UNIVERSIDAD CARLOS III DE MADRID

working papers

Reassessing the Differential Impact of Grandmothers and Grandfathers: The Old Age Program in Nepal

Yunrong Li and Ricardo Mora[1]

Abstract

We study the effects on infant mortality of the introduction in 1995 of a non-contributory universal pension scheme in Nepal known as the Old age Allowance Program. We use cross-sectional data from the 1996 and 2001 Nepal Demographic and Health Surveys. Following a standard diff-in-diffs approach, we find positive and significant effects on survival rates for the presence in the same household of a female beneficiary while negative and sometimes significant effects for the presence of a male beneficiary. When we conduct pre-treatment common trend tests, we find that we cannot reject it for the case of the female beneficiaries but we strongly reject it for the case of male beneficiaries. Following Mora and Reggio (2012), we then propose a more flexible model and identification strategy and find that there are no differences in the female and the male beneficiary effects. We interpret these results as suggestive that cross-sectional analysis may bias downwards the estimates of the effect of grandfathers because of gender differences in endogenous household formation.

Keywords: Infant mortality, difference-in-differences, gender differences.

---

1   Introduction

In their recent review of the anthropological literature, Sear and Mace (2008) report that many studies find correlational evidence that maternal grandmothers tend to improve child survival rates (in around 70% of the studies they review) while paternal grandmothers show somewhat more variation in their effects on child survival. What about grandfathers? They find that the statistical association between grandfather presence and child survival is much weaker: in 10 of 12 cases, the presence of a maternal grandfather had no significant effect on child survival rates while paternal grandfathers had no effect in 6 of 12 studies.

The evidence in economic studies of a gender differential in the impact of grandparents on children's health is, to our knowledge, limited to Duflo (2000, 2003). These two studies explore the effects on children's health of the expansion of the Old Age Pension program in South Africa. They find that pensions received by women had a positive impact on the health and nutritional status of children living in the same household. When the beneficiary of the pension is a man, however, no health effects are found.

A methodological concern for the causal interpretation of these results, which is shared with those from the anthropological studies, is that conditional on a household's having three generations, the presence of an elderly grandparent may be a sign of a relatively healthy household. Duflo (2003) takes advantage of the fact that the height for age of young children depends on accumulated investments over the life of the child. Hence, if households with eligibles have worse characteristics than non–eligible households, older children would be smaller in eligible households. The identifying strategy then is to compare the difference in height between children in eligible and those in non–eligible households among children exposed to the program for a fraction of their lives to the same difference among children exposed all their lives.

In this paper, we study the effects on child mortality of the introduction in 1995 of a non-contributory universal pension scheme in Nepal known as the Old age Allowance Program, OAP. Under the OAP all Nepalese with age 75 and above were eligible to a universal flat rate pension of 100 Rupees per month, around 2 dollars and 12% of the country's income per capita. We use cross-sectional data from the 1996 and 2001 Nepal Demographic and Health Surveys (NDHS).

We first follow a standard diff-in-diffs approach to estimate the effects on infant mortality of an exogenous increase in the income of an old female and an old male living in the same household. Our benchmark identification strategy consists of comparing the average changes in survival rates before and after the implementation of the OAP of children living in three-generation households with at most one male and one female beneficiaries of the OAP with four alternative population controls. Using this approach, we find positive and significant effects on survival rates for the presence of a female beneficiary of the OAP while negative and sometimes significant effects for the presence of a male beneficiary. These results are qualitatively similar across alternative definitions of the control group. We also obtain similar results when we restrict the sample for both boys and girls. Finally, the results are robust to changing the method employed to exploit retrospective information in our data, to whether the female (male) beneficiary is the only beneficiary in the household, and to the family status of the beneficiary.

We then conduct pre-treatment common trend tests to justify the validity of the Parallel Paths assumption in the benchmark diff-in-diffs approach and find that we cannot reject it for the case of the female beneficiaries but we strongly reject it for the case of male beneficiaries. This is consistent with a situation where endogenous composition of households together with economic progress create a downward sloping trend in the unobservable household quality on households with a male beneficiary. As this negative trend would be absent in households with only a female beneficiary of the OAP, the standard diff-in-diffs estimates would be appropriate to estimate the effect of the female beneficiary but would be inadequate to estimate the effects of the presence of a male beneficiary.

Following Mora and Reggio (2012), we propose a more flexible model and then conduct a test of pre-treatment common accelerations that would provide justification for a Parallel Growths assumption (i.e., assuming that without treatment, the change in growth for the treated would have equaled that of the controls). We cannot reject the presence of pre-treatment common accelerations for the male eligible effect and we strongly reject it for the female beneficiary effect. Hence, we implement a flexible identification strategy based on the Parallel Growths assumption for the male beneficiary effect and on the Parallel Paths assumption for the female beneficiary effect. The positive effects of the female beneficiary effect remain similar to those obtained using the benchmark diff-in-diffs approach. In contrast, the estimates of the male beneficiary effect become positive

and strongly significant. Thus, with a more flexible approach that standard diff-in-diffs estimation we do not find significant gender differences in the income effects on infants survival probabilities. We argue that our results can be interpreted as suggestive that under economic growth and gender differences in the presence of a beneficiary in the household, cross-sectional analysis may bias downwards the estimates of the effect of grandfathers.

The rest of the paper is structured as follows. We first describe the institutional setting in Section 2 and then present the data and the estimation strategy in Section 3. In Section 4 we report and discuss the results of the paper. Section 7 concludes.

2 Policy background

In the last decades, Nepal has steadily ranked as one of the least developed countries in the world. In 1995, the year the OAP program was introduced, GDP per capita was 200 US dollars in real terms, ranking Nepal as the 211th country in the world. Living conditions for children were also among the worst in the world. The infant mortality rate in Nepal at the time was 7.6%, higher than the average among Asian countries (5.4%). Malnutrition incidence among children under 5 years old was 64.5% using height for age as criterion and 44.1% using weight for age, when the average for other Asian developing countries was 42.9% and 28.8%, respectively.

The OAP scheme is initially announced on December 1994 as part of a five-year economic plan. All Nepalese citizens with age 75 and above become eligible to a universal flat rate pension of 100 rupees per month, i.e. around 2 dollars or 12% of the country's real GDP per capita. There were five (out of 75) pilot districts in which the program officially started in January 1995, although the actual payments were delayed until July. During the following Nepalese fiscal year (from 16th July 1995 to 15th July 1996), the OAP is extended to the entire country.[1]

---

[1]The Nepalese government introduces for the fiscal year 1996-1997 two additional social programs that could affect the economic conditions of the elderly. One of these two programs, the Helpless Widows Allowance, is only targeted at old widows who get neither any care from family members nor a widow pension. As we study only the income effects on children physically living with the elderly, in our data there are no individuals who can benefit from this allowance. The second social program introduced at the time, a Disabled Pension of 100 Rupees to adult disabled citizens, affected a very small proportion of the adult population. See, for example, Rajan (2003) for a more detailed description of these new

In the fiscal year 1999-2000, the government updates the OAP from 100 to 150 rupees per month (or 11% of real GDP per capita) presumably to accommodate the pensioners' accumulated loss in purchasing power due to the large increases in nominal GDP. There were two additional rate updates since 1999. In 2005 the OAP increases from 150 to 200 rupees and in 2008 from 200 to 500 rupees, or 34% of real GDP per capita. In addition, the age threshold is reduced from 75 to 70 years old in 2008.

There are no direct measures of the actual coverage of the program. Looking at the early stages in the implementation of the program, Rajan (2003) reports that some legitimate beneficiaries may have initially found difficulties to prove both their citizenship and date of birth. Although the number of OAP recipients is relatively stable since its inception until 2001 (between $170,000$ and $175,000$), then it abruptly increases by 10%. Based on census information, Rajan (2003) estimates the coverage of OAP to be ranging in that year from 83% to 86% in 2001. Hence, if the observed increase in the number of recipients in 2001 only reflects coverage improvements, then average coverage during the first years of the implementation of the OAP may have ranged from 75% to 78%, possibly with lower coverage in poor isolated areas, where ignorance about the program was presumably larger.

3   Data and estimation strategy

3.1   The Nepal Demographic and Health Surveys

The data come from the 1996 and 2001 Nepal Demographic and Health Surveys (NDHS).[2] Each survey is divided into two questionnaires. The household questionnaire provides demographic characteristics for every member of the household—such as current age, sex, education, and relation with the Household Head—and basic information on the characteristics of the household—such as its regional location and whether it is located in a rural or urban area. The individual or woman questionnaire is targeted at women of age between 15 and 49. In addition to their birth history, demographic information—like current age, education, major occupational category, and ethnic status—for the mother

---

policies.

[2]Both surveys are part of the worldwide Demographic and Health Survey (DHS) project. Additional information on the 2001 NDHS may be obtained from the Family Health Division, Department of Health Services, Ministry of Health, Nepal. Additional information about the DHS project may be obtained from ORC Macro (web site: http://www.measuredhs.com).

(and her husband if present) are included.

An important feature of the NDHS is that, for all interviewed women between 15 and 49, it contains birth information—such as the birth date, sex, birth order, and whether the child has a twin—on all their children, regardless of whether the children are alive or dead at the time of the interview. For those children who are dead, the dataset also contains their death date. Therefore, it is possible to reconstruct monthly survival histories for all the children born from the interviewed women. It is also possible to reconstruct some retrospective information of the children when they were infants: the number of siblings they had and the age of their mothers. If the father and the grandparents live in the household at the time of the interview, it is also possible to obtain their age at the time the child was an infant.

The data have several shortcomings. First, the NDHS data does not provide information on whether old people in the survey collected the benefits. We can identify eligible individuals in the household since eligibility is only based on age, but we cannot be sure that those eligible did collect the OAP pension. Moreover, since kin relations for each member of the household can only be reconstructed via his/her relation with the household head, we also cannot be sure that those eligible are the grand-parents of the infant. Hence, our results pertain only to the effect of eligibility status.

Second, apart form birth and death dates, the data set does not contain retrospective information. This is a potential problem for those variables whose value at the time of the interview may differ from the value at the time the child was under one year old. An important example refers to the presence of grandparents in the household at the time of interview, since this presence does not imply presence at the birth of the child.[3] Hence, when we attempt to capture the effect of the presence at birth of a grandparent on infant survival by controlling for the presence at the time of interview, we potentially incur in a measurement error that is likely larger the larger is the time span between the birth of the child and the time of the interview. One simple way to limit this measurement error is by restricting the estimation sample to births close enough to the interview date so that we expect that presence at the time of interview very likely implies presence at the time of birth. In the results section we present alternative restrictions of the estimation sample to discuss the sensitivity of our results to alternative assumptions on

---

[3]Other examples include variables that may change with time, such as the presence of the father, his occupational status, and the parent's educational highest achievements.

retrospective information.

Third, living (at the time of interview) infants whose mothers either do not live in the household (perhaps because they died before the interview) or are not eligible for the woman's questionnaire, cannot be included in the analysis. Although we do have their survival history, we do not have information for the corresponding population of dead children, i.e. dead infants whose mothers either do not live in the household at the time of interview or are not eligible for the woman's questionnaire. Hence, we do not include living children whose mothers do not live in the household because including them could potentially create sample selection bias.

Finally, there is no information on induced abortions. Before 2002, abortion was prohibited in Nepal and physicians could not recommend or perform it. Women seeking abortion did so clandestinely, frequently put their lives at risk, and suffered sometimes serious health or legal consequences (Thapa, 2004, Thapa and Padhye, 2001). Although there is no accurate direct information about the prevalence of abortion in Nepal at the time the OAP was implemented, the information available suggests that abortion was not a generalized method of birth control. Cross-country comparisons do show that pregnancy loss at the time was not high in Nepal (Casterline, 1989). Moreover, according to the 1996 and 2001 NDHS surveys, only around 18% of women in reproductive age reported to have had a pregnancy that terminated in a miscarriage, abortion, or still birth. Focusing on abortion-related hospital admissions, several studies show that in the last two decades of the 20th century only between 10 to 20 percent of these admissions were induced abortions (Thapa and Padhye, 2001, and the references therein). Thus, although it is impossible to know with precision the incidence of induced abortion, the available figures suggest that it has not been a generalized method of fertility control.[4]

3.2   The estimation sample

We combine two Surveys—the 1996 and the 2001 surveys—to create our data set. For the period before the government started implementation of the OAP, i.e. the pre-treatment

---

[4]The law changed in 2004, effectively liberalizing abortion on several general grounds. Importantly, the new bill recognized the right to terminate a pregnancy of up to 12 weeks voluntarily. Presumably, this may have made the use of abortion as a fertility control mechanism more general. Our results, however, cannot be driven by any changes in fertility control triggered by the change in the law, as we do not use data after the law changed,

period, we use the 1996 survey. In our benchmark estimation sample, we include all children born between July 1991 and June 1994 for the pre-treatment period. We do not include kids born between July 1994 and June 1995 because in the pre-treatment period we want children not affected by the policy before they are one year old. We also restrict the sample to kids born at most five years before the time of the interview to limit potential measurement error regarding retrospective information. Again for this reason, we do not use observations from the 2001 survey in the pre-treatment period.

For the period after the government started implementation of the OAP—i.e. the post-treatment period—we use the 2001 survey. We include all children born between July 1995 and June 1998. We do not include observations from children born after June 1998 because, as explained in Section 2, the government updated the amount of the OAP starting in July 1999. We do not include children born between July 1995 and June 1996 from the 1996 survey because we do not know whether they survived their first year of life.

In sum, in our benchmark estimation sample we use the survival histories of all kids from the 1996 survey born between July 1991 and June 1994 for the pre-treatment period and all kids from the 2001 survey born between July 1995 and June 1998 for the post-treatment period. One nice feature of this sample is that it covers births along a span of three years in the two periods. However, the minimum gap between the birth date and the time of the interview is 1.5 years for the pre-treatment period but 2.5 years for the post-treatment period. We will come back to this issue when we review the robustness of our results to the use of alternative samples.

3.3   A difference-in-differences strategy

Consider the case of how the presence in the household of an OAP eligible individual may affect an infant's survival status one year after birth.[5] Variable survival status after one year, S, is equal to 1 if the infant still lives one year after birth and is equal to 0 otherwise. Let S^0 denote survival status in the hypothetical case that the government does not introduce the OAP and let S^1 denote survival status in case the government

---

[5]In the empirical application, we restrict the sample to households with at most one male eligible and one female eligible and allow for gender differences in the effects on the child survival status. For notational simplicity, we present in this section the model assuming at most only one eligible individual.

introduces the OAP. Additionally, let $D = 1$ if the infant lives in a household with an eligible individual and $D = 0$ otherwise. We refer to infants for whom $D = 1$ as the treated and infants for whom $D = 0$ as the controls. Potential and observed survival statuses are related by $S = S^1 D + S^0 (1 - D)$.

We follow a standard latent-variable specification for both S^1 and S^0:

$$S^v \equiv \begin{cases} 1 & \text{if } S^{*v} \geq 0 \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

where S^{*1} is survival score in case the government implements the OAP and S^{*0} is survival score in case the government does not implement the OAP. Both survival scores S^{*1} and S^{*0} are unobservable latent-variables which, together with D, drive survival status S. Note that, given equation (1), S also follows a latent variable specification:

$$S \equiv \begin{cases} 1 & \text{if } S^* \geq 0 \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

where $S^* = S^{*1} D + S^{*0} (1 - D)$. We define the average effect on survival score for the treated as the expected change in survival score among those treated after implementation of the OAP:

$$\phi \equiv E\left[S^{*1} - S^{*0} | D = 1\right]. \tag{3}$$

The estimation of expectation $E\left[S^{*1} | D = 1\right]$ is not difficult because it equals $E\left[S^* | D = 1\right]$.[6] What makes identification of ϕ difficult is the identification of the average survival score for the treated in the absence of policy, $E\left[S^{*0} | D = 1\right]$.

Assume we have information on survival status S before and after the start of the OAP both for infants living with and without an eligible individual. Define ΔS^{*v} as the change in survival score S^{*v} when any given infant goes from being born before the implementation of the OAP to being born after the implementation of the OAP. The parallel path assumption in this context states that, conditional on a vector of individual characteristics x, the average change in survival score in the absence of treatment is the same for treated and controls:

$$E\left[\Delta S^{*0} | D = 1, x\right] = E\left[\Delta S^{*0} | D = 0, x\right]. \tag{4}$$

---

[6] One can, for example, assume $S^* \sim N\left(\beta_0, \sigma^2\right)$ and estimate $E\left[S^* | D = 1\right]$ by ML.

As a result of technological progress, survival scores improve with time. In Nepal, these improvements may have been overshadowed by the negative effects of the civil unrest that started in 1996 and ended in 2006. Moreover, development failure might have been the root of the civil conflict (Sharma, 2006). Equation (4) specifies that, in case the government never implements the OAP, the average changes in survival scores are similar for the population of infants who live with an OAP eligible and the population of infants who do not live with an OAP eligible.

Average survival score levels may still differ across the treated and the controls. This would likely be the case when there is endogenous formation of households. For example, if households with OAP eligibles are statistically associated with worse economic conditions, then average survival scores will tend to be lower for the treated than for the controls. Thus, the parallel-path assumption allows for group-specific, time-invariant, unobservable heterogeneity which may arise from endogenous formation of households.

Assuming equation (4) immediately leads to a difference-in-differences moment condition for ϕ based on changes in survival score S^*:

$$\phi = E\left[\Delta S^*|D=1, x\right] - E\left[\Delta S^*|D=0, x\right]. \tag{5}$$

We base our benchmark identification strategy on equation (5). Assuming linearity in $E\left[S^*|D, x\right]$, condition (5) leads to a linear standard diff-in-diffs specification for survival score:

$$S^* = \beta_0 + \beta x + \gamma_D D + \gamma_P Post + \phi D \times Post + \epsilon \tag{6}$$

where $E\left[\epsilon|D, x\right] = 0$ and $Post$ is a dummy variable for birth after the implementation of the OAP. Under the assumption of normality for error term ϵ we have a standard probit specification for the conditional expectation of observed survival status S:

$$Pr\left(S=1|D, x\right) = \Phi\left(\beta_0 + \beta x + \gamma_D D + \gamma_P Post + \phi D \times Post\right). \tag{7}$$

Consistent estimation by ML estimation of parameter ϕ in equation (7) gives a consistent estimate of the causal effect of the policy implementation on the survival score for the treated. To asses how this increase in the survival score affects survival probabilities, we focus our results on the average marginal effect of a score increase of size ϕ :

$$\alpha \equiv E\left[\Phi\left(\beta_0 + \beta x + \gamma_D D + \gamma_P Post + \phi\right) - \Phi\left(\beta_0 + \beta x + \gamma_D D + \gamma_P Post\right)\right] \tag{8}$$

3.4 Alternative control groups

To ensure a simple and tractable definition of treatment, we do not include in our analysis those infants who live with more than one eligible woman or with more than one eligible man. We only consider two types of treatments. An infant receives the first type of treatment if there is an eligible woman–i.e. a woman who is older than 75 at the time the infant is born–in the same household. An infant receives the second type of treatment if there is an eligible man.[7] Accordingly, there are three types of treated infants: those who live with an eligible woman, those who live with an eligible man, and those who live both with an eligible woman and an eligible man. As shown in Table 1, almost 100 infants both before and after treatment are treated.

Table 1: *Number of controls and treated (all observations)*

Control	Pre-treatment	Post-treatment	All
Treated infants	99	98	197
Control 1 infants	3424	4068	7492
Control 2 infants	598	680	1278
Control 3 infants	1965	2341	4306
Control 4 infants	487	546	1033

Note: Control 1 infants are infants who do not live with either an eligible woman or an eligible man. Control 2 infants are infants who live with people who were between 60 and 74 at the infant's birth date. Controls 3 infants are infants who do not live with people older than 60 in households where the household head is not older than 40 years of age. Control 4 infants are infants who live with non-eligible old people who were between 60 and 69 at the infant's birth date.

Although controls and treated may differ in levels in a typical diff-in-diffs setup, the usual parallel-path assumption still imposes group homogeneity in pre-treatment dynamics, so it is reasonable to look for controls that are as similar as possible to the treated. We consider four alternative control groups. The first control group—that we refer to as *control 1 infants*—includes all infants who do not live with either an eligible woman or an eligible man. Control 1 infants are a very large group because it includes households with old people (defining old people as those older than 60 at the infant's birth date) and households without old people.

An alternative control group—that we refer to as *control 2 infants*—would restrict the comparison to infants who live with old people who are still non-eligible, i.e. infants who live with people who were between 60 and 74 at the infant's birth date. The number of control 2 infants is around 600 both before and after treatment. Given that the age

---

[7]With complete retrospective information, we could separate those infants who have lived since birth with OAP eligibles from those who have only lived with OAP eligibles during a fraction of their first year. However, we can only look at children who live with OAP eligibles at the time of the interview.

eligibility limit, 74, was fixed by the government without considering how old people may help infants, control 2 infants are an interesting control group. One potential problem for control 2 infants, however, is that, with the introduction of the OAP program, old people who will soon become eligible may choose to increase their contributions to the household even before they become eligible because their permanent income raises with the announcement of the program. Hence, infants living in households where non-eligibles will soon become eligibles may not be convincing controls as they might actually receive benefits similar to those received by the treated.

One way to avoid the problem that arises with permanent income increases among old non-eligibles is to consider as controls those infants who do not live with people older than 60. To make this control group as homogeneous as possible, we additionally impose that the household head is not older than 40 years of age. Hence, most control 3 infants are newborns living in two-generation households.[8] Clearly, among the control 3 infants the introduction of the OAP program does not lead to an increase in the household available resources. However, in the presence of endogeneity in the member composition of the households, it can be argued that control 3 infants can be less appealing as a control group than control 2 infants. For example, suppose that young couples attempt to live in their own houses as soon as they reach a minimum income. As economic conditions improve nationwide, economic conditions in households with grandparents will worsen relative to households without grandparents and this trend differential will make the standard parallel-paths assumption inappropriate. Finally, grandparents who do not live in the household may still live close enough to have an influence in the welfare of the infant, so that control 3 infants we may have infants that could be considered to be under a weak version of treatment.

A less radical way to avoid the permanent income increase problem is to consider as control group only infants who live with non-eligible old people younger than 69. We refer to them as *control 4 infants*. Control 4 infants are a subset of control 2 infants, the sample size being, unsurprisingly, the smallest among the four control definitions (487 before treatment and 546 after treatment). Admittedly, there could still be an increase in the household resources driven by the elderly expectations to receive the benefits in the future. We believe, however, that this effect should be smaller than the effect for control

---

[8]Note that the sum of the number of control 2 and 3 infants is actually smaller than the number of control 1 infants because in the latter group there are also infants who live only with their non-eligible grand-parents.

2 infants for two complementary reasons. First, for the elderly living in households of control 4 infants, the minimum time interval before any benefits are obtained is five years. Second, life expectancy at 60 for the 1995 to 2000 period was around 16 years for women and 15 years for men.[9] Hence, a large proportion of non-eligible elderly between 60 and 69 do not survive to become eligible.

4 Diff-in-diffs results

4.1 The basic diff-in-diffs estimates

In Table 2 we report the estimated average marginal effects as defined by equation (8) under the four alternative control groups. We report the p-values for the significance tests of the estimates in parenthesis. We allow for different effects for male and female eligible and present two specifications. The unconditional specification allows for month of birth and region fixed effects.[10] In addition to the fixed effects included in the unconditional specification, the conditional specification includes two dummy variables for the education of the mother (for primary, for secondary, and for higher education), the mother's age at the child's birth and its square, a dummy for whether the child is female, the number of kids younger than five—at birth of infant—in the household, and two dummies for the ethnicity of the mother.[11]

We model survival status for four alternative time intervals: survival after 3, 6, 9, and 12 months. Consider, first, survival status after 3 months. Using control 1 infants as

---

[9]Figures obtained from the Gender Info database from the United Nations Statistical Division.

[10]In the original DHS surveys, two geographical variables are included: a binary variable that distinguishes between rural an urban areas and a dichotomous variable that distinguishes between mountain, hill, or plain terrain. We create regional dummy variables obtained from the interaction of these two geographical variables.

[11]To ensure comparability between the surveys, we constructed an ethnicity variable that considers 6 ethnic groups. The ancestors of the *brahmin/chhetri* come from India. The *Newar* and the *Janajati*— who include many of Nepal's indigenous nationalities, such as the *Gurung*, the *Magar*, the *Tamang*, the *Tharu*, and the *Rai*— are sometimes referred to as old Nepalese groups. The *Muslin* are a minority in Nepal, comprising about 4% of the total population. The *Dalit*, sometimes referred to as "untouchables", are the lowest caste in the Hindu caste system. Finally, all the other ethnic groups, who represent around 10% of the population, are classified together. In the specifications, we report the results after controlling for a binary variable for Dalit, and a dummy variable for others. Using all the other dummies for ethnic categories does not change significantly the results and none of the other ethnic variables is significant in any of the specifications (results are available upon request).

Table 2: *Average Marginal Effects. Basic difference-in-differences results*

| | 3 months | | 6 months | | 9 months | | 12 months | |
	Uncond.	*Cond.*	*Uncond.*	*Cond.*	*Uncond.*	*Cond.*	*Uncond.*	*Cond.*
Control 1	1	2	3	4	5	6	7	8
Female eligible	0.036	0.029	0.049	0.040	0.053	0.042	0.048	0.039
	(0.042)	(0.074)	(0.000)	(0.001)	(0.001)	(0.003)	(0.033)	(0.068)
Male eligible	-0.008	-0.015	-0.023	-0.036	-0.048	-0.064	-0.049	-0.091
	(0.873)	(0.777)	(0.700)	(0.572)	(0.502)	(0.413)	(0.489)	(0.307)
No. of obs.	7689	7424	7689	7424	7689	7424	7689	7424
Control 2								
Female eligible	0.056	0.042	0.068	0.049	0.067	0.049	0.072	0.053
	(0.011)	(0.129)	(0.000)	(0.030)	(0.000)	(0.034)	(0.001)	(0.031)
Male eligible	0.003	-0.028	-0.019	-0.078	-0.046	-0.112	-0.033	-0.126
	(0.965)	(0.743)	(0.795)	(0.459)	(0.577)	(0.330)	(0.666)	(0.289)
No. of obs.	1106	1015	1171	1079	1286	1183	1286	1206
Control 3								
Female eligible	0.033	0.030	0.046	0.041	0.048	0.042	0.042	0.038
	(0.114)	(0.085)	(0.002)	(0.001)	(0.003)	(0.003)	(0.107)	(0.094)
Male eligible	-0.016	-0.017	-0.032	-0.038	-0.058	-0.066	-0.062	-0.100
	(0.778)	(0.760)	(0.621)	(0.569)	(0.447)	(0.411)	(0.417)	(0.291)
No. of obs.	4369	4260	4443	4333	4503	4392	4503	4392
Control 4								
Female eligible	0.065	0.050	0.075	0.054	0.074	0.051	0.079	0.057
	(0.002)	(0.041)	(0.000)	(0.014)	(0.000)	(0.029)	(0.000)	(0.019)
Male eligible	0.000	-0.027	-0.029	-0.090	-0.052	-0.122	-0.038	-0.143
	(0.994)	(0.761)	(0.723)	(0.436)	(0.565)	(0.326)	(0.647)	(0.273)
No. of obs.	849	790	913	853	983	915	1003	933

Note: Average marginal effects as defined in equation (8). *p*-values are shown in parenthesis. See Table 1 for the definition of the control groups. *Uncond.* refers to the diff-in-diff model with month of birth and region fixed effects. The *Cond.* model additionally includes dummy variables for the education of the mother, the mother's age at the child's birth and its square, a dummy for whether the child is female, the number of kids younger than five—at birth of infant—in the household, and dummies for the ethnicity of the mother.

controls, we find that the grandmother effect is positive and significant. Inclusion of additional control variables does not change the results fundamentally, but reduces both the significance and the size of the effects (see columns 1 and 2). In contrast to what we observe for the eligible female, the eligible male effect is not significant. The sign of the effect is, nevertheless, negative and larger after controlling for additional covariates.

Estimates for the determinants of survival rates after 6 months follow a similar pattern. Interestingly, the female eligible effect increases around 5 percentage points after 6 months in the unconditional model (first row in column 3) and 4 percentage points in the conditional model (first row columns 4). The point estimate for the male eligible effect is still negative and not significant. Similar results are obtained when looking at the survival determinants after 9 and 12 months. These results suggest that the positive effect on survival rates take place in the first 6 months after birth and only in the presence of a female eligible.

We can study the robustness of these results to alternative definitions of the control group. When we include as controls only infants who live in households where there are no old people—i.e. control 3 infants—the results are very similar to those using control 1 infants, arguably the result of the large demographic weight of these households. When we include as controls only those infants who live in households where there are old non-eligible people, the estimates of the female eligible effect become larger and are estimated more accurately. The negative effect of the male eligible effect, however, remains insignificant.

The largest point estimates of the female eligible effect are obtained when we use as controls the control 4 infants. Survival rates after 3 months improve around 6.5 percentage points according to the unconditional model and around 5 percentage points according to the conditional model. This effect increases after 6 months up to 7.5 percentage points and then it stabilizes to between 7.4 and 7.9 in the first year of the newborn. According to the conditional model results, these effects are somewhat smaller although still important: around 5.7 percentage points after a year.

Different effects for male and female infants

In Table 3 we report estimates of the female and male eligible effects for subsamples of only boys and only girls. For brevity, we only report the marginal effects using control 1 and 4 infants using the full set of additional covariates.[12]

The female eligible effect on boys is positive and generally significant—the only exception being the effect after 12 months using control 1 infants. Using control 4 infants, the estimate of the marginal effect on boys survival rates more than doubles. For example, after 12 months, survival rates improve by 4.1 percentage points using control 1 infants but they improve by 9.2 percentage points using as controls the more credible control 4 infants.

For girls, we also find a positive effect when there is a female eligible in the household. Using control 1 infants, the effect is smaller than for boys, and it is significant at the 10% level only after 6 months. However, with control 4 infants the estimated marginal effects for girls are—in spite of the smaller sample—strongly significant. Interestingly,

---

[12]Results using other controls and the unconditional model are similar. They are available upon request.

Table 3: *Average Marginal Effects. Different effects for male and female infants*

	Control 1 infants				Control 4 infants			
	3 months	6 months	9 months	12 months	3 months	6 months	9 months	12 months
Only boys								
Female eligible	0.044	0.049	0.052	0.041	0.111	0.100	0.103	0.092
	(0.000)	(0.000)	(0.001)	(0.211)	(0.000)	(0.030)	(0.012)	(0.078)
Male eligible	-0.006	-0.058	-0.100	-0.150	-0.069	-0.290	-0.300	-0.329
	(0.913)	(0.527)	(0.372)	(0.252)	(0.705)	(0.247)	(0.214)	(0.159)
No. of obs.	3779	3779	3824	3824	315	336	364	390
Only girls								
Female eligible	0.018	0.036	0.035	0.038	0.084	0.080	0.080	0.146
	(0.624)	(0.039)	(0.123)	(0.128)	(0.024)	(0.030)	(0.018)	(0.000)
Male eligible	-0.055	-0.015	-0.007	-0.017	0.042	-0.275	-0.331	-0.382
	(0.135)	(0.598)	(0.771)	(0.572)	(0.482)	(0.442)	(0.311)	(0.203)
No. of obs.	3135	3257	3345	3423	152	171	184	202

Note: Average marginal effects as defined in equation (8) for subsamples of only boys and only girls. *p*-values are shown in parenthesis. See Table 1 for the definition of the control groups. Controls are those in model *Cond.* defined in Table 2 and include month of birth and region fixed effects, dummy variables for the education of the mother, the mother's age at the child's birth and its square, a dummy for whether the child is female, the number of kids younger than five—at birth of infant—in the household, and dummies for the ethnicity of the mother.

they become very close to those for boys (8 vs. 10 percentage points) after 3, 6, and 9 months and become larger after one year (14.6 vs. 9.2 percentage points). The male eligible effect is almost always negative. Perhaps more importantly, it is never significant, regardless of using the boys or the girls subsamples and both for control 1 and control 4 infants.[13]

The findings are broadly in line with results reported elsewhere. In particular, the asymmetry between the male and female eligible effects replicate the basic results found in Duflo (2000, 2003). We do find that conditioning the sample by infant's gender does not alter neither the importance of the presence of a female eligible, nor the apparent absence of any effect in the presence of a male eligible. In contrast, in Duflo (2000) and Duflo (2003) the female eligible effect is significant only for girls. We observe a larger estimated effect for girls one year after birth.

We claim in Section 6 that this asymmetric results are driven by assuming Parallel Paths both for female–eligible and for male–eligible treatments. Before we need to rule out two alternative potential explanations. First, the absence of effects in the case of male eligibles in our sample could result from males strategically exploiting gender role differences in society. In that case, the male eligible effect would not to be negative when

---

[13]In the unconditional model, not reported in Table 3, the negative effects of the male eligible effect for girls are around −0.075 and significant at the 5% significance level when control 1 infants are used as controls.

he is the only eligible individual in the household. Second, the estimates presented so far rely on the assumption that the presence at the time of interview of an eligible person coincides with her or his presence at the time of the infant's birth. This is a strong assumption and could lead to spurious asymmetric results if female and male eligible individuals are not equally likely to remain in three-generation households.

Specialization

It could be argued that the absence of effects in the case of male eligibles masks a type of specialization pattern within the household. In the presence of a female eligible individual, the male eligible would expect the female to be the only contributor to the additional resources for the newborn. However, in the absence of differentials in gender preferences, if the male eligible is the only eligible individual in the household we would expect the male eligible effect to be of a similar magnitude to the female eligible effect.

We present in Table 4 separate estimates using as treated three alternative subgroups. In the first specification, we use only those households in which the male eligible is the only eligible individual in the household. In the second specification, we use only those households in which the female eligible is the only eligible individual in the household. If the effect estimated in the previous specifications is just an artifact from gender specialization, then we would expect that the effects for the only–one–male eligible sample would be similar to the estimates reported until now for the female eligibles.

Due to the small treated sample, we encounter sample identification problems. As a consequence, we cannot report estimates for survival status after 3 months when the male eligible is alone. In those cases where the sample identification condition is met, the effects are usually negative for the male eligible sample and usually positive for the female eligible sample. However, due to the very small samples for the treated, the effects are never accurately estimated and we cannot reach any clear conclusion using this testing strategy.[14]

Alternatively, we look at the estimates of the effects when neither the male eligible nor the female eligible are reported as the household head. Nepal is a society where old

---

[14]Using a linear probability model we avoid the identification problems that we have with the probit model. For the only–one–male eligible sample, we find that the effects are negative and significant while in the only–one–female eligible sample the results are positive and significant. Results are available upon request.

Table 4: *Gender specialization in the provision of household resources*

	Control 1 infants				Control 4 infants			
	3 months	6 months	9 months	12 months	3 months	6 months	9 months	12 months
Male eligible alone								
Male eligible		0.007	-0.036	-0.090		-0.022	-0.135	-0.203
		(0.913)	(0.721)	(0.512)		(0.870)	(0.529)	(0.403)
No. of obs.	7261	7290	7290	7290	585	666	720	748
No. of Treated	29	29	29	29	29	29	29	29
Female eligible alone								
Female eligible	0.016	0.024	0.023	0.002	0.004	0.009	-0.013	-0.024
	(0.601)	(0.391)	(0.508)	(0.965)	(0.960)	(0.900)	(0.892)	(0.816)
No. of obs.	7291	7291	7291	7291	642	711	768	785
No. of Treated	28	28	28	28	28	28	28	28
Neither is HH								
Female eligible	0.019	0.034	0.034	0.025	0.030	0.041	0.033	0.032
	(0.471)	(0.069)	(0.128)	(0.461)	(0.576)	(0.279)	(0.483)	(0.556)
Male eligible		-0.009	-0.006	-0.056		-0.012	0.000	-0.056
		(0.910)	(0.940)	(0.622)		(0.916)	(0.997)	(0.714)
No. of obs.	7321	7344	7344	7344	659	749	807	825
No. of Treated	57	57	57	57	57	57	57	57

Note: Average marginal effects as defined in equation (8) using only those households in which the male eligible is the only eligible individual in the household, using only those households in which the female eligible is the only eligible individual in the household, and using only those households when neither the male eligible nor the female eligible are reported as the household head. p-values are shown in parenthesis. See Table 1 for the definition of the control groups. Controls are those in model *Cond.* defined in Table 2. Absence of marginal effect estimates signals that a perfect prediction problem impedes the sample identification of the effect.

people are frequently regarded as the most respected members of the family, even if they do not have a predominant economic position within the household. It is not rare to observe either a male or female eligible individual to be chosen as the household head, and we hypothesize that in many cases household head status is only a sign of respect to the elderly. We also assume that those elderly who are not chosen as household heads do not command a dominant economic position within the household.

If the specialization explanation is right, we would argue that in those households in which neither the male nor the female eligible are household heads, the differences between the effects of female and male eligible should be smaller. Turning to our results, we find that point estimates are again positive for the female eligible effect and negative for the male eligible. In the latter case, the effects are always non-significant. However, in the case of the female eligible effect, the effects are sometimes significant or borderline significant.

Retrospective information

The estimates presented so far rely on the assumption that the presence at the time of interview of an eligible person coincides with her or his presence at the time of the infant's birth. This assumption is less credible the larger the time span between the date of birth and the time of the interview. Therefore, in what follows we study the robustness of our results to alternative assumptions regarding retrospective information.

We consider in Table 5 three alternative samples. In the so-called *Minimized-delay sample*, we use for the pre-treatment period only infants born between June 1993 and June 1994. For the post-treatment period, we use infants born between June 1997 and June 1998. Defining our samples in this way, we make delay between births and the collection of information never larger than two and a half years for the pre-treatment period and never larger than three and a half years for the post-treatment period. Because of this asymmetry, we also consider the *Minimized-similar-delay sample* that includes all infants born between two and a half and three and a half years before the interview both for the pre-treatment period and for the post-treatment period. Consequently, in the *Minimized-similar-delay sample*, infants are born between June 1992 and June 1993 in the pre-treatment period and between June 1997 and June 1998 in the post-treatment period.

Both the *Minimized-* and the *Minimized-similar-delay* samples only include infants born within a period of 12 months. This greatly reduces the estimation sample and may potentially affect the accuracy of the estimates. Consequently, we additionally create a larger sample—that we refer to as *Similar-delay sample*—that includes children born between June 1991 and June 1993 for the pre-treatment period and children born between June 1996 and June 1998 for the post-treatment period. Births in this sample occur between two and a half and five years before the interview.

In Table 5 we present the basic diff-in-diffs estimates for the three alternative samples using the control 1 infants as the controls. When we consider the *Minimized-delay sample* we obtain positive point estimates for the female eligible effect in all specifications and negative point estimates for the male eligible effect in half of the specifications. However, all estimates are not significant with the exception of the male eligible effect 9 months after birth, which is positive and significant. Hence, changing the implicit assumption about retrospective information has an effect on the significance of our results.

Table 5: *Retrospective Information: Results using Control 1 Infants*

| | 3 months | | 6 months | | 9 months | | 12 months | |
	Uncond.	Cond.	Uncond.	Cond.	Uncond.	Cond.	Uncond.	Cond.
Minimize delay	1	2	3	4	5	6	7	8
Female eligible	0.011	0.014	0.021	0.021	0.021	0.022	0.001	0.008
	(0.804)	(0.626)	(0.565)	(0.394)	(0.607)	(0.451)	(0.982)	(0.860)
Male eligible	-0.047	-0.004	-0.066	-0.010	0.044	0.035	0.027	0.021
	(0.241)	(0.859)	(0.162)	(0.731)	(0.047)	(0.038)	(0.539)	(0.599)
No. of obs.	2657	2554	2657	2554	2674	2571	2674	2571
Minimize similar delay								
Female eligible	0.047	0.042	0.052	0.047	0.056	0.049	0.055	0.049
	(0.000)	(0.000)	(0.001)	(0.000)	(0.002)	(0.001)	(0.048)	(0.047)
Male eligible	-0.016	0.006	-0.013	-0.046	0.003	0.004	0.000	-0.053
	(0.556)	(0.774)	(0.615)	(0.383)	(0.971)	(0.953)	(0.996)	(0.670)
No. of obs.	2488	2403	2488	2403	2505	2420	2505	2420
Similar delay								
Female eligible	0.036	0.024	0.050	0.038	0.053	0.039	0.047	0.032
	(0.157)	(0.374)	(0.011)	(0.077)	(0.016)	(0.113)	(0.158)	(0.376)
Male eligible	-0.037	-0.045	-0.107	-0.126	-0.174	-0.191	-0.133	-0.253
	(0.710)	(0.648)	(0.441)	(0.383)	(0.292)	(0.259)	(0.301)	(0.171)
No. of obs.	5068	4891	5068	4891	5068	4891	5068	4891

Note: Average marginal effects as defined in equation (8) with alternative samples. The Minimized delay sample includes infants born between June 1993 and June 1994 for the pre-treatment period and infants born between June 1997 and June 1998 for the post-treatment period. The Minimized similar delay sample includes infants born between two and a half and three and a half years before the interview both for the pre-treatment period and for the post-treatment period. The Similar delay sample includes infants born between two and a half and five years before the interview. *p*-values are shown in parenthesis. See Table 1 for the definition of the control group and Table 2 for the variable specification of *Uncond.* and *Cond.*

The *Minimized-delay* pre-treatment sample differs in time span from the post-treatment sample. The implicit assumption on retrospective information is less credible for the post-treatment period because there are many observations in that period for which the distance between the date of birth and the date of interview is larger than the largest distance in the pre-treatment period. This asymmetry could create a bias in our diff-in-diffs estimations. In contrast, using the *Minimized-similar-delay* sample we assume that there are no relevant changes in the composition of the families in the last three years prior to the interview, regardless of whether the interview takes places before or after the start of the implementation of the OAP programme. Interestingly, although the sample does not change much, the female eligible effect is now larger than the original estimates and even more significant while the male eligible effect is never significant.

One could nevertheless argue that the lack of significance for the positive point estimates of the male eligible effect after nine months (see columns 5 and 6) might be due to the small size of the estimation sample. To look at this possibility, we use the *Similar-delay* sample, which increases the sample size with respect to the *Minimize-similar-delay* sample but makes the same assumption on retrospective information both before

and after the start of the policy. When we do this, the point estimates regarding the female eligible effect become very similar to the original results (although they are not significant in some cases). More interestingly, none of the point estimates of the male eligible effect are significant, and all of them are negative.

Assuming that the old people present in the household at the interview were already present at the date of the infant's birth is a potentially influential assumption. Our results show that alternative sample specifications (which imply alternative assumptions on retrospective information) lead to slight changes in the size of the effects and also to differences in the significance of the results. However, our results also suggest that the asymmetry between the female and the male eligible effects is not an artifact of this assumption. Moreover, in arguably the best alternative to our benchmark estimates—the estimates obtained from the *Minimized-similar-delay* sample—we find that the female eligible effect is positive and significant while the male eligible effect is not significant.

5 Testing common trends and an alternative identification strategy

So far, the identification of the effects of the OAP programme lies on the Parallel Paths assumption, which states that average changes in survival status among those treated if untreated are equal to the average changes in survival status among comparable controls. Violation of this assumption for male-eligibles would lead to inconsistent estimates of the male-eligible effect.

What economic process could motivate different pre-treatment trends for male treated and controls, but the same pre-treatment trends for female treated and controls? One plausible explanation is the existence of trends in unobservable quality differentials by type of households. Suppose that couples live with elderly people if they cannot afford to live separately or if the elderly person needs their assistance because no-one else can help. As the economy develops and wages improve, the proportion of three-generation households where the young couple cannot afford to live separately will tend to decrease. When looking at successive cross-sections of data, this effect creates a downward trend in the relative average household wealth in three-generation households where the young couple cannot live separately. In contrast, economic growth does not improve the wealth

of poor elderly who are not property owners. Hence, economic growth should not change the formation of three-generation households where the elderly requires assistance. As long as males are less affected by poverty than old females, old females will tend to live in three-generation households where the old person needs help while old males will live in households where the young couples are the ones who benefit economically from the association. When using successive cross-sections of three-generation households, economic progress will trigger a sample selection mechanism by which the average three-generation household with an old male will suffer a relative decline in wealth.

It is customary to test for common pre-treatment trends to justify the Parallel Paths assumption. The simplest way to do this is by conducting DID on the last pre-treatment period, a test that requires at least two periods before treatment. In our benchmark sample we include all children born between July 1991 and June 1994 for the pre-treatment period. We know the exact birth date for each observation so that for a sufficiently large sample we could consider as many periods as days in the pre-treatment period. However, since the number of treated is small even when we group them by month of birth, we opt for dividing the pre-treatment sample into only two periods: the first pre-treatment period includes all births from July 1991 to December 1992 while the second pre-treatment period includes all births between January 1993 and June 1994. We implement the test on pre-treatment common trends by using only the pre-treatment sample and then computing the diff-in-diffs estimator as if the policy had been implemented in January 1993 instead of July 1995. The test for common trends is the test on the significance of the diff-in-diffs estimate for the marginal effect α (see equation (8)).

Table 6 reports the results of the tests for pre-treatment common trends for the female eligible effect and the male eligible effect using all four alternative control groups and for both the unconditional and the conditional specifications.

We find no evidence of a female eligible effect in almost all specifications and periods considered. The only exceptions, for survival rates after 9 and 12 months, occur only in the unconditional model and using control 2 infants. With our preferred control group, control 4 infants, we cannot reject that controls and treated have common pre-treatment trends in survival status 3, 6, 9, and 12 months after birth.

In contrast, we find very strong evidence of a negative and significant male eligible effect

Table 6: *Tests for pre-treatment common trends*

| | 3 months | | 6 months | | 9 months | | 12 months | |
	Uncond.	Cond.	Uncond.	Cond.	Uncond.	Cond.	Uncond.	Cond.
Control 1	1	2	3	4	5	6	7	8
Female eligible	-0.001	-0.017	0.013	0.002	0.015	0.002	0.016	-0.001
	(0.984)	(0.811)	(0.796)	(0.969)	(0.781)	(0.968)	(0.784)	(0.990)
Male eligible	-0.952	-0.960	-0.946	-0.954	-0.939	-0.948	-0.933	-0.943
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No. of obs.	3523	3365	3523	3365	3523	3365	3523	3397
Control 2								
Female eligible	0.033	0.023	0.037	0.029	0.043	0.032	0.046	0.034
	(0.356)	(0.537)	(0.140)	(0.237)	(0.037)	(0.169)	(0.032)	(0.129)
Male eligible	-0.975	-0.980	-0.977	-0.982	-0.974	-0.979	-0.973	-0.979
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No. of obs.	503	473	558	523	610	571	610	571
Control 3								
Female eligible	-0.012	-0.036	0.005	-0.010	0.004	-0.010	0.005	-0.017
	(0.861)	(0.681)	(0.934)	(0.880)	(0.944)	(0.886)	(0.948)	(0.832)
Male eligible	-0.955	-0.962	-0.949	-0.957	-0.944	-0.951	-0.935	-0.945
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No. of obs.	2064	1997	2064	1997	2064	1997	2064	2009
Control 4								
Female eligible	0.023	0.022	0.028	0.026	0.038	0.033	0.041	0.036
	(0.651)	(0.660)	(0.451)	(0.439)	(0.195)	(0.297)	(0.180)	(0.249)
Male eligible	-0.976	-0.978	-0.980	-0.982	-0.977	-0.979	-0.976	-0.978
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No. of obs.	409	369	452	405	484	432	484	432

Note: Tests for pre-treatment common trends for the female eligible effect and the male eligible effect. *p*-values are shown in parenthesis. See Table 1 for the definition of the control groups and Table 2 for the variable specification of *Uncond.* and *Cond.*

before treatment for all specifications, time ranges, and control groups. In the presence of pre-treatment trend differentials, Parallel Paths becomes less attractive as it implies that differing pre-treatment trends become equal after treatment under no treatment. Hence, these tests suggest that the results reported so far for the female eligible effect are based on a true assumption but the results for the male eligible effect are based on an assumption that is false.

In practice, researchers who find pre-treatment trend differentials often formulate flexible econometric models to accommodate those trend differentials. In the next section, we follow Mora and Reggio (2012) and explore an alternative identification strategy using a flexible model and under an alternative assumption.

6 A flexible model with differing trends

Pre-treatment trend differentials in survival rates can be easily accommodated in the basic linear specification from equation (6) by including a time dummy for the last pre-treatment period, $LastPre$, and its interaction with the treated indicator D:

$$S^* = \beta_0 + \beta x + \gamma_D D + \gamma_L LastPre + \gamma_P Post + \phi_L D \times LastPre + \phi_P D \times Post + \epsilon. \quad (9)$$

Under the Parallel Paths assumption, $\phi = \phi_P - \phi_L$. However, as argued in the previous subsection, the Parallel Paths assumption is not appealing for the male eligible treatment because of the presence of pre-treatment trend differentials between treated and controls. Mora and Reggio (2012) show that an alternative assumption which identifies the policy effect in the presence of pre-treatment trend differentials is the Parallel Growths assumption. Intuitively, Parallel Growths states that under no treatment the survival status of the treated would have experienced the same acceleration as the survival status of the controls. Assuming Parallel Growths leads to a difference-in-double-differences moment condition for ϕ:

$$\phi = E\left[\Delta^2 S^* | D = 1, x\right] - E\left[\Delta^2 S^* | D = 0, x\right]. \quad (10)$$

Hence, under equation (9) and Parallel Growths, $\phi = \phi_P - 2\phi_L$. The parameter of interest α can then be estimated using equation (8).

For the female eligible effect we do not expect a very different estimate under Parallel Growths than under Parallel Paths because our pre-treatment trend differentials tests suggest that, for the female eligible treatment, $\phi_L = 0$. In contrast, the results of our common trend tests suggest that, for the male eligible treatment, $\phi_L < 0$, and, hence, that $\phi_P - 2\phi_L > \phi_P - \phi_L$. We thus expect a larger estimate of the male eligible effect under Parallel Growths than under Parallel Paths. Intuitively, the Parallel Growths assumption implicitly takes into account that the treated infants living with male eligibles would have experienced a relative average decline in survival score under no treatment.

The added flexibility in equation (9) comes with a cost: in our data, sample identification of ϕ_P fails for the male eligible treatment under the most flexible trend specification and the benchmark sample. We present two strategies to overcome this problem: a) to

extend the estimation sample to include observations from 1990; and b) to assume that, before 1993, infants face the same probability of survival regardless of the presence of a male eligible (i.e. $\gamma_D = 0$).

We provide support for the Parallel Growths assumption by testing that pre-treatment average acceleration was equal between the treated and the controls. As in the common-trends tests, we implement the test on pre-treatment common accelerations by using only the pre-treatment sample. We now partition the pre-treatment sample into three 12-month periods: from July 1991 to June 1992, from July 1992 to June 1993, and from July 1993 to June 1994. We then compute the diff-in-double-diffs estimator as if the policy had been implemented in July 1993 instead of July 1995. The test for common accelerations is the test on the significance of the diff-in-double-diffs estimate for the marginal effect α.

Table 7 reports the results of the tests for pre-treatment common accelerations for the female eligible effect and the male eligible effect using all four alternative control groups and for both the unconditional and the conditional specifications. When we tested common pretreatment trends, we found no evidence of a female eligible effect but very strong evidence of a negative and significant male eligible effect. In contrast, the results in Table 7 show that there is no evidence of differences in accelerations between treated and controls for the male eligible effects but the tests results suggest that the Parallel Growths assumption is not appropriate for the female eligible effect.

The results from Table 6 and Table 7 hint that the effect of the female eligible effect should be identified using the Parallel Paths assumption while the estimate of the male eligible effect should be identified using the Parallel Growths assumption. Hence, in Table 8 we report, using estimates of equation (9), estimated marginal effects under the Parallel Paths assumption for the female eligible effect and under the Parallel Growths assumption for the male eligible effect. For brevity, we only show the results using Control 4 infants.[15]

Table 8 corroborates the results so far concerning the female eligible treatment effect: results on survival status are always positive and significant for all time delays. Moreover, the size of the effects is very similar to the estimated effects assuming Parallel Paths and using both estimates from equation (6) and from equation (9).

---

[15]All results are available upon request.

Table 7: *Tests for pre-treatment common accelerations*

	3 months		6 months		9 months		12 months	
	Uncond.	*Cond.*	*Uncond.*	*Cond.*	*Uncond.*	*Cond.*	*Uncond.*	*Cond.*
Control 1	1	2	3	4	5	6	7	8
Female Eligible	0.066	0.059	0.074	0.067	0.082	0.074	0.089	0.080
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Male eligible	0.022	0.026	-0.042	-0.035	-0.072	-0.058	0.044	-0.061
	0.833	0.751	0.844	0.857	0.782	0.806	0.634	0.807
No. of obs.	4583	4382	4583	4382	4583	4382	4583	4428
Control 2								
Female Eligible	0.079		0.081	0.077	0.090	0.083	0.097	0.090
	0.000		0.000	0.000	0.000	0.000	0.000	0.000
Male eligible	0.064		0.017	0.026	-0.068	-0.040	0.049	-0.044
	0.128		0.907	0.838	0.811	0.873	0.606	0.866
No. of obs.	686		750	704	782	736	782	736
Control 3								
Female Eligible	0.067	0.060	0.076	0.068	0.084	0.077	0.094	0.085
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Male eligible	0.014	0.012	-0.068	-0.088	-0.086	-0.094	0.041	-0.114
	0.913	0.919	0.789	0.746	0.763	0.743	0.702	0.716
No. of obs.	2728	2643	2728	2643	2728	2643	2728	2661
Control 4								
Female Eligible	0.086		0.091	0.084	0.096	0.087	0.102	0.093
	0.000		0.000	0.000	0.000	0.000	0.000	0.000
Male eligible	0.063		-0.006	0.001	0.011	0.020	0.079	0.022
	0.343		0.978	0.994	0.954	0.901	0.103	0.898
No. of obs.	531		581	550	616	582	616	582

Note: Tests for pre-treatment common accelerations for the female eligible effect and the male eligible effect. *p*-values are shown in parenthesis. See Table 1 for the definition of the control groups and Table 2 for the variable specification of *Uncond.* and *Cond.*

The crucial novelty in Table 8 in relation to the results reported so far concerns the male eligible effect. Assuming Parallel Growths overturns the results obtained by using Parallel Paths: The male eligible effect changes from being negative and not-significant to being positive and strongly significant in most specifications. The only exceptions are estimates for the Only–boys sample after 6, 9, and 12 months in the conditional models where the point estimates are negative but very imprecisely estimated. These conclusions are similar to those obtained with alternative controls. Regarding, the size of the effect whenever is significant, it is closely similar to the size of the estimated effect for female eligible. In fact, we can never reject that the two effects are equal in size.

Finally, in the presence of different pre-treatment common trends it is usual in diff–in–diffs applications to extend the benchmark model for the male eligible effect by introducing a group–specific linear deterministic trend in equation (6). This is a more restrictive approach than identifying the male eligible effect using only the Parallel Growths assumption (Mora and Reggio, 2012) and this unnecessary restriction could bias the

Table 8: *Effects under Parallel Paths for female eligible and Parallel Growths for male eligible*

	3 months		6 months		9 months		12 months	
	Uncond.	*Cond.*	*Uncond.*	*Cond.*	*Uncond.*	*Cond.*	*Uncond.*	*Cond.*
Pre-treatment sample: July 1990-June 1994								
All								
Female effect	0.063	0.053	0.074	0.059	0.071	0.056	0.075	0.062
	0.006	0.030	0.000	0.009	0.006	0.035	0.011	0.026
Male effect	0.072	0.062	0.077	0.065	0.078	0.064	0.086	0.070
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Difference (*p*-value)	0.450	0.403	0.696	0.621	0.658	0.628	0.403	0.587
No. of obs.	978	916	1042	979	1115	1044	1135	1062
Only boys								
Female effect	0.112	0.107	0.117	0.109	0.116	0.111	0.094	0.101
	0.000	0.000	0.000	0.002	0.004	0.001	0.164	0.019
Male effect	0.105	0.094	0.066	-0.018	0.069	-0.024	0.113	-0.013
	0.000	0.000	0.608	0.947	0.580	0.932	0.014	0.962
Difference (*p*-value)	0.837	0.918	0.617	0.446	0.697	0.421	0.770	0.522
No. of obs.	426	379	450	400	487	430	508	456
Model with $\gamma_D = 0$								
All								
Female effect	0.064	0.051	0.073	0.054	0.069	0.050	0.072	0.056
	0.008	0.051	0.000	0.022	0.014	0.072	0.019	0.051
Male effect	0.062	0.052	0.062	0.045	0.058	0.036	0.075	0.039
	0.023	0.065	0.051	0.267	0.179	0.504	0.003	0.527
Difference (*p*-value)	0.939	0.913	0.899	0.901	0.918	0.851	0.810	0.821
No. of obs.	849	790	913	853	983	915	1003	933
Only boys								
Female effect	0.118	0.114	0.119	0.108	0.115	0.110	0.092	0.102
	0.000	0.000	0.002	0.008	0.018	0.003	0.237	0.027
Male effect	0.096	0.067	0.028	-0.127	0.022	-0.140	0.091	-0.167
	0.032	0.502	0.871	0.730	0.902	0.708	0.273	0.672
Difference (*p*-value)	0.840	0.562	0.497	0.297	0.546	0.268	0.948	0.289
No. of obs.	359	315	383	336	418	364	439	390

Note: Average marginal effects obtained using equation (9) and assuming Parallel Paths for the female eligible and Parallel Growths for the male eligible effect. Panel Pre-treatment sample: July 1990-June 1994 extends the estimation sample to include observations from 1990. Panel Model with $\gamma_D = 0$ restricts γ_D in equation (9). *p*-values are shown in parenthesis. Control infants are control 4 infants defined in Table 1. *Uncond.* refers to the diff-in-diff model with month of birth and region fixed effects. The *Cond.* model additionally includes dummy variables for the education of the mother, the mother's age at the child's birth and its square, a dummy for whether the child is female, the number of kids younger than five—at birth of infant—in the household, and dummies for the ethnicity of the mother.

estimates. In results we do not show for brevity, we find that the estimates are also positive and significant but the point estimates are around 33% larger than the point estimates using only the Parallel Growths assumption. Hence, although the basic result remains (i.e., the male eligible effect is positive) we suspect that the deterministic linear trend specification introduces a positive bias.

To sum up, our results show that the Parallel Path assumption is essential to find differences between grandmothers and grandfathers. Under the Parallel Growths assumption and a flexible specification for the econometric model, we find no gender differences in how a positive shock in income among old people affects the welfare of infants living

with them.

7 Conclusions

Many studies find evidence that presence of a grandmother is associated with higher child survival rates while no such association is found in the case of a grandfather. We exploit income variation from the introduction of a non-contributory universal pension scheme in Nepal in 1995. Using cross-sectional data from the 1996 and 2001 Nepal Demographic and Health Surveys, we obtain diff–in-diffs estimates that are consistent with these results: we find positive and significant effects on survival rates for an income increase of a female person older than 75 who lives in the same household while negative and sometimes significant effects for the income increase of an old male.

These results are qualitatively similar across alternative definitions of the control group, for both boys and girls, and do not depend on: a) how we exploit retrospective information in the data; b) whether the female (male) beneficiary is the only beneficiary in the household; or c) the family status of the beneficiary. However, the results are not robust to alternative assumptions for the diff–in–diffs estimates. More precisely, when we implement a flexible identification strategy based on the Parallel Growths assumption defined in Mora and Reggio (2012) for the male beneficiary effect and on the Parallel Paths assumption for the female beneficiary effect, we find no significant gender differences in how grandparents' economic conditions affect infant survival rates.

We validate the Parallel Growths assumption with a pre-treatment common acceleration test that is similar in spirit to the pre-treatment common tests used to validate the Parallel Paths assumption. We motivate the different results of these tests for female and male beneficiaries by the following argument. If couples tend to live with a male beneficiary when they have economic problems and tend to live with a female beneficiary when she has economic problems, then economic growth will result in successive cross-sections where three-generation households with an old male will suffer a relative decline in wealth. Hence, our findings can be interpreted as suggestive that cross-sectional analysis may bias downwards the estimates of the effect of grandfathers.

References

CASTERLINE, J. B. (1989): "Collecting data on pregnancy loss: a review of evidence from the World Fertility Survey," *Studies in Family planning*, 20(2), 81–95.

DUFLO, E. (2000): "Child Health and Household Resources in South Africa: Evidence from the Old Age Pension Program," *The American Economic Review*, 90(2), pp. 393–398.

———— (2003): "Grandmothers and Granddaughters: Old-Age Pensions and Intra-household Allocation in South Africa," *The World Bank Economic Review*, 17(1), 1–25.

MORA, R., AND I. REGGIO (2012): "Treatment effect identification using alternative parallel assumptions," Discussion paper.

RAJAN, S. I. (2003): "Oldage Allowance Program in Nepal," http://www.chronicpoverty.org/uploads/publication_files/CP_2003_Rajan.pdf, [Online; accessed 11-June-2013].

SEAR, R., AND R. MACE (2008): "Who keeps children alive? A review of the effects of kin on child survival," *Evolution and Human Behavior*, 29(1), 1 – 18.

SHARMA, K. (2006): "The political economy of civil war in Nepal," *World Development*, 34(7), 1237 – 1253.

THAPA, S. (2004): "Abortion law in Nepal: the road to reform," *Reproductive Health Matters*, 12(24), 85–94.

THAPA, S., AND S. M. PADHYE (2001): "Induced abortion in urban Nepal," *International Family Planning Perspectives*, pp. 144–151.