LIVING LONGER: THE EFFECT OF THE MEXICIAN CONDITIONAL CASH TRANSFER PROGRAM ON ELDERLY MORTALITY*

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Abstract

With both an aging population and a transition from communicable to chronic diseases, the health of the elderly is a growing issue in many developing countries. Conditional cash transfer programs are usually thought to benefit young people, but may also benefit other age groups since some programs required all household members to have regular preventive health check-ups. This paper exploits the phasing-in of the Mexican conditional cash transfer program, Progresa, between 1997 and 2000, and shows a 4 percent decline in average, municipality-level mortality for people aged 65 and older. The program not only reduced deaths due to more traditional infectious diseases, but also reduced deaths due to diabetes. Given diabetes deaths are a leading cause of death in Mexico, and now in the top 10 causes of death in many high- and middle-income countries, this is an important finding.

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1. Introduction

With improving living standards and higher life expectancy, many developing countries face the challenge of managing both an epidemiological (Omran, 1971, Gribble and Preston 1993) and a demographic transition. As the epidemiological transition takes place, the relative contribution of communicable and non-communicable diseases to death, as well as to burden of disease (loss of healthy life from death and disability), changes. The major causes of death and the burden of disease move from infectious diseases and under-nutrition to non-communicable diseases such as cardiovascular disease, diabetes, and over-nutrition. An epidemiological transition combined with an aging population means countries have the challenge of managing health care for a growing elderly population that suffers from traditional communicable disease as well as chronic degenerative diseases. This paper investigates whether the Mexican conditional cash transfer (CCT) program, initially known as Progresa and now called Oportunidades, helped with this challenge by reducing elderly mortality.

Mexico is an example of a middle-income country that is fairly advanced in the epidemiological transition. In 1991, non-communicable diseases already accounted for 47 percent of the burden of disease, communicable diseases for 32 percent, and the rest attributable to injuries and accidents (Lozano et al. 1995). By 2004, non-communicable diseases dominated, increasing to 68 percent of the burden of disease and 75 percent of total deaths, while communicable diseases decreased, accounting for 14 percent of deaths and 18 percent of the burden of disease (Stevens et al. 2008). In addition, infectious diseases and malnutrition went from being 3 of the top 10 causes of death (respiratory infections, acute diarrhea, malnutrition) in 1991, to being only 1 of the top 10 causes of death (respiratory infections) in 2004. However, the speed of the transition differs by region in Mexico, with communicable disease still accounting for up to a third of the burden of disease in 2004 in some of the poorer southern states (Stevens et al. 2008).

CCT programs are a popular social program in middle and low-income countries. They are usually thought of as a way to build the human capital of young children and break the intergenerational transmission of poverty. They aim to achieve these goals through the provision of cash transfers conditional on beneficiaries engaging in positive behaviors such as children's attendance at school or regular health care visits (Fiszbein and Schady 2009). However, the design of the Mexican CCT may also have led simultaneously to improvements in elderly health. For example, the program required all adults in a beneficiary household to seek regular preventive health visits, and the health services provided addressed some of the health concerns of the elderly. A household member was required to attend health education sessions to learn how to take better care of the health and nutrition of all family members, not just children (Adato, Coady, Ruel, 2000). And, the cash transfer increased the income of the entire household allowing all household members to potentially improve their nutrition and buy more health inputs.

Progresa is one of the first large-scale CCT programs. It started in 1997 mainly in poor rural areas. By 2000, the program reached approximately 2.5 million families or about 40 percent of rural families (Coady, 2000). Since then, the program expanded to include more urban areas, and served about 5 million households by 2007 (Adato and Hoddinott, 2009). Similar types of programs have been implemented in more than 30 middle- and low-income countries (Fiszbein and Schady 2009). A prominent feature of Progresa is the randomized evaluation built into the

design of the program. While many studies on Progresa take advantage of the data collected as part of the randomized evaluation, the sample size is insufficient to estimate the impact of Progresa on an important health indicator – elderly mortality – and does not contain information on cause of death. Due to these limitations, previous research has not examined the effect of Progresa on elderly mortality.

We exploit the phasing-in of Progresa throughout Mexico over time and location, and take advantage of the high quality death certificate data available in Mexico to estimate the effect of the Progresa on elderly mortality for those aged 65 and older. Similar to Barham (2011), we use the percent of households receiving Progresa transfers in a given year and municipality as the treatment variable and compare municipalities phased-in during 1998 to those in 1999 since they are the most similar to each other. Using a municipality-level dataset covering the period 1992-2002, and a municipality and time fixed-effects model, we show that Progresa led to a 4 percent reduction in the average, municipality-level elderly mortality rate; the effects are similar for men and women. Results by cause of death reveal that Progresa was successful at reducing deaths related to infectious diseases, diabetes, and nutrition and anemia, but not those related to transportation accidents, as would be expected given the program interventions. Given that the empirical model uses the second lag of the treatment variable, the results focus on the effect of Progresa through 2000, when the program mainly operated in poorer rural areas. We do not examine the effect of the program expansion into more urban areas after 2000 in this paper since the effect of Progresa on elderly health during this time period is confounded by the introduction of health reform in 2003. The health reform included an increase in public spending on health and a new public health insurance scheme, *Seguro Popular*, which benefited a similar population to that served by the CCT program (Frenk et al. 2006).

Previous research on the effects of CCTs on adult health and especially those 65 and older is limited, but the research that does exist focuses on Progresa and demonstrates that Progresa had short-run effects on possible pathways or health outcomes that may have led to a reduction in elderly mortality. For example, beneficiaries aged 50 or older experienced a 60 percent increase in health visit in the previous two months, a reduction in self-reported sick days, an increase in the number of days one was able to carry out normal activities, an increase in the proportion reporting that they could carry out vigorous activities, and a reduction in the proportion reporting high blood pressure (Gertler and Boyce, 2001; Behrman and Parker 2011). Fernald, Hou, Gertler (2008) also show the program resulted in a lower prevalence of uncontrolled hypertension among people aged 30-65 years old.[1,2]

The rest of the paper proceeds as follows. Section 2 summarizes the Progresa program and the pathways through which program interventions may affect elderly mortality; section 3 describes the data; section 4 lays out the identification and estimation strategy; the findings are discussed in section 5; and section 6 concludes.

[1] Fernald, Hou, Gertler (2008) found significant reductions in the body mass index and prevalence of obesity due to the program, but results are not significant once covariates are included.

[2] Some of these analyses used short-term experimental evidence on the rural Mexican population from the randomized evaluation data, while others used matching methods that compared Progresa beneficiaries to people who were never eligible for the program in rural and urban areas.

2. The Progresa program

2.1 Background

Progresa was introduced in 1997, and between 1997 and 2000 gave preference to rural localities since poverty was highest in these areas, though a limited number of urban localites were included. The program was expanded starting in 2001 to include more urban areas. The program was originally designed to alleviate short-term poverty and to break the intergenerational transmission of poverty by improving the health and development of children. A secondary objective of the program was to improve adult health (Fernald, Hou, Gertler, 2008). Progresa combines two traditional methods of poverty alleviation: cash transfers and free provision of health and education services. Conditioning receipt of the cash transfers on children attending school, all family members obtaining regular preventative health care, and at least one family member attending health education training sessions, links the two methods. Therefore, the income transfer not only relaxes the household budget constraint, but also helps improve utilization of health and education services. There are separate transfers provided for the health and education conditionalities, so receiving the health transfer does not depend on the family also meeting the education conditionalities. The conditionalities were provided to a designated woman in the beneficiary household. Together, these conditionalities led to an increase in average beneficiary income levels of 22 percent in rural areas (Parker and Teruel, 2005).

2.2 The Progresa health and nutrition component

The health component of Progresa was designed to address health issues of all members of the family. The conditionalities required all adults, including senior citizens, to have one preventative health check-up a year, and at least one family member to attend regular health education training sessions (Adato, Coady, Ruel, 2000). Nutritional supplements were also given to mothers and young children. Transfers based on the health conditionality were paid every two months and were only paid if all the members of the beneficiary households attended the required health care visits and health education training sessions for that two-month period. Health clinics were required to provide a minimum package of services to ensure a basic quality of care. This package did not cover all types of health issues faced by families or the elderly, but did include: family planning; education on basic sanitation, and accident prevention; prenatal, childbirth and puerperal care; growth monitoring; vaccinations; anti-parasite treatment, and prevention and treatment of diarrhea, respiratory infections, tuberculosis, high blood pressure, and diabetes (Adato, Coady, Ruel, 2000). The health package was expanded with the introduction of health reform in 2003 to cover among other health issues, some cancers, cardiovascular problems, and dialysis (Knaul, Arreola-Ornelas, Mendez 2005). However, this came after the time period covered in this study.

Since it was expected that health care utilization would rise as a result of the program, Progresa coordinated with other government ministries to increase the health care supply to ensure that the quality of health care in program areas did not deteriorate. The improvement in supply included the use of mobile clinics and foot doctors to reach marginalized communities that did not have access to permanent health clinics. Despite improved access to health care, many health services were still out of the reach for the poor. This is because the health care system available to the poor during this time period relied heavily on out-of-pocket spending and

these costs were often beyond the reach of the poor, and coverage of service was not comprehensive as described above in the basic package of services (Knaul et al. 2006).

2.3 Program targeting, phase-in, and take-up

For logistical and financial reasons, Progresa rolled out over time across localities (Skoufias, Davis, Vega 1999). Progresa used a two-stage process to identify eligible households. In 1997, localities were selected based on a marginality index,[3] access to primary and secondary schools as well as to a permanent health care clinic.[4] The program used population density data and information on the proximity of localities to each other to determine the geographic isolation of the locality. Any locality with less than 50 inhabitants, or that was determined to be geographically isolated was excluded from the program. In 1998, the condition that the locality had to have access to a permanent health care clinic was relaxed since mobile clinics and foot doctors were available. And, some localities that were eligible but previously excluded due to geographic isolation were incorporated into the program.[5]

Once localities were selected, beneficiary households in each community were identified based on their poverty status as determined by household income and characteristics collected in a census of the program localities. Only those classified as poor became eligible for benefits. Recertification of eligibility was supposed to take place after three years of participation (Coady, 2000). As a result of this process, the program covers a different percent of the population in each locality. Households were informed they were eligible using door-to-door methods. And, research in areas where the randomized intervention took place, report that take-up of the program was remarkably high at 97 percent (Gertler and Boyce, 2001).

Figure 1 shows the program expanded over time across localities.[6] The program started in a limited number of localities in 1997, with the first transfers being provided between September and October of 1997. In 1998, the program was greatly expanded, reaching around 35,000 localities in all but two states, and over 95% of these localities were rural localities. Not all localities were incorporated in the same month in 1998, rather the expansion was spread throughout the year. For this reason, some beneficiaries received transfers starting in February 1998, while others had to wait until September or October of 1998. There was another expansion of localities and beneficiaries in 1999. The next major expansion took place in 2001 and 2002 with the incorporation of more urban localities.

2.4 Program mechanisms to reduce elderly mortality

The Progresa program could affect elderly mortality rates through a number of mechanisms. The health conditionalities (annual preventive visit and health education sessions) could have improved health since many of the services in the basic health services package addressed

[3] The marginality index included the following variables: the literacy rate; percent of dwellings with running water, drainage, and electricity; average occupants per room; percent of dwellings with a dirt floor; and percent of labor force working in the agriculture sector.

[4] A locality was considered to have access to a health care clinic if the clinic was either in the locality or in a neighboring locality at most 15 kilometers away (Skoufias, Davis, Vega 1999).

[5] See Skoufias, Davis, Vega (1999) and Coady (2000) for more details on program targeting.

[6] These numbers are based on Progresa administrative data provided to the first author by the central Oportunidades administrative office in 2004 and represent localities that had at least one household registered for the program by December of each year.

common health problems of the elderly in Mexico, including some risk factors for cardiovascular diseases, hypertension and obesity (WHO, 2011). In particular, the package included services for hypertension, diabetes, and respiratory infections, and recipients received more regular monitoring of their health. In addition, the health education trainings covered health issues relevant to the elderly, including nutrition (eating well for good health), accident prevention, and chronic disease detection and prevention (Adato, Coady, Ruel, 2000). While not specifically documented, the health education training sessions or doctors at the preventive check-ups may have provided specific information on important health behaviors for reducing chronic diseases like cardiovascular diseases, such as the importance of exercise, diet, and controlling tobacco and alcohol use (WHO, 2011).

The cash transfer may also have improved elderly health by enabling beneficiary households to buy more nutritious foods and spend more money on health inputs. However, it is possible that the income transfer may have exacerbated problems of overweight and obesity and increased the risk of death if households increased consumption of non-nutritious foods or calories. This possible negative effect may have been tempered by the health education training sessions, since healthy eating behaviors and encouragement of families to spend part of the cash transfer on nutritious foods were promoted during these sessions (Adato, Coady, Ruel, 2000). Indeed, prior research shows that Progresa lead to a 6.4% increase in household caloric intake, but that the increase was greatest for higher quality foods such as vegetables, fruits, milk, eggs and meat (Hoddinott and Skoufias, 2004).

Finally, the schooling conditionality could also affect elderly health if children's required attendance at school provided the elderly with more time to engage in positive health behaviors or required them to do more exercise due to the need to take-up household chores usually done by the children. Overall, these are plausible pathways for the Progresa CCT to have impacted the mortality of Mexican elders.

3. The data

We construct a municipality-level data set covering the 1992-2002 time period using vital statistics information, Progresa administrative records, and census data.[7] We examine the effect of the program on the elderly mortality rate (EMR) for three age groups, those aged 65 and older (EMR 65+), those aged 65-74 (EMR 65-74) and those aged 75 and older (EMR 75+), and for both sexes together and well as separately.

The EMR is constructed at the municipality level by sex using vital statistics data on deaths and census data on population. The vital statistics data are from a nation-wide database containing information on every certified death in Mexico and were provided by the Mexican Ministry of Public Health starting in 1990. These data have municipality level identifiers and are not publicly available. The population data are from the 1990, 1995, 2000, and 2005 census and are public available from the Mexican Statistical Agency (INEGI) at the municipality level by age and sex. Linear interpolation was used to determine population values for the years the data are not available.[8]

[7] Localities in Mexico are grouped into municipalities. The 2000 census recorded that there were 199,391 localities in 2,445 municipalities in Mexico.

[8] Linear interpolation of the population data will not be an accurate estimation if there are sizable migration flows of the elderly that are correlated Progresa. Given the short duration of the analysis, and that migration of the elderly

Mexico has a long-standing vital registration system that has met the World Health Organization's (WHO) international standards since the 1950s and is ranked in the top 20 in the world for its quality and completeness (Mathers et al., 2005; Braine, 2006). Despite this high rating, quality of the cause of death data does vary depending on whether the death is certified by medical personnel versus a lay-person authorized by the Ministry of health, and under-reporting remains an issue (Herandez et al. 2011; Braine 2006). The percent of elderly deaths in rural areas certified by a medical practitioner is high but did rise from 87 percent in 1992 to 92 percent in 2002. To account for this improvement in cause of death reporting during the study period, we control for percent of deaths certified by a medical practitioner as a robustness check when examining the effects of the program by cause of death. If a cause of death is not determined, it is recorded as ill-defined. The percent of elderly deaths that are ill-defined has remained fairly stable over the study period at 6 percent, however we do examine the effect of the program on ill-defined cause of death in a similar manner to other causes of deaths. Finally, we discuss how changes in under-reporting may bias the results in section 5.4.

The intensity of treatment indicator, referred to as *program intensity* in the tables, is the ratio of the number of households receiving Progresa benefits to the total number of households in a municipality. This variable ranges from zero to one, where one indicates that Progesa covers all households in a locality.[9] The Progresa administrative data includes the number of households registered in a locality for the program in December of each year and is available since program start in 1997. These data are not publically available, but provided by the central Progresa administrative office. We use 1995 census population data to determine which Progresa localities are rural (have less than 2500 in habitants) and which are urban (have 2500 or more in habitants). This data is aggregated to the municipality level using municipality identifiers in the administrative data. Using INEGI census data on the number of households in a municipality for 1990, 1995, 2000, and 2005 the number of households for each year between 1992 and 2002 is linearly interpolated.[10] While the study covers the period 1992-2002, variation in program intensity is only used until 2000 since the estimation equation uses the second lag of program intensity.

The municipality characteristics used as controls in the robustness analyses are created from locality level census data from INEGI for the years 1990,[11] 1995, and 2000. These variables are aggregated to the municipality level for those localities that received Progresa benefits before 2001 to better approximate change in characteristics of Progresa areas of municipalities over

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is low, any measurement error should be minimized. In addition, Stecklov et al. (2005) find that for people under age 60, Progresa had no effect on domestic migration.

[9] There are a handful of observations for which the values of program intensity is slightly greater than 1. These are mostly municipalities in which all localities participated in the program and there may have been some measurement error due to the linear interpolations or due to locality boundaries being unclear. We top code these values at one, but the results are same if we set them to missing.

[10] If Progresa induced elderly individuals who were previously living on their own to move into a household with other family members, then the changes in number of households may not be accurately captured by the linear interpolations. Given that the proportion of elderly living on their own prior to the program in 1994 was 7 percent in Mexico (Palloni, 2002), and that Progresa had little effect on the living arrangements of those aged 70 and older (Rubalcava and Teruel, 2006), any measurement error will be small.

[11] The 1990 locality data was matched by name to the 1995 and 2000 data. Locality names and codes changed over time and these changes were incorporated when identified.

time. Again, linear interpolation is used to obtain values for the years the data are not available.[12] The variables created using these data are: municipality density (population per square kilometer); the percent of households with no piped water, with no electricity, with no wastewater disposal,[13] and with a dirt floor; the percent of population over 14 that are illiterate; the percent of population over 4 who speak an indigenous language; the percent of employed working in the primary sector; and, the number of occupants per household.[14]

Yearly health supply data are from the Ministry of Health and Instituto Mexicano del Seguro Social (IMSS)-Oportunidades for the years 1992-2002 and are not publically available. These data do not include information on the total supply of health care in Mexico, rather the data only cover the providers that care for Progresa beneficiaries. These data are at the locality level but aggregated to the municipality level and then converted to be per 1000 population using census population data. The data are used to control for the number of clinics, hospitals, mobile clinics, foot doctors, doctors and nurses in contact with patients, and medical residents in a municipality. To help control for the phase-in rule, we also use these data to determine the percent of Progresa localities in a municipality that have a permanent health facility (clinic or hospital) in a given year.

Using these data sources, we construct a municipality-level panel dataset for the years 1992-2002.[15] Municipality boundaries were redefined during this time period. In order to make a consistent panel of municipalities from 1992-2002, municipalities that were split in a particular year are amalgamated. This results in a balanced panel of 2,376 municipalities each year. The research design only uses data on municipalities that were incorporated into Progresa in 1998 and 1999, leaving a panel of 1,961 municipalities.

3.1 Summary Statistics

Figure 2 shows the trends in the EMR for the 65 plus age group from 1992 to 2002 for all of Mexico, as well as, separately for municipalities that were incorporated into Progresa prior to 2001 (Progresa municipalities) and those that were not (non-Progresa municipalities). The general trend in EMR is similar for Mexico as a whole and for those municipalities where Progresa was operating. In Progesa municipalities, the EMR was 49 deaths per 1000 population prior to Progresa in 1996, and was fairly stable in the five years prior to the program. The EMR declined between 1996 and 2000 to 44 deaths per 1000 in 2000. A similarly sized decline was not experienced in the non-Progresa municipalities between 1996 and 2000. These trends just show the raw data and do not mimic the research design, but demonstrate that there was a decline in the EMR over the period of the program in municipalities where Progresa operated.

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[12] Due to the linear interpolation, if there is a large non-linear change in the covariates that is correlated with the treatment variable, the results could be biased. Given that Progresa localities were determined prior to the program based on pre-program characteristics, this possibility is minimized.

[13] No wastewater disposal means that the house does not have a drainage system that removes wastewater via the public sewage network, a septic take, creek, river, or some other water body.

[14] The percent of households with a dirt floor and percent employed working in the primary sector are only available in the 1990 and 2000 Censuses.

[15] Mortality data is available starting in 1990, and we take advantage of these two extra years in the robustness check which uses lagged mortality. Results are robust to which pre-intervention years (1990-1996) are included in the analysis.

The EMR is lower in Progresa affect municipalities than for Mexico as a whole. However, not all localities within the municipality received Progresa, and the Progresa areas of a municipality were worse off compared to the municipality as a whole. As shown in Table 1, municipality characteristics generated for all localities in a municipality compared to characteristics for only the localities with Progresa beneficiaries, Progresa areas were worse off in that a greater percent of households had no water, no electricity and no wastewater disposal, and more of the population was illiterate and worked in the primary sector. So the EMR may be higher in Progresa areas of a municipality than for the municipality as a whole.

Table 2 presents mean municipality EMR for municipalities that are the focus of this study (municipalities incorporated into Progesa in 1998 and 1999). We present the data for the year prior to municipalities receiving Progresa in the sample, 1997, and for last year in the study, 2002. Panel A highlights that the EMR 65+, EMR 65-74, and EMR 75+ declined between 1997 and 2000, but the decline was larger for those aged 75 and older. Panel B shows the EMR by cause of death for those aged 65 and older for common causes of death and for causes the program was more likely to affect. While the EMR for those aged 65 and older declined over time for most causes of deaths, it increased over the 1997 to 2002 time period for deaths related to diabetes and transportation accidents.

4. Identification strategy

We estimate the impact of Progresa on elderly mortality at the municipality level, rather than the locality level – the level at which it was rolled out – due to data limitations. As a result of the unit of analyses being the municipality level, the treatment variable, *program intensity*, combines three sources of variation. One source comes from the variation in treatment status across municipalities over time. A second source is derived from the variation in the number of rural localities covered by the program over time within a municipality. And, a third source results from the percent of households benefiting from the program differing between localities.

4.1 Sources of variation

Municipalities were incorporated into the program over time starting in 1997. A municipality is defined as being incorporated into Progresa the year the first locality, in that municipality, was phased-in. Following Barham 2011, we compare municipalities phased-in during 1998 to those phased-in during 1999 since mortality rates between these sets of municipalities are more similar to each other than to municipalities phased-in either earlier, in 1997, or later. This is not surprising since, as described in Section 2.3, in 1998 the conditional that the locality had to have access to a permanent health care clinic was relaxed (so localities phased-in before 1998 had better health care access) and municipalities incorporated after 1999 tended to be more geographically isolated or urban.

To identify the program effect using this municipality variation, we use municipalities yet to be treated as comparison municipalities. The identifying assumption in this case is that changes in mortality observed after the program was introduced in municipalities incorporated in 1998 would be the same as in municipalities incorporated in 1999, if the municipalities incorporated in 1998 had not received the program. Although it is not possible to test this assumption, we test that the differences in pre-intervention means by year between municipalities that were incorporated in 1998 and 1999, controlling for municipality fixed-effects, are similar and present

the differences in Table 3. The differences in means are fairly small and not statistically significant between the two groups for the years 1992 to 1996 providing some confidence that the municipalities incorporated in 1998 are similar to those incorporated in 1999.

Another source of variation used to identify the program impact is the phasing-in of Progresa localities, within a municipality, over time. This source of variation arises because localities were incorporated into the program in different years within a municipality. Results may be biased if the trends in mortality in localities that were phased-in during different years within a municipality are not similar. Since mortality data is not available at the locality level, as a robustness check we lag the dependent variable 3 years to check that the treatment variable is not correlated to mortality trends prior to the program. In addition, in Table 4 we present evidence similar to Barham (2011) on pre-intervention trends in locality characteristics for municipalities incorporated in 1998 and 1999. The locality characteristics include literacy; access to services such as electricity, piped water, and wastewater disposal; household size; and, the ethnicity of the population. These variables are proxies for income and education, two important determinants of elderly mortality. The table displays the change in means between 1990 and 1995 for localities that were incorporated into the program in 1998 in the first row. The subsequent rows show how these changes differ between localities that were incorporated into the program in 1998 versus later years, and indicate if the changes are statistically different between the two groups. The differences in means are adjusted for municipality fixed effects to mimic the regression analysis.[16]

The changes in means are statistically different for many variables in Table 4. This is not surprising given the large sample size (approximately 50,000), however the magnitudes are arguably small. We examine two other measures of the size of the difference since statistical tests, such as t-tests, are affected by sample size. First, we examine normalized differences, difference in the changes between groups divided by the standard deviation for the sample, since they are not affected by sample size. As a rule of thumb normalized differences greater than 0.25 may lead to sensitive results (Imbens and Wooldridge, 2009). The normalized differences are less than a quarter and all but 3 less than 0.1. Second, the difference in the change in means is compared to the difference in quartile cut-off values (i.e. forth quartile cut-off minus third quartile cut-off, third quartile cut-off minus second quartile cut-off etc.) for each variable for 1990. The differences in the change in means are smaller than the non-zero differences between the quartiles of the distribution, indicating that they are not large enough to move a locality to a different part of the distribution. Lastly, these variables are included as controls in the regressions as a robustness check that any differences in time varying observables are not biasing the results.

Finally, there is variation in the treatment variable, *program intensity*, across localities. This variation is in part due to the poverty targeting by the program within the locality. The poverty targeting means that the percent of households covered by the program in a given locality may be endogenous. It could also be a result of differential program take-up between localities. The program take-up rate is high in areas that were part of the randomized evaluation, 97 percent, so selection into the program is likely to be the smaller issue. Regardless, to address these issues,

[16] The magnitude of the differences in *locality characteristics* between localities phased-in in 1999 and 1998 in Table 4, is similar to the magnitude of the differences in *municipality characteristics* between municipalities incorporated in 1999 and 1998 (results not reported).

we create a different treatment variable that does not depend on the number of beneficiary households, the percent of localities that have Progresa beneficiaries in a municipality, which we refer to as *locality intensity*. It is possible that locality intensity is correlated with poverty at the municipality level, however the inclusion of municipality fixed-effects will control for non-time varying differences in poverty between the municipalities.

Using these sources of variation the treatment variable, *program intensity*, ranges from zero to one, where one indicates that 100 percent of households in a locality were covered by Progresa. Given, the regression model uses the second lag of program intensity, the analysis only uses variation in this variable until 2000. As shown in Table 5, the average program intensity in 2000 for municipalities phased-in during 1998 and 1999, was 35 and 27 percent.

4.2 Empirical Model

The municipality-level average treatment effect is estimated using the following equation:

$$(2)\ EMR_{mt} = \alpha_t + \tau_m + \beta program\ intensity_{m,t-2} + \varepsilon_{mt}\ ,$$

where *EMR* is the elderly mortality rate in municipality *m* in time *t* and *program intensity* is the measure of the intensity of treatment (percent of households receiving Progresa transfers in a municipality).[17] The second lag of program intensity (or *lag 2 program intensity*) is included since some new beneficiaries started receiving benefits in the second half of the year, and it can take some time for health care visits to have an effect on health.[18] Year fixed effects, α_t, are included to control for general time trends common to all municipalities, and municipality fixed effects, τ_m, time-invariant municipal unobservables. The error term is clustered at the municipality level to account for likely intracluster and serial correlation, as well as, heteroskedasticity that is inherent in models with aggregated data.

The effect of Progresa is given by β and shows the effect of the program if the program intensity rose from zero to one (i.e. 100 percent of households in municipality are Progresa beneficiaries). To estimate the average, municipality-level program effect we multiply β by the average of the program intensity in 2000 (0.32). The estimate of the treatment effect will be unbiased if there are no unobserved time-varying municipality characteristics that are correlated with the treatment variable. The similarity of the mortality and locality characteristics trends for the quasi-experimental treatment and comparison groups prior to the program provide some confidence the unobservables are not biasing the results. However, to further test that time varying unobservables are not biasing the results, we include municipality controls and individual municipality time trends in the regression model as a robustness check.

[17] This specification assumes that the treatment effect is linear. Examining the treatment effect by 0.2 bins of program intensity shows this assumption holds.

[18] Specification tests on the number of lags of program intensity support using the second lag, however the results are similar to the first lag.

5. Results

5.1 Program impacts

Table 6 presents the effects of Progresa on the EMR for those age 65 and older. Column (1) presents the effect of the program controlling for year and municipality fixed effects only.[19] The point estimate on lagged program intensity is statistically significant at the 1 percent level and is 6.37 which means that a one percentage point increase in program intensity leads to a 0.0637 per 1000 individual decrease in EMR. At the municipality level, the percent of households covered by the program reached an average of 32 percent in 2000, so the average municipality program effect represents approximately a 4 percent decline in the EMR over a pre-program level of 47. Alternatively, a standard deviation increase in program intensity led to a reduction in the EMR of 1.47 deaths per 1000, which is a 3 percent decline over the pre-program EMR. Column (2) and (3) show the program effect for males and females are similar at 6.42 and 6.46 respectively.

5.2 Robustness checks

In this section, we explore a number of threats to the validity of the estimates. Given the similarity in results between males and females, we do not present results by male and females in this section. The results by sex are consistent with those for both sexes together.

First, as discussed in section 4.1, to test that pre-existing trends are not related to the treatment variable variation, the dependent variable is lagged three years. The coefficient on the treatment variable in column (4) is -0.98.[20] It is small and not significantly different from zero, suggesting that pre-existing trends are not biasing the results.

Second, section 4.1 also highlighted that the percent of households covered by the program in a given locality could be endogenous. To address this potential bias a new treatment variable is created which is not based on the number of households in a municipality: the percent of localities that have Progresa beneficiaries in a municipality, *locality intensity*. The point estimate on the treatment variable in column (5), -4.91, shows a slightly smaller reduction in deaths due to the program (3 percent), but again is not statistically different from column 1.

Third, we test if the point estimates are sensitive to the inclusion of time varying municipality characteristics in column (6) by controlling for the phase-in rule (population density and the percent of Progresa localities in a municipality with access to a permanent health facility in a given year), and variables that are likely correlated with poverty and mortality from the census (see section 3 for a list of these variable). These do not include the health supply controls which are likely endogenous (see section 5.3). It is possible that these controls are also endogenous if Progresa led to changes in these variables, so we include the controls only as a robustness check. To better approximate changes that took place in Progresa areas, instead of including a municipality controls that reflects characteristics of all the localities in the municipality, we include municipality controls that only reflect characteristics for localities that received Progresa benefits before 2001. The point estimate on lagged program intensity is -6.13. The similarity in

[19] To the extent that non-eligibles (non-poor in a locality) benefit from the improved health supply or the health education program, the program effects may be over-estimated. Bobonis and Finan (2002) find no evidence of health spillover effects on the non-eligibles in Progresa localities using the Progresa randomized evaluation database.

[20] The expansion of health supply prior to the program could have affected pre-program trends.

results with and without observable time varying characteristics provides some confidence that differences in the time varying observables are not biasing the results.[21]

Fourth, to further test that unobservable time varying characteristics are not biasing the results, individual municipality time trends are included in column (7). The program effect is slightly smaller at -5.14 but not statistically different from column (1), and shows a program effect of 3.5 percent.

Finally, in general localities phased-in during 2000 were likely to be more geographically isolated and worse off than those phased in earlier. While, the research strategy does not include municipalities incorporated in 2000, there are some localities that were phased-in during 2000 in the municipalities in the sample. The percent of localities phased-in during 2000 is small at less than 1 percent of localities in municipalities phased-in during 1998 and 1999. However, in order to exclude these localities from the analysis, in column (8) we restrict the sample to exclude 2002. This excludes the use of variation in program intensity from 2000 because we use the second lag of program intensity in the regression analysis. Again, the point estimate on the treatment variable excluding 2002 is similar to column (1) at -6.95.

5.3 Changes in health supply

It is possible that the program effects are partly a result of the expansion of health care in rural communities that preceded Progresa. Supply was increased to ensure the quality of health care did not deteriorate with the increase in health care utilization resulting from the program (Bautista-Arredondo et al, 2006), and that beneficiaries could meet the health conditionalities. Health care was expanded in both treatment and comparison municipalities (i.e. municipalities incorporated in 1998 and 1999), but if the timing of expansion varied, it is difficult to determine if the reductions in mortality are due solely to the demand-side incentives. Health supply variables are likely to be endogenous but are included in Table 6 column (9) to examine if the increase in health supply is a mechanism through which Progresa reduced mortality. The point estimates on the second lag of program intensity for the model including health supply controls, -6.59, is similar to the point estimate for the model without these controls in column (1), and are not statistically different providing some evidence that changes in health care supply is not the mechanisms driving the results.[22]

5.4 Biases from under-reporting of vital statistics data

Vital statistic data on deaths usually suffers from under-reporting. To the extent that under-reporting does not change over time within a municipality, it is controlled for by the municipality fixed-effects. Under-reporting of deaths in Mexico occur if the time or money costs associated with reporting the death to a health official are too high (Braine, 2006). Progresa may actually lead to an increase in reporting of deaths among beneficiary families because all members of the family must attend a regular preventive care visit or the family will not receive the cash transfer for the health component. So, it is in the financial interest of the family to report a death to the health official. As a result, it is possible that in areas where there are more Progresa beneficiaries

[21] If we use municipality controls that represent the whole municipality rather than just Progresa localities within the municipality the results are again similar (-6.39).

[22] Results are similar when health supply variables are lagged one or two years.

there will be more deaths reported than before the program began, likely causing an under-estimate of the program effect.

5.5 Impact of the program disaggregated by age

Results for those aged 65-74 and age 75 plus are in Table 7. The point estimate on lagged program intensity for the 65-74 year olds shows a smaller program effect for the 75 plus group, at -4.13 compared to -5.44. However, because the mortality rate is higher for the older age group, the average municipality program effect is higher for the 65-74 year olds than the 75 plus group at 5 percent compared to 3 percent. The program effect was also higher for the younger age group for males (6 compared to 2.5 percent) and females (5 compared to 4 percent).

5.6 Impact of the program on selected causes of deaths

To better understand the types of illnesses Progresa was more effective at addressing, Table 8 presents the program impact on the EMR for those aged 65 and older by cause of death. We include 5 major cause of death categories that Progresa may have affected based on services provided in the minimum package of health services and other program interventions (infectious diseases; heart disease, stroke and hypertension – core components of cardiovascular disease; diabetes; respiratory infections; and nutrition and anemia). We also examine the effect of the program on two causes of death less likely to be affected by Progresa. One that is health related, cancer, and one that is non-health related, deaths due to transportation accidents. Finally, we examine the effect of the program when the cause of death was not determined (ill-defined cause of death), and include a category "all other causes", which includes deaths due to any cause that was not specifically reported in the table.

Results are presented for both sexes together as well as separately. We discuss the results for both sexes together unless the results differ by sex. There is a statistically significant reduction (at the 1 percent level) of deaths per 1000 of 1.70 due to infectious diseases (or 22 percent), 1.03 due to diabetes (or 12 percent), and 1.66 due to nutrition and anemia (or 15 percent). There is little effect of the program on heart disease, stroke and hypertension. This is somewhat surprising given that the basic health package focused on two important risk factors, hypertension and diabetes. However, services to address hypertension as well as prevention of cardiovascular diseases, such as life style changes and statin therapy, was inadequate during this time period (Mexican Ministry of Health 2006, Gomez-Perez et al. 2009).

As expected, the point estimates for death due to cancer and transportation accidents are small and insignificant at, -0.04 and -0.01. To the extent that the unobservables driving the trends for these two causes of death are similar for the other causes of death, it provides some further evidence that trends in unobservables are not biasing the results.

The program did reduce the number of deaths that were ill-defined. While the percent of deaths that were ill-defined was fairly stable over the time period for the sample as a whole, it is still difficult to determine if the decline in ill-defined deaths reflects a real reduction in deaths from ill-defined causes, or if there were less deaths recorded as ill-defined because the quality of determining the cause of death improved with the program. However, both interpretations of the reduction in ill-defined deaths could potentially lead to an under-estimate of the program effect for the other causes of death. For example, if deaths due to diabetes are better diagnosed as a

result of the program, deaths that were recorded as ill-defined prior to the program, but actually due to diabetes, may be recorded as due to diabetes after the program was introduced.

As discussed in section 3, the quality of the cause of death information varies depending on whether the cause of death was certified by a medical practitioner or a lay-person authorized by the Ministry of Health. We control for the percent of deaths in the municipality certified by a medical practitioner each year since this percent rose over time. The results are similar for most of the cause of death except ill-defined deaths. The point estimates on lagged program intensity are slightly smaller and no longer statistically significant for males (results not reported).

5.7 Heterogeneity of the treatment effect by pre-intervention levels of the mortality rate

It may be is easier to reduce the mortality rate when it starts at a higher level, so we examine if the impact of the program varies depending on the pre-program level of the elderly mortality rate. To answer this question we interact the treatment variable by an indicator variable: if the municipality had an elderly mortality rate in 1995 that was above the sample median for 1995. The findings in Table 9 indicate that the program led to a -3.44 decline in the EMR for those aged 65 and older in program areas that had a lower mortality prior to the program. But, the decline for municipalities that had higher EMR to start, was larger by 7.42 deaths, and the difference is statistically significant.[23] Given a mean death rate of 53 per 1000 live births in the years prior to the program and an average program intensity of 30% in 2000 in municipalities that had higher mortality rates to start, the average program effect is slightly higher than for the sample as a whole, at 6 percent. Results by sex are again similar.

5.8 Heterogeneity of the treatment effect by pre-intervention characteristics

To provide some insight into what types of municipalities the program may have had greater success, we examine if the program had heterogeneous effects for available determinants of poverty and mortality. To create the pre-intervention characteristics, locality data from the 1995 census are aggregated to the municipality level for all localities incorporated into Progresa by 2000.[24] Terciles of each characteristic, X_{mt}^{95}, are created so that higher terciles reflect a worse off state, and the third tercile is interacted with the treatment variable to compare those in the worse off tercile to the two better terciles. To take into account the correlations among the variables, we include all variables in one regression, but results are similar if the heterogeneous effects are examined for each variable in a separate regression.

The results in Table 10 highlight that the program effect was not statistically different for most observable characteristics. However, the program effect was larger in areas that prior to the program had less households with some kind of a wastewater disposal system and electricity (though the latter is only significant at the 10 percent level). Results disaggregated by sex have a similar pattern to both groups together, however the result on electricity is not significant for either sex.

[23] The relative ranking of the death rate at the municipality level does not necessarily correlate with the relative ranking of the death rates in localities in which Progresa operated in those municipalities.

[24] Data on the percent of households with a dirt floor and percent of the rural population working in the primary sector is from 1990 since 1995 data are not available.

6. Conclusions

This paper investigates the effect of the Mexican conditional cash transfer program, Progresa, on the mortality rate for those aged 65 and older. Taking advantage of the rollout of the program at the national level between 1997 and 2000, we show the program led to a 4 percent reduction in the average municipality-level mortality rate. Results were similar for males and females. Results by cause of death showed the program reduced deaths due communicable diseases (e.g. infectious diseases) and nutrition and anemia, as well as, due to one non-communicable disease, diabetes. This is important, given that death due to diabetes is now in the top 10 causes of death in many middle income countries and is one of the leading causes of death in Mexico (WHO, 2012, World Life Expectancy 2012). It is surprising the program was not effective at reducing deaths due to typical cardiovascular diseases such as heart disease, stroke and hypertension. Cardiovascular diseases are the leading cause of death in Mexico. However, quality of health care and high out-of-pocket costs to address these diseases may have been an impediment.

As may be expected, the decline in the elderly mortality rate was larger, 6 percent, in municipalities whose average pre-program mortality rates were above the sample median. However, Progresa also led to significant, though smaller, declines in mortality in those municipalities whose pre-program mortality rate was below the sample median, indicating that even municipalities that may have been better off to start benefited from the program.

The fact that the program was able to address both communicable and non-communicable disease, as well as issues of nutrition, is important from a development perspective. Many middle- and low-income countries are in the process of the epidemiological transition and face the double burden of addressing both communicable and non-communicable diseases. Developing countries also tend to have few programs in place to help the elderly, and these results suggest that modest increases in household income along with regular preventive check-ups and health education, as provided by Progresa, can lead to significant mortality declines. Furthermore, for countries that have a CCT program, but the conditionalities only focus on children, the results highlight that requiring elderly family members to also have regular health care visits may be an effective way to improve elderly health.

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Tables and Figures

Figure 1
Trends in the Number of Progresa Beneficiary Families and Localities

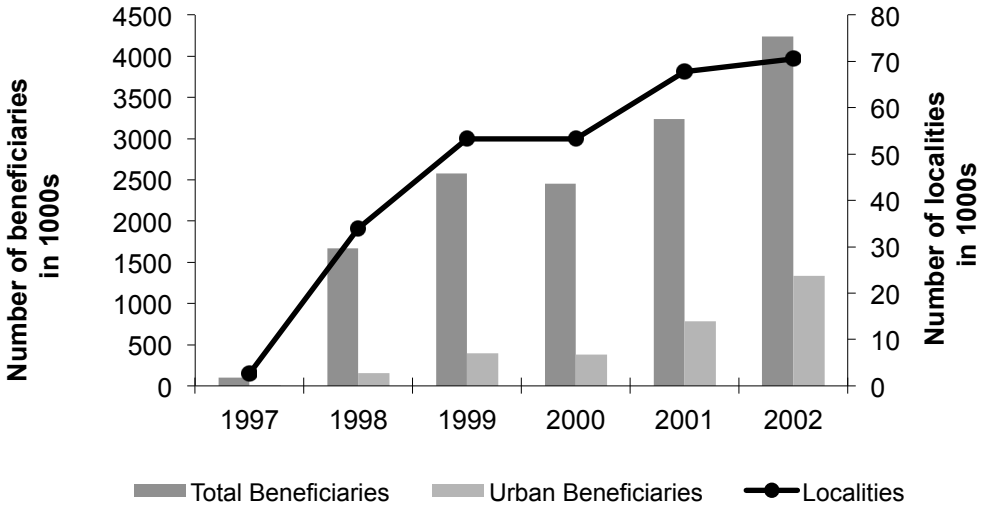
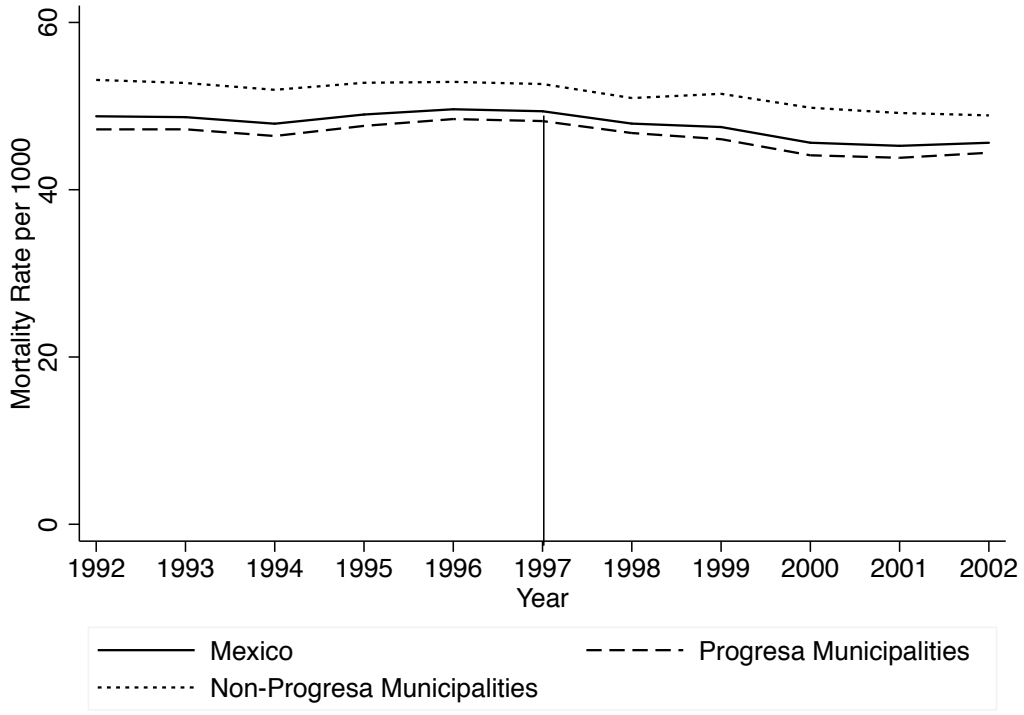


Figure 2
Trends in EMR 65+



Note: Progresa municipalities include municipalities that had at least one Progresa locality between 1997 and 2001. Non-Progresa municipalities are all other municipalities. Analysis for this paper only includes municipalities incorporated in 1998 and 1999. The EMR 65+ is the mortality rate for people aged 65 per 1000.

Table 1
Pre-Program Municipality Characteristics

	All Localities		Progresa Localities	
	Mean	SD	Mean	SD
Percent of the households with:				
No water	28.50	(23.53)	42.60	(28.58)
No electricity	13.68	(15.40)	19.64	(20.14)
No wastewater disposal	56.86	(29.82)	78.85	(21.08)
Dirtfloor	38.77	(25.30)	49.50	(23.94)
Percent of population:				
> 4 that speaks an indigenous language	18.37	(28.13)	19.51	(29.28)
> 14 that are illiterate	21.67	(14.29)	25.35	(13.79)
Percent of employed working in primary sector	51.49	(22.87)	68.02	(17.60)
Average number of occupants in a household	5.71	8.30	5.89	8.29

Notes: N= 1961. These data are for 1995 and are for the municipalities used in the analysis (municipalities incorporated into Progresa in 1998 and 1999). The "Progesa Localities" column means that the data are aggregated to the municipality level only for localities that eventually received Progresa by 2000.

Table 2
Trends in Elderly Mortality Rate (EMR)

	1997		2002	
	Mean	SD	Mean	SD
Panel A: All cause mortality				
EMR 65+	47.5	(15.34)	42.5	(13.52)
EMR 65+, males	49.3	(21.95)	44.4	(17.63)
EMR 65+, females	46.0	(19.66)	40.7	(17.11)
EMR 65-74	25.0	(13.57)	23.2	(12.45)
EMR 75+	54.2	(20.98)	49.2	(20.17)
Panel B: EMR 65+ for selected causes of death				
Infectious diseases	1.6	(2.92)	1.2	(2.24)
Hearth, stroke and hypertension	14.4	(8.51)	13.0	(7.81)
Diabetes	3.0	(3.05)	4.1	(3.57)
Respiratory	5.9	(4.76)	4.8	(4.14)
Nutrition and anemia	3.6	(5.64)	2.9	(4.80)
Cancer	5.1	(4.19)	5.1	(3.87)
Transportation accidents	0.2	(0.73)	0.3	(0.75)
Ill-defined	2.9	(6.92)	2.9	(5.78)
All other causes	10.9	(6.86)	8.2	(5.23)

Notes: N= 1961. These data are only for the municipalities used in the analysis EMR 65+ and EMR 75+ refer to the mortality rate for people aged 65 and older and 75 and older respectively. Data from 1997 is for the year before the municipalities in the sample received Progresa, and 2002 is the last year of data in the sample.

Table 3
Difference in Mean EMR 65+ Between
Municipalities Incorporated into Progresa in 1998 and 1999, by Year

Year	Difference (Incorporated 1999 – Incorporated 1998)
1992	-1.10
	(0.89)
1993	0.33
	(0.83)
1994	-0.35
	(0.87)
1995	-0.85
	(0.83)
1996	0.31
	(0.78)

Notes: These results are the differences in means controlling for municipality fixed effects. Standard errors are in parentheses and are clustered at the municipality level. **, * and + denote significance at the 1%, 5% and 10% level respectively. EMR 65+ is the elderly mortality rate for those aged 65 and older.

Table 4
Difference in Changes in Pre-Intervention Locality Characteristics,
by Year Locality was Incorporated into Progresa

	Percent of population who are		Average number of occupants in a household	Percent of households with		
	Indigenous speakers	illiterate		No piped water	No wastewater disposal	No electricity
Change in mean for 1998 localities	0.1	-3.5	-0.1	-13.5	-5.3	-21.6
	(0.0)	(0.1)	(0.0)	(0.2)	(0.2)	(0.2)
Difference in change in means between localities incorporated in later years and those incorporated in 1998						
1999 localities - 1998 localities	-0.1	0.4**	-0.1**	-1.1*	-5.0**	6.0**
	(0.1)	(0.1)	(0.0)	(0.5)	(0.4)	(0.5)
2000 localities - 1998 localities	0.0	0.4	-0.0	-1.2	-4.8*	2.3
	(0.5)	(0.5)	(0.1)	(2.4)	(1.9)	(2.2)
2001 localities - 1998 localities	0.1	0.4*	-0.1**	3.0**	-2.6**	7.7**
	(0.1)	(0.2)	(0.0)	(0.6)	(0.4)	(0.6)
N	50,284	50,280	50,299	50,299	50,299	50,299

Notes: Standard errors are in parentheses. **, * and + denote significance at the 1%, 5% and 10% level respectively. The change in means is the difference in means between 1995 and 1990. Means are adjusted for municipality fixed effects. Data on the indigenous language speakers and the illiterate population are for the population over age four and fourteen respectively.

Table 5
Trends in Program Intensity
by Year Municipality Incorporated was into Progresa

	Municipalities Incorporated in 1998		Municipalities Incorporated in 1999	
	Mean	SD	Mean	SD
1998	0.28	(0.23)		
1999	0.38	(0.23)	0.27	(0.24)
2000	0.35	(0.22)	0.26	(0.23)

Notes: Program intensity is the percent of households within a municipality that receive Progresa benefits.

Table 6
Impact of Progresa on EMR 65+

	EMR 65+			EMR 65+ All					
	All	Male	Female	Lag 3					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2nd lag of program intensity	-6.37**	-6.42**	-6.46**	-0.98		-6.13**	-5.14**	-6.95**	-6.59**
	(1.04)	(1.42)	(1.31)	(1.07)		(1.07)	(1.51)	(1.20)	(1.06)
2nd lag of locality intensity					-4.91**				
					(0.88)				
N	21,571	21,571	21,571	19,610	21,571	21,571	12,571	19,610	12,571
R^2	0.36	0.24	0.27	0.37	0.35	0.36	0.44	0.37	0.36
Mean dependent variable	47	48	46	47	47	47	47	47	47
Municipality characteristics	N	N	N	N	N	Y	Y	Y	N
Municipality time trend	N	N	N	N	N	N	Y	N	N
Year 2002 excluded	N	N	N	N	N	N	N	Y	N
Health Supply	N	N	N	N	N	N	N	N	Y

Notes: All regressions include municipality and time fixed effects. Standard errors are in parentheses and are clustered at the municipality level. **, * and + denote significance at the 1%, 5% and 10% level respectively. The municipality characteristics include controls for population density, percent of Progresa localities with access to a permanent health facility, and municipality controls presented in Table 1 for Progresa localities. Health supply controls are per 1000 population and include: health clinics, hospitals, mobile health clinics, health brigades, doctors, residents, and nurses.

Table 7
Impact of Progresa on Elderly Mortality Rate, by Age

	EMR 65-74			EMR 75+		
	All	Male	Female	All	Male	Female
2^{nd} lag of program intensity	-4.13**	-5.11**	-3.45**	-5.44**	-4.20*	-6.73**
	(0.90)	(1.25)	(1.19)	(1.45)	(2.03)	(1.88)
R^2	0.28	0.21	0.21	0.36	0.25	0.27
Mean dependent variable	25	27	23	54	53	55

Notes: N=21,571. Following column 1 in Table 6, all regressions include municipality and time fixed effects. Standard errors are in parentheses and are clustered at the municipality level. **, * and + denote significance at the 1%, 5% and 10% level respectively.

Table 8
Impact of Progresa by Cause of Death, EMR 65+

	All	Male	Female	All	Male	Female
	Infectious Disease			Cancer		
2nd lag of program intensity	-1.70**	-1.27**	-2.11**	-0.04	-0.46	0.32
	(0.23)	(0.31)	(0.28)	(0.25)	(0.35)	(0.34)
R^2	0.25	0.18	0.19	0.26	0.22	0.15
Mean of dependent variable	2.5	2.6	2.4	4.8	5.1	4.5
	Heart Disease, Stroke, and Hypertension			Transportation Accidents		
2nd lag of program intensity	-0.31	-0.76	0.12	-0.01	-0.02	-0.00
	(0.58)	(0.74)	(0.76)	(0.06)	(0.10)	(0.06)
R^2	0.31	0.22	0.23	0.14	0.12	0.10
Mean of dependent variable	13.9	13.6	14.4	0.2	0.3	0.1
	Diabetes			Ill–Defined Cause of Death		
2nd lag of program intensity	-1.03**	-0.98**	-1.10**	-1.34**	-0.86+	-1.80**
	(0.18)	(0.26)	(0.28)	(0.48)	(0.52)	(0.61)
R^2	0.39	0.27	0.29	0.41	0.31	0.34
Mean of dependent variable	2.6	2.1	3.1	3.1	2.8	3.5
	Respiratory			All Other Causes		
2nd lag of program intensity	-0.28	-0.23	-0.34	-0.01	-0.52	0.49
	(0.34)	(0.45)	(0.43)	(0.41)	(0.66)	(0.47)
R^2	0.25	0.20	0.19	0.22	0.19	0.17
Mean of dependent variable	5.5	5.9	5.1	10.7	12.5	9.1
	Nutrition and Anemia					
2nd lag of program intensity	-1.66**	-1.32*	-2.05**			
	(0.46)	(0.53)	(0.55)			
R^2	0.29	0.20	0.24			
Mean of dependent variable	3.5	3.2	3.8			

Notes: N=21,571. Following column 1 in Table 6, all regressions include municipality and time fixed effects. Standard errors are in parentheses and are clustered at the municipality level. **, * and + denote significance at the 1%, 5% and 10% level respectively.

Table 9
Impact of Progresa by Pre-Program Levels, EMR 65+

	All	Male	Female
2^{nd} lag of program intensity	-3.44*	-3.69*	-3.17+
	(1.37)	(1.88)	(1.86)
2^{nd} lag of program intensity * high pre-program EMR	-7.42**	-6.91*	-8.25**
	(2.01)	(2.80)	(2.59)
R^2	0.38	0.26	0.29
Mean of dependent variable low pre-program EMR	41	42	39
Mean of dependent variable high pre-program EMR	53	54	52

Notes: N=12,571. Following column 1 in Table 6, all regressions include municipality and time fixed effects. Standard errors are in parentheses and are clustered at the municipality level. **, * and + denote significance at the 1%, 5% and 10% level respectively. High pre-program EMR are municipalities for which EMR 65+ was greater than the median EMR 65+ for the sample in 1995.

Table 10
Heterogeneity of the Impact by Pre-Intervention Characteristics, EMR 65+

	Tercile	All	Male	Female
2nd lag of program intensity	1/2	-4.09*	-5.59*	-2.67
		(1.84)	(2.54)	(2.34)
Interaction with above median indicator of pre-program municipality characteristics				
Percent of households in Progresa localities with:				
No piped water	3	1.12	2.10	0.44
		(1.64)	(2.21)	(2.02)
No electricity	3	-2.97+	-3.48	-2.62
		(1.67)	(2.25)	(2.11)
No wastewater disposal	3	-5.35**	-6.37**	-4.27*
		(1.58)	(2.05)	(2.04)
Dirt floor	3	0.00	0.36	-0.45
		(1.70)	(2.34)	(2.31)
Percent in Progresa localities of:				
Population >4 that speak an indigenous language	3	-0.98	1.28	-3.53
		(1.65)	(2.39)	(2.32)
Population >14 that are illiterate	3	2.54	4.76+	0.83
		(1.72)	(2.43)	(2.24)
Employed working in primary sector	3	-0.33	-1.40	0.56
		(1.57)	(2.15)	(2.02)
Average number of occupants in rural households	3	1.69	0.45	2.67
		(1.46)	(1.96)	(1.85)
R^2		0.36	0.24	0.27

Notes: N=21,571. The regression includes municipality and time fixed effects, and municipality controls. Standard errors are in parentheses and are clustered at the municipality level. **, * and + denote significance at the 1%, 5% and 10% level respectively. Preprogram data is from 1995 for all variables except dirtfloor and the percent of population that is employed in the primary sector.