Causal effects of universal health insurance: Evidence on child health in Mexico

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Abstract
Public health insurance programs are being expanded across the globe despite limited evidence regarding their impacts on population health. Exploiting the roll-out of Seguro Popular, a large-scale program that provides public health insurance to about half Mexico's population, this research isolates the causal impact of the program on child health and nutrition measured by height-for-age. Drawing on insights from the biology of human linear growth during the first few years of life, we use rich longitudinal population-representative data, the Mexican Family Life Survey, in combination with administrative program data and establish that Seguro Popular has had, at best, a modest impact on child nutritional status. Program effects in a community are larger after the program has been established for several years, suggesting supply-side factors may be critical impediments to fulfilling program goals. The results have important implications for the design, roll-out, and evaluation of public health programs.

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INTRODUCTION

In 2002, the Mexican government initiated a very large-scale and ambitious public health insurance program, Seguro Popular, to address large and long-standing inequalities in access to health care and health outcomes across the country. In comparison with similar programs in other developing countries, including Latin American, Seguro Popular is unusual in terms of both its size and its scope (Atun et al. 2015). The program is intended to cover around 60 million people, almost half the population of Mexico, and it is designed to provide access to a very large number of health interventions, medications, and treatments. The program has taken nearly a decade to fully implement.

During this time, the literature on universal health coverage has grown dramatically and a consensus is emerging that argues there are substantial health and economic benefits of universal coverage and therefore calls for substantial public investment in low-income countries where universal coverage is unlikely to be achieved by the private sector alone. For example, the Lancet Commission on Investing in Health concludes that universal health coverage is an efficient way to achieve large population health gains in developing countries, in addition to offering financial protection for families and achieving overall economic growth (Lancet, 2013). These conclusions are echoed in the World Innovation Summit for Health in 2015 and World Health Organization reports (WISH 2015; WHO 2014).

This research provides evidence on the effect of Seguro Popular on child height-for-age, an indicator of the health of a child during early life. We focus on child height for two primary reasons. First, the nutrition and biology literatures have established that the trajectory of linear growth is largely determined by the time a child is 24 to 36 months old. Absent a large nutrition or disease shock, the child's attained height as an adult is largely determined by this time and, exploiting this fact in combination with the timing of the roll-out of Seguro Popular, it is possible to credibly isolate the causal impact of access to universal health coverage. This directly addresses a key challenge confronting studies that investigate the impact of universal health coverage on health outcomes.[1] Moreover, with multiple waves of data over time, it is possible to exploit variation in linear growth of children to measure both the immediate and longer-term impacts of the program. This is another important contribution to the literature since the impact of an ambitious program like Seguro Popular on population health is likely to vary over time as health services are expanded to meet increasing demands of those who are newly covered. Our second primary reason for examining child height is that it has been shown to be an important marker of health and nutrition that is associated with a broad array of indicators of well-being in later life including educational attainment, earnings, health in adulthood and mortality (Fogel 1994; Strauss and Thomas 1995; Hoddinott et al. 2008). The use of a measure of physical health as an outcome is also preferred to self-reported health status, which is commonly used but prone to error and can be influenced by many individual characteristics (including experience with the health care system).

In addition to these contributions to the universal health coverage literature, we provide new evidence on the impact of Seguro Popular in particular on population health, an issue that has received very little attention. Most existing studies have investigated the impact of the program on health insurance coverage, use of health care services, and household spending on health care. In addition, the few studies that exist generally focus on a shorter time horizon than we are able to cover with this work.

This research uses data from three waves of the Mexican Family Life Survey (MxFLS), a large-scale population representative longitudinal study that spans the temporal and spatial roll-out of Seguro Popular. The survey collects detailed information about socio-economic status of each household as well as information about the health of every member including anthropometric measures, biomarkers and self-assessed health along with health care utilization, insurance coverage and payments for health services. We find little evidence that the introduction of Seguro Popular in a community had a

[1] See, for example, Frankenberg, Suriastini, and Thomas (2005), who adopt a parallel approach to estimate the effect of the expansion of public maternal and child health services in Indonesia.

measurable impact on the height of young children, relative to similar children in other communities or to their older peers in the short term. However, we do find that when the program has been in place for at least 5 years, significant benefits emerge. We interpret the evidence as indicating that there are adjustment costs to providing health services to the population. These likely include expansion of health services as well as changes in health-related behaviors of those who are newly covered. We provide evidence suggesting that these are relevant concerns in the Mexican case. Importantly, our results indicate that evaluations of large-scale program like Seguro Popular that focus only on immediate impacts are potentially misleading and that tracking impacts over time is important to understand the effects of providing universal health coverage.

SEGURO POPULAR

Prior to the introduction of Seguro Popular in 2002, the vast majority of Mexican families who were covered by health insurance were employed either in the formal sector by private firms or in the public sector. Workers in the formal sector are covered by benefits provided by the Mexican Institute for Social Security (IMSS) while public sector workers are covered by a similar program run by the Institute of Social Security and Services for Civil Servants (ISSSTE). By 2000, approximately 40% of the Mexican population was covered by IMSS, 7% was covered by ISSSTE, and 3% was covered by private insurance (Knox, 2008). As a result, half of the Mexican population (more than 50 million people) remained without any form of health insurance (Frenk, 2006). This prompted the creation of Seguro Popular, a government-implemented insurance program available for all Mexican households who do not otherwise have health insurance. The program has several main goals, including protecting individuals and families from catastrophic healthcare spending, lowering out-of-pocket healthcare expenditures, and ensuring access to accredited healthcare providers and facilities (King et al., 2007).

The costs of the program are primarily shared between the state and federal governments, although a small amount is contributed by annual fees from enrollees. The state governments administer the program and decide how to allocate funds. However, they are required to ensure that there are enough accredited service providers and infrastructure so that people who affiliate actually have access to the services guaranteed. As a result, a significant amount of program funds have been allocated to investment in health infrastructure, although most observers believe that there were areas, particularly in rural municipalities, where the minimum requirements were not always met (Pfutze 2014; Barros 2008 for instance). These supply-side impediments to implementation and effectiveness of the program will be discussed in more detail in the results and discussion sections.

Enrollees in Seguro Popular have access to a large number of treatments, medical services, and medicines without co-pay. Upon enrollment, households or individuals are assigned to a primary-care physician and public clinic (Conti and Ginja, 2014). The number of interventions and medicines covered has grown as the program has rolled out. By 2006, Seguro Popular covered the treatments for diseases responsible for 95% of the burden of disease in the country (King et al., 2007). The program encompasses four main components. The first and largest is Catálago Universal de Servicios de Salud (CAUSES) which has been in place since the start of Seguro Popular and is operated at the state level. In 2004, CAUSES covered 91 interventions and 142 drugs; by 2012 it had expanded to 284 interventions and approximately 400 drugs. The second component, Fondo de Protección contra Gastos Catastróficos (FPGC), has also been in place since the program began but is managed at the federal level. FPGC is primarily a trust fund to cover catastrophic events (such as cancer treatment or surgery), and by 2012 included 58 interventions (Pfutze, 2014).

There were two modules added to Seguro Popular after the program began. Seguro Médico para una Nueva Generación (SMNG) began December 1, 2006 and focuses on infant and child health. Every child born after SMNG began and their immediate families (if they are not affiliated with any social security insurance through the government) are now automatically covered by Seguro Popular. SMNG covers children and their families until the children are five years old (National Commission of Social

Health Protection). This module covers 131 interventions that focus on early childhood health. The primary goals of SMNG were to lower infant and child disease and mortality while preventing catastrophic expenditures for families of young children. Seguro Popular and SMNG both include interventions and treatments for young children, while Seguro Popular also includes some interventions for pregnant women. SMNG includes treatment for a very wide range of conditions, including genetic issues, complications from birth, nutrition and metabolic disorders, hearing and vision problems, and other potentially high-cost health issues. Children covered by SMNG are also covered by Seguro Popular, which includes the treatments for the most common childhood diseases, a complete physical after birth, infant and child vaccinations, neonatal screenings, and training for mothers on breastfeeding and nutrition (CAUSES 2012). They are also covered for 44 interventions (for high-cost, low-likelihood illnesses) focusing specifically on young children through FPGC (Rodríguez-Ortega, 2012; Muñoz-Hernández, 2012). Embarazo Saludable (ES), added in May 2008, does not cover new interventions but simply extends automatic coverage to pregnant women and their families if they do not have insurance coverage (Pfutze, 2014).

There are a number of services covered under Seguro Popular that could be possible pathways for the program to affect child nutrition. For instance, the program covers a range of interventions relating to childbirth, neonatal care, and infant preventive health care like vaccinations and screenings. The program also covers education on breastfeeding and infant nutrition, which could very directly affect child nutrition. Similarly, for children who are older than infants but younger than five years of age the program covers complete health check-ups, which include measurement of height and weight and training of parents on nutrition and feeding. Childhood diarrhea is another possible mechanism that could affect nutrition and height. Seguro Popular includes treatment for diarrhea, including oral rehydration and training for parents on how to prevent and recognize diarrhea (CAUSES 2014). More indirectly, if Seguro Popular lowers out-of-pocket expenditure for health care more generally and reduces the chances of catastrophic healthcare spending, this could also potentially impact child nutrition simply by freeing up resources within a household.

To be covered by Seguro Popular, individuals or households must actively affiliate with the program, with a few exceptions. In addition to the automatic coverage of pregnant women, infants, and their families added to the program later and discussed above, individuals already enrolled in Oportunidades are affiliated with Seguro Popular automatically. In general, however, people must choose to enroll and, as part of that process, are required to pay an annual fee based on self-reported income. The lowest two income quintiles do not have to pay anything (Barros 2008). As a result of income being self-reported, although there is technically a fee for households with higher incomes, the Department of Health estimates that the vast majority of households (more than 99%) do not pay for coverage (Pfutze, 2014). In our sample (described in more detail below), for instance, the mean per capita monthly expenditure among households with Seguro Popular coverage in 2009 was approximately $1,005 compared to $1,506 among households without Seguro Popular. Although these means are significantly different, there is still quite a bit of overlap in the distribution of per capita expenditure between the two groups, suggesting it was not only the poorest households who took up the program.

The program began with a pilot in five states in 2002 and continued with full roll-out beginning in 2004. There were, however, 15 states which began roll-out before there was an official agreement with the federal government in 2002 and 2003 (Bosch et al., 2012; Conti and Ginja, 2014). Due to financial and logistical constraints, the program was to be implemented gradually over seven years, with an annual cap on new enrollees. Each year, the state and federal governments negotiated the target number of households the state would enroll in each quarter of the following year until states reached their official final target number (theoretically equal to the number of uninsured households in their state). Officially, states with low insurance coverage, high numbers of uninsured individuals in the first six income deciles, the ability to provide services, high demand for enrollment, and a sufficient budget were to be targeted first (Conti and Ginja, 2014). Within states, Seguro Popular was rolled out at the municipality level, meaning that a municipality could begin affiliating enrollees once their health care facilities met minimum infrastructure and human resource requirements (Knox, 2008).

While Seguro Popular was intended to be rolled out first to poorer states and states with high numbers of uninsured households, there is disagreement about whether this happened in practice. Some observers have suggested that politics played a role in determining when and how quickly the program rolled out to states and municipalities. For instance, Barros (2008) argues that smaller states and those affiliated with the governing party tended to have the program implemented sooner. His explanation of this phenomenon is that for smaller states, it was easier to achieve full coverage, and these states could be used by the federal government as examples of the success of the program. Knox (2008) notes that the five states included in the pilot program had health ministers who were affiliated with the Minister of Health. Officially, these five pilot states were chosen because they had the infrastructure to provide the guaranteed services, they had large semi-urban populations, and because they had a large number of households who participated in social programs such as Oportunidades (Conti and Ginja 2014). Although the Inter-American Development Bank (2012) concluded that program roll-out was essentially random, it is not clear that this is in fact the case.

To get at this issue, we can look at whether date of roll-out seems to be related to municipality resources for the municipalities in our sample. Table 1 (which uses data described in more detail below) shows municipality averages for a select number of relevant characteristics at three dates, stratified by when Seguro Popular was rolled out. For instance, the first two panels show measures of household resources, including average education of the household head and average per capita expenditures. Generally, there is little evidence that Seguro Popular was rolled out first to municipalities that were poorer. If anything, in our data the opposite seems generally true. For instance, the average education (in years) of the household head was 6.9 for communities that got Seguro Popular in 2002 versus 5.4 for municipalities that got the program after 2005 (and this difference is statistically significant). The municipalities that got Seguro Popular earlier also had slightly higher average per capita expenditures than municipalities that got it later, contrary to the government's stated policy of introducing Seguro Popular into poorer areas first. This may reflect the fact that roll-out was conditional on municipalities having the infrastructure to provide the required services.

Panels C and D show some of the municipality characteristics that were officially supposed to influence roll-out, including the fraction of households informally employed and the fraction of households without medical insurance. Similar to what was found for household characteristics it seems that if anything municipalities that had higher rates of formal employment and insurance coverage and lower rates of households being enrolled in Oportunidades were targeted first. This does not align with the official stated policy of the government but is generally consistent with previous empirical evidence on the roll-out of the program.

DATA

The primary data source for this project will be the Mexican Family Life Survey (MxFLS), a panel survey launched in 2002. The first wave (MxFLS1) was conducted in 2002, the second (MxFLS2) in 2005-2006, and the third (MxFLS3) in 2009-2012. The baseline sample was representative at the regional, rural, urban, and national levels. It contained a sample of 35,677 individuals in 8,440 households, located in 150 communities spread over 16 states. During the second and third rounds, attempts were made to relocate all individuals from the first round even if they had migrated within Mexico or to the United States. The attempts to follow and find households who had moved were quite successful; MxFLS2 and MxFLS3 found and re-interviewed nearly 90% of the original sample.[2]

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[2] For more information on MxFLS, see Rubalcava and Teruel, 2006; Rubalcava and Teruel, 2008; and Rubalcava and Teruel, 2013.

MxFLS contains a rich variety of demographic and economic measures. Critical for this project is that an array of health and anthropometric information was collected from all household members. This includes information on height (or length for young infants), which was used to construct height-for-age z-scores. The survey also included extensive information on other health-related outcomes, including self-reported health, illness, use of health care (such as prenatal care), healthcare expenditures, and vaccinations. There is also information on household members, including education, income, and employment. For each household member at least 14 years old, the survey collects information on whether household members received benefits from social programs (such as Oportunidades) and if they are covered by health insurance (and if so, from what source). There were also surveys of municipality facilities, including health care facilities, which can provide some information on the availability of health care services and infrastructure within a municipality.

The data from MxFLS are supplemented with administrative data on the date that Seguro Popular was initially rolled-out within each municipality. This allows the creation of a measure, for each child, of how much potential exposure they had to the program during their early childhood based on when Seguro Popular was introduced into their community.

MEASURING SEGURO POPULAR'S EFFECT ON CHILD HEIGHT

Isolating the causal effect of Seguro Popular is complicated by a number of identification challenges, only some of which the previous literature has been able to address. While a few studies find positive health effects of the program (Sosa-Rubí et al., 2009; Aguilera and Marrufo, 2006; Pfutze, 2014; Pfutze, 2015; Bleich et al., 2007; Knox, 2016), others find no effect at all (Barros, 2008; King et al., 2009). It is difficult to compare these results since they focus on different outcomes and use a range of methodologies. Unfortunately, for the health outcomes these previous papers focused on, the effect of the program cannot be identified using observational data without making additional assumptions. For instance, several papers use the date of roll-out as an instrument for enrollment and then look at the relationship between enrollment and health outcomes. However, roll-out also coincided with an increase in spending on health infrastructure that may have affected families who were not enrolled as well. Therefore, the exclusion restriction is almost certainly violated. Similarly, a number of the papers assume that the date of roll-out was essentially random. If roll-out is related to the health environment, however, this will cause problems for their estimation. While two papers use experimental data (King et al., 2007 and King et al., 2009), they suffer from external validity concerns (the experiment was run in a small number of carefully selected municipalities which are very unlikely to be representative of the entire country) and only cover ten months (arguably not enough time for many health outcomes to respond to roll-out of health insurance). Reliance on additional and likely unrealistic assumptions is a shortcoming of the previous literature on Seguro Popular.

Looking at how the prevalence of various health outcomes changed over time can help to illustrate how analysis depending on random roll-out or other additional assumptions might lead to incorrect conclusions. Figure 1 shows, as an example, the percent of children aged 0 to 2 who had a cough in the previous four weeks in 2002, 2005, and 2009. The children are stratified based on when their municipality got Seguro Popular. Looking at children in municipalities with the program in 2002, there is a dramatic decline in reported cases of cough between 2002 and 2005. It might be tempting to conclude that this is the impact of the program. However, there is a very similar decline between 2002 and 2005 for children who got the program after 2005, although it could not have been caused by the program. Something else seems to have been going on as well. It is possible that the difference in improvement (a difference-in-difference) is significant if the areas that got Seguro Popular improved more. However, attributing that additional improvement to Seguro Popular requires a parallel trends assumption that may not be reasonable if areas that got Seguro Popular earlier were on a different trajectory even before receiving the program.

An additional complication in measuring the impact of the program is that one cannot simply compare the height of children in households enrolled in Seguro Popular with children in households that have not enrolled. This comparison would be difficult to interpret because it does not take into account differences between households who do and do not choose to enroll in the program. The households who opt in are likely to be different than households who do not in ways that are not observed (for instance, they may have greater need for health insurance because of pre-existing health conditions). The primary analysis in this project will therefore be intent-to-treat, where treatment status will be a function of age of the child and of when Seguro Popular was introduced to their community, regardless of whether their family reports actually enrolling in the program.

The intent-to-treat analysis can be supplemented by running similar regressions with households who, at baseline, were likely to be eligible because they worked in the informal sector or reported not having health insurance. Similarly, we can run regressions restricting the sample to households with children who might, *a priori*, be expected to benefit more from the program. For instance, we can look at whether the program's impact is different for children whose families were enrolled in Oportunidades at baseline. These families were already given access to some very basic health care services through Oportunidades but Seguro Popular greatly expanded those services, and we know these families were quite poor (as determined by their eligibility for Oportunidades).

The comparison of children who were likely to benefit from Seguro Popular with those unlikely to benefit are still complicated by the non-random roll-out of the program. For instance, the official policy was that Seguro Popular be introduced in areas with higher levels of poverty first. If that policy was actually implemented, children in areas that got Seguro Popular right away might initially be at a disadvantage compared to children in areas that got Seguro Popular later, making it appear that the program is having little to no effect when that is not in fact the case. If the observable characteristics that were used to plan roll-out were known, it might be possible to control for those characteristics in an analysis. However, the existing literature suggests that the roll-out process was far from transparent, so even if the model includes a wide range of municipality-specific characteristics, there might still be unobservable differences that cannot be accounted for.

Unfortunately, much of the previous literature relies on the roll-out being random for identification. As previously discussed, it is not clear what municipality characteristics determined roll-out, in addition to the ones that were officially stated. Figures 1 and 2 suggest that municipalities that got the program at different points in time may have had very different health environments in 2002 when the program began. Comparing children aged 0 to 2 in 2002 who got the program in their municipality earliest with those that got it in their municipality latest we can see there is a large difference in the reported rates of diarrhea and cough for children before the program was implemented. This suggests that the date of roll-out was not random and that there is reason to be concerned that date of roll-out is correlated with the health status of children.

To address this concern, we will construct groups of municipalities that are likely to be fairly similar (based on their date of roll-out) and look at the impact of the program within these municipality-groups. The first group will consist of communities in which Seguro Popular was rolled out at some point in 2002. The municipalities in the second group had Seguro Popular introduced between 2003 and 2005 (between the first and second waves of MxFLS). The third group consists of municipalities which got Seguro Popular after 2005. Our empirical specification (discussed in more detail below) will also include municipality fixed effects to help eliminate the impact of unobserved, time-invariant municipality characteristics.

Finally, the use of panel data helps in a few ways. First, panel data allows us to control for household, municipality, and state characteristics. In addition, panel data helps in the interpretation of height-for-age z-score. It has been found that in low and middle-income countries, height-for-age tends to decline until weaning before recovering slightly and then remaining fairly constant (Martorell and

Habicht, 1986). This makes it more challenging to interpret any comparisons of younger and older children. Controlling for exact age in the analysis can partially account for these dynamics of height-for-age over childhood. Using the three waves of MxFLS can strengthen the interpretation of the estimates by comparing children in different communities measured at approximately the same age at different points in time.

To this end, children in our sample will be separated into four bands based on their ages during the different rounds of MxFLS. This will allow the separation of children based on whether they are at an age where nutritional interventions should affect their height. The oldest children in the analysis are nine years old. The upper age limit is imposed so that none of the children in the analysis should have hit puberty, which causes additional complications in terms of the dynamics of growth. At each round of MxFLS, children are divided into those who are *very young* (between 0 and 2), *young* (between 3 and 4), *old* (between 5 and 6) and those who are *very old* (between 7 and 9).

The relationship between exposure and age is key for the analysis. The expected exposure for the various age groups, which depends on whether their community got Seguro Popular while they were in the critical window (0 to 4 years old), is outlined in Figure 3. For instance, there should be essentially no effect of the program on the height of children when they are observed in 2002 (since they will have experienced at most only a very short period of exposure to the program) and they are therefore labeled in the figure as having experienced no impact of the program. However, the story is different for children who were less than three years old in 2005 who lived in a municipality that got the program in 2002 (the cell in the row labeled "2005: 0 to 2 years old" and the first column). They would have lived their whole lives with the program in place and there should be a full effect on them. Slightly older children in those same locations, such as those who are between 3 and 4 years old in 2005 (the cell in the row labeled "2005: 3 to 4 years old" and first column), would have experienced only a partial effect of the program since it would have arrived to their municipality after they were born but still during their critical early years. They are labeled as experiencing some effect of the program. In the third column are children who live in municipalities that got Seguro Popular after 2005. For those children there should be no effect of the program during the first two waves, and they will serve as a useful control group.

SAMPLE AND METHODS

Description of the sample population

A selection of descriptive household and child characteristics is presented in Table 2. In the first panel, height-for-age z-scores (calculated using US Centers for Disease Control and Prevention references) are shown for each wave of the survey, split into two age groups and into three municipality-groups based on when their municipality got Seguro Popular. In every column, there is a decline in height-for-age moving from the children who got the program in 2002 to those who got it after 2005. This is suggestive of worse off communities tending to receive the program later, contrary to the official policy but consistent with the literature on the roll-out. There is also a general increase over time in height-for-age in most of the subgroups. The other household and child characteristics (in the second panel) are relatively stable over time, with the average child in the sample being 4.6 years old in a household with roughly 6 members. The average education level of the household head is about 6.7 years and average monthly log per capita expenditure is about 6.7.

Estimation

Given the definitions of age bands and municipality groups outlined above, we turn to the regression equation:

$$\theta_{icvt} = \alpha_{ct}^{3} + \alpha_{ct}^{1} I_1 + \alpha_{ct}^{2} I_2 + \gamma_c^{'} X_{ivt} + \gamma_v + \varepsilon_{ivt} \qquad (1)$$

where θ_{ictv} is the height-for-age z-score of child *i*, in age band *c*, in municipality-group *v*, at time *t*. The superscripts on the α terms represent the municipality-groups (group 1 through group 3). The α_{ct}^{3} terms are twelve different age group-time intercepts. There are intercepts for very young, young, old, and very old at time 0 (2002), intercepts for very young, young, old, and very old at time 1 (2005), and intercepts for very young, young, old, and very old at time 2 (2009). These coefficients estimate the height-for-age z-scores of children in group 3. The next two terms are interactions between age group-time intercepts and indicator variables for belonging to one of the two groups that did get Seguro Popular before MxFLS3. For instance, the α_{ct}^{1} terms represents the interaction of each of the age group-time intercepts with an indicator for belonging to group 1. The subscripts for the age groups are A for very old, B for old, C for young, and D for very young. As a specific example, α_{C0}^{2} represents the difference in average height-for-age at time 0 (MxFLS1) between young children who lived in community-group 2 (who got Seguro Popular between 2003 and 2005) and young children who lived in control communities (group 3 communities that did not get Seguro Popular until after 2005). Each of the interaction terms can be interpreted in this fashion. The X covariates include individual gender and age in months. Finally, the model includes municipality fixed effects to help deal with municipality-level characteristics that might have affected roll-out. It is important to note these are municipality fixed effects, not municipality-group fixed effects (which would introduce issues of multicollinearity). The same municipality of residence from the first round is maintained throughout in case there is any selective migration to areas that got Seguro Popular earlier. Standard errors are clustered at the municipality-time-age group level (clustering at the municipality, municipality-time, or municipality-age group level produces very similar results).

The first version of equation (1) includes all children who are nine or younger during the three waves. This is the truly intent-to-treat version of the model. These estimates can be compared with coefficients from the same regression limited to the sample most likely to benefit from the program. In one version, this will be children who live in households where no one works in the formal sector at baseline (since formal employees have health insurance by law). The second version will limit the sample to children who live in households where none of the adults report having health insurance at baseline. Comparing these estimates can give some suggestive evidence about whether the estimates from the intent-to-treat are being driven downward by including children whose families cannot benefit from the program.

Difference-in-difference estimates

The coefficients in equation (1) cannot simply be read as the effect of the program on child height because roll-out was not random. A modified version of a difference-in-difference will be used instead to get estimates of the causal impact of the program. There are a number of variations of the difference-in-difference that could be estimated; a few possible examples are outlined below.

The first comparison is between children of different ages in the same community group at the same time, which uses the variation in exposure which is the result of the critical window for nutrition and height. The gradual roll-out of the program allows the comparison of younger children who were likely to be fully or partially exposed with older children who were unexposed. As a specific example, $\alpha_{D1}^{1} - \alpha_{A1}^{1}$ provides an estimate of the effect of full exposure by comparing (in 2005) very young children with very old children within communities that got the program in 2002 (group 1). This approach has the advantage that it sweeps out any characteristics common to municipalities within the same community group (which may have influenced when the program was rolled out). However, one possible concern is with life-cycle dynamics that can cause younger and older children to have different height-for-age z-scores even in the absence of any program effect. Previous literature suggests that, particularly in lower and middle-income contexts, height-for-age z-scores tend to decline until weaning, increase slightly, and then stabilize. This can affect the comparison of younger and older children regardless of program impact. In addition, any time-varying community characteristics or time-trends that affected the height of older children differently than the height of younger children may also influence this estimate.

This suggests a second comparison that could provide an alternative estimate of the program effect. Instead of comparing children of different ages within a community group at the same point in time, children of the same age can be compared at different points in time. For instance, $\alpha_{D1}^{1}-\alpha_{D0}^{1}$ gives an estimate of the effect of full exposure by comparing children who were very young in 2005 with children who were very young in 2002 (all within communities that got the program in 2002). This addresses the issues of life-cycle dynamics but this comparison raises the concern of confounding time effects. If any changes occurred within these communities between 2002 and 2005 that affected child height besides the introduction of Seguro Popular, that could bias this estimate.

One way to look at whether time effects are influencing the estimate is to do another, similar comparison. Instead of comparing children who were likely exposed to those unlikely to be exposed, however, this comparison will look at older children who were all unlikely to have experienced an effect of the program. For instance, $\alpha_{A1}^{1}-\alpha_{A0}^{1}$ is the difference in height-for-age comparing very old children in 2005 with very old children in 2002, all within communities that got the program in 2002. None of these children should have been exposed to the program, so this difference can give an idea of time effects that were operating in the background that might be influencing the estimate of the program effect.

By combining these comparisons into various difference-in-difference estimates, a number of estimates of the program impact can be constructed. Depending on which combination of differences is used, these estimates will be unbiased under different assumptions and robust to different potential confounding factors. To get an estimate of the impact of the program on children who were fully exposed, for instance, we can calculate $(\alpha_{D1}^{1}-\alpha_{A1}^{1})-(\alpha_{D0}^{1}-\alpha_{A0}^{1})$. The first term gives an estimate of the program impact if there is no age effect. Subtraction of the second term gets rid of any age effect. This comparison is of children in the same community group across time. For this to be capturing only the effect of the program, it needs to be true that there were no time effects that influenced the difference between very young and very old children in a different way in 2002 versus 2005.

Another version of the difference-in-difference is $(\alpha_{D1}^{1}-\alpha_{A1}^{1})-(\alpha_{D1}^{3}-\alpha_{A1}^{3})$. The first term is the same as before, but now the second term looks at children in 2005 living in communities that got Seguro Popular after 2005. This has the benefit that if time trends were the same in the two types of communities, this difference eliminates any age effects and time trends that affected older versus younger children differently. However, for this estimate to be unbiased it must be true that in the absence of the program the difference between very young and very old children would have been the same in 2005 in these two types of communities. This might not be true if there are different trends over time in these two types of locations.

For each of these types of difference-in-difference estimates outlined above, we can compare whether this estimate changes depending on how much time has passed since the program was first rolled out. This gives an idea of whether the impact of the program was changing over time (if, say, the program became more effective as it became more established or well-known). We can also construct similar difference-in-difference estimates of partial exposure. For instance, the effect of partial exposure can be estimated by calculating $(\alpha_{D1}^{2}-\alpha_{A1}^{2})-(\alpha_{D0}^{2}-\alpha_{A0}^{2})$ and $(\alpha_{D2}^{2}-\alpha_{A2}^{2})-(\alpha_{D0}^{2}-\alpha_{A0}^{2})$. Both of these estimates of the effect of partial exposure are within communities that got the program between 2003 and 2005, but the first looks at the program impact in 2005 and the second looks at the program impact in 2009. If these are significantly different, it might suggest that the effect of the program changed depending on when it was rolled out.

RESULTS

The results of estimating model (1) for the full sample are presented below in Table 3. The first column gives the differences in height-for-age z-scores for children in communities that got Seguro Popular in 2002 compared to children whose communities got the program after 2005 (the reference group in the third column). The second column compares the height-for-age of children in the reference group to children in communities who got Seguro Popular in 2003-2005. The coefficients in bolded blue are those that should reflect full exposure to the program. The coefficients in italicized red are those that should reflect partial exposure to the program.

First, there are some patterns consistent with the dynamics of height-for-age over the life course. Looking at children in municipalities that received Seguro Popular after 2005, for instance, their height-for-age z-scores tend to be higher for older children. This is consistent with the existing literature on height-for-age over childhood in developing and middle-income countries.

Looking at the coefficients that should reflect some impact of the program, most are positive but there is no clear relationship in the size of the coefficient and whether the children were fully or partially exposed (and a few coefficients are insignificant or negative). However, as discussed above, simply reading these coefficients out of the table will not provide program effects. Instead, we will have to estimate some of the difference-in-difference comparisons that were outlined in the previous section. This will help disentangle selective roll-out, age effects, and time trends to see if there was in fact an effect of the program. While there are a great number of possible comparisons that could be done, the few that were discussed above are presented in Table 4. The first row in each panel of estimates includes all children in the sample, while the second only includes children in families without anyone working in the formal sector in MxFLS1. The third row in each panel includes children in families that had no one with health insurance in MxFLS1. The fourth and fifth rows show results of quantile regressions at the 25^{th} and 75^{th} percentile. The sixth and seventh rows show the same regressions stratified into urban and rural households, while the final two rows splits the sample based on whether the household was enrolled in Oportunidades at MxFLS1.

Some of the estimates of program effect (in Table 4) are positive and significant, but many others are not different from zero (and some even have a negative sign). Columns 1 and 2 compare children within the same community-type across time while column 3 compares children in 2005 across community types. Column 1 compares children in 2005 to children in 2002, while column 2 compares children in 2009 to children in 2002. Panel A performs these comparisons for children in community type 1 (who got the program in 2002) so should have experienced the full program impact while panel B shows comparisons for children in community type 2 (who got the program between 2003-2005) so should have experienced only a partial impact of the program. As mentioned in the empirical specification, the two types of comparisons require different assumptions. Columns 1 and 2 require that the difference in height-for-age z-score between young and old children would have been the same at two points in time within a community group if the program had not been implemented. Column 3 requires that the difference in height-for-age z-score between young and old children would have been the same for the different community types in 2005 in the absence of the program.

Looking first at columns 1 and 2, we see that generally the estimates in column 2 (2009 to 2002) are larger than the estimates in column 1 (2005 to 2002), suggesting that even in communities got the program very early, it took time to have a measurable impact on health. The effect is also much stronger when we are comparing within communities that got the program very early (in panel A) rather than in communities that got the program between 2003 and 2005 (in panel B). This is as expected, given panel A measures full program impact and panel B measures partial impact. While the partial program estimates in panel B are less consistent, the full program impact estimates in panel A and column 2 show that generally the effects are larger for households we would expect to be more likely to benefit from the program (informally employed, rural, and with Oportunidades). A similar story emerges comparing the results from the two quantile regressions in lines four and five. In both column 1 and 2 in every case the coefficients for the 25th percentile are larger than those for the 75th percentile, which is consistent with our expectation that children who were worse off in terms of their height-for-age may be more impacted by the program.

In column 3 we compare children who got the program earlier (2002 in panel A and 2003-2005 in panel B) with children who got the program after 2005 all in 2005. These estimates are quite large and suggest that the program effect was stronger among urban households and households not enrolled in Oportunidades. The crucial identifying assumption for these estimates, as mentioned above, is that the difference in height-for-age z-scores between younger and older children in the absence of the program would have been the same across different types of communities. Given that we know these community types were very different at baseline, this assumption may not be justified and may explain some of the inconsistent results we see in column 3.

Estimating $(\alpha_{D1}^3 - \alpha_{A1}^3) - (\alpha_{D0}^3 - \alpha_{A0}^3)$, which compares children across time within communities that got the program after 2005, can serve as a robustness check on whether differential time trends are influencing the estimates in the first two columns of Table 4, since there should be no effect of the program on these children. This is a useful comparison only under the assumption that the time trends affecting the height difference between younger and older children would have been parallel in the different community-types in the absence of the program. These estimates are very small and insignificant, suggesting that differential time trends are not affecting our estimates.[3] Estimating similar difference-in-differences for adults is a further placebo check, since there should be no effect of the program on their height. These estimates are generally not significant, and when they are

[3] The exception is that the 25th percentile quantile regression is significantly negative and the 75th percentile quantile regression is positive (although insignificant). This is not what we would expect if the program were having a larger effect on worse off children since it should be making very young children healthier while not affecting older children, but since we do this comparison only for 2005 to 2002 it is consistent with our observation that significant effects of the program do not appear until 2009.

significant they are actually negative (which would suggest the program made adults shorter, although it is more likely a reflection of the fact that taller adults are slightly more likely to leave the sample over time).

We can also look directly at the attrition of children from the sample. Specifically, we would be concerned if attrition was related to their height-for-age or to the date of introduction of Seguro Popular. We do not find any evidence that attrition is related to when the program was rolled out within a community (see Table 6 in the Appendix for a selection of these attrition models). Another way to look at attrition is to see whether children who left the sample are similar in height-for-age to children who entered the sample. This is not a perfect comparison because we are not including children over 10 years old in our sample because of puberty and therefore are looking at cross-sections rather than true panels. However, it can give us some sense of whether selective attrition is a problem for us. Once again, we find no evidence that this is the case. We find, for instance, that children who are measured in 2002 but are gone from the sample by 2005 have very similar height-for-age z-scores as children who enter the sample between 2002 and 2005. The same pattern holds for children who leave the sample before 2009 compared to the children who enter the sample by 2009.[4] These two sets of analysis suggest that differential attrition of children either by height-for-age or by date of program implementation is not a major concern for this analysis.

Taken together, these difference-in-difference estimates provide evidence that the program did have an effect on child height. In addition, the effect seems concentrated among children in families that we would expect to be most likely to enroll (the informally employed, households without insurance, and households enrolled in Oportunidades) and among children who were at the lower end of the height-for-age distribution. However, the impact of the program is not significant when we

[4] For instance, average height-for-age z-scores of children in 2002 who are not measured in 2005 is -0.41 while average height-for-age z-scores of children in 2005 who were not measured in 2002 is -0.46. Similarly, children in 2005 who are not measured in 2009 have an average height-for-age z-score of -0.35 compared to children in 2009 who were not measured in 2005 who have an average height-for-age z-score of -0.33.

compare 2005 to 2002, suggesting that it did not emerge consistently until after the program had been in place for many years.

Other health outcomes

There are a number of other child and infant health outcomes and behaviors that are of interest that could potentially be affected by Seguro Popular. These effects are more difficult to estimate, however, because there is not the critical window for impact as there is for child height. There is some previous work on adults suggesting that Seguro Popular increased the use of primary care and health-seeking behavior (Knox, 2016) but fairly little work looking specifically at use of care for children (Knox 2008; Gakidou et al., 2011; Danese-DlSantos et al., 2011, for instance). Future work could look at vaccination rates, neonatal care, or overall use of care to get at whether the program resulted in children receiving more care. However, without the benefit of a critical window as we have for child height, any results on these outcomes using observational data will be suggestive rather than causal. Given these caveats, very preliminary results on use of care suggests that overall health-seeking behavior declined substantially for all communities between 2002 and 2005 before increasing somewhat by 2009 (see Figure 4 for children under 6 and Figure 5 for adults, for instance). This decline seems to be larger in areas that got the program in 2002 (compared to those that got it after 2005). A similar but smaller decline is seen in the areas that got the program between 2003 and 2008. This decline could reflect a number of factors, including an overall decline in the economy during that interval as well as a change in the health environment over time. However, if this decline in use of care is also related to the roll-out or implementation of the program, it provides some suggestion of a possible mechanism for why it took so long to see an effect of Seguro Popular on child height. The supply-side factors which might have mediated the program's impact which will be discussed in more detail in the next section. However, considerable additional work is needed to determine the cause of the fall in healthcare use and how it was related to the implementation of the program.

Implementation and supply-side constraints

Many commentators and critics have pointed out supply-side constraints and imperfect implementation as problems that have plagued Seguro Popular from the beginning. These may provide some of the explanation for why clear evidence of a program impact is not apparent until the program has been officially in place in a community for many years. One of the initial motivations for the program was the inequality in healthcare spending, availability of services, and quality of care and infrastructure in different states. Before Seguro Popular was rolled out, even among the insured there were high levels of out-of-pocket healthcare spending among those who sought out care from the private sector because of long wait times, prescription drug shortages, and generally poor health infrastructure and quality of care in the public sector (Knaul et al., 2012). Through the implementation of the program there was certainly some progress in redistributing health care spending and addressing the inequality in access to care (Frenk et al. 2009 provide some specific examples). For instance, there was investment in infrastructure including renovations of existing facilities and construction of new hospitals and specialty centers. Critics of the program, however, suggest that the infrastructure investment was not nearly enough and often the new units were small and understaffed, so the numbers paint a more optimistic picture of possible access than is accurate (Laurell, 2007). At a broader level, there were also management issues such as the slow movement of funds from the federal level to the states and poor oversight of funds at the state level, including under-spending and a lack of transparency and accountability (Knaul et al., 2012).

Another challenge to the implementation of Seguro Popular was a fairly severe shortage of human and organizational resources in many areas. A large number of the personnel hired by states to implement the new program were hired on short-term contracts. Specialist medical personnel have been in particularly short supply, leading to underuse of specialty facilities. These problems have been much more severe in rural areas, which had worse health infrastructure to begin with. In 2009, only about half of rural health centers were actually accredited for the Seguro Popular program (which likely partially reflects that they did not meet minimum program requirements). What is more, officially, capacity constraints were taken into consideration when deciding which interventions to cover in Seguro Popular and when they should be added. Given the real likelihood of implementation constraints, the program included the option of “accelerated accreditation” which allowed health care units to provide services as part of Seguro Popular for one year while they improved their infrastructure enough to apply for regular accreditation. Similarly, there is a conditioned certification of medical personnel in some areas to address a shortage of specialists. One probable result of the delay in implementation in places without necessary infrastructure combined with the fact that capacity constraints were considered when deciding which interventions to cover meant that implementation may have been biased toward covering the needs of urban and wealthier communities first (González-Pier et al., 2006).

One result of these supply-side issues is that there are still higher than desirable levels of out-of-pocket spending because of issues of access to and quality of care provided under Seguro Popular (Knaul et al., 2012). In their 2015 paper, Servan-Mori and coauthors looked at availability of prescribed medicines for Seguro Popular affiliates and non-affiliated households without insurance. The basic facts seem to be that in 2012 there were still quite high rates of prescription medications being unavailable to households who sought care at Ministry of Health centers (something around 35%). In addition, a fairly sizeable number of Seguro Popular affiliates were still choosing to get care at non-affiliated health facilities, which suggests there were supply-side or quality limitations.

Enrollment

The previous analyses were all versions of intent-to-treat, since there is likely selection into enrollment. However, it is still of some interest to know what characteristics are associated with enrollment to get suggestive evidence about which households actually enrolled in and benefited from the program. Enrollment in Seguro Popular during MxFLS3 was used as an outcome in a linear probability model (results are in Table 5). The unit of observation is now the household, since households were enrolled in the program together. The results presented below are for all households, but results limited to households with children in our sample are very similar. There are three versions of the model: one which includes basic household information like per capita expenditure and health insurance coverage, a second which adds household health characteristics, and a third which adds community healthcare characteristics. Everything in the model except enrollment in Seguro Popular (the outcome) is measured at baseline (in 2002).

Generally, household resources (education and per capita expenditure in this model) are negatively associated with being enrolled in the program. Having health insurance at baseline, as expected, is also strongly and negatively related to the likelihood that households have Seguro Popular at MxFLS3. The later the program was rolled out in their community, the less likely the household was to have enrolled by 2009, which is consistent with the results suggesting it took time for results of the program to develop. Most of the individual health characteristics are not significantly related to enrollment, other than the female household head having hypertension and the male household head having been diagnosed with cancer, both of which are positively associated with enrollment. Households also seem to be less likely to enroll if the female head reports being in good health but more likely to enroll if the female head has had a serious accident in her life. They are also more likely to be enrolled if they live in a community with health care facilities providing key services like mother and child care. They generally are more likely to be enrolled if they live in communities with higher quality health care facilities, as measured by characteristics like having good ventilation and clean floors.

CONCLUSION AND DISCUSSION

The Mexican public health insurance program Seguro Popular was a large and ambitious attempt to ensure that all of Mexico's citizens had access to basic health care and services. It required a great deal of time and money to implement and resulted in significant investment in health care infrastructure. Many other middle-income countries have also implemented similar public health insurance programs, so lessons from Mexico are relevant to other contexts. Despite this, very little is known about its effects, and particularly its impact on child health. While there is some evidence that the program has had an effect on birth weight and infant mortality, these results are fairly sparse and often suffer from methodological shortcomings.

This analysis dealt with potential problems in this literature in a number of ways. The first was the decision to focus on child height as an indicator of child nutrition. Poor child nutrition has very real consequences both for health in childhood and for later-life outcomes like adult height, earnings, and longevity. It is also known from the nutrition and biology literature that height is affected by nutrition during a critical period in early childhood. Using this fact, in combination with the gradual roll-out of the program and the multiple rounds of data, allows the comparison of children who would have benefited from the program with children who would not have benefited in a modified difference-in-difference analysis. The critical window for child height allows us to estimate a causal effect using observational data, which would not be possible for other health outcomes that do not share this trait.

Another challenge with this type of analysis is that public programs like Seguro Popular are generally not introduced randomly, and are often supposed to reach areas with greater need first. While this was officially the case with Seguro Popular as well, in practice it is more complicated and not at all clear how roll-out was actually implemented. Combining communities into three groups based on when the program was first introduced helps to deal with this issue if communities that got the program around the same time are more likely to be similar in terms of the characteristics that determined the order of the program's introduction. Including community fixed-effects sweeps out any time-invariant community traits.

Another issue to be dealt with is that families who choose to enroll in a public health insurance program are likely different from families who do not, and plausibly in ways that are related to our health outcomes of interest. For this reason, all the estimates are intent-to-treat. Running the same regressions for the full sample of children and comparing them to the estimates for children whose families worked entirely in the informal sector at baseline and to those for children whose families did not have health insurance at baseline helps to get a sense of who might have benefited from the program and whether the estimates are being driven down by households who could not benefit from the program. However, our estimates are generally similar for the full sample and for the selected sub-samples.

The results provide little evidence for immediate or wide-spread benefits of the program in terms of child height. There are some positive effects of the program, but they are seen primarily in children who were exposed to the program their entire lives and who lived in areas that had had the program established for many years.

There are a number of possible explanations for these results. Given the existing evidence on implementation and infrastructure constraints, for instance, it seems very possible that it took time for the program to become fully established and for the infrastructure to catch up with the program requirements. This would be consistent with the preliminary, suggestive evidence on use of care which shows that health-seeking behavior initially fell in MxFLS2 communities that got the program earlier (compared to those communities that got the program after 2005). The empirical evidence on supply-side constraints is relatively thin, but further work on use of care, wait times, costs per visit, and community characteristics like number of clinics and doctors could help shed some light on this issue. It might be helpful to supplement this analysis by looking at other countries that have attempted to achieve universal health coverage and their experiences with expanding the supply side of the health care system (for instance, supply-side expansions for the case of Turkey are explored by Atun et al.

2013). It is also possible that there were demand limitations as well. For instance, it may have taken some time for demand to increase as households learned about the program. Households may also have waited to enroll until the program was well established. Addressing the issue of demand generally, Lakin (2010) suggests that one reason so few households paid the fee to enroll is that officials quickly realized demand was too low for any price higher than zero.

The existence of the Oportunidades program in Mexico might provide another possible explanation for a lack of clear effect of Seguro Popular. There is significant overlap in the populations covered by the two programs, and Oportunidades already provides some services that might affect the health of infants, young children, and their mothers. For these families, Seguro Popular may have more of an effect on healthcare spending and resources within the household in the event of an extreme health shock, while having less of an effect on health and health care for very young children. Unfortunately, these various supply and demand explanations cannot be easily disentangled with these data.

These findings have serious implications for the design and evaluation of health insurance programs and of government policies more generally. It is frequently the case that evaluations of policies and randomized controlled trials are done in the very short term (including the randomized controlled trial done to evaluate Seguro Popular that took place over only 10 months). These results suggest that policy makers should anticipate the possibility that it may take years for significant results to be seen or measured. This may be particularly true in contexts where implementation and management of new programs is imperfect or infrastructure needs to be scaled up, as is particularly likely in low and middle-income countries.

Overall, this analysis provides evidence of a limited causal impact of the program on child height. It also suggests that the impact took several years to be established. Although some preliminary evidence on possible explanations for this small effect is provided, more research is needed to definitively disentangle the possible mechanisms. This is particularly important if other countries look to Mexico's experience for lessons on public health provision. All of this points to a need for researchers and policy makers to think harder about how similar programs are implemented and run, how beneficiaries are targeted, and how long program evaluators might have to wait to see any real health benefits.

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TABLES AND FIGURES

Table 1: Municipality characteristics in 2002 by date of roll-out	
Panel A – Average education of household head	
Received SP by 2002	6.93 (0.19)
Received SP by 2005	6.64 (0.13)
Received SP after 2005	5.44 (0.18)
Panel B – Average log of per capita expenditure	
Received SP by 2002	7.06 (0.01)
Received SP by 2005	6.93 (0.01)
Received SP after 2005	6.58 (0.01)
Panel C – Percent of adults informally employed	
Received SP by 2002	62% (1.00%)
Received SP by 2005	64% (0.62%)
Received SP after 2005	66% (1.04%)
Panel D – Percent of adults without insurance	
Received SP by 2002	42% (1.00%)
Received SP by 2005	53% (0.64%)
Received SP after 2005	64% (1.07%)
Panel D – Percent of the population with Oportunidades	
Received SP by 2002	7% (0.31%)
Received SP by 2005	12% (0.26%)
Received SP after 2005	24% (0.56%)
Numbers in parentheses are standard deviations. An informal worker is someone who reports working on their own plot, working as a family worker without payment, being self-employed, or otherwise working without payment. All numbers are weighted using 2002 household weights.	

Table 2: Descriptive statistics of the sample of children and their households									
	MxFLS 1 (2002)		MxFLS 2 (2005)		MxFLS 3 (2009)		Full sample		Sample size
	Age 0-2	Age 3-9	Age 0-2	Age 3-9	Age 0-2	Age 3-9	Age 0-2	Age 3-9	
Panel A – Average height-for-age z-scores									
Received SP by 2002	-0.28	-0.24	-0.40	-0.22	0.15	-0.24	-0.15	-0.24	4,280
	(0.10)	(0.04)	(0.13)	(0.06)	(0.10)	(0.05)	(0.06)	(0.03)	
Received SP by 2005	-0.66	-0.53	-0.41	-0.39	-0.34	-0.34	-0.45	-0.43	9,221
	(0.08)	(0.03)	(0.07)	(0.04)	(0.05)	(0.03)	(0.04)	(0.02)	
Received SP after 2005	-0.84	-0.92	-0.70	-0.77	-0.69	-0.79	-0.73	-0.83	4,261
	(0.10)	(0.05)	(0.11)	(0.05)	(0.12)	(0.05)	(0.07)	(0.03)	
Overall	-0.66	-0.61	-0.51	-0.50	-0.40	-0.49	-0.50	-0.54	
	(0.06)	(0.02)	(0.05)	(0.03)	(0.05)	(0.03)	(0.03)	(0.01)	
Sample size	1,359	4,795	1,304	4,036	2,063	4,205	4,726	13,036	17,762
Panel B – Household and child characteristics									
Age in years	4.72		4.61		4.36		4.57		21.696
	(0.05)		(0.05)		(0.05)		(0.03)		
Education of household head	5.97		6.57		6.85		6.68		15,569
	(0.07)		(0.08)		(0.06)		(0.05)		
Log per capita expenditure	6.68		6.30		7.08		6.68		21,131
	(0.01)		(0.01)		(0.01)		(0.01)		
Household size	5.52		5.77		5.90		5.79		21,285
	(0.03)		(0.04)		(0.04)		(0.03)		

Numbers in parentheses are standard deviations. All numbers are weighted using 2002 household weights. Education is measured in years while per capita expenditure is measured in dollars.

Table 3: Height-for-age z-scores (Full sample of children 0 to 9 years old at each wave)					
			(1) Received SP in 2002	(2) Received SP in 2003-2005	(3) Received SP after 2005
Notation	Period	Age group			
α^{v}_{A0}	2002	A: 7 to 9	0.65***	0.62***	-0.28
			(0.19)	(0.17)	(0.19)
α^{v}_{B0}		B: 5 to 6	0.37*	0.44***	-0.44**
			(0.19)	(0.17)	(0.18)
α^{v}_{C0}		C: 3 to 4	0.50**	0.55***	-0.83***
			(0.20)	(0.16)	(0.17)
α^{v}_{D0}		D: 0 to 2	0.45**	0.31	-1.22***
			(0.22)	(0.20)	(0.20)
α^{v}_{A1}	2005	A: 7 to 9	0.38*	0.47***	0.03
			(0.21)	(0.16)	(0.18)
α^{v}_{B1}		B: 5 to 6	0.54**	0.50***	-0.37**
			(0.22)	(0.17)	(0.17)
α^{v}_{C1}		C: 3 to 4	0.38*	0.69***	-0.70***
			(0.20)	(0.17)	(0.17)
α^{v}_{D1}		D: 0 to 2	0.17	0.42**	-1.05***
			(0.23)	(0.19)	(0.20)
α^{v}_{A2}	2009	A: 7 to 9	0.35*	0.38**	0.05
			(0.20)	(0.17)	(0.19)
α^{v}_{B2}		B: 5 to 6	0.40*	0.82***	-0.50***
			(0.21)	(0.17)	(0.17)
α^{v}_{C2}		C: 3 to 4	0.45**	0.68***	-0.75***
			(0.20)	(0.17)	(0.18)
α^{v}_{D2}		D: 0 to 2	0.62***	0.48***	-1.07***
			(0.20)	(0.17)	(0.20)
		F-statistic [column] (p-value)	1.59 (0.09)	2.94 (0.00)	6.69 (0.00)
		F-statistic [full model] (p-value)	43.98 (0.00)		
		Observations	17,702		

This table is the result of running one ordinary least squares regression on the sample of children age 0 to 9 at each wave of MxFLS. The regression also includes gender, age in months, and municipality fixed effects. Robust standard errors are clustered at the municipality-year-cohort level (clustering at the municipality, municipality-year, or municipality-cohort levels give essentially the same result). Numbers in blue reflect the full impact of the program, while numbers in red reflect partial exposure. All numbers are weighted using 2002 household weights.

Table 4: Difference-in-difference estimates of the program impact

Panel A: Full program effect

	(1) $(\alpha^1_{D1}-\alpha^1_{A1})-(\alpha^1_{D0}-\alpha^1_{A0})$	(2) $(\alpha^1_{D2}-\alpha^1_{A2})-(\alpha^1_{D0}-\alpha^1_{A0})$	(3) $(\alpha^1_{D1}-\alpha^1_{A1})-(\alpha^3_{D1}-\alpha^3_{A1})$
Full sample	-0.02	0.46*	0.86***
	(0.27)	(0.24)	(0.33)
Informally employed	-0.17	0.73	0.61
	(0.48)	(0.46)	(0.50)
No insurance	-0.41	0.51	0.18
	(0.41)	(0.49)	(0.41)
Quantile (25%)	-0.09	0.19	0.89**
	(0.37)	(0.31)	(0.44)
Quantile (75%)	-0.23	0.49	0.65
	(0.41)	(0.38)	(0.48)
Rural	0.12	0.60**	0.19
	(0.34)	(0.29)	(0.38)
Urban	0.09	0.49	1.39***
	(0.33)	(0.30)	(0.42)
Oportunidades	0.20	1.33**	0.68
	(0.44)	(0.53)	(0.58)
No Oportunidades	0.04	0.50*	1.13***
	(0.29)	(0.27)	(0.36)

Panel B: Partial program effect

	(1) $(\alpha^2_{D1}-\alpha^2_{A1})-(\alpha^2_{D0}-\alpha^2_{A0})$	(2) $(\alpha^2_{D2}-\alpha^2_{A2})-(\alpha^2_{D0}-\alpha^2_{A0})$	(3) $(\alpha^2_{D1}-\alpha^2_{A1})-(\alpha^3_{D1}-\alpha^3_{A1})$
Full sample	0.26	0.41*	1.03***
	(0.24)	(0.22)	(0.30)
Informally employed	0.07	0.49	0.53
	(0.36)	(0.37)	(0.42)
No insurance	0.27	0.54	0.74**
	(0.27)	(0.35)	(0.32)
Quantile (25%)	0.21	0.37	0.84**
	(0.29)	(0.27)	(0.41)
Quantile (75%)	0.13	0.49*	0.81*
	(0.32)	(0.28)	(0.47)
Rural	-0.32	0.24	0.09
	(0.28)	(0.26)	(0.35)
Urban	0.54*	0.53*	1.60***
	(0.29)	(0.28)	(0.38)
Oportunidades	0.07	0.08	0.84
	(0.34)	(0.36)	(0.51)
No Oportunidades	0.34	0.56**	1.29***
	(0.25)	(0.26)	(0.33)

To get these estimates, first the ordinary least squares regression outlined above was calculated to obtain the coefficients used to construct the difference-in-difference estimates. Nine versions of this model were run (each row in each panel represents one version). Robust standard errors are clustered at the municipality-year-cohort level (except for the quantile regressions, for which the standard errors were bootstrapped). Informal employment, insurance status, rural/urban, and Oportunidades are from baseline (2002). All numbers are weighted using 2002 household weights.

Table 5 – Household was enrolled in Seguro Popular by 2009 (100 = yes)			
	(1) Basic	(2) Basic + health	(3) Basic + health + community
Basic household characteristics			
Education of the male head	-1.46*** (0.19)	-1.38*** (0.19)	-0.85*** (0.18)
Education of the female head	0.39* (0.22)	0.41* (0.21)	0.29 (0.19)
Age of the male head	-0.13 (0.10)	-0.15 (0.10)	-0.24** (0.11)
Age of the female head	0.13* (0.07)	0.12 (0.08)	0.08 (0.09)
No one in the household works in the formal sector	1.26 (1.48)	1.07 (1.53)	0.16 (1.33)
The household has health insurance	-16.80*** (1.37)	-16.85*** (1.38)	-13.81*** (1.30)
Seguro Popular arrived in 2003 (omitted category is 2002)	-2.99 (5.35)	-3.43 (5.22)	-0.76 (6.94)
Seguro Popular arrived in 2004	-0.46 (5.24)	-0.79 (5.10)	4.18 (5.34)
Seguro Popular arrived in 2005	-5.33 (5.89)	-5.63 (5.71)	-2.52 (5.21)
Seguro Popular arrived in 2006	-2.40 (6.94)	-2.48 (6.73)	7.89 (7.03)
Seguro Popular arrived in 2007	-11.42 (7.91)	-12.22 (7.85)	-8.76 (9.15)
Seguro Popular arrived after 2007	-28.40*** (8.16)	-28.17*** (8.06)	-32.76*** (11.50)
Log of per capita expenditure	-8.29*** (0.98)	-9.98*** (1.03)	-5.83*** (1.12)
Household health characteristics			
Height of the male head		0.05 (0.09)	0.01 (0.10)
Height of the female head		0.01 (0.09)	-0.03 (0.09)
If the male head has been diagnosed with hypertension		-1.48 (3.23)	1.34 (3.07)
If the female head has been diagnosed with hypertension		3.97** (1.78)	3.27 (2.12)
If the male head has been diagnosed with cancer		29.02*** (10.71)	25.72* (13.43)
If the female head has been diagnosed with cancer		-1.67 (4.38)	-1.71 (4.34)
If the male head reports being in good health		-1.98 (1.80)	-1.15 (2.08)
If the female head reports being in good health		-3.64** (1.59)	-3.17 (2.06)
If the male head has had an illness in the past 4 weeks		3.36 (2.80)	3.12 (3.11)
If the female head has had an illness in the past 4 weeks		2.22 (1.95)	2.30 (1.99)
If the male head has had a serious accident in their life		-0.41 (2.66)	0.49 (3.27)
If the female head has had a serious accident in their life		3.55* (2.00)	4.05 (2.61)
Community healthcare characteristics			
At least one SSA institution			-2.20 (4.28)
At least one IMSS/ISSTE institution			-5.90 (5.48)
At least one private institution			-4.09 (4.00)
At least one other type of institution			-5.56** (2.80)
At least one center with a family planning area			5.27 (4.66)
At least one center with mother-child services area			6.61 (5.37)
At least one center with good ventilation in the exam room			11.13* (6.08)
At least one center with a very clean floor in the exam room			2.41 (4.93)
At least one center with a lab			-11.81** (5.22)
At least one center without frequent blackouts			1.92 (2.47)
At least one center with community outreach			-0.23 (5.94)
Constant	95.81*** (11.46)	103.06*** (18.97)	81.38*** (25.98)
Adjusted R-squared	0.20	0.20	0.20
Number of observations	7,957	7,957	6,119

Additional covariates in both models not shown in the table include whether the female and male head exist; number of household members of each gender from 0-4 years old, 5-9 years old, 10-14 years old, 15-24 years old, 25 to 64 years old, and 65 or older; whether someone in the household works; and state dummies. Additional covariates in model (2) not shown in the table include waist circumference, BMI, and blood pressure of the household heads, whether the household heads smoke or exercise, whether the household heads think their health will be worse next week, whether the household heads have a permanent injury, and whether the household heads have been diagnosed with diabetes or heart disease. An informal worker is someone who reports working on their own plot, working as a family worker without payment, being self-employed, or otherwise working without payment. All variables are measured at baseline except the outcome. Robust standard errors clustered by municipality are in parenthesis. All numbers weighted using 2002 household weights.

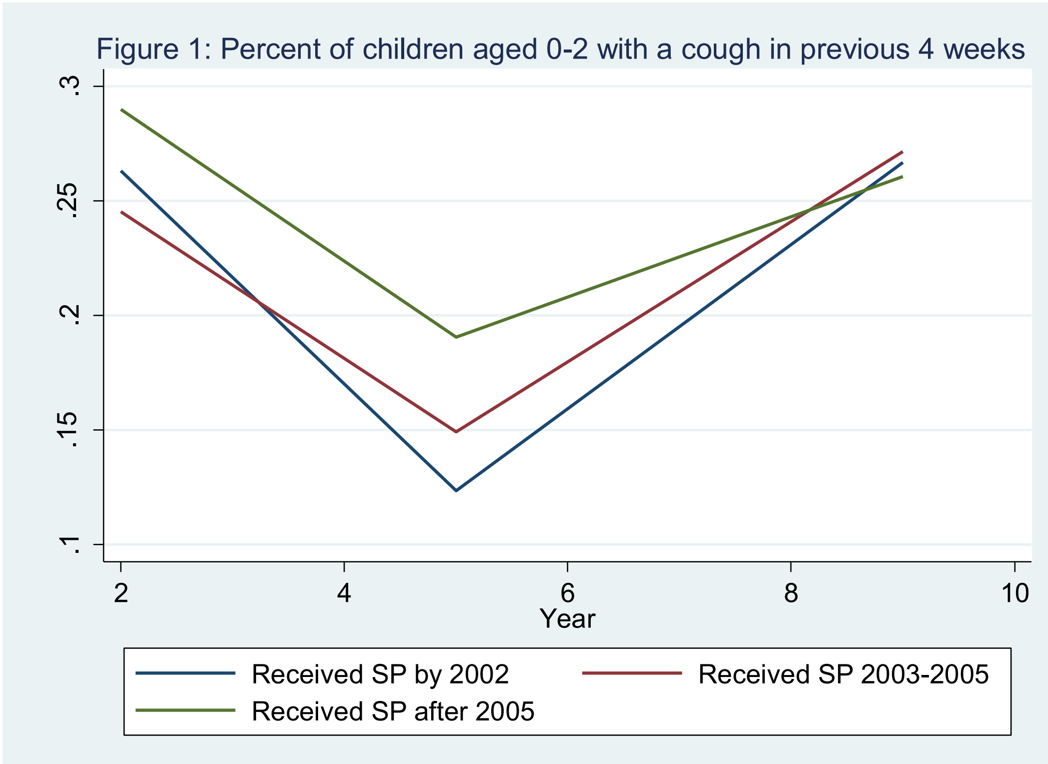

Figure 1: Percent of children aged 0-2 with a cough in previous 4 weeks
.3
.25
.2
.15
.1
2
4
6
8
10
Year
Received SP by 2002
Received SP 2003-2005
Received SP after 2005

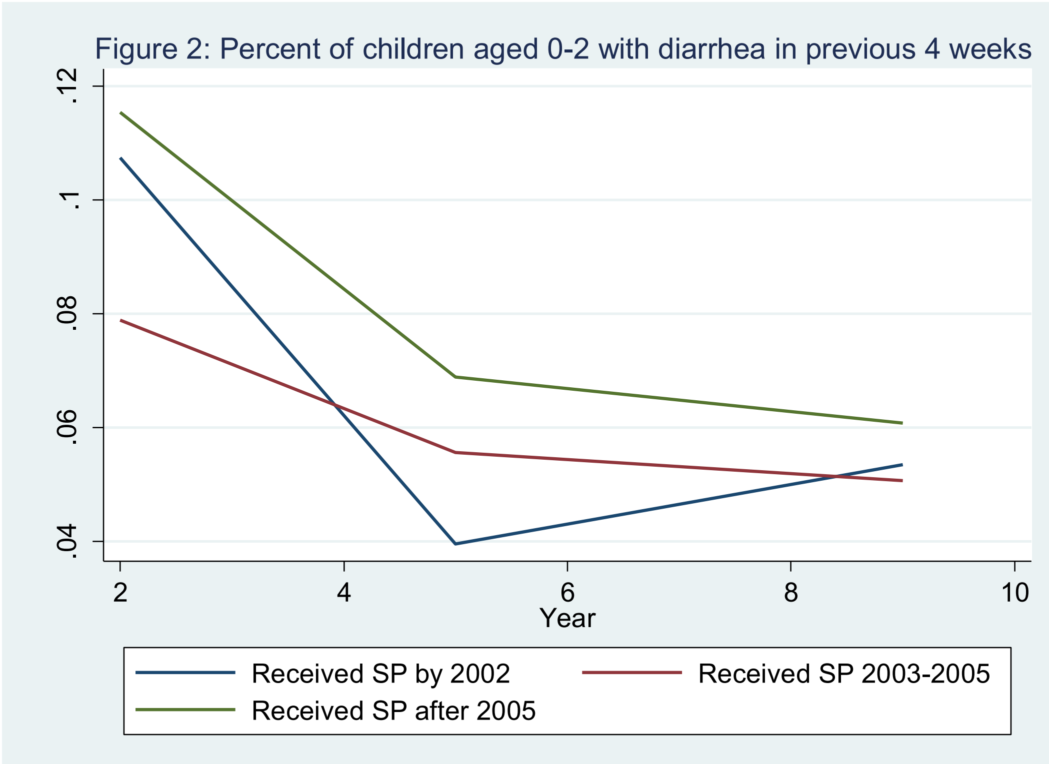

Figure 2: Percent of children aged 0-2 with diarrhea in previous 4 weeks
.04
.06
.08
.1
.12
2
4
6
8
10
Year
Received SP by 2002
Received SP 2003-2005
Received SP after 2005

Figure 3: Expected impact of Seguro Popular on height-for-age z-scores

	(1) Received SP in 2002	(2) Received SP in 2003-2005	(3) Received SP after 2005
2002: 7 to 9 years old	None	None	None
2002: 5 to 6 years old	None	None	None
2002: 3 to 4 years old	None	None	None
2002: 0 to 2 years old	None	None	None
2005: 7 to 9 years old	None	None	None
2005: 5 to 6 years old	*Some*	*Some*	None
2005: 3 to 4 years old	*Some*	*Some*	None
2005: 0 to 2 years old	**Full**	*Some*	None
2009: 7 to 9 years old	*Some*	*Some*	*Some*
2009: 5 to 6 years old	**Full**	*Some*	*Some*
2009: 3 to 4 years old	**Full**	**Full**	*Some*
2009: 0 to 2 years old	**Full**	**Full**	*Some*

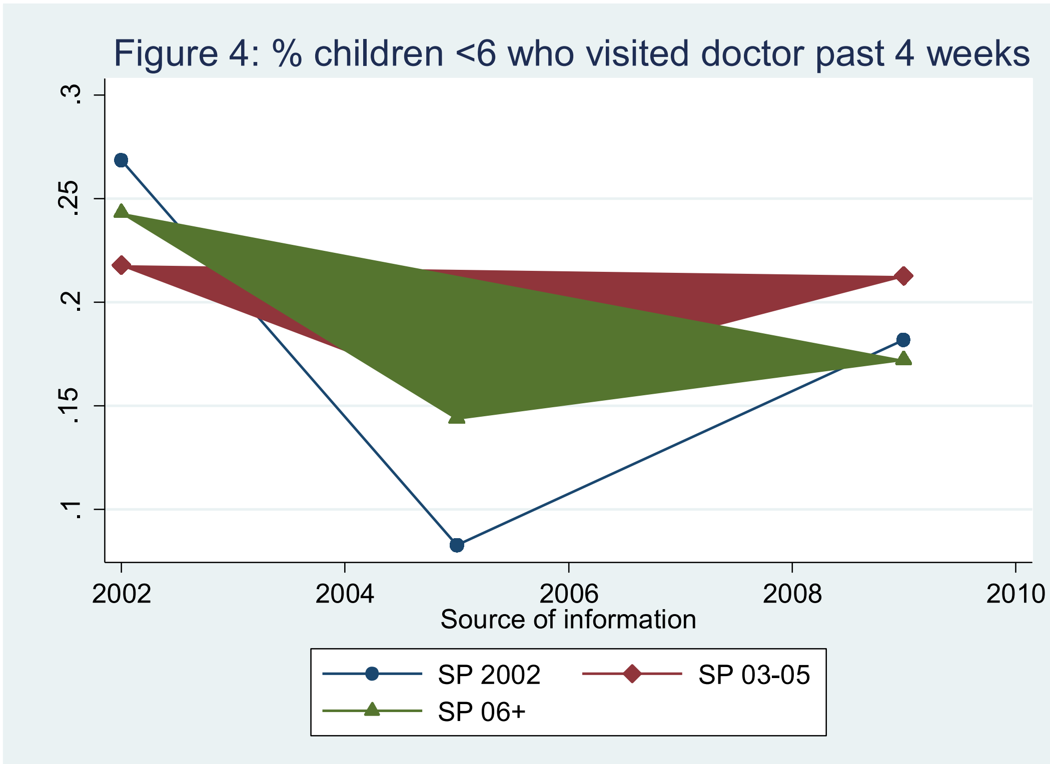
Figure 4: % children <6 who visited doctor past 4 weeks
.3
.25
.2
.15
.1
2002
2004
2006
2008
2010
Source of information
SP 2002
SP 03-05
SP 06+

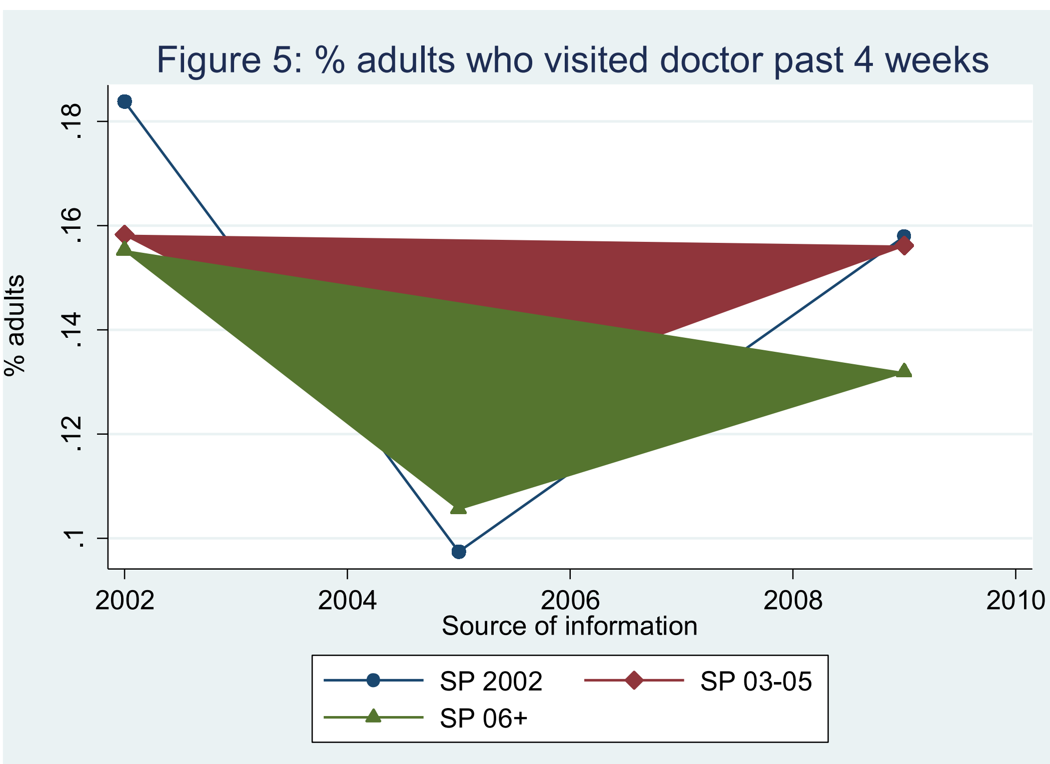
Figure 5: % adults who visited doctor past 4 weeks
% adults
.18
.16
.14
.12
.1
2002
2004
2006
2008
2010
Source of information
SP 2002
SP 03-05
SP 06+

APPENDIX

Table 6: Attrition of children and the roll-out of Seguro Popular

	No information in 2009	
	(1)	(2)
VARIABLES	Children in 2002 or 2005	Children in 2002
Seguro Popular in 2003-2005	0.00	-0.00
	(0.04)	(0.05)
Seguro Popular in 2006-2008	-0.04	-0.02
	(0.04)	(0.05)
Constant	0.18***	0.23
	(0.03)	(0.16)
Observations	10,018	7,294
R-squared	0.00	0.20

Robust standard errors clustered by municipality are in parenthesis. All numbers weighted using 2002 household weights. The omitted category is received Seguro Popular in 2002.