Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs

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Targeted transfer programs for poor citizens have become increasingly common in the developing world. Yet, a common concern among policy-makers and citizens is that such programs tend to discourage work. We re-analyze the data from seven randomized controlled trials of government-run cash transfer programs in six developing countries throughout the world, and find no systematic evidence that cash transfer programs discourage work. JEL codes: J22, I38, H53, C93

Governments in the developing world are increasingly providing social assistance programs for their poor and disadvantaged citizens. For example, in a recent review of programs worldwide, Gentilini, Honorati, and Yemtsov (2014) find that 119 developing countries have implemented at least one type of unconditional cash assistance program, and 52 countries have conditional cash transfer programs for poor households. Thus, on net, they find that 1 billion people in developing countries participate in at least one social safety net.[1]

These programs serve to transfer funds to low-income individuals and have been shown to reduce poverty (Fiszbein and Schady 2009) and to improve educational outcomes (Schultz 2004; Glewwe and Olinto 2004; Maluccio and Flores 2005) and access to health services (Gertler 2000, 2004; Attanasio et al. 2005). However, despite these proven gains, policy-makers and even the public at large often express concerns about whether transfer programs discourage work. In fact, these types of beliefs tend to be associated with less extensive and less generous social assistance programs: figure 1 shows a negative relationship between spending on cash transfers as a fraction of GDP and the share of the population in a country

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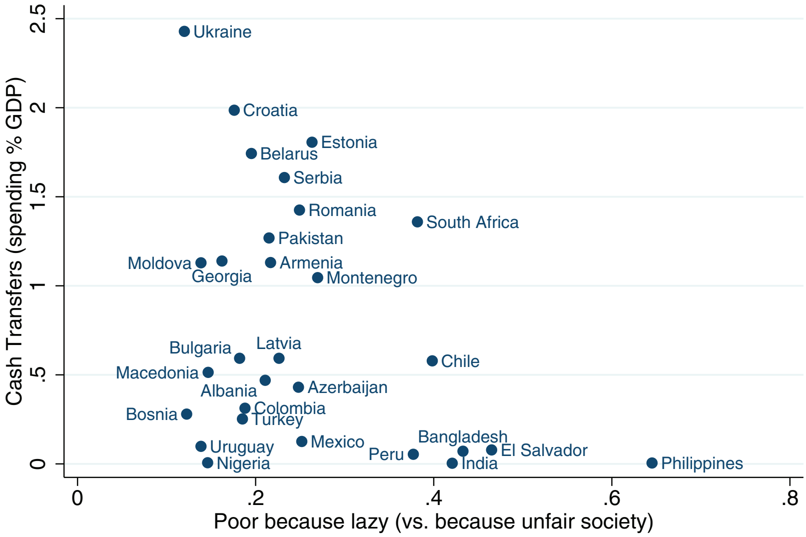
Figure 1. Cash Transfers and Attitudes to the Poor



Note: This figure is constructed with data on beliefs from the World Values Survey (WVS), and data on national spending on social assistance from the ASPIRE dataset for the latest available year. The horizontal axis plots the national average answer to the WVS question "Why, in your opinion, are there people in this country who live in need? Poor because of laziness and lack of will power (=1), OR, Poor because of an unfair society (=0)" from the 1995 WVS wave.

who believe that poverty is due to laziness (as opposed to an unfair society). But are these beliefs justified? Is this what the theory would predict? What does the evidence say?

On the one hand, transfer programs could reduce work incentives: individuals may not work—or exit visible forms of work—to ensure that they keep the benefits, or they may stop work simply through the income effect. On the other hand, these programs could have positive effects on work if they help relieve the credit constraints of the poor to allow them to invest in small enterprises or if they have spillover effects. Given that the theory has some ambiguity, it is then imperative to turn to the evidence. In developed country policy contexts, some transfer programs have indeed been shown to have small, but statistically significant, effects on work.[2] However, there is little rigorous evidence showing that transfer countries in emerging and low-income countries actually lead to less work.

In this paper, we re-analyze the results of seven randomized controlled trials of government-run non-contributory cash transfer programs from six countries worldwide to examine their impacts on labor supply.[3] Re-analyzing the data allows us to make comparisons that are as similar as possible, using harmonized data definitions and empirical strategies. It also allows us to use a cutting-edge, statistical technique to pool the effects across studies to analyze in a systematic way the effects in different countries to obtain tighter statistical bounds than would be possible from any single study, while still allowing for the possibility that the different programs worldwide could have different treatment effects given the differing contexts.

We bring together data on this issue from all randomized control trials (RCT) that we identified that met three criteria: (1) it was an evaluation of a (conditional or unconditional) government-run cash transfer program in a low-income country that compared the program to a pure control group; (2) we could obtain micro data for both adult males and females from the evaluation; and (3) the randomization had at least 40 clusters. This yielded data for transfer programs from six countries: Honduras, Indonesia, Morocco, Mexico (two different programs), Nicaragua, and the Philippines.[4] All of these programs are non-contributory transfers programs, rather than social insurance programs.

Across the seven programs, we find no systematic evidence of the cash transfer programs on either the propensity to work or the overall number of hours worked, for either men or women. This is a particularly stark finding, given the differences in context and program design across the differing settings. Importantly, pooling across the seven studies to maximize our statistical power to detect effects if they exist, we find no observable impacts on either work outcome. We can reject with high confidence any moderate negative effects for the elasticity of work outcomes with respect to income for men. If anything, the point estimates are positive. For women, more uncertainty persists even after aggregating: the point estimates are negative and small, with wide credible intervals that cover both negative and positive values. The overall low effects on work behavior may be, in part, due to the fact that the eligibility to receive (or stay on) one of the programs does not appear to be closely tied to current income levels.

Theoretically, the transfers could have different effects on work "outside the household" versus self-employment or work "within the household." For example, one could imagine that the effect for the outside-work sector may be larger, as individuals fear—rationally or otherwise—that visible employment outside the household could disqualify them from receiving future transfers. Looking at the pooled sample, we find no aggregate effect on either outcome, although the analysis points to large dispersion in impacts across programs. Indeed, for most individual programs we do not find any significant effect for either outcome, and for one program we find a small shift towards work inside the household, while for another program we find a small shift towards work outside the household.

In short, despite the rhetoric that cash transfer programs lead to a massive exodus from the labor market, we do not find evidence to support these claims.

Coupled with the benefits of transfer programs that are well-documented in the literature, this further suggests that cash transfer programs can play an effective role in providing safety nets in developing and emerging countries.

Theoretical Frameworks and Existing Literature

While much of the discourse around transfer programs is centered on people working less, the theory is more ambiguous. On one hand, cash transfers may reduce work for two key reasons. First, these programs provide unearned income, and recipients may "spend" some of this extra income on leisure.[5] That is, the pure income effect may lead recipients to work less if leisure is a normal good.[6] Second, cash transfers may decrease labor supply if they act as a "tax" on labor earnings. Specifically, if people believe that higher earnings will disqualify them from receiving benefits, they will have a disincentive to work.

On the other hand, cash transfer could increase work through a number of mechanisms. First, cash transfers could help households escape the classic poverty trap problem elucidated by Dasgupta and Ray (1986) by allowing them to have a basic enough living standard to be productive workers. Second, an infusion of cash could reduce credit constraints to starting or growing a business. Indeed, Gertler, Martinez, and Rubio-Codina (2012) provide some evidence that *Mexico's Oportunidades* program led poor households to be able to invest in productive assets. Third, cash transfers can also finance risky but profitable endeavors such as migration, which may lead to increases in the adult labor supply. For example, Ardington, Case, and Hosegood (2009) shows that the cash infusion from South African old-age social pension led to prime-aged adults having higher employment, mainly through migration. Finally, additional cash could have spillover effects within poor regions by providing additional cash that can spark increases in sales in local businesses.

The theoretical effect of transfers on work is thus ambiguous, suggesting that both the sign and magnitude of the treatment effects may be driven by the details of the program design (e.g., the targeting methods, the size of the transfers), as well as the underlying economic conditions (e.g., how cash constrained households are, how risk averse they are). Therefore, it is important to turn to the empirical evidence and to look at the evidence across a variety of contexts.

We now turn to evidence from previous studies on the impact of cash transfers on adult labor supply. Table 1 summarizes results from 21 studies, covering 17 conditional or unconditional cash transfer programs that do not have explicit work requirements.[7] The last column summarizes the evidence on overall labor supply indicators, and on shifts in the allocation of labor supply. While not necessarily exhaustive, we included all published studies we could find with a rigorous

Table 1. Summary of Findings of Labor Supply Impacts from Other Studies

Paper	Country and Program	Program Type	Research Design	Summary of Findings
Garganta and Gasparini (2015)	Argentina, AUH	CCT	Difference-in-difference	The program reduces the proportion of informal households that acquire formal jobs, for families with children, relative to families without children.
Foguel and Barros (2010)	Brazil, Bolsa Família	CCT	Difference-in-difference	Small increase in working probability of less than 1 percentage point for women and between 2 and 3 percentage points for men. Decrease of 0.6-2.6 hours of work per week for women, and an 0.6-1.6 hours increase for men.
Ribas and Soares (2011)	Brazil, Bolsa Família	CCT	Propensity Score	No detectable effect on work probability or hours of work. Reduction in formal sector participation.
de Brauw et al. (2015)	Brazil, Bolsa Família	CCT	Propensity Weighting	No detectable effect on work probability or hours of work. Shift of 8 hours per week of work away from the formal sector and into the informal sector.
Ferreira, Filmer and Schady (2009)	Cambodia, CESSP	CCT	Regression Discontinuity	No detectable effect on work probability or hours of work, both for pay and not for pay.
Chen et al (2006)	China, Di Bao	UCT	Propensity Matching	Results on total income consistent with no behavioral response. Authors note that selection issues are not accurately resolved by matching.
Attanasio and Gómez (2004)	Colombia, Familias en Acción	CCT	Difference-in-difference	Mostly no effect on working probability, except for a 3 percentage point increase for rural men.
Galiani and McEwan (2013)	Honduras, PRAF II	CCT	RCT and Regression Discontinuity in Municipality Poverty index	No detectable effect on work outside the household. Small increase in work inside the household for men, no detectable effect for women.
Alzua, Cruces, and Ripani (2013)	Honduras, PRAF II	CCT	RCT	No detectable effect on overall probability of work or hours of work.

Continued

Table 1. *Continued*

Paper	Country and Program	Program Type	Research Design	Summary of Findings
Asfaw et al. (2014)	Kenya, Cash Transfer for Orphans and Vulnerable Children	UCT	RCT and Propensity Score Matching	Reduction in wage work for men and women. Non-farm activities increase for women but decrease for men. No detectable effect on own farm work for either men or women. Effect on total work not reported.
Haushofer and Shapiro (2013)	Kenya, GiveDirectly	UCT	RCT	No detectable effects on whether primary income source is wage labor, own farm labor, or non-agricultural business.
Covarrubias et al. (2012)	Malawi, Social Cash Transfer (SCT)	UCT	RCT and Propensity Score Matching	Reduction in wage work between 3 and 5 days from a base of 7 days per month. No data on total work.
Skoufias et al. (2008)	Mexico, PAL	UCT and In-Kind	RCT	No detectable effect on overall probability of work. Some evidence of substitution from agricultural to non-agricultural work.
Parker and Skoufias (2000)	Mexico, Progresa	CCT	RCT	No detectable effect on overall probability of work.
Skoufias and Vincenzo Di Maro (2008)	Mexico, Progresa	CCT	RCT	No detectable effect on overall probability of work or participation in wage work.
Alzua, Cruces, and Ripani (2013)	Mexico, Progresa	CCT	RCT	No detectable effect on overall probability of work, or on agricultural employment. Small increase of hours of work for eligible women of 0.4 hours on a base of 42 hours per week
Maluccio and Flores (2005)	Nicaragua, RPS	CCT	RCT	No detectable effect on overall probability of work. No significant effect on hours of work for women. Reduction of 5 hours of work per week for men.
Maluccio (2007)	Nicaragua, RPS	CCT	RCT	No detectable effect on overall probability of work. Reduction of 4 hours of work per week for women. Reduction of 8 hours of work per week for men.

Continued

Table 1. *Continued*

Paper	Country and Program	Program Type	Research Design	Summary of Findings
Alzua, Cruces, and Ripani (2013)	Nicaragua, RPS	CCT	RCT	No detectable effect on overall probability of work or hours of work, for men or women. For hours of work, large but statistically insignificant point estimates between −1.5 and −2.7 hours for men and between −4 and −5.7 hours for women. No detectable effect on agricultural employment.
Hasan (2010)	Pakistan, Punjab CCT targeted to female students	CCT	Difference-in-difference	Decrease in time spent on paid work by 24-32 minutes, from a base of 47 minutes per day. Significant increase in the amount of housework by 100-120 minutes, from a base of 600 minutes per day. Effect on total work not reported.
Chaudhury et al. (2013)	Philippines, Pantawid Pamilya Program (PPP)	CCT	RCT	No detectable effect on work probability or hours of work.
Amarante et al. (2011)	Uruguay, PANES	UCT	Regression Discontinuity	The program reduces formal earnings. Data on informal work not available.
American Institutes for Research (2013)	Zambia, Child Grant Program	UCT	RCT	Significant decrease in wage labor, compensated by increase in participation in non-farm enterprises and labor on household farms.

experimental or natural-experiment-based research design. In terms of geographic cover, thirteen studies are from Latin America, four are from Africa,[8] one is from South Asia, one is from China, and two are from South-East Asia.[9]

Overall, these studies suggest little to no effects on overall labor supply. From among the fourteen studies with data on overall working probability or hours of work, nine do not find any significant effect, two find a combination of positive and null results, two find only negative results, and one finds a combination of positive and negative effects. For eight studies, we do not have explicit results on overall work probability or hours of work.

Those studies that do find an effect tend to find effects on the type of work done, rather than the total amount of work. For example, several studies have documented a shift from formal to informal labor for programs that explicitly exclude formal workers. Levy (2006), among others, argued that transfers targeted at informal workers discourage formalization. Evidence from *Bolsa Família* in Brazil, the Plan de Atención Nacional a la Emergencia Social (PANES) program in Uruguay, and the Universal Child Allowance in Argentina supports this hypothesis (Foguel and Paes de Barros 2008; Ribas and Soares 2011; de Brauw et al. 2015; Amarante et al. 2011; Garganta and Gasparini 2015). These studies find a reduction in formal work; when data is available, they also find no overall effect on work.

Several studies also document shifts away from work outside the household towards work within the household. Galiani and McEwan (2013) find a small switch to within-household work for men due to the PRAF program. Skoufias, Unar, and González-Cossío (2008) identify a switch from agricultural to non-agricultural work for the PAL program in Mexico.

Two studies in African countries find similar patterns of reductions in wage labor, together with increases in self-employed activities (Covarrubias et al. 2012; American Institutes for Research 2013). Hasan (2010) finds that a conditional cash transfer program in Pakistan decreased the time spent by mothers on paid work, while significantly increasing the amount of housework. Asfaw et al. (2014) also finds a large decrease in wage work, especially for men; nevertheless, there is little evidence of a compensatory increase in within-household work, especially for men.

Data, Empirical Strategy, and Sample Statistics

We now turn to systematically re-analyzing the labor supply effects of government-run transfer programs that have previously been experimentally evaluated. In this section, we first describe the data and then detail our empirical strategy. In the last sub-section, we provide sample statistics to provide a descriptive picture of each program area.

Figure 2. Summary of Included Studies

Country	Program	Evaluation Years	Number of Households at Endline	Targeting Method	Transfer Type and Amount	Transfer Consumption Ratio
Honduras	Programa de Asignación Familiar - Phase II (PRAF II)	2000–2002	3,185	Geographic and family demographics	CCT ranging from $4 to $23 per month depending on family structure	4%
Morocco	Tayssir	2008–2010	4,268	Geographic	CCT and labelled CCTs: between $8 to $13 per month per child (depending on age of child)	5%
Mexico	Progresa	1998–1999	18,351	Geographic and PMT	CCT: $12.5/month + $8–$30.5/month per child (depends on child grade) + $11-$20.5 grant for school materials per child, Max grant per HH (1999): $75/month	20%
Mexico[1]	Programa de Apoyo Alimentario (PAL)	2004–2005	2,866	Geographic	UCT: $13 per month	11.50%
Philippines	Pantawid Pamilyang Pilipino Program (PPPP)	2009–2011	1,410	Geographic and PMT	CCT: $11–$30 per month depending on number of kids	11%
Indonesia	Program Keluarga Harapan (PKH)	2007–2009	14,665	Geographic and PMT	CCT: $44–$161 per year	17.50%
Nicaragua	Red de Protección Social (RPS)	2000–2002	1,433	Geographic. All except 6% who owned vehicle or ≥ 14ha land	CCT: $224/year + $112/year (school attendance) + $21/child/year	20%

Notes: The Mexico PAL experiment included two treatments: a food transfer and a cash transfer. We focus on the cash transfer treatment only.

Sources: Honduras: Galiani and McEwan (2013), Glewwe and Olinto (2004); Morocco: Benhassine et al. (2015); Mexico Progresa: Parker and Skoufias (2000); Skoufias and di Maro (2008); Mexico PAL: Skoufias and González-Cossío (2008), Skoufias, Unar, and Gonzalez-Cossio (2013); Philipinnes: Chaudhury, Friedman, and Onishi (2013); Indonesia: World Bank Office Jakarta (2011); Nicaragua: Maluccio and Flores (2005)

Data and Program Description

We began by identifying randomized evaluations of cash transfer programs in low-income and emerging nations. For a study to be included, it needed to have both a pure control group and at least one treatment arm of a conditional or unconditional cash transfer program.[10]

In total, we identified 18 randomized control trials that met the above criteria.[11] Of these, three were excluded because they did not include variables on both male and female adult labor supply in the public datasets,[12] three were excluded because the evaluated programs were not run by the government,[13] two were excluded due to baseline imbalance caused by a small number of clusters or different sampling in the control and treatment groups,[14] and we have been unable to obtain data for another three studies.[15] Online Appendix table 1 lists these excluded studies.

Therefore, we included seven RCTs in this analysis: Honduras' Programa de Asignación Familiar II (PRAF II), Morocco's Tayssir, Mexico's Progresa and Programa de Apoyo Alimentario (PAL), Philippines' Pantawid Pamilyang Pilipino

Program (PPPP), Indonesia's Program Keluarga Harapan (PKH), and Nicaragua's Red de Protección Social (RPS). A notable characteristic of all seven programs is that they are implemented by national governments (as opposed to NGOs) either as pilot or expansion programs, and thus are representative of "real-world" cash transfers. Figure 2 provides some details about the programs and evaluation data and provides references to key academic papers for each program (Online Appendix 2 provides additional information on the data).

In terms of program type, most of the programs that we include are conditional cash transfer (CCTs), where benefits are "conditional" on desirable social behaviors, such as ensuring that the recipient's children attend school and get vaccinated. The two exceptions were: (1) Mexico's PAL program, where benefits were not conditioned on behaviors, and (2) Morocco's Tayssir program, which had two treatment arms consisting of a CCT and a "labeled" cash transfer in which the conditions were recommended but were explicitly not enforced.[16] In general, it is important to note that there is considerable variation in how stringent conditions are enforced across countries, so that even in programs that are conditional "on the books", beneficiaries may still receive the full stipend amount regardless of whether they meet them.

A first challenge in these types of programs is finding the poor ("targeting"). Unlike developed countries, where program eligibility can be verified from tax returns or employment records, developing country labor markets often lack formal records on income and employment, and thus alternative targeting methods must be used (see Alatas et al. 2012, for a description). For all of the programs in our study, regions were first geographically targeted based on some form of aggregate poverty data. After that, in five out of the seven programs, eligibility was determined by a demographic criterion (e.g., a woman in the household was pregnant or there were children below an age cutoff) and/or an asset-based means test (e.g., not owning land over a certain size).

Once a household becomes eligible for any of the programs that we study, the amount of benefit that one receives is the same regardless of actual income level and lasts at least a period between two and nine years, depending on the program. This differs from many U.S. transfer programs (e.g., Earned Income Tax Credit (EITC), Supplemental Nutrition Assistance Program (SNAP)), where the stipend depends (either positively or negatively) on family income, and is updated frequently. This discrepancy likely stems from the greater difficulty in ascertaining *precise* income levels in data-poor environments. However, similar to the U.S programs, the level of the transfer received was determined, at least in part, by the number of children in the family and their ages. On net, the programs were fairly generous, ranging from 4 percent of household consumption (Honduras' PRAF II) to about 20 percent (Mexico's Progresa), though all were intended to supplement other sources of income, rather than providing sufficient income that a household could subsist on the transfer alone.

For each evaluation, we obtained the raw evaluation micro-datasets from either online downloads or personal correspondence with the authors. Two features of the evaluation design affect the analysis. First, all of the studies that we consider are clustered-randomized designs, that is, the program was randomized over locations rather than individuals. Thus, in the analysis below, we cluster our standard errors by the randomization unit. Second, we obtained both baseline and endline data for five of the studies. Baseline data were not collected for the Philippines' PPPP. Moreover, the baseline data for the treatment group of the Honduras' PRAF II study was collected in a different agricultural season than for the control group (Glewwe and Olinto 2004). Alzua, Cruces, and Ripani (2013) point out that this leads to a small but statistically significant imbalance in labor supply between the two groups and, therefore, we decided not to use the baseline for this program. Therefore, as we discuss below, we use a different empirical strategy for the programs with baseline data and those without.

While some of the studies explored impacts on some of the work variables, the sample composition and work variable definitions varied across the studies. We therefore harmonized the datasets in several ways. First, we aimed to restrict our datasets to include all adult males and females aged 16 to 65 from *eligible* households. We have two exceptions to this, where we included adults in all surveyed households (regardless of eligibility status): First, Nicaragua's RPS contains a random sample of households. About 6 percent of households were excluded from the cash transfer program based on a proxy means test, but we cannot identify them in the data. Second, Honduras' PRAF II has a random sample from households in the geographically-targeted areas; we attempted to code the eligibility rules within the evaluation dataset, but did not feel fully confident in our ability to identify eligible households and thus include all individuals.

Next, for these samples we coded consistent variables for employment status and hours worked per week for each included individual.[17] Importantly, our sample includes all individuals, regardless of whether or not they are in the labor force. Thus, if the cash transfer programs induce individuals to exit the labor force, this will be captured by our employment variable. Similarly, individuals who do not work are counted as "zero" hours of work in our analysis; thus, this variable is capturing both the decision to work (extensive margin) and the number of hours worked (intensive margin). Note that we lack information on hours of work for Indonesia's PKH program, so it is only included in the analysis on employment status.

In the poor areas where the programs that we analyze are located, a significant share of people work in agriculture (in rural areas) or in self-employment. We include both these activities in the employment status, and we later analyze two outcome variables that differentiate between household work (any self-employed activity) and work outside the household (casual or permanent employment).

Empirical Strategy

We begin our analysis by first estimating the effect of being randomized to receive a transfer program on labor market outcomes, estimating the following regression:

$$y_{ic} = \beta Treat_c + \mu_{s(c)} + \gamma \cdot \boldsymbol{X}_{ic} + \varepsilon_{\mathrm{ic}} \quad (1)$$

where i is an individual in cluster (randomization unit) c, y_{ic} is individual i's labor market outcome, either an indicator variable that takes the value of one if the individual is employed or a continuous variable on the hours an individual worked per week. Further, $Treat_c$ is an individual variable that equals one if the individual was randomly assigned to the treatment group, and zero otherwise; β is the parameter of interest, providing the difference in work outcomes between the treatment and the control group. Given the randomization, the treatment and control groups should be similar along observable and unobservable baseline characteristics. Thus, β provides the causal estimate of the program on work outcomes.

Note two features of the specification. First, while the randomization should ensure that β captures the causal program impacts, we can include additional control variables to improve our statistical precision. Specifically, we include strata fixed effects ($\mu_{s(c)}$) and a number of individual-level control variables, ($\gamma \cdot \boldsymbol{X}_{ic}$), including age, age squared, household size, years of education, and marital status dummies. For each control variable, we code missing values at the variable mean and include a dummy variable that indicates the observations with missing values. Standard errors are clustered at the randomization unit level.

We run this basic specification for the two programs for which we do not have reliable baseline data (Philippines' PPPP and Honduras' PRAF II). For the other five programs, we can take advantage of the fact that baseline data were also collected. Specifically, we stack the individual baseline and endline data and estimate the following difference-in-difference specification:

$$y_{ict} = \mu_c + Post_t + \beta(Treat_c \times Post_t) + \gamma \cdot \boldsymbol{X}_{ict} + \varepsilon_{ict} \quad (2)$$

where i is an individual in cluster c at time t. While the randomization implies that equation (1) would provide a causal estimate of the program effect, the difference-in-difference specification allows us to better control for any baseline imbalances between the treatment and control group and thus provides us with even greater statistical precision. We now include the randomization unit fixed effects, μ_c, and all of the same control variables as before, and continue to cluster our standard errors at the randomization unit.[18] The parameter of interest is again β, which provides the difference in work outcomes across the treatment and control relative to their baseline values and conditional on our control variables.

A benefit of harmonizing and re-analyzing the various micro-datasets is that we can pool the data across studies and estimate an underlying treatment effect. This allows us to potentially generate tighter statistical bounds than would be possible from any one study, which is important if we want to try to identify a real zero—or very small effect—from just noise in the data. If cash transfers have the same impact across programs, then ordinary least squares analysis on the pooled data weighs the data optimally to estimate the underlying (universal) treatment effect.

However, it is unlikely that programs across different countries and contexts have the same effect, so our pooling approach needs to model this possibility explicitly. Therefore, we use a Bayesian hierarchical model to aggregate the results from the seven studies (Rubin 1981; Meager 2016). In this model, the treatment effect τ_p in program p is allowed to vary across programs. Treatment effects corresponding to different programs are nevertheless related by a "parent distribution;" specifically, each τ_p is drawn iid from a normal distribution with mean τ and standard deviation σ_τ, $\tau_p \sim N(\tau, \sigma_\tau)$. We aim to estimate the unknown parameters τ and σ_τ that describe the parent distribution; τ captures the mean treatment effect across the programs, and

Table 2: Descriptive Statistics for Non-Program Areas

	Honduras PRAF (1)	Morocco Tayssir (2)	Philippines PPPP (3)	Mexico PAL (4)	Indonesia PKH (5)	Nicaragua RPS (6)	Mexico Progresa (7)
Panel A: Work Outcomes							
Worked last week	0.59	0.63	0.56	0.52	0.61	0.55	0.48
Worked for Self/Family	0.42	0.51	0.26	0.17		0.26	0.07
Worked Out of HH	0.26	0.16	0.29	0.27		0.29	0.38
Hours/Week	19.80	20.86	22.73	21.63		23.63	17.87
Observations	4,174	2,757	2,293	3,567	20,246	4,183	53,226
Panel B: Work Outcomes for Men							
Worked last week	0.90	0.85	0.72	0.80	0.82	0.93	0.86
Worked for Self/Family	0.67	0.63	0.31	0.30		0.46	0.10
Worked Out of HH	0.38	0.32	0.39	0.46		0.47	0.70
Hours/Week	31.70	34.29	29.51	35.80		39.51	34.56
Observations	2,132	1,272	1,215	1,647	10,198	2,131	25,850
Panel C: Work Outcomes for Women							
Worked last week	0.27	0.44	0.38	0.27	0.39	0.16	0.12
Worked for Self/Family	0.16	0.42	0.19	0.06		0.05	0.03
Worked Out of HH	0.13	0.02	0.18	0.10		0.11	0.08
Hours/Week	7.37	9.49	15.09	9.72		6.96	3.66
Observations	2,042	1,483	1,078	1,920	10,048	2,052	27,305

Note: This table reports descriptive statistics from the control group at endline. Panels A, B, and C restrict the sample respectively, to all adults, men, and women, between 16 and 65 years old. The binary work indicator is equal to 1 if the respondent reported working during the last week (last 30 days for Morocco Tayssir); the other work variables are reported for the same time frame.

σ_τ captures the dispersion in the treatment effects. Intuitively, the hierarchical model allows the data to speak about the degree of similarly of the impacts across programs, while also reaping the benefits of improved precision from pooling the data. Details of this procedure can be found in Online Appendix 1.

Descriptive Picture

Table 2 provides descriptive statistics for the standardized work variables across the studies, using data from the control group at endline to show work outcomes in the absence of the program.[19]

Many of the program recipients would have worked in the absence of the program. Pre-program employment ranged from 48 percent in Mexico Progresa to 63 percent in Morocco, with a weighted mean of 56 percent across all programs. This figures includes all adults aged 16 to 65, including those not in the labor force due to being in school, having a disability, or being retired, and thus includes people who would likely not change their status, regardless of the presence of cash transfers. Across everyone regardless of employment status, we observe about 20 hours of work per week, implying about a 40-hour work week for those who are employed.

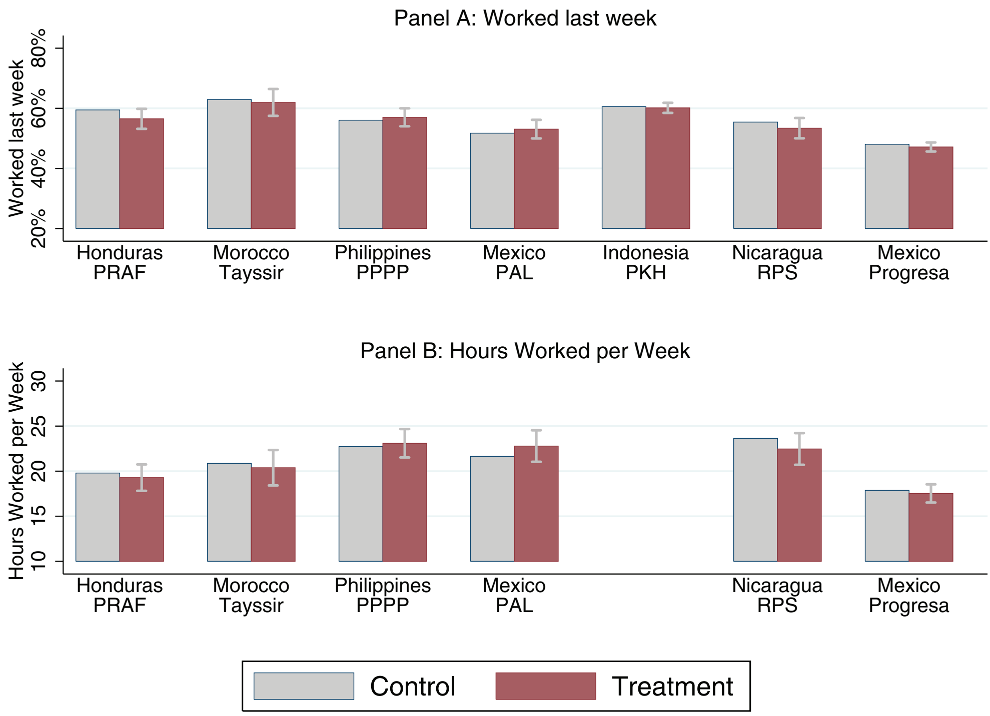
However, these means mask considerable heterogeneity in work patterns. First, *male* employment rates are high, with a weighted average of about 84 percent. In contrast, female employment rates tend to be much lower, ranging from 12 percent in Mexico Progresa to 44 percent in Morocco. Second, work outcomes tend to be split between self-employment/family work and outside work, with some exceptions: men in Honduras and both men and women in Morocco tend to be more engaged in work inside the house, while men in Mexico's Progresa program tend to be more engaged in outside work.[20]

Do Cash Transfers Reduce Work?

Overall Findings

Figure 3 provides a graphical summary of our main findings. In panel A, we graph the employment rate for all eligible adults in both the control and treatment arms for each evaluation. The evaluations are listed in order from the least generous in terms of benefits relative to consumption levels (Honduras' PRAF) to the most generous (Nicaragua's RPS and Mexico's Progresa). Panel B replicates panel A, but for hours of work. The graphs suggest that the overall numbers for both employment rate and hours of work are similar across the treatment and control groups across *all* of the programs.[21]

Figure 3. Experimental Estimates of Cash Transfers on Work Outcomes



Note: The "Control" (light, left) bars report the mean of the outcome variable (probability of work and hours worked in Panels A and B, respectively) in the control group, at endline. The "Treatment" (dark, right) bars report the control mean plus the treatment effect from in Table 3. The segments represent 95% confidence intervals.

Table 3 provides the corresponding regression analysis underlying figure 3. Panel A presents the analysis for the binary employment outcome for each individual program, while panel B does so for hours of work per week. Remember that the hours of work variable captures both intensive and extensive work decisions, thereby providing the treatment effect on total work activity.

Consistent with figure 3, we do not observe a significant effect of belonging to a transfer program on employment in six of the programs (panel A). We only find an impact in one program: in Honduras—the least generous program—we find a 3 percentage point decrease in probability of work that is significant at the 10 percent level; note that when analyzing multiple coefficients, this is roughly what we may expect by pure chance. Panel B also shows no effect on hours worked per week: none of the individual coefficients are significant, even in the Honduras data where we observed a decrease in employment status.

Table 3. Experimental Estimates of the Impact of Cash Transfer Programs on Work Outcomes

	Honduras PRAF (1)	Morocco Tayssir (2)	Philippines PPPP (3)	Mexico PAL (4)	Indonesia PKH (5)	Nicaragua RPS (6)	Mexico Progresa (7)
Panel A. Worked last week							
Treatment Effect	−0.0295*	−0.0097	0.0096	0.0135	−0.0043	−0.0202	−0.0089
	(0.0164)	(0.0227)	(0.0151)	(0.0156)	(0.0085)	(0.0167)	(0.0076)
Observations	8,486	29,832	4,527	15,598	80,851	12,979	182,940
Control Group Mean	0.59	0.63	0.56	0.52	0.61	0.55	0.48
Panel B. Hours worked per week							
Treatment Effect	−0.51	−0.48	0.37	1.15	–	−1.17	−0.34
	(0.72)	(1.00)	(0.80)	(0.88)	–	(0.87)	(0.51)
Observations	8,473	30,235	4,527	15,402	–	12,979	130,127
Control Group Mean	19.8	20.86	22.73	21.63	–	23.63	17.87
Method	endline	DD	endline	DD	DD	DD	DD

Note: This table reports regression results of the impact of cash transfers on a dummy for working (panel A) and on the number of hours worked per week (panel B). Each column reports results from a separate program. The treatment effect is the coefficient on Treatment x Follow-up for difference-in-difference (DD), and the coefficient on Treatment otherwise. Controls are age, age squared, years of education, marital status dummies (single, married or with partner, divorced or separated, and widow), household size, and survey wave fixed effects, as well as dummies for missing values for each control variable. Columns 1 and 3 include randomization strata fixed effects, columns 2, 4, 5, 6, and 7 include randomization unit (village) fixed effects. The sample is all adults between 16 and 65 years old, excluding domestic workers. The Control Group Mean reports the mean of the panel variable in the control group, at endline. Standard errors clustered at the randomization unit level are reported in round parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Even if overall labor force participation did not change, the type of work that households participate in could change as a result of the transfers. In particular, households may choose not to work outside the household due to fears that this form of employment could disqualify them from receiving benefits, regardless of whether this fear is rational or irrational according to program rules. Therefore, in table 4, we disaggregate work type by whether the work is self-employed/within the household (panel A) or outside of the household (panel B). We do this for all programs, except Indonesia's PKH, where the disaggregated data do not exist.

No clear systematic patterns emerge. In the four programs that had the least generous benefits (columns 1–4), we find no statistically observable impacts on either type of work. We find an increase in outside work and an associated decrease in within household work in Mexico's Progresa program, but the opposite pattern holds for Nicaragua's RPS program (which has a similar transfer size).

Finally, we consider men and woman separately, given the differences in baseline labor force participation. It is not clear ex ante whether we would expect

Table 4. Experimental Estimates of the Impact of Cash Transfer Programs on Household and Private Market Work Outcomes

	Honduras PRAF (1)	Morocco Tayssir (2)	Philippines PPPP (3)	Mexico PAL (4)	Indonesia PKH (5)	Nicaragua RPS (6)	Mexico Progresa (7)
Panel A. Worked in household							
Treatment Effect	0.0203	−0.0073	−0.0197	0.0054	–	0.0263**	−0.0235**
	(0.0190)	(0.0252)	(0.0191)	(0.0168)	–	(0.0130)	(0.0096)
Observations	8,483	29,832	4,527	15,598	–	12,979	182,533
Control Group Mean	0.42	0.51	0.26	0.17	–	0.26	0.07
Panel B. Worked outside the household							
Treatment Effect	−0.0335	−0.0035	0.0299	−0.0131	–	−0.0465*	0.0191*
	(0.0254)	(0.0143)	(0.0197)	(0.0178)	–	(0.0235)	(0.0103)
Observations	8,486	29,832	4,527	15,598	–	12,979	182,533
Control Group Mean	0.26	0.16	0.29	0.27	–	0.29	0.38
Method	endline	DD	endline	DD	DD	DD	DD

Note: This table reports regression results of the impact of cash transfers on a dummy for working for self/family (panel A) and on a dummy for working outside the household (panel B). See table 3 notes for specification details. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

larger effects for men or women. For example, the additional income may allow a woman who previously had to work the ability to choose to stay home with the children if she prefers, or the additional income may make it possible for her to afford additional child care and actually work more. Moreover, the literature often paints a picture of the lazy male, who uses transfer stipends to shirk and instead waste money on cigarettes and alcohol, and thus it is important to understand if these stereotypes are borne out in the data.

Table 5 replicates table 3, but disaggregates by gender. Panels A and B report results on employment for men and women, and panels C and D report results on hours for the two groups. The impact of the cash transfer programs on men's labor supply is only significantly different from zero in one program (Philippines), where it is positive. However, overall hours worked do not significantly change. For women, the impact is only significantly different from zero in one program (Honduras PRAF), where it is negative. However, none of the programs significantly affected hours worked. We also disaggregate the gender results by whether work is conducted within or outside the household (Online Appendix table 4). For men, we find a shift from working outside to inside the household in Nicaragua, but we find the exact opposite for Progresa. For women, we find slightly lower rates of working within the household in two of the six programs (Philippines PPPP and Mexico Progresa), and similarly lower rates of working outside the household in two programs (Honduras PRAF and Morocco Tayssir).

Table 5. Experimental Estimates of the Impact of Cash Transfer Programs on Work Outcomes, by Gender

	Honduras PRAF (1)	Morocco Tayssir (2)	Philippines PPPP (3)	Mexico PAL (4)	Indonesia PKH (5)	Nicaragua RPS (6)	Mexico Progresa (7)
Panel A. Worked last week - MEN							
Treatment Effect	0.0116	0.0022	0.0301*	0.0115	−0.0077	−0.0025	−0.0024
	(0.0108)	(0.0162)	(0.0175)	(0.0224)	(0.0074)	(0.0144)	(0.0074)
Observations	4,279	13,879	2,377	7,306	40,560	6,632	89,621
Control Group Mean	0.90	0.85	0.72	0.80	0.82	0.93	0.86
Panel B. Worked last week - WOMEN							
Treatment Effect	−0.0535*	−0.0166	−0.0096	0.0233	0.0010	−0.0276	−0.0174
	(0.0297)	(0.0368)	(0.0229)	(0.0223)	(0.0141)	(0.0261)	(0.0118)
Observations	4,207	15,951	2,150	8,292	40,291	6,347	93,104
Control Group Mean	0.27	0.44	0.38	0.27	0.39	0.16	0.12
Panel C. Hours worked per week - MEN							
Treatment Effect	1.16	−0.90	1.43	1.71	–	−0.67	0.37
	(0.93)	(1.56)	(1.14)	(1.53)	–	(1.26)	(0.62)
Observations	4,269	14,073	2,377	7,163	–	6,632	61,327
Control Group Mean	31.7	34.29	29.51	35.8	–	39.51	34.56
Panel D. Hours worked per week - WOMEN							
Treatment Effect	−1.56	0.05	−0.68	1.15	–	−1.18	−0.83
	(1.00)	(1.05)	(1.06)	(0.88)	–	(0.96)	(0.63)
Observations	4,204	16,158	2,150	8,239	–	6,347	68,610
Control Group Mean	7.37	9.49	15.09	9.72	–	6.96	3.66
Method	endline	DD	endline	DD	DD	DD	DD

Note: This table replicates table 3, separating results by gender. See table 3 for specification details. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Pooling the Results

Table 6 reports the results for work outcomes from pooling the results for the seven programs using the Bayesian hierarchical model described above. In pooling the programs, to make them comparable we scale the estimated treatment effect for each program by the size of the transfer.

The presented coefficients correspond to the impact of a hypothetical new cash transfer program worth 13.6 percent of household consumption, which is the average transfer size across the programs. Columns (2)–(4) provide effects on the work outcomes in levels. Columns (5)–(7) report the implied elasticities from the estimates in columns (2)–(4).[22]

The pooled estimates further confirm little program impact on work. First, the estimated impact on the extensive margin decision to work in panel A is a decrease of 0.4 percentage points from a base of 56 percent. In fact, with 95

Table 6. Pooled Impact of Cash Transfer Programs on Work Outcomes (7 Programs)

	(1) Weighted control mean	(2) Effect size (τ_p)	(3)	(4)	(5) Elasticity (τ_p)	(6)	(7)
Statistic of the posterior distribution:		Mean	5th percentile	95th percentile	Mean	5th percentile	95th percentile
Panel A. Worked last week							
Full sample:	0.56	−0.004	−0.023	0.014	−0.06	−0.30	0.18
For Men:	0.84	0.001	−0.020	0.026	0.01	−0.17	0.23
For Women:	0.29	−0.008	−0.039	0.024	−0.21	−0.99	0.60
Panel B. Hours worked per week							
Full sample:	21.1	−0.077	−1.734	1.356	−0.03	−0.60	0.47
For Men:	34.2	0.470	−1.702	2.965	0.10	−0.37	0.64
For Women:	8.7	−0.430	−2.588	1.611	−0.36	−2.18	1.36

Note: This table reports results from a Bayesian hierarchical model used to aggregate the results from the seven programs. The impact for each program from table 3 or table 5 is first scaled according to the size of the transfer, such that for each program the scaled coefficient corresponds to a transfer worth 13.6% of consumption. (The program transfer size is defined as the average transfer value relative to average consumption.) Column (1) reports the mean of the row variable in the control group at endline, averaged over the seven programs. Columns (2)-(4) present the mean, and the 5th and 95th percentiles of the posterior distribution of the site effect τ_p, which measures the impact for a hypothetical new program. Columns (5)-(7) report the same statistics for the elasticity of the work outcome with respect to the size of the cash transfer. Bayesian posteriors are computed using the rstan package, 20,000 iterations on four chains, thinning the result by a factor of two.

percent probability, a new program has an impact no lower than a 2.3 percentage points reduction in work status. Conversely, with 5 percent probability a new program will tend to increase work status by at least 1.4 percentage points. Similarly, for hours of work, the point estimate corresponds to a decrease of five minutes of work per week, from a base of 21 hours. With 95 percent probability, a new program will not reduce hours of work by more than 1 hour and 42 minutes per week.

In terms of elasticities, the estimates in columns (5)–(7) indicate that on average, a new program worth 10 percent more of household consumption will tend to reduce work status by 0.6 percent, and with 95 percent probability this effect will not be lower than a 3 percent decrease in work. For hours of work, on average such a program will tend to reduce work by 0.3 percent, and with 95 percent probability this effect is no lower than a 6 percent reduction in hours. These effects are broadly symmetric around zero, offering very little evidence of a negative impact of cash transfers on work outcomes.[23]

Looking at results separately by gender, for men the average effects are positive and more precise. Indeed, the results show a 0.1 percentage point increase in work status, and a positive elasticity of + 0.01, while with 95 percent probability the impact in a new program will not reduce work by more than 2 percentage points, and the elasticity will not be lower than -0.17. We further find a half hour increase per week due to cash transfers in panel B, and a positive elasticity of $+0.10$. Once again, we can reject moderate negative effects with a high probability.

For women, the average effects are negative but small, corresponding to a 0.8 percentage point decrease in work status and half an hour of work less per week. Due to the low mean of these work outcome variables in the control group for women, the implied elasticities are moderately negative, between -0.2 and -0.36. However, the Bayesian meta-analysis points to significant uncertainty in the impact of a new cash transfer program for women, with estimates for work status in columns (3)–(4) and (6)–(7) between a 3.9 percentage point reduction and a 2.4 percentage point increase, and an elasticity between -1 and $+0.6$. Similar results for hours worked indicate that the existing data covers a high range of effects for women.

Understanding Mechanisms: Exploring the "Tax" Rate

As described above, transfer programs can have a negative effect on work for two reasons: (1) the income effect, and (2) individuals choosing to work less in fear of losing their benefits ("the tax rate" or "benefit withdrawal rate"). As we found little evidence of a systematic negative effect of the transfer programs across all of the countries that we examined, we now test to see whether this is rational given the expected "tax rates" of these programs.

To examine the tax rate, it is important to examine two aspects of the program. First, consider how individuals are added and subtracted from the list ("targeting"). In developed countries, programs are targeted based on income measured from administrative sources and recertified frequently. In contrast, obtaining frequent or real-time information about income is challenging in developing countries, and so targeting is often conducted infrequently through alternatives methods—proxy means tests, geographic targeting, etc. Bosch and Manacorda (2012), Grosh et al (2008), and Alderman and Yemtsov (2013), among others, have argued that the fact that targeting is less connected to current income suggests that taxes are low, and therefore, these programs are less likely to cause negative labor supply effects. Second, it is important to understand the size of the transfers. For example, Alderman and Yemtsov (2013) argue that the size of the transfer programs is often insufficient to live from, and thus, a small gain of income from the program is not enough to keep people out of the workforce.

Turning to the programs we consider, the way the targeting rules are designed suggests that the tax is, if anything, very small, since eligibly is rarely based directly on current observable income. In two out of the seven programs (Morocco Tayssir and Mexico PAL), targeting is purely geographic, meaning that everyone within a chosen region received the program. This implies that any individual's behavior is likely not to affect the probability of their receipt and thus the implied tax rate on labor income is effectively zero. Similarly, the Honduras PRAF selects beneficiaries within geographically-targeted regions if the household includes a pregnant woman or children under age three, and so eligibility is not driven by work status. In the Nicaragua RPS, after the geographic targeting, a small fraction of households (6 percent) were excluded based on a simplified asset test, and thus most households are not going to lose eligibility status if they work more. In short, for about half of the programs, eligibility is not directly related to current employment or income, effectively implying no tax. Thus, one would expect close to no labor supply effects unless income effects were unusually large.

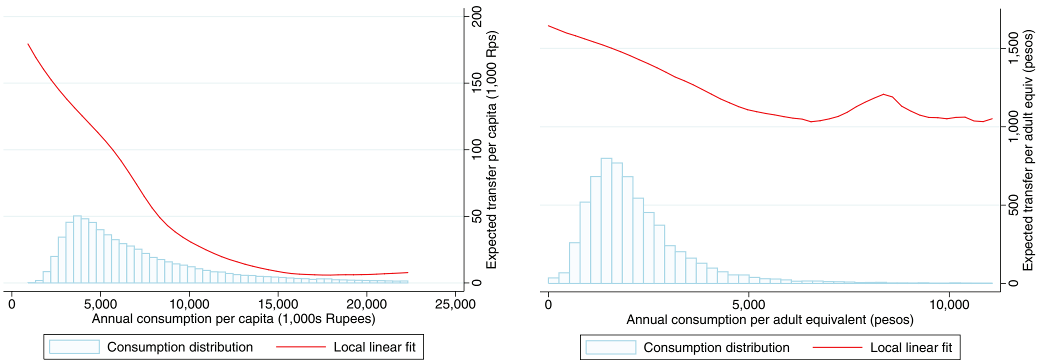
In the remaining three programs (Philippines PPPP, Indonesia PKH, and Mexico Progresa), beneficiaries are selected based on a full-fledged asset test (proxy means test or PMT). For two of these studies (Indonesia PKH and Mexico Progresa), we can examine the perceived implicit tax rate with respect to consumption by graphing the relationship between the expected total transfer for households at different consumption levels. The slope of the relationship represents the perceived, implicit tax rate with respect to consumption. Note that in so doing, we assume that households know exactly when they will be assessed for targeting purposes, so we assign as the 'cost' of working more the potential loss of the full net present value of the program for all the years they would then receive it.[24]

We document a weak relationship for both programs in figure 4, as households with higher consumption have only marginally lower total expected transfer size. Starting with Indonesia's PKH, a household with Rp. 1,000 higher per capita annual consumption will receive in expectation Rp. 40 less in net present value transfers, calculated over a period of six years.[25] This is not surprising, as only 4.5 % *percent* of households receive the cash transfer, and among recipients the transfer is on average 10 percent of consumption.[26] These factors attenuate the relationship between consumption and expected transfers.

In the case of Progresa, the fraction of eligible is higher (60 percent), the cash transfer is a larger fraction of household consumption (25 percent), and households receive benefits for nine years. Nevertheless, the implied tax rate is 15 percent.[27] Thus, even for large transfers that cover about half the population, imprecise targeting attenuates the relationship between poverty and expected transfers.

In short, the targeting rules of these programs, coupled with the size of the transfers, provides one reason why we do not observe systematic negative effects of the transfer programs across the differing settings. Our findings on the implicit tax rates in Indonesia and Mexico echo Ravallion and Chen (2013), who measure the tax rate imposed by the Chinese Di Bao cash transfer program. The largest estimate that they find is 15 percent, much lower than the theoretical 100

Figure 4. Consumption and Expected Cash Transfer Receipt



Notes: Each panel displays a local linear regression of the expected transfer per capita as a function of consumption per capita (solid line), and a histogram of consumption per capita (bars). Panel (A) reports results from the PKH program in Indonesia using SUSENAS 2013 data in all districts where at least 1% of respondents report being PKH beneficiaries ($N = 101{,}568$); we coded the transfer size (in 2013) for each household, depending on whether they report receiving PKH, and on the number of children and their ages. The total transfer is the net present value of transfers over six years, assuming a discount factor $\delta = 0.9$. Panel (B) reports results from the Progresa program in Mexico, using data from the October 1998 ENCEL survey (which is included in our main results). We use the eligibility variable together with the average per adult equivalent transfer size of 32.5 pesos per month reported in Angelucci and De Giorgi (2009). The total transfer is the net present value of transfers over nine years, assuming a discount factor $\delta = 0.9$. We use per adult equivalent consumption from the same study. In both panels, we drop the top 5% of the consumption distribution.

percent tax rate implied by its goal of providing a means tested guaranteed minimum income.

Comparison with Asset Transfer Programs

Our analysis has focused on cash transfers programs that provide small amounts of money either monthly or quarterly to poor households. However, a policy alternative to cash transfers is an *asset transfer program*, which is typically a one-time intervention where the beneficiary receives a productive asset (or money to buy such an asset), with the idea that they will benefit from the asset's future income stream. The labor supply effect of an asset program could be quite different from that of a cash transfer because it is a lump sum or a lumpy asset (e.g., livestock or tools for a business), an amount of which savings market failures might prevent households from accumulating from the transfer funds. If it is a productive asset that requires complementary household labor to use, the presence of the asset would quite naturally encourage additional work effort. Labor supply could also increase if the household combines the lump sum with a loan to purchase a consumer durable that complements the asset, but then needs to work harder to pay down the loan.

We can, thus, qualitatively compare the effects of cash transfers with these asset programs. One version of the program is the so-called graduation model, developed by BRAC in Bangladesh. Under this model, households, chosen for being the poorest members of poor communities, are given an asset of their choosing (from a set of affordable assets) as well as some training and support, including a small income stipend for a period of no more than six months. An RCT of this program by Bandiera et al. (2013) reports, "After four years, eligible women work 170 fewer hours per year in wage employment (a 26% reduction relative to baseline) and 388 more hours in self-employment (a 92% increase relative to baseline). Hence total annual labor supply increases by an additional 218 hours which represents an increase of 19% relative to baseline." Another RCT by Banerjee et al. (2015) of this program in six different countries (Ethiopia, Ghana, Honduras, India, Pakistan, and Peru), reports that total labor supply across the six sites went up by 10 percent of the control group mean (or about 85 hours a year), two years after the start of the program. Consistent with this, both the Bangladesh study and the multi-country study also find increases in income and consumption of commensurate magnitudes in these households.

There is also evidence from a small number of lump sum cash transfer programs. Blattman et al. (2016) carry out a randomized evaluation of a program where women in Northern Uganda—most of whom had never run a business before—were given a package comprised of $150 in cash, five days of business training, and ongoing supervision. These authors find that hours worked per week go up by a stunning 10 hours and, correspondingly, there is a doubling of new nonfarm enterprises and a significant rise in income. Blattman, Fiala, and Martinez (2014) evaluate the Youth Opportunities Program (YOP), a government program in northern Uganda designed to help unemployed adults become self-employed artisans. The government invited young adults to form groups and prepare proposals for how they would use a grant to train in and start independent trades. Funding was randomly assigned among 535 screened, eligible applicant groups. Successful proposals received a one-time unsupervised grants worth $7,500 on average—about $382 per group member, roughly their average annual income. After four years the treatment group had 57 percent greater capital stocks, 38 percent higher earnings, *and 17 percent more hours of work* than did the control group.

Perhaps not surprisingly, these programs have a strong and clear positive effect on labor supply, in contrast with the more or less zero effect we find from the income support style cash transfer programs. However, it is very important to note two aspects of these programs. First, all of these programs combined assets (or cash for assets) with training and support, and so the evidence is not yet available as to whether supervision is needed to achieve these increases in work or just the asset transfer would be enough. Moreover, it is likely that labor supply is a complementary input to the asset; for example, a cow or goat needs to be fed and taken care of. Future research is needed to disentangle the contributions of the various aspects of the programs. Second, in thinking through large-scale implementation across governments, physical assets (and in-kind transfers, in general) are often more expensive to distribute than cash. Moreover, we often observe leakages in the distribution of in-kind goods in many developing countries, with the goods never reaching program beneficiaries. New advances in technologies for distributing cash, such as mobile money, may make it easier to provide cash *directly* to beneficiaries with both potentially low leakage and low costs. Thus, research into understanding how large-scale physical asset distribution programs fare against these newer ways to distribute cash is also important for policy.

Conclusion

In recent years, there has been a large growth in transfer programs across the developing world. If anything, we might expect this trend to increase as countries grow: Chetty and Looney (2007) show that social insurance as a fraction of GDP rises as countries get richer, suggesting that safety nets may be increasingly important as countries grow and develop.

As transfer programs have increased, so has the debate about whether they simply discourage work, enabling a "lazy poor." Aggregating evidence from randomized evaluations of seven government cash transfer programs, we find no systematic evidence of an impact of transfers on work behavior, either for men or women. Moreover, a 2014 review of transfer programs worldwide by Evans and Popova (2014) also shows no evidence—despite claims in the policy debate—that the transfers induce increases in spending on temptation goods, such as alcohol and tobacco. Thus, on net, the available evidence implies that cash transfer programs do not induce the "bad" behaviors that are often attributed to them in the policy space. Combined with the positive effects of transfer programs documented in the literature, this suggests that transfers can be an effective policy lever to help combat poverty and inequality.

Notes

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1. Note that this includes both in-kind and cash transfer programs.

2. See for example, the Ashenfelter and Plant (1990) analysis of the Seattle–Denver Maintenance Experiment, or Imbens, Rubin, Sacerdote (2001) estimates of the effect of unearned income on work from studying lottery winners.

3. This extends Alzua, Cruces, and Ripani (2013), which explores the program impacts on labor outcomes for three of the programs that we include. While we use slightly different specifications to harmonize across the full set of datasets that we include, our findings echo theirs.

4. Our sample covers countries from Latin America, Asia, and the Middle East. Unfortunately, randomized control trials for South Asia or for African countries do not exist, do not include labor supply information, or do not have publically available data. This is an important area for future research to extend this type of analysis to these settings, which are on net lower income than the countries in our sample.

5. Note that households may not necessarily shift to "leisure," but could shift to spending their time in productive ways. For example, a benefit of cash transfers could potentially be a reduction in child labor and a concurrent increase in the child's education (see Behrman, Parker, and Todd 2011).

6. Evidence from developed countries tries to isolate this effect by looking at lottery winners. These studies generally find that the pure income effect on labor supply is modest (Imbens, Rubin, and Sacerdote 2001; Cesarini et al. 2015). In developing countries, Haushofer and Shapiro (2013) study a (non-governmental) large unconditional cash transfer in Kenya and do not detect any impact on total business profit or wage labor as primary income. Yang (2008) finds that in the Philippines there is no impact on aggregate household labor supply due to changes in remittances due to exchange rate shocks.

7. We have excluded studies of programs that contain explicit work requirements, such as India's National Rural Employment Guarantee Act (NREGA) and Argentina's *Jefes y Jefas*.

8. Other randomized studies of cash transfers in African countries do not report results on adult labor supply decisions: Evans et al. (2014) in Tanzania, Akresh, De Walque, and Kazianga (2013) in Burkina Faso, Baird, McIntosh, and Ozler (2011) in Malawi, and Gilligan and Roy (2013) in Uganda.

9. In India, cash transfers are extremely rare. Only 0.0035% of GDP goes to cash transfers (compared to 0.72% on social assistance in general), which ranks India below the 5th percentile in the ASPIRE dataset of 88 countries in terms of spending on cash transfers.

10. Some studies experimentally compare different ways of running a transfer program on recipients. While these provide valuable information on program design, they do not allow us to assess the full impact of introducing the program to begin with.

11. We apologize in advance if we have missed a particular study that meets our criterion. We tried to be as complete and systematic as possible.

12. Ecuador's Bono de Desarrollo Humano (BDH) (Edmonds and Schady 2012; Schady and Caridad Araujo 2008), Nicaragua's Atención a Crisis (Macours, Schady, and Vakis 2012), and (Baird, McIntosh, and Ozler 2011) in Malawi.

13. Subsidios Condicionados a la Asistencia Escolar (SCAE) in Colombia (Barrera-Osorio et al. 2011), GiveDirectly in Kenya (Haushofer and Shapiro 2013), and a cash transfer for preschool in Uganda (Gilligan and Roy 2013).

14. Treatment status was randomized over eight communities in Malawi's Social Cash Transfer Scheme (SCT) program (Covarrubias, Davis, and Winters 2012). Despite having a larger number of households, the small number of randomization units led to baseline imbalance on a number of indicators, and biases one towards not being able to measure a statistically significant effect unless the effect size is very large; therefore, we did not include this study. Sampling of beneficiaries and potential beneficiaries was done differently in treatment and control groups in Kenya's CT-OVC (Asfaw et al. 2014), which led to large baseline imbalances.

15. The data for Tanzania's Tanzania Social Action Fund (TASAF) (Evans, et al. 2014) is not yet available, and we were not able to obtain data for Burkina Faso's Nahouri Cash Transfers Pilot

Project (NCTPP) (Akresh, De Walque, and Kazianga, 2013) and Zambia's Child Program (American Institutes for Research 2013).

16. Mexico's PAL program also had an in-kind treatment, which we do not utilize for this analysis.

17. All programs except for Morocco's ask about the number of hours worked during the last week. In Morocco, the reference period is the last 30 days, and we normalize the response by 7/30.

18. There are two additional differences across specifications. First, as Mexico's Progresa includes three endline waves and Nicaragua's RPS has two endline waves, we additionally include wave dummy variables in these specifications. Second, we weight observations in Morocco's Tayssir to account for the sampling structure as in Benhassine et al. (2015).

19. We provide the control group statistics since we do not have baseline data for two of the programs and the definitions of work are not the same in the baseline and endline for one of the evaluations (Morocco's Tayssir).

20. Appendix tables 2 and 3 report the baseline balance check by program, or in the case of the two programs without baseline, the balance on demographic characteristics at endline. With the exception of PAL and Progresa—for which the analysis in tables 3–6 uses the difference-in-difference specification—the joint significance tests do not reject balance.

21. Appendix figure 1 considers hours of work conditional on working status. The same pattern of results emerges.

22. These elasticities are $\frac{d \log ProbabilityWork}{d \log Income}$ and $\frac{d \log HoursWorked}{d \log Income}$ in panels A and B, respectively. To compute these elasticities, we take the estimated treatment effect in columns (2)–(4), divide by the mean of the outcome (probability work or hours worked) from column (1), and divide by the average increase in income due to the transfer (13.6 percent).

23. Online Appendix table 5 presents results for pooled results inside the household and work outside the household for men and women. The average impacts listed in column (2) are always close to zero, slightly positive for men and slightly negative for women. While there are a range of possible impacts on work (see columns 3 and 4), in all cases the zero effect is comfortably within the distribution of impacts. That is, there is no consistently negative effect of the transfer programs on work for any of the subgroups considered here.

24. An alternative assumption would be to assume that, ex-ante, households do not know in which year the targeting will take place. This assumption would yield effective tax rates that are 6 and 3-9 times smaller than the estimates reported here, for Indonesia and Mexico Progresa, respectively.

25. This is calculated over the steepest part of the graph in figure 3. We obtain essentially the same value when we include census area fixed effects.

26. This fraction is lower than the one reported in figure 1, because we use a different data source than for the main results, namely the SUSENAS national survey from 2013.

27. The fraction of eligible is larger than the one reported in figure 1, because we only used one of the follow up surveys (October 1998 ENCEL). Households are re-certified after three years, yet they continue to receive benefits for at least six more years. The implied tax rate with village fixed effects is 13 percent.

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