approach, the analysis in Lanjouw and Ravallion (1999) was based on India's National Sample Survey (NSS) for 1993-94. This survey includes standard data on consumption expenditures, demographics and education attainments, including school enrolments. This particular NSS round also asked about participation in various anti-poverty programs. Participation in three key programs can be identified from the survey: public works schemes, a means-tested credit scheme called the "Integrated Rural Development Programme" (IRDP), and a food rationing scheme, the "Public Distribution System" (PDS).[8] The data on participation in these programs can be collated with data on total consumption expenditure per person at the household level.

Sampled households in the NSS were ranked by total consumption expenditure (including imputed values of consumption from own production) per person normalized by state-specific poverty lines. Quintiles were then defined over the entire rural population, with equal numbers of people in each. So the poorest quintile refers to the poorest 20% of the national rural population in terms of consumption per capita.

The analysis was done at the level of the NSS region, of which there are 62 in India, spanning 19 states and with each NSS region belonging to only one state.[9] So in the basic model, for any given combination of quintile and program, the participation rates across the 62 NSS regions were regressed on the average participation rate (irrespective of quintile).

Let us first consider primary schooling. Table 1 gives the average and marginal odds of enrollment amongst children 5-9 from the 1993-94 NSS data. Enrollment rates rise with household expenditure per capita and they tend to be higher for boys than girls.

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[7]   However, Ordinary Least Squares regression will give a biased estimate of the MOP, since the region and quintile specific participation rate (on the left-hand side) is implicitly included when calculating the overall mean participation rate across all regions and quintiles (on the right hand side). To deal with this problem Lanjouw and Ravallion (1999) use an Instrumental Variables Estimator in which the "leave-out mean" is used as the instrumental variable for state average participation rate. (The leave-out mean is defined as the mean for the state excluding the region and quintile specific participation rate corresponding to each observation in the data.)

[8]   Participation in public works programs is based on whether any household member worked for at least 60 days on public works during the preceding 365 days. Participation in the IRDP program is defined as whether the household received any assistance during the last 5 years from IRDP. Participation in the PDS program is defined as whether the household purchased any commodity from a ration/fair price shop during the last 30 days.

[9]   The sample size (rural areas only) of the 1993-94 NSS was 61,464 households.

The average odds of enrollment suggest that subsidies to primary schooling favor the non-poor.[10] However, that is not the case once we look at the estimated marginal odds of being enrolled, which are also given in Table 1. The MOP can be interpreted as the gain in subsidy incidence per capita for each quintile from a one Rupee increase in aggregate spending on each program. For example, an extra 100 Rupees per capita spent on primary schools will increase the public expenditure per capita going to the poorest quintile by 110 Rupees. The average odds indicate that the share of the total subsidy going to the poorest quintile is only 14% (0.71 times 0.2). However, the marginal odds imply that the poorest quintile would obtain about 22% of an increase in the total subsidy going to primary education. The MOP estimates suggest that an aggregate contraction in primary schooling would be borne heavily by the poor.

There are also gender differences. Average enrollment rates tend to be higher for boys than girls (Table 1). However, the marginal odds are almost identical for boys and girls from the poorest quintile (1.09 versus 1.08). And the marginal odds are higher for girls than boys in families from the richest quintile (0.70 versus 0.53). The boys are clearly favored first, by both rich and poor parents. For the poor, the gender gap does not change as the overall primary enrolment rate increases. Amongst the richest quintile, by contrast, the girls catch up as the program expands.

Table 2 gives the corresponding results for each of the anti-poverty programs. For both public works programs and IRDP, participation rates fall as expenditure per person increases. However, the rate of decline is not large; the odds of the poorest quintile participating in public works programs is 1.23, versus 0.83 for the richest quintile; the rate of decline is even lower for IRDP. Participation rates amongst the richest 20% in terms of consumption per person are high even for public works programs. For PDS, the participation rate is actually lowest for the poorest quintile, with highest participation amongst the middle expenditure quintile.

Table 2 also gives the estimated marginal odds of participation. These fall far more rapidly than the average odds as expenditure rises. The MOP for the poorest quintile is highest for public works programs, while IRDP dominates for the three middle quintiles; the PDS has the highest MOP for the richest quintile. The MOP coefficients broadly confirm the conclusion from the average odds of participation that the public works programs perform best at reaching the poorest, while IRDP is more effective in reaching the middle quintiles, including those living at India's poverty line (at roughly the 40th percentile).

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[10]   Notice that Lanjouw and Ravallion could not split public from private schooling in their data, and that public school enrollments may well be lower for the well off.

The difference between the MOP numbers for any two programs gives the estimated gain from switching one Rupee between the two programs. For example, switching 100 Rupees per capita from PDS to public works programs would increase public spending per capita on the poorest quintile by 10 Rupees (116-106=10, using the basic model).

For both the public works programs and IRDP, it is notable that the marginal odds of participation tend to fall more steeply as one moves from the poorest to the richest quintiles than do the average odds (Table 2). Thus the average odds underestimate how much the poor would lose from a cut in total spending on each of these programs. This is particularly strong for IRDP, for which the average odds of participation are only slightly higher for the poorest quintile than the richest (1.03 versus 0.89), while there is a large difference in the MOP (1.11 versus 0.39), though with less difference amongst the first four quintiles. As compared to the average odds of participation, the share of the total IRDP spending imputed to the poorest 40% of the population is 11% higher, while that imputed to the richest 20% is 56% lower. For PDS, however, there is less difference between the average and marginal odds.

For the purpose of the present discussion, what is most striking from these results is that for all these social programs in India, higher aggregate outlays (as reflected in higher aggregate participation rates) tend to be associated with more pro-poor incidence of benefits. By the same token, aggregate cuts tend to be associated with worse targeting. Similar methods are used by Lanjouw et al., (2001) on data for primary health and education spending in Indonesia. They too find evidence of early capture by the non-poor in that the marginal odds of participation by the poor tend to exceed the average odds, with the reverse for upper quintiles.

Bangladesh's Food-for-Education Program

The second piece of evidence comes from a study of Bangladesh's Food-for-Education Program (FFE) program. This was one of the first of many school-enrollment subsidy programs now found in both developing and developed countries. The official aim was to keep the children of poor rural families in school. On paper, the program distributes fixed food rations to selected households conditional on their school-aged children attending at least 85% of classes. Over two million children participated in 1995-96 (13% of total primary school enrolment). There is evidence of significant gains in terms of school attendance with only modest foregone income through displaced child labor (Ravallion and Wodon, 2000). There were two stages of targeting. First economically backward

areas were chosen by the center. Second, community groups—exploiting idiosyncratic local information—select participants within those areas.

Similarly to the method in the last section, we will examine how incidence varies geographically with aggregate program outlays, but this time the analysis will be done at village level. Galasso and Ravallion (2001) compare the performance of villages in reaching the poor with how the program was allocated across villages. Survey data from Bangladesh's Household Budget Survey for 1995/96 was used to assess program incidence within villages.

Targeting performance was measured by the targeting differential defined above, namely the difference between the per capita allocation to the poor and that to the non-poor. Two poverty lines are considered, one at the median and the other at the quantile of the 25th percentile from the bottom. It can be shown that this is exactly decomposable between a "within-village" and "between-village" components (Ravallion, 2000). The targeting differential is also interpretable as a measure of association for the 2x2 contingency table formed by comparing who is poor or not and who gets the program (Galasso and Ravallion, 2001).

Table 3 summarizes the results on overall targeting performance. The aggregate targeting differential is positive and significantly different from zero.[11] Amongst all villages, 12% of the poor receive the program, as compared to 8% of the non-poor (in participating villages, the proportions are 46% and 32%). Virtually all of the aggregate targeting differential is accounted for by the intra-village component.

Performance differed greatly across villages; indeed, the targeting differential was negative in 24% of the villages. Table 4, column 1, summarizes the results of a regression of targeting performance against the budget allocation to the village and the village poverty rate (proportion of village population living in poor households, by the above definition). The participation rates of both the poor and non-poor rise with the overall participation rate in the program (which closely approximates the aggregate budget allocation to the village, given that all participants received roughly the same amount). However, the rate of increase is higher for the poor, so that targeting performance (the difference between the amount going to the poor and that going to the non-poor) tends to improve as the aggregate outlay on the program expands.

To allow for heterogeneity in village characteristics, Table 4 (column 2) also gives the results when controls are added for program eligibility criteria, structural characteristics of villages

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[11] This is based on the standard chi-square test statistic for the test of independence in a contingency table (Galasso and Ravallion, 2001).

(education, cropping, modernization in agriculture) and institutional characteristics (land inequality, and indicators of local social capital). With these controls, it remains the case that targeting performance rises with aggregate outlays. However, the effect is no longer statistically significant at a reasonable level.

Again there are concerns about assuming that the aggregate allocation (in this case across villages) is exogenous, even in the model with controls. Galasso and Ravallion exploit seemingly plausible assumptions about the information structure to test exogeneity of allocations across villages, and are unable to reject the null of exogeneity (though they do find signs that information provided by local authorities to the center is manipulated). Their identifying assumption is that a village's relative position in terms of the eligibility criteria influences the allocation across villages but does not influence outcomes conditional on that allocation.

The results for both India and Bangladesh point in a similar direction in suggesting that targeting performance deteriorates with program contraction. However, these tests cannot be considered conclusive. As I have emphasized, the main problem is the possibility that aggregate participation is endogenous; for example, local governments with a political preference in favor of the poor may simultaneously spend more and assure that the spending is more pro-poor. While both the India and Bangladesh case-studies performed tests for this, and found that the results were reasonably robust, there are intrinsic limits to how far one can go in addressing this problem using cross-sectional data. By exploiting a panel-data structure, the next case study will be more robust to the possible endogeneity concerns.

A safety net program in Argentina

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. The program provides an interesting case study for the present purpose, both 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.

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, Trabajar I, II and III. 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. 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. 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 (Jalan and Ravallion, 2002). The results have indicated that Trabajar jobs are well targeted to the poor; 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. The program's targeting also appears to be better than any other targeted programs in Argentina (Ravallion, 2002).

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 how well 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.

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Measuring 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, 2000).

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:

$$G_{ij}^n = G_j^n + \varepsilon_{ij}^n \tag{8.1}$$

$$G_{ij}^p = G_j^p + \varepsilon_{ij}^p \tag{8.2}$$

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:

$$G_{ij} = H_{ij}G_{ij}^p + (1 - H_{ij})G_{ij}^n \tag{9}$$

Using equations (8.1) and (8.2) we can re-write (9) in the form of a simple linear regression across all departments in province j:

$$G_{ij} - G_j = T_j(H_{ij} - H_j) + v_{ij} \tag{10}$$

where

$$v_{ij} = \varepsilon_{ij}^{n} + (\varepsilon_{ij}^{p} - \varepsilon_{ij}^{n})H_{ij} \tag{11}$$

where $T_j = G_j^p - G_j^n$ is the targeting differential already introduced, i.e., 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 (11.) 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) (Ravallion, 2000). Thus H_{ij} is exogenous in (10) and so one can estimate T_j from an OLS regression of G_{ij} on H_{ij} across all departments within a given province.[12]

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. The targeting differential for province j is then given by:

$$T_j \equiv \frac{\sum\limits_{i=1}^{M_j}(G_{ij} - G_i)(H_{ij} - H_j)}{\sum\limits_{i=1}^{M_j}(H_{ij} - H_j)^2} \tag{12}$$

One can similarly define a national inter-departmental targeting differential, by calculating (12) 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. Poverty is measured by the proportion of the population deemed to have unmet basic needs (UBN), based on the 1991 census.

Results

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

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[12]   Equation (11) indicates that the error term will not be homoscedastic. Standard errors of the targeting differential were corrected for heteroscedasticity.

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.

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.

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. 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. The fact that the above test is robust to latent geographic heterogeneity in targeting performance (as embodied in the province fixed effects) makes it more convincing than the earlier tests using cross-sectional data.

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:

$$T_{jt} = \alpha + \beta G_{jt} + \eta_j + \mu_{jt} \qquad (j=1,..,22; t=1,..,6) \tag{13}$$

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.

Table 5 gives the results, both for the combined sample and split between TII and TIII. 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.

I also tested whether the estimated value of β 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.

Again we have found that targeting performance tends to worsen when aggregate spending on the program is cut, broadly consistent with the earlier evidence for India and Bangladesh. The results of all three studies are suggestive of an underlying tendency in the political economy to protect spending on the non-poor.

Conclusions and lessons for safety net policy

Even when they have the power to do so, it is not obvious that it will be in the interests of the non-poor to shift the burden of fiscal adjustment to the poor. This depends on the preferences of the non-poor, 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. 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.

As this paper has demonstrated, the incidence of fiscal contraction, and hence the case for action to protect public spending on the poor at a time of overall fiscal austerity, is an empirical question. The paper has tried to address that question drawing on case-studies for various social programs in Argentina, Bangladesh and India. The case studies have used a range of data and methods but share the common feature that they study the way in which the incidence of spending (notably how much goes to the poor relative to others) varies with the aggregate level of spending. While further work is needed in other settings before one can be confident in making generalizations, there are some common findings in these case studies that are at least suggestive.

The results reinforce the view that extra public action is warranted to protect public spending on the poor at times of aggregate fiscal contraction. In all the cases studied here, one finds signs of early program capture by the non-poor, but that targeting tends to improve as the program expands; in short, it appears from the evidence reviewed here that it is spending on the non-poor that is protected. In terms of the theoretical model, this suggests that the "utility effect" dominates the "power effect;" declining marginal utility of spending on the non-poor tends to mean that there is a switch in spending away from the poor during an aggregate contraction.

One implication of these findings concerns impact evaluations of add-on programs intended to compensate losers from fiscal adjustment. The results of this paper suggest that evaluations that ignore the political economy of fiscal adjustment can greatly underestimate the impact on poverty of successful add-on programs, relative to the counter-factual of no intervention. Past performance in reaching the poor is clearly not a reliable guide to outcomes in the absence of the intervention. Restoring the pre-adjustment level of public spending on the poor is consistent with large gains relative to what would have happened without intervention. That is implied by this paper's repeated finding that targeting performance tends to deteriorate when aggregate spending declines.

Another implication is that achieving a pro-poor shift in spending during an aggregate contraction will not be politically easy. While the above results strengthen the case for efforts to change the composition of spending at times of aggregate fiscal adjustment, they also point to the difficulty of doing so. If the empirical regularities found in the various data sets studied in this paper prove to be general, then one is led to conclude that attempts to combine the short-term spending cuts required for macroeconomic adjustment with better targeting will meet opposition. As the case study for one such program in Argentina suggests, even successful add-on programs are not immune to the same underlying forces in the political economy that help protect spending on the non-poor from aggregate fiscal contractions. The Argentina program did help the families of poor unemployed workers at a time of need; given the pattern of past public spending, it appears unlikely that they

would have received such help otherwise. But the local political economy tends to protect allocations to non-poor areas when the program contracts (and program expansions tend to favor poor areas). Deeper institutional and policy reforms may then be called for if the poor are to be more protected from fiscal adjustment.

This begs the question of whether it might be possible to design permanent policies that automatically protect the poor from short-term fiscal adjustments. This is not so far fetched. With the better household survey data currently available (or feasible to generate) it is not difficult to identify ex-ante the key line items of public spending on health, education and social protection that will need to be protected. Investing in credible program impact assessments can create the information base needed to more effectively resist short-term political pressures during a time of fiscal adjustment.

There is also scope for establishing permanent programs that can respond endogenously to the income shocks facing the poor. Some promising clues can be found from developing country experience, notably in the success stories found in famine prevention and relief (Ravallion, 1997). Indeed, automatic protection is the essential idea of an important and long-standing class of anti-poverty programs, typified by the famous Employment Guarantee Scheme in the state of Maharashtra in India. This aims to assure income support in rural areas by providing unskilled manual labor at low wages to anyone who wants it. The scheme automatically contracts in good agricultural years and expands in poor years, and has provided effective protection when there is a threat of famine (Dréze and Sen, 1989). Design features are crucial, notably that the wage rate is not set too high.[13] The scheme is financed domestically, largely from taxes on the relatively well-off segments of Maharashtra's urban populations, who perceive benefits from effective social protection, such as in attenuating migration to cities in times of stress in rural areas (Ravallion, 1991). So this is essentially a public insurance scheme in which outlays are fully endogenous to the aggregate shocks to the economy. This means of course that funding must be secure, and that there is a permanent institutional capability for rapid disbursement when needed.

There are limits to social protection by this means alone. The work requirement means that not all those in need will be able to participate. A complementary set of transfers in cash or food may well be needed, targeted to specific groups who either cannot work, or should not be taken out of

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[13]   Ravallion et al., (1993) provide evidence on how this scheme responds to aggregate shocks, and on how the ability of the scheme to insure the poor without rationing was jeopardized by a sharp increase in the wage rate.

other activities (notably school) to join relief work. Those who cannot be expected to work can be fairly easily identified by their age or disability, though even then the administrative and political-economic difficulties in doing so should not be under-rated. The judgment might also be made that certain groups should not work to obtain benefits even though they are able. Cash or food-for-education programs in poor areas (such as the Bangladesh program studied in this paper) can help keep poor kids in school during times of macroeconomic contraction. These transfer schemes will need to be allocated by administrative means, and turned off and on according to indicators of crisis. The demand for relief work can provide a useful signal for this purpose. A rapid expansion of demand for low wage relief work is a good signal that other transfers also need to kick in.

While effective short-term social protection from aggregate shocks may well be the exception rather than the rule, some developing countries have demonstrated that it is possible to protect those who are poor or vulnerable at such times. The claims sometimes heard that this is beyond the means of developing countries are demonstrably wrong; the evidence found in this paper that it is the non-poor who are protected belies the case that differential protection is unfeasible on narrow economic grounds.

The real challenge ahead is to assure that an effective safety net is a permanent institution, dealing simultaneously with macro crises and the more routine problems of idiosyncratic risk in normal years. The cost to the budget need not be higher than existing schemes, though in many settings current spending on social protection may well be too low. The budgetary outlay could well be highly variable over time, though possibly no more so than for poorly prepared and designed relief operations in which large sums of money have to be injected into an overdue response. To cover the variability in disbursements, a central safety-net fund can be established. There will no doubt be low probability events for which extra external help will be needed. However, the fund should be sufficient to cover a normal sequence of shocks as well as modest demand in normal years.

What is the role of donors and the International Financial Institutions in all this? Only coming into the picture in an *ad hoc* and delayed way during emergencies, and naively running against the local political economy, hardly constitute a credible external response. A potentially far more important role for external assistance is in assuring that an effective permanent safety net is in place with secure funding, as a crucial element of sound domestic policy making even in normal times. Setting up the capacity for effective social protection is arguably no less important to the ultimate welfare objectives of economic policy in risk-prone economies than is aiming to assure sustainable trajectories for macroeconomic aggregates. Neither goal should be compromised to placate short-term political interests.

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Table 1: Average and marginal odds of primary school enrollment in rural India

Quintile	Boys		Girls		Total	
	Average odds	Marginal odds	Average Odds	Marginal odds	Average odds	Marginal odds
Poorest	0.75	1.09 (6.90)	0.66	1.08 (9.65)	0.71	1.10 (8.99)
2nd	0.93	0.91 (6.05)	0.91	0.91 (6.99)	0.90	0.97 (7.92)
3rd	1.07	0.92 (5.85)	1.06	0.84 (6.54)	1.08	0.87 (7.65)
4th	1.16	0.66 (4.10)	1.26	0.66 (4.28)	1.21	0.67 (4.77)
5th	1.23	0.53 (4.08)	1.38	0.70 (5.53)	1.31	0.67 (5.69)

Note: The average odds are based on the average primary school enrollment rates as a percentage of children aged 5-9, and the odds of enrollment, defined as the ratio of the quintile-specific enrollment rate to the mean rate. The marginal odds are estimated by an instrumental variables regression of the quintile-specific primary school enrollment rates across regions on the average rate by state for that program. The leave-out mean state enrollment rate is the instrument for the actual mean. The numbers in parentheses are t-ratios. Households were ranked by total expenditure per person in forming the quintiles.
Source: Lanjouw and Ravallion (1999) based on the 1993-94 National Sample Survey.

Table 2: Average and marginal odds of participation for India's anti-poverty programs

Quintile	Public Works Programs		Integrated Rural Development Program		Public Distribution System	
	Average odds	Marginal odds	Average odds	Marginal odds	Average odds	Marginal odds
Poorest	1.23	1.16 (3.27)	1.03	1.11 (15.49)	0.92	1.06 (8.14)
2nd	1.13	0.93 (3.64)	1.13	1.28 (17.73)	1.01	0.99 (7.26)
3rd	1.04	0.80 (2.98)	1.03	1.21 (23.52)	1.03	0.91 (6.88)
4th	0.86	0.92 (4.32)	0.96	0.96 (19.09)	1.00	0.86 (7.16)
5th	0.83	0.55 (3.29)	0.89	0.39 (8.06)	1.00	0.81 (6.27)

Note: See Table 1.

Table 3: Targeting performance of Bangladesh's Food-for-Education Program

	Participation rate of the poor	Participation rate of the non-poor	Targeting differential	Intra-village	Inter-village	ϕ	prob. value
50% poverty line:	0.118	0.079	0.039	0.036	0.003	0.004	0.000
25% poverty line:	0.136	0.086	0.050	0.037	0.013	0.005	0.000

Table 4: Intra-village targeting performance of Bangladesh's Food-for-Education Program

	(1) Without controls for village characteristics			(2) With controls for other village characteristics		
	Targeting performance	Participation rate for the poor	Participation rate for the non-poor	Targeting performance	Participation rate for the poor	Participation rate for the non-poor
Budget allocation to village	0.324**	1.177**	0.853**	0.156	1.013**	0.857**
	(2.30)	(16.30)	(9.63)	(0.71)	(8.04)	(6.03)
Poverty rate in the village	0.081	-0.145	-0.226**	0.314*	0.055	-0.258**
	(0.43)	(0.99)	(2.51)	(1.92)	(0.49)	(2.32)
R^2	0.07	0.73	0.66	0.57	0.87	0.81
N	62	62	62	52	52	52

Note: Robust t-statistics in parentheses. * denotes significant at 10% level; ** at 5% level.
Source: Galasso and Ravallion (2001).

Table 5: Budget effects on poor-area targeting of Argentina'sTrabajar Programs

	Full sample	Trabajar II	Trabajar III
Program spending (deviation from time mean TII+TIII)	3.13	3.55	10.39
	(4.81)	(5.32)	(4.44)
R^2	0.778	0.813	0.903
N	132	66	66

Note: Robust t-statistics in parentheses. 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. Each regression included province fixed effects. 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.

Figure 1: Has social spending in Argentina been protected from aggregate fiscal contractions?
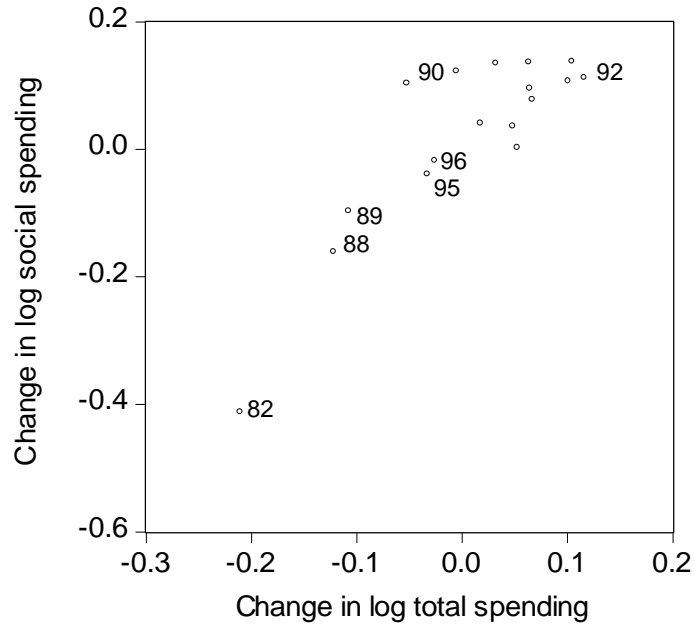
Note: Annual data 1980-97; 1997 prices; selected years indicated. "Social spending" comprises "social insurance" (pensions, public health insurance, unemployment insurance) and "social services" (education, health, water and sewerage, housing and urban development, social assistance, and labor programs).
Source: Government of Argentina (1999)

Addendum (not intended for publication but available on request)

Derivation of equations 6.1 and 6.2

The necessary conditions for a maximum of

$$U^n[Y^n - (G^n + G^p), G^n] + \lambda(Y^n, Y^p)U^p(Y^p, G^p)$$

with respect to G^n and G^p are that

$$U_Y^n[Y^n - (G^n + G^p)] = U_G^n(G^n) = \lambda U_G^p(G^p)$$

(recalling that utility is assumed to be separable). The implicit solutions give G^n and G^p as functions of Y^n. On differentiating w.r.t. Y^n we have:

$$U_{YY}^n \frac{\partial G}{\partial Y^n} + U_{nn}^n \frac{\partial G^n}{\partial Y^n} = U_{YY}^n \tag{A1}$$

$$U_{YY}^n \frac{\partial G}{\partial Y^n} + \lambda U_{pp}^n \frac{\partial G^p}{\partial Y^n} = U_{YY}^n - \lambda_n U_G^p \tag{A2}$$

where $G = G^n + G^p$. Equations (A1) and (A2) imply that:

$$U_{GG}^n(\frac{\partial G^n}{\partial Y^n} / \frac{\partial G}{\partial Y^n}) - \lambda U_{GG}^p(\frac{\partial G^p}{\partial Y^n} / \frac{\partial G}{\partial Y^n}) = \lambda_n U_G^p / \frac{\partial G}{\partial Y^n} \tag{A3}$$

Also:

$$(\frac{\partial G^n}{\partial Y^n} / \frac{\partial G}{\partial Y^n}) + (\frac{\partial G^p}{\partial Y^n} / \frac{\partial G}{\partial Y^n}) = 1 \tag{A4}$$

(since $G = G^n + G^p$). Solving (A3) and (A4) and noting that:

$$\Pi_G^i = \frac{\partial G^i}{\partial Y^n} / \frac{\partial G}{\partial Y^n} \quad (i=n, p)$$