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Cumulative Impacts of Conditional Cash Transfer Programs: Experimental Evidence from Indonesia[†]

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Conditional cash transfers provide income and promote human capital investments. Yet evaluating their longitudinal impacts is hard, as most experimental evaluations treat control locations after a few years. We examine such impacts in Indonesia after six years, where the program rollout left the experiment largely intact. We find static effects on many targeted indicators: childbirth using trained professionals increased dramatically, and under-15 children not in school fell by half. We observe impacts requiring cumulative investments: stunting fell by 23 percent. While human capital accumulation increased, the transfers did not lead to transformative economic change for recipient households. (JEL I21, I38, J13, J24, O15)

Perhaps the most remarkable innovation in welfare programs in developing countries over the past few decades has been the invention and spread of conditional cash transfer programs (CCTs). These programs provide regular cash transfers to poor households to help reduce poverty but condition the transfers on households making a series of human capital investments in their young children. These conditions typically begin before birth—prenatal care and deliveries by trained midwives or doctors are usually conditions—and continue through early childhood health investments (for example, immunizations and growth monitoring) and enrollment in primary and junior secondary school. These programs began in the 1990s in Mexico, Bangladesh, and Brazil, and today over 63 countries have at least one CCT program

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(Bastagli et al. 2016), covering millions of families worldwide (Robles, Rubio, and Stampini 2015; World Bank 2018).

The theory behind these conditions—and the reason they start before birth and continue throughout childhood—is that static investments in human capital at every stage of the life cycle will accumulate as children grow, and the cumulative investments in human capital will eventually lead to improvements in child outcomes that may affect intergenerational poverty. For example, Santiago Levy, who helped create the CCT model with Mexico's PROGRESA program in the 1990s, argued, “clearly, achieving good health is a cumulative process, and temporary investments in nutrition are of little help. The same is true of education: children must be supported year after year.... [PROGRESA's] central effects will gradually occur through the accumulation of human capital” (Levy 2006, 18).

Given the worldwide scope of CCT programs, there has been substantial interest in understanding whether static CCT conditions actually lead to cumulative improvements in child outcomes, but it is empirically challenging to answer this question. Many CCTs, starting with PROGRESA, began with randomized controlled trials on a pilot basis prior to scale-up, and the vast majority of the evidence on their impacts comes from these trials (for example, see Behrman and Todd 1999, Gertler 2004). However, most of these programs extended the CCT to the control group after a relatively short pilot period—18 months in the PROGRESA case, for example.¹ While this phase-in experimental design is useful for studying the short-run, static impacts of the CCT on the health and education behaviors they incentivize, the fact that the control group is ultimately treated makes it much harder to credibly estimate cumulative effects from sustained exposure to the programs over time.²

A second, related question is whether these government welfare programs themselves continue to be effective—even in the static sense of maintaining increased compliance with incentivized behaviors. Some have argued that interventions are often less effective when implemented by the government at scale than in a smaller pilot stage, when researchers are more likely to be paying attention to the implementation (see, for example, Bold et al. 2015 and the related discussions in Banerjee et al. 2017 and Muralidharan and Niehaus 2017). More generally, a CCT program's effects could weaken over time after people's initial excitement of being in the program fades or once people learn that the conditions placed on health and education behaviors are not always enforced perfectly. Since most CCT experiments extend

¹To our knowledge, there are two notable exceptions. First, Barrera-Orsorio, Linden, and Saavedra (2019) experimentally evaluate the long-run effect (8–12 years) of an unusual CCT and savings program in Bogotá, Colombia that focused on incentivizing high school enrollment and studied the effect on tertiary enrollment in universities. Second, in a paper contemporaneous to this one, Molina Millán et al. (2018) study the education effects of the Honduran CCT, which was implemented for five years.

²Some papers exploit the time gap between treatment and control groups receiving the program (even if for a short period) and thus look at medium- or long-run effects for children affected for longer periods by the program (for example, see Behrman, Parker, and Todd 2011; Barham, Macours, and Maluccio 2017; Kugler and Rojas 2018). While this approach is very informative in terms of some outcomes, it could underestimate impacts as the control group is also exposed to the program just a few years later. Others exploit nonexperimental variation using discontinuities in who received the transfers (e.g., Filmer and Schady 2014) or cohort analysis across areas with higher and lower program intensity (Parker and Vogl 2017). A third approach is to study long-run effects of temporary programs, such as in Baird, McIntosh, and Özler (2016), who compare treatment and control areas two years following the end of a two-year temporary transfer program.

the program to the control group after a relatively short time, understanding whether the programs continue to be effective even in a static sense after a short experimental initial period is also challenging.

This study aims to answer these questions using an unusual, large-scale policy experiment. We study Indonesia's conditional cash transfer program, known as *Program Keluarga Harapan* (Family Hope Program, or PKH). Starting in 2007, the government introduced PKH in 438 subdistricts across Indonesia (selected randomly from a pool of 736 subdistricts) to a total of about 700,000 households. The unit of randomization, the Indonesian subdistrict, is large—a subdistrict has about 50,000 people, and the 736 subdistricts in the experimental sample have a total population of over 36 million people. Targeted households received between 600,000 and 2,200,000 rupiah (approximately US\$60 to US\$220) per year, with typical CCT conditions for children (pre- and postnatal care, deliveries with trained birth attendants, regular growth monitoring, immunizations, enrollment and attendance of children in primary and junior secondary school). Households that were enrolled into the program in 2007 continue to receive quarterly benefits today. The World Bank conducted a follow-up survey in 2009, about 2 years after the rollout, in a randomly chosen subset of 360 subdistricts, intended to be the end of the evaluation (Alatas 2011).

Crucially, while PKH has subsequently been expanded to many more areas in Indonesia—by 2013 it had reached over 3,400 subdistricts, spread over 336 districts in all of Indonesia's provinces, and covered over 2.3 million households—60 percent of the initial control subdistricts were still not treated nearly 6 years later. The reason for this expansion to new provinces and districts, rather than to the control group, was that the government prioritized the expansion of the program to new areas such that the program would be spread throughout the country rather than focused intensely in a small number of geographic areas.

For research, however, this presents a unique opportunity because the initial randomization of subdistricts to treatment and control status continued to induce random variation in program placement six years later. To study how sustained CCT benefits affect families over this longer time horizon, in 2013 we resurveyed households that were in the initial experiment. Notably, we resurveyed successfully at least 1 household member from 95 percent of the original 14,326 households in the baseline survey. We show that the experimental first stage—the regression of whether a household is receiving PKH on whether the household's subdistrict was randomized in 2007 to be in the treatment group—is strong (F -statistic over 400). This unique setup—wherein the experiment continued to run at scale by the government for over six years without any researcher intervention in the program implementation—allows us to examine whether the static effects of CCTs on targeted indicators persisted even as the program continued over time as well as whether these human capital investments began to cumulate as children grew up exposed to the program.

We start by examining whether conditional cash transfers continued to have impacts on incentivized behaviors, even after the program had been running at scale for six years. We find remarkable effects on several of the incentivized indicators, which remain significant even accounting for multiple hypothesis testing. In

particular, treated households were more likely to have childbirth assisted by a skilled birth attendant (doctor or midwife; increased by 23 percentage points) and delivery at facility for those who had given birth (increased by 17 percentage points). These are dramatic increases—they imply that the CCT program reduced the share of children not born at a health facility by 62 percent and virtually eliminated all births not delivered by a trained midwife or doctor. Conditional cash transfers also had large impacts on reducing the share of children not in school: school enrollment rates for the targeted age group—7- to 15-year-olds—were about 4 percentage points higher for the treatment group than for the control group in the 6-year follow-up. Since 92.4 percent of control group children were already enrolled in school, this means that the program eliminated 53 percent of nonenrollment.

We then turn to whether continued exposure to the conditional cash transfers began to lead to results on outcomes that require cumulative investments. We find very large impacts on children's propensity to be stunted or severely stunted.³ In fact, we observe a 23 percent reduction in the probability of being stunted (defined as being 2 standard deviations less than the WHO's height-for-age standard) and a 56 percent reduction in the probability of being severely stunted (3 standard deviations less than the WHO's height-for-age standard). There were no detectable stunting effects in the two-year follow-up. We find no impact on malnourishment (two standard deviations less than the WHO's weight-for-age standard).

To capture the cumulative effects of educational investments, we look at impacts on older children—aged 15–21 at the time of our surveys, who were in the target age range (aged 9–15) at the time PKH began. We find evidence that children aged 15–17 were about 10 percentage points more likely to still be enrolled in school, reducing the nonenrollment rate by 27 percent. We also find some evidence that high school completion rates increased—by 7 percentage points, representing a 29 percent increase (p -value 0.14 after adjusting for multiple hypothesis testing). We find no evidence that this translated to a higher likelihood of wage employment for those aged 18 to 21 years, nor do we find impacts on early marriage.⁴

The final piece of our analysis is to examine whether the continued cash transfers—which add up to an average of US\$970 per household over the six-year period—had a transformative effect on the recipient households themselves. For example, Gertler, Martinez, and Rubio-Codina (2012) find that PROGRESA households invested a fraction of accumulated transfers in productive assets, which could affect the overall poverty status of the household. However, we find no evidence of this here. While the point estimates of the impact on consumption are positive, we cannot distinguish the measured impacts from zero; we also cannot reject effects equal to the size of the transfer, which was about 7 percent of household consumption. What we can rule out definitively, however, are transformational impacts on household consumption: given our confidence intervals, we can reject that household per capita consumption increased by more than about 10 percent. We also find

³Weight-based measures respond more quickly to nutrition and health status, whereas stunting is thought to respond to accumulated early childhood conditions (UNICEF 2013, Hoddinott et al. 2013a).

⁴Note that unlike the results on stunting, where children were on PKH most of their lives, the children in the 15–21 age category were older to begin with when PKH started. If we believe that cumulative effects come from PKH's focus on early childhood, these children are less likely to have been impacted than the younger children.

no observable effect on productive household assets, such as livestock owned, or on fixed assets, such as land.

In short, conditional cash transfers in Indonesia continued to have impacts on the incentivized health and educational investments of households six years after program introduction: in particular, the program continued to impact primary and secondary school education attainment and deliveries in a facility by trained birth attendants. This occurred despite the fact that the level of benefits fell from 14 to 7 percent of monthly household consumption and also despite the fact that the program was being run with business-as-usual practices by the government (without any researcher involvement). And, perhaps more importantly, after continued investment in children over time, we begin to see some substantial results on “cumulative outcomes,” which was the original rationale for sustained payments over time. In particular, stunting was greatly reduced, suggesting large health gains, and school enrollment for high school-age children increased. On the other hand, we see no transformational effects over six years of repeated cash transfers on the incomes of the beneficiary households themselves. Combined, this suggests that if conditional cash transfers are going to indeed break the cycle of poverty, this effect is going to happen through impacts on the subsequent generation rather than through impacts on households themselves.

The rest of the paper proceeds as follows. We describe the setting, experimental design, and data in Section I. We provide the findings in Section II, while Section III concludes.

I. Setting, Experimental Design, and Data

A. Program

We study the cumulative (six-year) effects of the Indonesian government’s conditional cash transfer program, Program Keluarga Harapan (PKH or “Family Hope Program”). Launched in 2007, the program provides quarterly cash transfers to very poor households with children or pregnant and/or lactating women, with a fraction of the payment conditional on a number of health- and education-related obligations. By providing a sustained flow of payments to families over many years, the program aims “(a) to reduce current poverty and (b) to improve the quality of human resources among poor households” (Alatas 2011, 11).

The government targeted extremely poor households, approximately in the bottom 10 percent of the per capita consumption distribution. To determine their eligibility, Statistics Indonesia (BPS) conducted a door-to-door survey of potentially eligible households; the survey included 29 asset and demographic questions.⁵ They applied a proxy-means test formula to this data, and households that were below a predetermined cutoff were deemed to be financially eligible. Statistics Indonesia then kept households that also met demographic requirements: households with a

⁵BPS visited all households that had been included in a previous 2005 unconditional cash transfer (UCT) program, and they also worked with local officials to visit any potentially poor households that may have not been included.

pregnant and/or lactating woman, with children aged 0–15 years, and/or with children aged 16–18 years who had not completed 9 years of basic education.

Eligible households began receiving quarterly cash payments through the nearest postal office. The amount of cash was designed to be about 15 to 20 percent of annual household income, depending on the age of the children; payments ranged from Rp 600,000 (US\$60) to Rp 2,200,000 (US\$220) per household per year. The payments were made to women in the household. As with most conditional cash transfer programs, households were informed that they had to complete a number of conditions to continue receiving the transfers. For example, households with children aged zero to six years needed to ensure that children completed childhood immunizations and take vitamin A capsules a minimum of twice per year, and also must take children for growth monitoring checkups (see online Appendix Figure 1 for the full list of conditions). Trained facilitators provided beneficiaries with information and advice and also verified compliance with conditions: one violation would result in a warning letter, a second violation would lead to a 10 percent cut in benefits, and a third violation would lead to program expulsion. However, in practice, the verification system did not begin until at least 2010, and even afterwards, conditions were not always enforced. In that sense, this program is more akin to a “labeled” CCT program, such as the Moroccan program studied by Benhassine et al. (2015).

B. Sample, Experimental Design, and Timing

The government of Indonesia first introduced the conditional cash transfer program in six provinces (West Java, East Java, North Sulawesi, Gorontalo, East Nusa Tenggara, and the capital city of Jakarta). Within each province, the government excluded the richest 20 percent of districts and then determined which subdistricts within the remaining districts were “supply-side ready” (based on availability of midwives, doctors, and middle schools) to participate in the program. A total of 736 subdistricts (with a total population of about 36 million individuals in 2005) were included in their sample, and 438 of these subdistricts were selected randomly for the treatment group. About 700,000 households in these selected subdistricts were enrolled in the conditional cash transfer program.

Out of the 736 sample subdistricts, 360 subdistricts were randomly chosen for data collection (180 treatment, 180 control). Online Appendix Figure 2 shows the distribution of sample subdistricts, by experimental assignment, across Indonesia, including both on and off Java.

As shown in online Appendix Figure 3, the World Bank conducted a baseline survey from June to August 2007, and the program was launched in these subdistricts soon afterward. The World Bank conducted a follow-up survey from October to December 2009, about two years after the start of the program; the results are described in Alatas (2011), and we reanalyze these data below to ensure comparability with our analysis. We conducted a follow-up survey from September to November 2013, about six years after the intervention, using identical survey questionnaires.

The evaluation we conduct is possible because subsequent program expansions kept the control group largely intact. Online Appendix Figure 4 shows the evolution

of the PKH program over the time period that we study based on administrative data on the program's expansion. In 2009 (at the time of the two-year follow-up), the program was operating in 99 percent of the locations randomized to treatment and in 22 percent of locations randomized to control, which implied a subdistrict-level "first stage" of 77 percentage points in 2009. By 2013 (six-year follow-up), the program had expanded somewhat, but the experiment still remained intact: the program was operating in 99 percent of locations randomized to treatment and in 39 percent of locations randomized to control for an implied subdistrict-level "first stage" of 60 percentage points. Thus, after six years, the original subdistrict-level randomization still had substantial bite. Moreover, because the program reached fewer households in areas in the control subdistricts that received the expansion between 2009 and 2013, the first stage for receiving PKH at the household level is virtually identical in both 2009 and 2013. As described below, we use the original subdistrict randomization as an instrument for treatment.

C. Data, Data Collection, and Experimental Validity

The World Bank collected both a baseline survey and initial follow-up survey in the 360 subdistricts to assess PKH's short-run program impacts (see Alatas 2011 for more details). These surveys were conducted using the same survey instruments as, and in tandem with, the evaluation of the *Generasi* community block grant program, which was being carried out in 300 separate subdistricts but was targeting similar indicators (see Olken, Onishi, and Wong 2014).

As shown in online Appendix Table 1, 14,326 households (73,578 individuals) were surveyed at baseline in the 360 sampled subdistricts between June and August 2007. To create this sampling frame, eight randomly selected villages were drawn from each subdistrict, and then one subvillage was selected within each village.⁶ From within each village, four households were selected randomly from the government's interview lists, stratified such that two households included a pregnant or lactating mother or a married woman who was pregnant within the last two years, and the other two included children aged 6–15. Note that since the survey was conducted in both treatment and control areas (and we do not know who would have received the conditional cash transfers in the control areas), households were selected randomly to be surveyed from the initial asset listing (not the beneficiary list), so not all households would have ultimately received the CCT. There was very little attrition of households in the first follow-up survey that was conducted from October to December 2009: 13,971 households (97.5 percent of baseline) were found and surveyed, and households that split and moved within the subdistrict were also surveyed (so the sample size increases slightly in each round).

Both the baseline and follow-up surveys included modules for consumption, demographics, assets, education, and health outcomes. Additionally, they included anthropometric data (height and weight measurements) for children aged 0 to

⁶If there were fewer than eight villages sampled in a subdistrict (since there were not enough eligible households in enough villages) or if there were fewer than five potential households to survey in the subvillage, additional subvillages from the same village were randomly selected.

36 months in the baseline survey and for children aged 0 to 60 months in both follow-up surveys.

This paper focuses on the medium-run follow-up, which we conducted from September to November 2013. The survey is a panel and tracked the original households included in the baseline. Overall household attrition was again low: 13,619 households, or 95.1 percent of baseline households, were found, with the attrition rates nearly identical across the treatment and control groups (see online Appendix Table 1).⁷

While household attrition was low, it could be that some household members left the subdistrict and thus were not tracked. This is not an issue for the young children born after the baseline since we measure all children present in any household we track, and we track 95 percent of baseline households. Thus, attrition should likely not be a concern for outcomes such as completed vaccinations or stunting.⁸

However, attrition could be relevant potentially for the oldest children at baseline, who are now teenagers and young adults, and for outcomes in this age range such as high school completion or teenage marriage. In online Appendix Table 3, we examine attrition for those who were initially aged 6–15 years in the baseline. We find and resurvey 90 percent at the two-year follow-up (column 1) and 72 percent at the six-year follow-up (column 2) but with no differential attrition between the treatment and control group in either survey, either overall or differentially between boys and girls (columns 3 and 4).⁹

Online Appendix Table 6 shows that the final sample is balanced across treatments in terms of baseline characteristics. In column 2 we provide the control group mean for the variable listed in that row, while column 3 provides the mean for the treatment group. Column 4 provides the difference between the treatment and control for that variable (clustered by subdistrict, which is the level of randomization), and column 5 provides this difference conditional on strata (districts). Of the 14 variables considered, only one difference is statistically significant at the 10 percent level, consistent with chance.

II. Results

We first outline our empirical strategy and show the first-stage results. We then examine results on three key dimensions: ongoing impacts on incentivized health

⁷We followed entire households or household members who moved within the same subdistrict. In addition, we surveyed 362 households that were added to the sampling frame in the two-year follow-up and 751 households that were added in the 6-year follow-up in order to compensate for household attrition.

⁸An alternative concern for younger children is that the CCT affected infant mortality differentially and this biases the results for the young children. However, this does not appear to be the case: in online Appendix Table 2, we show that there was no observable difference in fertility, miscarriage, stillbirth, or infant mortality rates in either follow-up survey.

⁹We can further disaggregate by age at baseline to determine if attrition is worse for older children, who may be more likely to leave the household for work or marriage. In online Appendix Table 4, we observe that attrition does indeed increase with baseline age. However, this attrition does not appear differential by treatment group: only 4 out of 40 regression coefficients are significant, which is consistent with chance. We also show in online Appendix Table 5b that there are no differences in the reasons for child migration across the treatment and control group. In short, while older children were more likely to have migrated than younger children, the probability of migration as well as the reasons for doing so do not appear to be associated with treatment status.

and education behaviors, cumulative effects on proxies for human capital, and cumulative effects on household economic outcomes.

A. Empirical Strategy and First-Stage Results

While compliance with the randomization protocol was generally high, it was not perfect, and some control areas were treated. In addition, only a subset of households on the initial Statistics Indonesia interview lists ultimately became beneficiaries as there was a subsequent screening step to determine categorical eligibility. Therefore, we conduct an instrumental variable analysis in which we instrument program receipt ($ReceivedCCT_{hsd}$) with whether households were located in an initial treatment subdistrict:

$$(1) \quad Y_{hsd} = \beta_0 + \beta_1 ReceivedCCT_{hsd} + \mathbf{X}_{hsd}'\boldsymbol{\gamma} + \alpha_d + \varepsilon_{hsd}.$$

The variable Y_{hsd} is the outcome of interest for household h in subdistrict s in district d ; $ReceivedCCT_{hsd}$ is a dummy variable for whether the household has ever received the CCT program, while α_d is a set of district fixed effects. For additional precision, we include the following baseline control variables in \mathbf{X}_{hsd} : house roof type, wall type, floor type; household head's education level; household head works in agriculture; head of household works in services; log monthly per capita expenditure; log household size; and dummies for whether the household has clean water, has its own latrine, has a square latrine, has its own septic tank, and has electricity from the state electric company.¹⁰ We cluster the standard errors by subdistrict, the level of the randomization. We adjust p -values for multiple hypothesis testing within each panel of results using the step-down method of Romano and Wolf (2005, 2016) and report the resulting adjusted p -values in brackets.¹¹ We also report the p -value for the difference between the two- and six-year effects.

Table 1 provides our first-stage estimates. Column 1 shows the results of our analysis of the World Bank's data from the two-year follow-up for comparison, while column 2 provides our six-year follow-up results. In the last row, we also provide the F -statistic from a test of the instrument.

The regressions show a strong—and almost identical—first stage in both the short run and medium run. By the two-year follow-up (column 1), about 9 percent of the control group reported receiving the CCT, with a 37.5 percentage point increase in the treatment group (p -value less than 0.001). The results are similar in the 6-year data (column 2), with a 36.8 percentage point increase in the treatment group relative to the 13.1 percent of households having ever received the CCT in the control group (p -value less than 0.001). The instruments are strong, with F -statistics over

¹⁰ For missing baseline data, we fill in the control variables with zero and create a dummy variable to indicate missing values for each variable.

¹¹ We do this for each family of outcomes. In Tables 2, 4, and 5, we group outcomes by survey round (i.e., columns). In Tables 3, 6, and 7, we group outcomes by survey round and panel since some of the panels are essentially summary variables of other variables (e.g., enrollment for ages 7–15 is a weighted average of enrollment by categories, with the categories broken down in subsequent panels) or because the outcomes are from a different family of outcomes (e.g., marriage versus education).

TABLE 1—FIRST-STAGE REGRESSIONS, HOUSEHOLD LEVEL

| Outcome: Received CCT | 2-year (1) | 6-year (2) |
|-----------------------|------------------|------------------|
| Treatment | 0.375 (0.017) | 0.368 (0.017) |
| Observations | 14,757 | 15,667 |
| R ² | 0.258 | 0.242 |
| Control mean | 0.091 | 0.131 |
| F-statistic | 507.797 | 456.783 |

Notes: This table reports first-stage regressions of CCT receipt status (“Received CCT”) on baseline subdistrict treatment assignment. Column 1 reports results from the two-year follow-up survey, and column 2 reports results from the six-year follow-up survey. In the final row, we report *F*-statistics from a Wald test of simple hypotheses involving the strength of our chosen instruments. Baseline controls include the following: household roof type, wall type, floor type; head of household’s education level; head of household works in agriculture; head of household works in services; household has clean water; household has own latrine; household has square latrine; household has own septic tank; household has electricity from PLN; log monthly per capita expenditure; and log household size. Includes district fixed effects. Standard errors are clustered by subdistrict.

450. Moreover, it is largely the same households continuing to receive the program over time: 83.6 percent of those households that report currently receiving the CCT in the two-year survey also report receiving it currently in the six-year survey; conversely, 85.2 percent of the panel households in our data that receive the CCT at six years also reported receiving it at two years.

It is important to note that the randomization is at the subdistrict level, and hence control households come from other subdistricts. Virtually all health and education services (health clinics, schools) are contained within subdistricts, so spillovers *across* subdistricts are extremely unlikely in this context, and indeed, this was the reason the randomization was done at such a high level.

A second question is whether there are spillovers to nontreated households *within* treated subdistricts. This assumption of no within-subdistrict spillovers is important for the exclusion restriction implicit in estimating equation (1) with instrumental variables. While in other contexts this has been a concern—see, e.g., Angelucci and De Giorgi (2009) in the PROGRESA case—there are two reasons why even within-subdistrict spillovers seem very unlikely here. Unlike PROGRESA, which treated over 60 percent of households in treatment villages, PKH was targeted at the poorest of the poor households and as such treated a far smaller fraction of households in a village—in 2009, for example, the typical treated village had only 78 PKH beneficiary households out of a mean of 1,200 households, meaning PKH treated only 6.5 percent of households in a village on average.¹² General equilibrium effects (e.g., congestion at schools or health clinics or positive spillovers

¹²One would expect that the average effect on a nonbeneficiary household is therefore likely at least an order of magnitude smaller than the average effect on beneficiary households. Since our household survey intentionally sampled households that were likely to be beneficiaries, beneficiary households are about 50 percent of the sampled households in the treatment area, even though they are only 6.5 percent of the total population. This means that the average effect in our sampled households will be driven almost entirely by the effect on treated households.

through supply-side changes) are therefore likely to be very small in our case, given how small the share of treated households is.¹³

B. Impacts on Incentivized Human Capital Behaviors

Health.—We begin by examining incentivized health-seeking behaviors. Table 2 reports results of IV regressions, where we estimate separate regressions for the outcomes of interest listed in each row. Our key estimates from the six-year follow-up survey are shown in column 2; each cell presents the IV effect of PKH treatment analyzed using equation (1).¹⁴ For ease of comparability, in column 1, we show the results from the two-year survey conducted by the World Bank, which we reanalyzed using the same IV specification as (1).

Our analysis shows continued effects of the CCT program on a range of health-seeking behaviors in the medium run, particularly with regard to maternal health-seeking behaviors. We first examine health-seeking outcomes for women who became pregnant or gave birth within the 24 months prior to each follow-up survey. We find that the transfers continued to have large, positive effects on the probability that childbirths were assisted by trained personnel (doctors or midwives) in the six-year follow-up and that deliveries were more likely to take place in a health facility. Specifically, the estimates imply that the CCT program led to a 17 percentage point increase in delivery at a health facility at the six-year follow-up (24 percent increase) and a 23 percentage point increase in the probability a birth was assisted by a trained midwife or doctor (about double the effect at the two-year mark; *p*-value of two-year versus six-year difference 0.079). These are dramatic changes—they imply that the CCT program reduced the share of children born outside a health facility by 62 percent and virtually eliminated births not assisted by trained midwives or doctors. These effects remain statistically significant even after adjusting for multiple hypothesis testing.

However, unlike in the short run, we do not find statistically detectable impacts on pre- and postnatal visits, though the point estimates are positive and we cannot reject that the two- and six-year effects are statistically equal. One potential reason is that the control group increased their overall number of visits in the intervening

¹³Triyana (2016) studies whether there are changes in service provision as a result of PKH in the two-year follow-up. She finds no effect on the number of doctors or traditional attendants but finds a small increase in the number of midwives. In online Appendix Table 7, we examine the effect of PKH on the number of doctors, midwives, traditional birth attendants, and schools in the six-year follow-up, and do not find an increase on the level of any of these measures of supply-side service availability, suggesting little presence of spillovers through supply responses. In addition, we also conduct an alternative identification strategy that uses baseline assets interacted with subdistrict-level treatment status to predict treatment at the individual level, not at the aggregate level. As shown in online Appendix Tables 8 and 9, this produces very similar results to the univariate treatment-versus-control subdistrict-level instrument (and, in particular, these results are not systematically smaller than the univariate instrument results), further suggesting empirically that spillovers are very small in our context.

¹⁴Our results are robust to the model specification choices that we made. For example, in online Appendix Table 10, we replicate Table 2 using “currently receiving” a CCT, rather than “ever received,” as our variable of interest because some households received PKH in the two-year follow-up survey but had stopped receiving it by the six-year follow-up; the results look nearly identical (which is not surprising given that the overlap of households in both categories is high). Similarly, in online Appendix Table 11, we replicate Table 2 but drop baseline controls. Again, we find similar coefficients, but sometimes we lose some statistical precision when omitting the baseline controls.

TABLE 2—IV EFFECT OF CCT ON HEALTH-SEEKING BEHAVIORS

| Outcome | 2-year (1) | 6-year (2) | <i>p</i> -value (2-yr. = 6-yr.) (3) |
|--|---------------------------------------|---------------------------------------|--|
| Number of prenatal visits | 1.048 (0.473) [0.140] 6.493 | 0.560 (0.582) [0.706] 7.147 | 0.485 |
| Delivery assisted by skilled midwife or doctor | 0.115 (0.056) [0.166] 0.640 | 0.233 (0.059) [<0.001] 0.770 | 0.079 |
| Delivery at health facility | 0.112 (0.062) [0.230] 0.457 | 0.171 (0.066) [0.058] 0.725 | 0.430 |
| Number of postnatal visits | 0.842 (0.272) [0.023] 1.234 | 0.403 (0.317) [0.663] 1.778 | 0.275 |
| 90+ iron pills during pregnancy | 0.025 (0.049) [0.831] 0.179 | −0.035 (0.044) [0.706] 0.131 | 0.356 |
| Percent of immunizations received for age | 0.038 (0.029) [0.444] 0.754 | 0.048 (0.029) [0.427] 0.786 | 0.788 |
| Times received vitamin A (6 months–2 years) | −0.022 (0.208) [0.903] 1.639 | −0.095 (0.205) [0.706] 1.817 | 0.799 |
| Times weighed in last 3 months (0–60 months) | 0.919 (0.130) [<0.001] 1.791 | 0.250 (0.192) [0.663] 1.954 | 0.001 |

Notes: Each row in this table represents a separate outcome variable. Each table entry includes (i) the regression coefficient, (ii) the cluster-robust standard error, (iii) an adjusted *p*-value controlling the family-wise error rate (FWER) within each column as described by Romano and Wolf (2005, 2016), and (iv) the control mean. Outcomes from “number of prenatal visits” to “90+ iron pills during pregnancy” are coded for women in our sample who had been pregnant within the past two years. Outcomes from “percent of immunizations received for age” onward are coded for children who were ages 0–36 months at baseline. These child-related regressions also include age-bin controls for each month of age up to one year and for each quarter-year of age for ages one and above in addition to baseline controls and fixed effects listed in Table 1. Standard errors, clustered by subdistrict, are shown in parentheses.

years (for example, the control mean increased from 6.5 to 7.1 prenatal visits) and essentially caught up to the treatment group. In terms of the care women and children received, we observe no effect on receiving a full set of iron pills during pregnancy, either in the two- or six-year follow-up.

Health inputs into young children also appear to have improved in the medium run for children who had ever been covered by PKH since the baseline survey, but any observed effects are relatively weak. While there was no observable impact on immunizations in the two-year follow-up survey for children in our baseline sample, we observe about a 5 percentage point increase in the percent of age-recommended immunizations completed, though this effect does not survive multiple inference adjustment, and we cannot reject that the two- and six-year mark effects are

statistically equal. We observe no increase in the number of times children between the ages of six months and two years received vitamin A. We observe increases in the number of times a child was weighed by a health professional in the last three months (for those aged 0 to 60 months), though these increases are no longer significant and smaller in the six-year follow-up compared to the two-year follow-up (the difference between the two- and six-year effects is statistically significant with a p -value of 0.001).

Education.—The second component of the CCT incentives focused on increasing enrollment and attendance of primary and junior secondary school-age students, i.e., those students aged 7–15 years. Table 3 presents the results for these children. In panel A, we examine enrollment and attendance for children aged 7–15 (panel A) and then disaggregate further by ages 7–12 (panel B) and 13–15 (panel C).¹⁵

We find substantial increases in enrollment for all children aged 7 to 15: the CCT program increased enrollment rates by 4 percentage points in the 6-year survey. Since 92.4 percent of control group children were enrolled in school, the 4 percentage point increase in enrollment represents a 53 percent decrease in the fraction of students who were not enrolled in school; that is, the CCT program eliminated more than half of nonenrollment, making a large dent in the last-mile enrollment problem.¹⁶ This effect is statistically different (slightly smaller) from what was observed at two years—a 6.4 percentage point increase in enrollment, which represents a 66 percent decline in the nonenrollment rate (the p -value of the difference in effects is 0.081)—though the decline in treatment effect from year 2 to year 6 is entirely due to an increase in enrollment in the control group rather than a decline in the treatment group. It is important to note that these increases are not just nominal enrollment: we observe substantial increases in the percentage of children who reported attending school at least 85 percent of the time in the last two weeks in both the two- and six-year follow-ups.¹⁷

Disaggregating the effects by age group, we see that the six-year effects are concentrated among older students (panel C). For students aged 13–15, we see increases in school enrollment of 9 percentage points, representing a 52 percent decline in the nonenrollment rate. For students aged 7–12, we do not observe a statistically significant increase in enrollment but note that the enrollment rate in the control group is 97.2 percent in this age range, so obtaining gains in this age group is likely to be difficult.¹⁸

¹⁵Online Appendix Tables 12 and 13 provide the results using the “currently receiving” CCT variable and with no baseline controls, respectively, and show similar findings to Table 3.

¹⁶Note that if we redo the FWER multiple inference adjustment among all incentivized indicators, i.e., combining Table 2 and panel A of Table 4, the statistical significance levels remain unchanged. The multiple-inference adjusted p -values for enrollment and attendance become <0.001 and 0.012, respectively, and the results on assisted deliveries and deliveries in facility in Table 2 remain statistically significant.

¹⁷Note that this attendance measure is defined as 0 for those nonenrolled in school. This measure therefore captures the combination of enrollment and attendance decisions since both can respond to the CCT program.

¹⁸Online Appendix Table 14 examines the effects for boys and girls separately. As shown in panel B, younger boys in households receiving the CCT program were more likely to be in school, while we find no effect for younger girls—however, girls had very high rates of enrollment to begin with (98 percent were enrolled in some form of school). While somewhat larger in magnitude for older boys than for older girls, we nonetheless observe treatment effects of the CCT program on the enrollment and attendance of both older boys and girls (panel C).

TABLE 3—IV EFFECT OF CCT ON INCENTIVIZED EDUCATION INDICATORS

| Outcome | 2-year (1) | 6-year (2) | p-value (2-yr. = 6-yr.) (3) |
|--|---------------------------------------|---------------------------------------|--------------------------------|
| <i>Panel A. Enrollment for ages 7–15</i> | | | |
| Enrolled in school (any level) | 0.064 (0.013) [<0.001] 0.903 | 0.040 (0.012) [<0.001] 0.924 | 0.081 |
| >85 percent attendance last two weeks | 0.070 (0.016) [0.001] 0.830 | 0.057 (0.017) [<0.001] 0.856 | 0.492 |
| <i>Panel B. Outcomes for ages 7–12</i> | | | |
| Enrolled in school (any level) | 0.037 (0.009) [<0.001] 0.960 | 0.012 (0.008) [0.181] 0.972 | 0.013 |
| Enrolled in primary school | 0.012 (0.014) [0.356] 0.887 | 0.011 (0.016) [0.505] 0.879 | 0.928 |
| >85 percent attendance last two weeks | 0.041 (0.016) [0.023] 0.881 | 0.034 (0.017) [0.102] 0.895 | 0.745 |
| <i>Panel C. Outcomes for ages 13–15</i> | | | |
| Enrolled in school (any level) | 0.121 (0.032) [<0.001] 0.783 | 0.090 (0.027) [0.002] 0.826 | 0.383 |
| Enrolled in secondary school | 0.075 (0.037) [0.042] 0.585 | 0.054 (0.034) [0.106] 0.609 | 0.651 |
| >85 percent attendance last two weeks | 0.132 (0.033) [<0.001] 0.723 | 0.099 (0.029) [0.002] 0.777 | 0.364 |

Notes: This table examines school enrollment and attendance outcomes. See Table 2 notes for explanation of table entries. Baseline controls and fixed effects are as listed in Table 1. *p*-values are adjusted within each panel rather than within entire columns. Standard errors, clustered by subdistrict, are shown in parentheses.

In sum, the conditional cash transfer remained highly effective at reducing nonenrollment in school for those in the targeted age category, particularly for older students (age 13–15), for whom nonenrollment is a substantial issue. More generally, despite the fact that the program has been running at large scale by the government for six years and with no researcher monitoring or intervention, it continues to be effective in improving targeted health-seeking and education behaviors.

C. Cumulative Impacts on Proxies for Human Capital

Anthropometric Impacts.—The results thus far have shown that health-seeking behaviors continued to be positively affected by the CCT program. This implies

that at the time of the six-year survey, young children (those under five years old) had spent their entire lives with higher levels of improved health services at various points in their life cycle. A natural question is whether this increased health utilization accumulated and led to changes in health outcomes.

We examine this question in Table 4. We explore anthropometric outcomes for children aged 0 to 60 months.¹⁹ We start by examining measures of stunting. Stunting is considered a measure of cumulative health investments during the first few years of life (Hoddinott et al. 2013, Jayachandran and Pande 2017); it is also thought to be correlated with worse cognitive and economic outcomes later in life (Case and Paxson 2008, Glewwe and Miguel 2007, Hoddinott et al. 2013b, Guven and Lee 2013). We follow WHO definitions and define stunting as being more than two standard deviations below the WHO-standardized height-for-age median; severe stunting is defined as being more than three standard deviations below the WHO-standardized height-for-age median.

We observe very large reductions in stunting among children aged 0 to 60 months in the six-year follow-up survey. Stunting declined by roughly 9 percentage points, i.e., a 23 percent reduction in the probability of being stunted. Severe stunting declined by approximately 10 percentage points, i.e., a 56 percent reduction.²⁰ Both boys and girls benefited from the CCT program in terms of decreased stunting and severe stunting, although the point estimates are slightly larger in magnitude for boys than for girls (see online Appendix Table 17). While the point estimates indicate stunting reductions of about 3 percentage points after the program had been in effect for 2 years, these estimates are not statistically significant. While we cannot reject that the two- and six-year effects on stunting are the same (p -value of 0.221), we find a statistically significant difference between the two- and six-year effects on severe stunting (p -value of 0.072).

We observe no impacts on malnourishment (i.e., weight for age more than two standard deviations below WHO standard)—which responds more quickly to health investments—in either the two- or six-year follow-up.²¹

Potential Mechanisms for Stunting Effects.—Given that a conditional cash transfer is a bundled intervention (cash + incentives), it is hard to disentangle which specific channels could drive the observed stunting reductions. However, we can explore three potential mechanisms. First, it could be that the increased

¹⁹ Online Appendix Table 15 provides the results for those currently receiving the CCT (as opposed to those who ever received the CCT), and online Appendix Table 16 does so without controls. The findings remain robust to these specification changes.

²⁰ Online Appendix Figure 5 estimates the impacts on stunting nonparametrically by child age and finds similar reductions in stunting across 0- to 60-month-olds; if anything, the figures suggest somewhat larger reductions for older cohorts.

²¹ Most experimental evaluations of CCTs to date have not shown large effects on stunting, but there are a few exceptions. Fernald, Gertler, and Neufeld (2008) look at stunting in the PROGRESA context in 2003, comparing families that were initially part of the 1997 rollout with those in the control group that received PROGRESA 18 months later, and find that longer exposure to PROGRESA led to reductions in stunting. Kandpal et al. (2016) measure experimentally the effect of the Philippines' Pantawid program on stunting 30–31 months after its introduction and find reductions in stunting for children aged 6 to 36 months. They argue that part of the reason for the impact could be the program's focus on nutrition (particularly dairy) in family development sessions. Baird, McIntosh, and Özler (2016) do not report any effect of a UCT or CCT program in Malawi on child height, though the transfers in the program they study were in place for only two years and ended two years prior to their survey.

TABLE 4—IV EFFECT OF CCT ON CHILD NUTRITION AND HEALTH OUTCOMES, 0–60 MONTHS

| Outcome | 2-year (1) | 6-year (2) | p-value (2-yr. = 6-yr.) (3) |
|-----------------------|---------------------------------------|--|--------------------------------|
| Stunted | −0.028 (0.035) [0.822] 0.513 | −0.089 (0.039) [0.061] 0.390 | 0.229 |
| Severely stunted | −0.023 (0.034) [0.855] 0.306 | −0.100 (0.029) [<0.001] 0.180 | 0.072 |
| Malnourished | −0.008 (0.028) [0.946] 0.332 | −0.009 (0.033) [0.943] 0.274 | 0.981 |
| Severely malnourished | 0.004 (0.018) [0.946] 0.097 | −0.003 (0.020) [0.943] 0.068 | 0.786 |

Notes: This table examines child anthropometric outcomes. “Stunted” indicates children with height-for-age z-scores below −2, and “Severely stunted” indicates children with height-for-age z-scores below −3. “Malnourished” indicates children with weight-for-age z-scores below −2, and “Severely malnourished” indicates children with weight-for-age z-scores below −3. See Table 2 notes for explanation of table entries. Baseline controls and fixed effects are as listed in Table 1. Regressions also include age-bin controls for each month of age up to one year and for each quarter-year of age between one and five years. Standard errors, clustered by subdistrict, are shown in parentheses.

health-seeking behaviors (shown in Table 2) increased interaction with medical professionals, which in turn reduced stunting through increased maternal knowledge and health behaviors. We do find that treatment mothers are more likely to know and report their child’s birthweight, but on net, we do not observe large changes in other indicators of maternal knowledge or behavior (online Appendix Table 18). Second, it could be that PKH drives improved nutrition (see online Appendix Table 19). We do find changes in child protein intake in response to the CCT program: children aged 18–60 months were roughly 10 to 11 percentage points more likely to have consumed milk and 10 to 12 percentage points more likely to have consumed eggs in the week prior to the two-year follow-up survey. Note, however, we do not find similar results in the six-year follow-up survey. Finally, we explore whether reported illness of the children declined, under the hypothesis that sick children would have more stunted growth paths. Online Appendix Table 20 shows no observable declines in reported acute illness rates for children under five.

Impacts on Child Labor.—Child labor is thought to be an important concern since it likely crowds out human capital accumulation. We therefore examine whether the CCT, which we saw led to substantial gains in schooling for children aged 13–15, is also associated with changes in child labor.²² Table 5 shows that the program

²² We explore this outcome among children aged 13–15 since child labor among younger children is extremely rare. Online Appendix Table 21 presents the results for younger children (aged 7 to 12). We do not find any changes for this age group in the six-year follow-up, but this is not surprising as only 1.6 percent of children in the control group report working for a wage at all, and only 0.4 percent report working more than 20 hours for wage in the past month.

TABLE 5—IV EFFECT OF CCT ON CHILD LABOR, AGES 13–15

| Outcome | 2-year (1) | 6-year (2) | <i>p</i> -value (2-yr. = 6-yr.) (3) |
|--------------------------------------|---------------------------------------|---------------------------------------|--|
| Worked for wage last month | −0.041 (0.021) [0.050] 0.098 | −0.044 (0.020) [0.057] 0.092 | 0.932 |
| Worked 20+ hours for wage last month | −0.046 (0.016) [0.002] 0.061 | −0.030 (0.017) [0.081] 0.055 | 0.382 |

Notes: This table examines the effect of the conditional cash transfer on child labor outcomes based on survey responses. Outcomes are dummy variables indicating if children ages 13–15 performed any work for wage (or 20+ hours of wage work) in the past month. This definition does not include household labor. See Table 2 notes for explanation of table entries. Baseline controls and fixed effects are as listed in Table 1. Standard errors, clustered by subdistrict, are shown in parentheses.

reduced the fraction of children engaged in wage work by 4.4 percentage points, i.e., a reduction of 48 percent. For those working extensively, which we define as working for a wage at least 20 hours in the past month, the effects are similar: a reduction of 3.0 percentage points, i.e., a decline of 44 percent. Online Appendix Table 22 shows that reductions in wage work are primarily found for boys, who are more likely to be working for a wage than girls.²³ The fact that we see both substantial increases in school enrollments *and* declines in wage work for the same age groups suggests that the two effects may be related.²⁴

Impacts on High School Education, Labor, Early Marriage, and Early Fertility.—Our final set of results for children explores outcomes for children who were aged 9 to 15 when the CCT program was initially rolled out and hence were between ages 15 and 21 at the time of the six-year follow-up. This allows us to explore the cumulative effects of the CCT on final educational attainment and early adulthood outcomes after the incentives have ended.

We begin by exploring educational outcomes for this cohort, shown in panel A of Table 6.²⁵ We find large increases in the probability of those aged 15 to 17 attending any kind of school in the 6-year follow-up, with some of this effect driven by increases in high school enrollment.²⁶ We also find some evidence of an increase in high school completion rates for those aged 18 to 21 (about 7 percentage points, with an FWER-adjusted *p*-value of 0.139). As shown in online Appendix Table 26, most

²³In online Appendix Table 23, we explore alternative measures of nonwage work. In the medium run, we observe some reductions in working more than 20 hours a month for a family business for those aged 13–15 (but this is not significant in all specifications) and no effect on “helping out at home.”

²⁴In online Appendix Table 24, we show that the CCT program led to a decline in the number of students who were both enrolled in school *and* working for a wage at the same time. This fact—that the CCT program reduces, not increases, the number of students both doing wage work and being enrolled in school—suggests that the effects we see on enrollment and work are not coming exclusively from a time-budget constraint but rather may be related to the income effects of the CCT.

²⁵We omit outcomes for ages 18 to 21 from our reported 2-year regressions because virtually no respondents who were aged 9 to 15 at baseline had reached age 18 by the time of the two-year follow-up survey.

²⁶As shown in online Appendix Table 25, we observe increases in 15-year-olds and 17-year-olds attending any type of school. The high school effect appears largely driven by 17-year-olds.

TABLE 6—IV EFFECT OF CCT ON MEDIUM-RUN EDUCATION, WORK, AND MARRIAGE OUTCOMES

| Outcome | 2-year (1) | 6-year (2) | <i>p</i> -value (2-yr. = 6-yr.) (3) |
|---|---------------------------------------|---------------------------------------|--|
| <i>Panel A. School enrollment/completion outcomes</i> | | | |
| Enrolled in school (ages 15–17) | 0.069 (0.047) [0.234] 0.536 | 0.105 (0.045) [0.052] 0.616 | 0.481 |
| Enrolled in high school (ages 15–17) | 0.016 (0.039) [0.648] 0.301 | 0.074 (0.041) [0.139] 0.393 | 0.207 |
| Completed high school (ages 18–21) | | 0.074 (0.041) [0.139] 0.258 | |
| <i>Panel B. Labor outcomes (ages 16–21)</i> | | | |
| Worked for wage last month (ages 16–17) | –0.068 (0.053) [0.286] 0.258 | 0.032 (0.041) [0.665] 0.221 | 0.104 |
| Worked 20+ hours for wage last month (ages 16–17) | –0.063 (0.049) [0.286] 0.188 | 0.004 (0.038) [0.914] 0.172 | 0.228 |
| Worked for wage last month (ages 18–21) | | –0.059 (0.048) [0.482] 0.478 | |
| Worked 20+ hours for wage last month (ages 18–21) | | –0.043 (0.047) [0.665] 0.423 | |
| <i>Panel C. Marriage outcomes (ages 16–21)</i> | | | |
| Married (ages 16–17) | –0.026 (0.020) [0.211] 0.041 | –0.012 (0.025) [0.835] 0.056 | 0.624 |
| Married (ages 18–21) | | –0.017 (0.036) [0.835] 0.186 | |

Notes: This table explores schooling, labor, and marriage outcomes for children who were between the ages of 6 and 15 (i.e., schooling age) during the baseline survey and initial CCT rollout. Outcomes for ages 18–21 are omitted from column 1 (two-year follow-up) because virtually none of these children had turned 18 by the time of the follow-up survey. See Table 2 notes for explanation of table entries. Baseline controls and fixed effects are as listed in Table 1. *p*-values are adjusted within each panel rather than within entire columns. Standard errors, clustered by subdistrict, are shown in parentheses.

of the increases in educational attainment for these age categories are driven by boys, who show very large impacts on high school enrollment (13 percentage points, representing a 38 percent increase) and completion rates (9.7 percentage points, 42 percent increase). We find no impact on high school enrollment or completion rates for girls.

We then explore work outcomes for this age group, shown in panel B of Table 6. We find no impacts on the probability of wage work for either 16- to 17-year-olds or 18- to 21-year-olds. As shown in online Appendix Table 26, we find no effect for either boys or girls. For 16- to 17-year-olds, one might expect a decrease in wage work due to the increases in school enrollment documented above; while we do not observe this, as shown in panel A of Appendix Table 27, we do observe some decreases in helping out with the family business or housework (particularly for girls). For 18- to 21-year-olds, who are more likely to be out of school, we may have expected higher employment rates for wage work. However, this does not appear to be the case.²⁷ It is worth noting, however, that these 18- to 21-year-olds were already teenagers at the time the program started and thus spent fewer of their formative years in the program than the young children for whom we observe reductions in stunting.

In panel C of Table 6, we explore whether the CCT program led to changes in age of marriage. Age of marriage could be delayed from the cash transfer's income effect (Baird, McIntosh, and Özler 2011) or from a delay in marriage due to the practical side of being enrolled in school longer. However, we find no evidence that the CCT program changed the propensity to marry for those aged 16 to 17 or for those aged 18 to 21, though standard errors are large relative to the mean. Finally, we investigate changes in fertility (online Appendix Table 28), and while we observe qualitatively postponement of births—decreases in fertility for girls aged 16 to 17 and increases for those aged 18 to 21—we cannot reject that these coefficients are different from 0.

D. Cumulative Impacts on Recipient Households: Consumption, Work, and Assets

The third main question we explore is whether the accumulation of repeated cash transfers had transformative effects on the economic condition of the recipient households themselves. The CCT program provides a quarterly transfer of cash to households for around six years. The cash payment is around 7 to 15 percent of total household consumption, adding up cumulatively to between \$360 and \$1,320—an average of \$970—per household. We, therefore, ask whether this assistance was large enough to have a “transformative effect” on households, shifting them out of poverty. One mechanism for this could be that households save part of the transfers over time and use this to invest in productive assets. For example, Gertler, Martinez, and Rubio-Codina (2012) find that PROGRESA beneficiary households invested a fraction of their accumulated transfers in productive assets.

In Table 7 we examine the impacts of the transfer on household consumption, adult employment, and household assets.²⁸ While we observe positive impacts of the transfer on overall log per capita consumption (panel A), we cannot distinguish these measured impacts from zero; on the other hand, we cannot reject an increase

²⁷ We do observe that the boys within this age category were more likely to help out with the family business (panel B of online Appendix Table 27), while girls were somewhat less likely to help out in any family business or housework.

²⁸ In online Appendix Table 29, we disaggregate expenditure outcomes by province. While we find noisy estimates, the increase in log consumption appears largest in the relatively poorer province of East Nusa Tenggara.

TABLE 7—IV EFFECT OF CCT ON HOUSEHOLD ECONOMIC OUTCOMES

| Outcome | 2-year (1) | 6-year (2) | <i>p</i> -value (2-yr. = 6-yr.) (3) |
|---|--|---------------------------------------|--|
| <i>Panel A. log per capita household expenditure</i> | | | |
| log per capita expenditure | −0.006 (0.035) [0.951] 12.353 | 0.037 (0.037) [0.485] 12.898 | 0.329 |
| <i>Panel B. Household land + livestock investment</i> | | | |
| Owens any land | −0.011 (0.017) [0.770] 0.915 | 0.007 (0.021) [0.916] 0.909 | 0.406 |
| Head of household employed | 0.001 (0.014) [0.939] 0.940 | −0.004 (0.011) [0.916] 0.943 | 0.793 |
| Total number of livestock owned | −0.529 (0.468) [0.574] 3.883 | −1.203 (1.575) [0.814] 4.753 | 0.670 |

Notes: This table reports effects on various household-level consumption and investment outcomes. In panels A and B, households above the ninety-ninth percentile for each category of expenditure are dropped from the regressions for that specific category. See Table 2 notes for explanation of table entries. Baseline controls and fixed effects are as listed in Table 1. *p*-values are adjusted within each panel rather than within entire columns. Standard errors, clustered by subdistrict, are shown in parentheses.

of consumption equal to the amount of the transfer at the six-year follow-up. As shown in panel B, we do not observe changes in employment rates, although even in the control group, nearly 94 percent of the control group was employed regardless. Finally, the CCT program did not lead to increases in assets as transfers accumulated over time, such as livestock ownership.

In short, we do not observe transformational effects on household economic outcomes. To the extent that the CCT program leads to substantially large changes in material household welfare, these will likely come through the effects on the next generation, who experience increased health and education, rather than a reduction in overall poverty of the current generation.

III. Conclusion

The decision to redistribute through targeted transfers is a complex one. Some arguments are at core ethical, arguing that a society should protect the vulnerable and give them some additional help. But other arguments are economic, asserting, for example, that transfers allow households to make business investments that can have transformational impacts on household income and reduce poverty. Still others make arguments based on the intergenerational transmission of poverty, with transfers as a mechanism to help increase investments in child health and education.

We evaluate these claims in the context of a large-scale, government-run conditional cash transfer program, which provides moderately sized, regular financial assistance to households that adhere to conditions that aim to improve investments

in child health and education. We find that even though the program has been running for six years—without any researcher involvement in later years and with a dynamic economic landscape—the program continues to promote remarkable health and educational investments in children explicitly targeted by the program. For example, six years after the program launched, we observe dramatic increases in usage of trained health professionals and facilities for childbirth and a reduction of more than half of the share of children aged 7 to 15 who are not enrolled in school.

Perhaps even more importantly, for children who have grown up their entire lives in households receiving these transfers, we also begin to observe impacts on outcomes that may require cumulative investments: for example, six years after the program began, we observe large reductions in stunting for young children and increased school enrollment for older teenagers. While this does not yet translate to increases in employment for individuals who have just started to enter the labor force, these are children who were already teenagers at the time the program started and have thus spent fewer of their formative years in the program. The stunting results suggest that effects may be larger in the very long run for children who benefited from the program during early childhood.

In contrast, we do not observe any impact on beneficiary households' current consumption, employment, or assets—suggesting that the additional help that the program provides does not have a transformational poverty reduction effect for those currently on the program. Rather, given that our results show that CCTs help poor households make significant investments in their children's health and education, an important part of the economic gains of CCTs likely could come from reductions in the intergenerational transmission of poverty.

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