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The Old-Age Security Motive for Fertility: Evidence from the Extension of Social Pensions in Namibia

Pauline Rossi and Mathilde Godard *

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Abstract

The old-age security motive for fertility postulates that people's needs for old-age support raise the demand for children. We exploit the extension of social pensions in Namibia during the nineties to provide a quasi-experimental quantification of this widespread idea. The reform eliminated inequalities in pension coverage and benefits across regions and ethnic groups. Combining differences in pre-reform pensions and differences in exposure across cohorts, we show that pensions substantially reduce fertility, especially in late reproductive life. The results suggest that improving social protection for the elderly could go a long way in fostering fertility decline in Sub-Saharan Africa.

Keywords: Fertility, Old-age pensions, Social security, Africa, Difference-in-differences.

JEL Codes: D15, H55, I38, J13, O15, O55.

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1 Introduction

This article connects two debates on Africa's development: how to foster fertility decline, and whether governments should provide universal old-age pensions. Currently, both debates are discussed in isolation. This is surprising since the idea that people have children partly in order to secure old-age support is a longstanding hypothesis in social sciences (Leibenstein 1957, Caldwell 1978, Caldwell 1982, Nugent 1985). We lack credible evidence on the importance of this old-age security motive for fertility, though. Are the needs for old-age support a first-order or a negligible driver of fertility?

We exploit the extension of social pensions in Namibia in the nineties to study this question. The reform took place after the end of the apartheid and eliminated inequalities in coverage across regions and inequalities in benefits across ethnic groups. Restricting our attention to the Black population, we combine the variation in pre-reform pensions with the variation in exposure across cohorts in a difference-in-differences framework. We examine the number as well as the timing of births, and find that social pensions substantially reduce fertility, especially after age 30. Our estimates predict that completed fertility could be reduced by around one child in other Sub-Saharan African countries if they implemented a pension system similar to the ones currently in place in Southern Africa.

Quantifying the strength of the old-age security motive is challenging. In theory, this motive can be important under some conditions: (i) there is no better way to secure well-being in old-age than relying on children; and (ii) the quality-quantity trade-off is limited so that more children do provide more support. Whether these conditions hold in practice and influence reproductive behaviors is an open empirical question.[1] Field experiments are difficult to implement due to the lag between the time when people make their reproductive decisions, and the time when they receive their pensions. Young couples would have to trust that pensions will actually be paid twenty to forty years later, and it would take long and be costly to build such a trust. A more feasible option is exploiting the introduction of real pension systems as natural experiments, although this raises simultaneity and reverse causality issues. Indeed, the introduction of old-age pensions is often part of a broader struc-

[1] Old-age support has long been considered as the least thoroughly analyzed motive for fertility (Stolnitz 1983). In a recent review article, Piggott and Woodland (2016) conclude that "The empirical evidence supporting this hypothesis is surprisingly scarce. [...] More research is certainly needed to understand how much the growing availability of substitutes for old-age security affects the decline in the demand for children".

tural change in the economy and in the society, which makes it difficult to isolate the impact of pensions. Moreover, the fall in fertility can precede and drive the demand for social security. These issues are salient in before-after comparisons and cross-country comparisons, as explained by Guinnane (2011) in the context of the European fertility transition. We argue that they can be mitigated by tracking the extension of pensions to different parts of a national population.

In Sub-Saharan Africa, only eight countries currently have well-developed social pension systems, with a coverage between two thirds and universal, and benefits between 15 and 20% of the average income.[2] As shown by Figure 1, these countries turn out to have much lower fertility rates than the rest of the continent: three compared to five or six children per woman. To study whether this correlation partly captures a causal effect, the most interesting settings are South Africa and Namibia, because there is variation in the inclusiveness of pensions across periods, regions and ethnic groups. Under the apartheid, the pension system was characterized by high inequalities in generosity and in access, not only between Whites and Blacks but also within the Black population. De jure, benefits were defined at the ethnic group level (e.g. Damara, Nama, Herero, Owambo etc); de facto, the administration was decentralized at the regional level and left behind substantial shares of eligible people, particularly in rural areas. After the apartheid, the system was extended to ensure that everyone receives the same social pension: equal benefits and quasi-universal coverage were achieved by the end of the nineties.[3]

We argue that the Namibian context provides a clean difference-in-differences design. First, we can exploit information from the national Household Income and Expenditure Survey conducted in 1993-94 to construct a measure of initial pensions that varies at the regional and ethnicity levels within the Black population. This is not feasible in South Africa.[4] Second, four waves of the Demographic and Health

[2]These are either Southern African countries: South Africa (since 1928), Namibia (1949) Botswana (1996), Lesotho (2004), and Swaziland (2005), or small islands: Mauritius (1950), Seychelles (1979), and Cape Verde (2006). The vast majority of African countries have no social pension system. See Figure A.1 in Appendix for more details.

[3]Case and Deaton (1998), Duflo (2003), Jensen (2003), Hamoudi and Thomas (2014) and World Bank (2017) study the South African pension system but they do not look at fertility responses. They document that the extension of pensions was a large income shock to the elderly. We come back to this point when discussing the interpretation of our estimates as an insurance effect rather than an income effect.

[4]The same household survey was carried out in both countries in 1993-94. At that time, the extension had just started in Namibia, whereas it had already been implemented in South Africa so there was no variation in pensions anymore. Earlier surveys exclude Black homelands and are therefore not representative.

Surveys collected between 1992 and 2013 provide retrospective information on birth histories of relevant cohorts. We have a long time window to study the whole reproductive period of women, and analyze the timing and the total number of births. The extension was implemented in just a few years during the mid-nineties. This sudden change is predicted to generate a marked pattern across years and cohorts that can be distinguished from a secular decline and from a global convergence in fertility rates of sub-populations. Third, we are able to control for many potential confounding factors. We show that omitted variables tend to generate an underestimation bias in this context, because people previously disadvantaged by the pension system benefited less from other socio-economic changes. Last, the extension was driven by equity motives, with the explicit goal of granting the same rights to everyone. An extension driven by economic motives and targeting vulnerable groups could have raised concerns about reverse causality.

We find that before the reform, people disadvantaged by the pension system had more children; the difference in birth rates was particularly strong in the second half of women's reproductive life, between ages 30 and 45. The equalization of pensions is followed by a fast convergence in the number of births. The convergence starts precisely when the reform is announced and was completed within a decade. Cross-cohort patterns coincide with exposure to the new pension system during the late reproductive years. We check that these results are not driven by specific regions or ethnic groups, and do not reflect composition effects. Changes in age at last birth, ideal family size, and childbearing intention confirm that the main mechanism is stopping rather than starting or spacing. We further examine female labor supply and household income, and conclude that the negative impact of pensions on fertility does not operate through these channels, but rather through the old-age insurance motive. Our estimates predict that improving pension coverage to at least one half, and raising benefits above the poverty line, reduces completed fertility by an order of magnitude of one child. We provide evidence that the impact of pensions on fertility is non-linear and explain theoretically why this may be the case. The key insight is that the lack of pensions and the risk that children default on their parents reinforce each other in driving the precautionary demand for children upwards. This suggests that introducing – even more than expanding – social protection for the elderly in other Sub-Saharan African countries could lead to a substantial, rapid decline in fertility.

Our article provides robust quasi-experimental evidence that old-age pensions can reduce fertility. A large literature discusses the relationship between pensions and fertility, but most articles study either the opposite causal link – the impact of low fertility on the sustainability of pension systems – or correlations.[5] Another strand of the literature is interested in the substitution between formal and informal old-age insurance, assuming that fertility is exogenous.[6] Only a few papers study whether parents change their fertility behaviors when they expect to be less dependent on children's support. One experimental article investigates the No Birth Bonus Scheme in South India (Ridker 1980) and two quasi-experimental studies use the introduction of pensions in rural Mexico in the sixties (Nugent and Gillaspy 1983) and in rural China in the nineties (Ebenstein and Leung 2010). All find fertility responses but the empirical designs are not ideal.[7] Conversely, a working paper by Billari and Galasso (2008) examines the impact of *reducing* pension benefits in a lowest-low fertility society, Italy, and finds that cohorts entitled to less generous replacement rates had more children. Finally, we add to the debate on the direction of intergenerational altruism in overlapping generation models. Boldrin, Nardi, and Jones (2015) show that different assumptions on the direction of transfers lead to different predictions. The Boldrin and Jones (2002) model, where transfers go from children to parents, predicts that social pensions are a quantitatively important driver of fertility; in contrast, the Barro and Becker (1989) model, where transfers go from parents to children, predicts that pensions explain very little variation in fertility. Our quasi-experimental estimates from Namibia provide support for the former.

Our findings contribute to two strands of policy discussions. First, old-age pensions are rarely included in the policy tools to reduce fertility. To achieve their goal of containing population growth, African governments target an improvement

[5]See review in Boldrin, Nardi, and Jones (2015). Using cross-country or before-after comparisons, these articles consistently find that better social security is associated with fewer children.

[6]Researchers have shown that children do support parents in old-age (Hoddinott 1992, Oliveira 2016), especially if the pension system is absent or failing (Cox and Jimenez 1992, Juarez 2009, Cai, Giles, and Meng 2006). In particular, Jensen (2003) finds evidence of crowding-out in South Africa: transfers by children are reduced by 0.25-0.30 rand for each rand paid by the old-age pension system. Furthermore, Bau (2019) shows that intergenerational co-residence practices tend to decline when formal pensions become available. Last, parents change their old-age arrangements if they have fewer kids, especially fewer sons (Ebenstein and Leung 2010, Banerjee, Meng, Porzio, and Qian 2014).

[7]In the Indian experiment, women received money on a bank account that was blocked until retirement if they had no child during the year. The sample was small and not representative, the treatment was not randomly allocated, there were large imbalances in female education favoring the treatment group, and old-age security needs cannot be disentangled from the effect of financial incentives. In Mexico, pensions were bundled with a family planning program. In China, pensions were introduced in villages with specific features; moreover, the number of children was strictly regulated by population policies.

in birth control through better access to contraceptives and female empowerment. The assumption is that people have more children than what they want.[8] Our analysis suggests that exploring why people want many children and what can be done to change these incentives could help renewing population policies. Second, the impact on fertility is not mentioned in the discussions on pensions. Social protection for the elderly figures prominently in the global strategy to fight poverty and inequalities in Africa.[9] Decision makers tend to focus on narrow, static outcomes, weighing the gains in terms of protection and redistribution against the corresponding fiscal costs. This article calls for considering a broader picture: pensions do not only affect the well-being of the elderly today, but also the reproductive behavior of the next generation. Including fertility responses in cost-benefit analysis may change the conclusions, since these responses potentially affect both the intergenerational transmission of poverty and the evolution of dependency ratios.

The outline of the paper is as follows. Section 2 describes the Namibian pension system and the historical context. Section 3 presents the data and important descriptive statistics. Section 4 sets up the empirical strategy. Section 5 reports the results and the robustness tests. Section 6 discusses external validity through the lens of a Lexicographic Safety First model. Section 7 concludes.

2 Context

This section draws upon Devereux (2001) to describe the main features of the Namibian social pension system. Before 1990, Namibia was de facto a colony of South Africa and subject to the apartheid regime. Pensions were first introduced in 1949 for white residents only. The eligibility was extended to all residents in 1973, but the system remained highly unequal, both in theory and in practice. In theory, different ethnic groups were entitled to different benefits, depending on whether they were favored or not by the regime. In practice, different regions had different coverages because the administration of the pension system for non-whites was decentralized at the *bantustan* level. Depending on the corruption and inefficiency of

[8] See for example United Nations (2013) or United Nations (2015) recommending to "invest in reproductive health and family planning, particularly in the least developed countries, so that women and couples can achieve their desired family size".

[9] Old-age social pensions are highlighted as a special theme in the 2018 edition of the State of Social Safety Nets (World Bank 2018) and monitored as a key indicator to reach the Sustainable Development Goal target 1.3 calling for the implementation of appropriate social protection systems and measures (International Labour Organization 2018).

the local authorities, eligible people were more or less likely to receive their pensions. Moreover, delivery systems tended to neglect isolated communities, given the logistical problems arising from the provision of payments to unbanked people in sparsely populated areas. Everything changed in the nineties after the end of apartheid and the independence of Namibia. In 1992, inequalities were removed in theory: the Pension Act states that all Namibian residents above age 60 are entitled to the same social pension. A universal, non-contributory system is created. In practice, inequalities persisted until 1996, when the government decided to outsource the pension delivery system to a private company, with the stated goal of reaching universal coverage. This was achieved within a couple of years thanks to two innovations: mobile payment in cash improved access, and biometric identification reduced corruption. In concrete terms, escorted vehicles with cash dispensing machines monthly visit a dense network of payment points. Pensioners wait in line and receive money if their fingerprints match the ones in the computerized database.

Table 1 provides more details about the variation in official benefits across different ethnic groups. Before the Pension Act, Owambo, Kavango and Caprivi were entitled to the lowest amount: 55 rands, which corresponds to the poverty line, one dollar per day, or 14% of the average income. Herero and Nama received roughly 20% more (65 rands), and Damara 40% more (75 rands). At the top, Whites were entitled to 382 rands. In 1992, when the Pension Act was adopted, the amount was raised to 120 rands for the bottom four categories, reducing the ratio of highest to lowest from 7:1 to 3:1. Two years later, the rate was truly equalized to 135 rands for all, meaning that it decreased substantially for Whites. The final amount was set in 1996 at 160 rands, 3 times the poverty line, or 40% of the average income. This was a generous amount by any standards, which enabled the elderly to become financially independent (Devereux 2001). In our data, we can only distinguish four categories: we observe Damara; Herero – Nama; Owambo – Kavengo – Caprivi; and non-Black (an aggregation of White, Coloured and Baster). Tswana, who account for less than 0.5% of the population, are not present.

Turning to the variation in coverage, Table 2 shows the fraction of eligible people who actually received a pension in each of the 13 regions of Namibia in 1993-94, after the Pension Act but before the outsourcing of delivery. The national average was one half, and there is a large variation between regions in the North, just above 30%, and in the South, just below 80%. In 1998, two years after the outscourcing, the national coverage jumped to 88% and became very quickly universal. The ILO Social Assistance Database reports that 95% of eligible individuals received their pensions in 2001. The initial coverage is therefore an excellent predictor of the change in coverage.[10] Importantly, the initial situation in a region partly reflects the fraction of White residents (see maps in Appendix, Figure A.3). During the apartheid, the pension system for Whites was administered at the national level and funded by individual taxation; the vast majority of eligible White people therefore claimed and obtained their pensions (Morgan 1991). Assuming universal coverage for Whites, and based on the average regional coverage and the fraction of Whites among eligible people, we create a measure of initial coverage for non-Whites. The picture remains unchanged in Northern regions, where the fraction of White residents is very low. But in South and Central regions, the high coverage on average masks a lower coverage for Blacks. In particular in Khomas, the region of the capital city Windhoek where most Whites live, discrimination is extreme: only 15% of eligible Black people have access to pensions.

To sum up, while everybody ends up at the same point after the extension, there is some initial variation in amount at the ethnicity level and in coverage at the regional level. Two groups appear as outliers: Whites at the top, and Khomas at the bottom. As discussed in the next sections, both turn out to be very specific in terms of covariates and trends, and we exclude them from the final analysis sample. For Black people living in the provinces, there is a substantial initial variation, between groups well included and groups totally marginalized: coverage varies from 31% to 65% across 12 regions, and annual benefits vary from 660 to 900 rands (1 to 1.4 times the poverty line) across three categories of ethnic groups. To combine both sources of initial variation, we construct a variable called expected pension. We assume that people form myopic expectations about their future pensions based on what they observe. The current fraction of recipients in the environment is a good proxy for the probability of getting a pension, and the current amount received by pensioners a good proxy for future benefits. These quantities are public information: the distribution of cash is a noticeable event, and pensions are an important source of income for many households, where grand-parents, parents and children live together (Devereux 2001). We define the expected pension as the effective coverage times the official annual amount. This variable varies from 200 to 585 rands across 35 clusters.[11] Finally, we compute how far the expected pension is from the poverty

[10]Figure A.2 in Appendix plots, for each region, the coverage in 1993-94 on the x-axis and the change in coverage between 1993-94 and 2003 on the y-axis. Most regions are very close to the line $y = 100\% - x$ meaning that universal coverage was reached almost everywhere.

[11]We have 12 regions and 3 categories of ethnic groups, and in our sample, one category is absent from one region.

line (660 rands per year). We call the difference $InitialNeeds$: it measures the informal transfer that people should target in order to maintain their expected income in old-age above the poverty line.[12]

3 Data

3.1 Datasets

We use two datasets: the Demographic and Health Survey (DHS) and the Namibian Household, Income and Expenditure Survey (NHIES).

The DHS are ideal to compute fertility measures. These are nationally representative household surveys, with waves in 1992 (Katjiuanjo, Titus, Zauana, and Boerma 1993), 2000 (Ministry of Health and Social Services - MOHSS/Namibia 2003), 2007 (Ministry of Health and Social Services - MOHSS/Namibia and Macro International 2008) and 2013 (The Nambia Ministry of Health and Social Services - MoHSS - and ICF International 2014). They record the exhaustive birth history of women aged 15 to 49 (up to 64 in the 2013 wave). The main limitation is that regions are defined in only four categories instead of 13 in the first wave, 1992. This implies that we do not have data at a fine regional*ethnicity level for cohorts born before 1950 and for years before 2000, apart from retrospective information. We use birth histories collected in 2000, 2007 and 2013, to reconstruct birth rates in previous years.

We complement the DHS with NHIES to get information on potential confounders, like consumption, financial inclusion, access to health, education, occupation and urbanization. There are four cross-sectional waves: 1993-94 (Namibia Statistics Agency 1993-1994), 2003-04 (Namibia Statistics Agency 2003-2004), 2009-10 (Namibia Statistics Agency 2009-2010) and 2015-16 (Namibia Statistics Agency 2015-2016). Regions and ethnic groups have a narrow definition, making it possible to aggregate data at the same regional*ethnicity level as in DHS.[13] We also collected

[12] $InitialNeeds$ is the main explanatory variable in the empirical analysis. Alternatively, we can use the expected pension variable; estimates have the same magnitude and the opposite sign.

[13] In theory, it should have been possible to construct the initial pension coverage at a more disaggregated level, for instance separating urban and rural areas, to have more variation and more clusters. NHIES collected individual data on pension receipt in 1993-94, and this information was used by Subbarao (1998) to come up with the estimates in Table 2. But the income module of the questionnaire was lost. We thank Alwis Weerasinghe, who contributed to the computations in Subbarao (1998) for his help in searching for both the physical and digital versions of the questionnaires. Individual data on pension receipt could only be found for the wave 2015-16, a

additional data on HIV prevalence at the regional level from the 1994 National HIV prevalence survey conducted by the Ministry of Health and Social Security.

3.2 Descriptive statistics

Table 3 provides information about key variables. The first three columns describe drivers of fertility before the extension of pensions, separating regions with high initial coverage (hence low initial needs for old-age support), regions with low initial coverage (hence high initial needs), and Khomas, the region of the capital city. Indicators of development, notably wealth and urbanization, are worse in low coverage regions. This is partly because there is virtually no White residents, while they account for 15% and 39% of the population in high coverage regions and in Khomas, respectively. When we exclude White people, in the last three columns, high and low coverage regions become more similar, but Khomas remains different: less poor and much more urban. If we kept Whites and Khomas in the sample, the correlation between initial pension coverage and fertility would therefore capture, to a large extent, differences in socio-economic development. To limit the scope of imbalances, we restrict our sample to Blacks living outside the capital city in the rest of the analysis.[14]

Following the demography literature, we analyse fertility from two different perspectives. First, a period analysis (*tempo*): the outcome variable is the fertility rate, a measure of the flow of births, and the time variable is calendar year. Second, a cohort analysis (*quantum*): the outcome variable is completed fertility, a measure of the stock of births, and the time variable is woman's year of birth. Both perspectives complement each other. The period analysis focuses on the timing of fertility responses: when do adjustments happen and in which age groups? The cohort analysis indicates whether these responses are transitory or lead to a permanent change in the number of births per women.

Figure 2 compares the evolution of fertility between regions with low initial pension coverage (North) and regions with high initial pension coverage (Northeast

period when there is very limited variation. Our identification strategy therefore relies on the only piece information that remains - the initial coverage by region - combined with variations in official amounts.

[14] Alternatively, we can use a matching methodology to improve the balance between high and low coverage regions. Table A.1 in Appendix identifies which clusters are not in the common support when we predict the likelihood of high initial coverage using indicators of wealth and urbanization (Panel A). The procedure always leads to dropping ethnic groups in Khomas, but not all of them or not only them depending on the specification. We show in Panels B and C that our main results hold-up to this sample selection method.

and South). The graph on the top left plots the total fertility rate, which is a synthetic measure of how many births a woman would have if she was subject to current age-specific fertility rates at all ages throughout her reproductive life. Before the extension of pensions, in 1992, there was a large gap: just above six children in regions with a low coverage compared to just below five children in regions with a high coverage. After the extension, fertility decreased everywhere, and especially in regions that used to lag behind. The gap was reduced to 0.3 child in 2000 and has completely disappeared since 2006.

In the graph on the top right, we zoom in on the period 1992-2000 and look at age-specific fertility rates over five-year periods. Before the extension, fertility rates were higher in low coverage regions, except at a young age. The gap was the largest between ages 35 and 45: in high coverage regions, fertility rates steadily declined after age 35, whereas in low coverage regions, they remained as high as between ages 25 and 30. Women had more children in low coverage regions because they stopped later, not because they started earlier. After the extension, the situation changed. Fertility decreased across the board, and more in the low coverage regions for women between ages 30 and 45. The age profiles became much more similar between the two groups. The catch-up in total fertility rates is therefore driven by women between 30 and 45 years old in the mid-nineties. In other words, cohorts born between 1950 and 1965 responded the most strongly.

We reach the same conclusion when we look at completed fertility. The graph on the bottom plots the evolution of total number of children for women above age 45. For cohorts 1945-50, we observe a one-child gap between low and high pension coverage regions. This gap gradually shrinks and dies out after 1960-65. For youngest cohorts, there is no difference anymore. The raw data analysis gives us a first hint that those groups who were marginalized in the old pension system initially had a much higher fertility, and rapidly caught up after the extension. To go one step further, the next section formalizes the empirical strategy and provides support for the identification assumptions.

4 Empirical Strategy

We implement a difference-in-differences (DD), examining how groups with different initial needs for old-age support respond to the pension reform.

4.1 Period specification

The period specification is at the mother*year level and writes as follows:

$$
\begin{aligned}
Birth_{i,t} =& \gamma InitialNeeds_{c(i)} \times Post_t + \beta_t + \alpha_{c(i)} \\
&+ \nu Z_i + \delta X_{c(i),pre} \times Post_t + \mu_{w(i)} + \epsilon_{i,t}
\end{aligned} \tag{1}
$$

The outcome variable is $Birth_{i,t}$, a dummy equal to 1 if woman i surveyed in cluster c (=ethnicity*region) in wave w gave birth in year t. β_t, α_c and μ_w are respectively year, cluster and wave fixed effects. Z_i denotes time-invariant individual controls and $X_{c(i),pre}$ denotes cluster-level controls measured before the extension. $Post_t$ is a dummy equal to 1 after the year of the Pension Act, 1992. We use the $Post_t$ dummy in the main specification; we then allow the coefficient γ_t to vary year-by-year to look at the time profile. The explanatory variable of interest is $InitialNeeds_c$, the difference between the poverty line and the expected annual pension before the extension, expressed in thousand rands and measured at the level of cluster c. When we split the sample on the median initial needs, the "Low" and "High" needs groups have average needs of 0.28 and 0.43 thousand rands, respectively. We use survey weights and we cluster standard errors at the level of the treatment, i.e. ethnicity*region.[15]

We always control for year and cluster fixed effects to account for the evolution over time and for initial differences in levels across clusters in the most flexible way. In addition, we gradually include time-invariant individual controls as well as cluster-level controls measured before the extension and interacted with the $Post_t$ variable. We thus allow characteristics that are initially imbalanced between clusters to have varying effects over time.

Note that each wave provides information on the birth history of women aged 15 to 49 at the time of the survey. If we want to use retrospective data to compute comparable birth rates in the past, we need to pay attention to the age structure of the sample. For instance, the 2000 wave gives information about the probability to give birth in 1999 for women aged 14 to 48 at that time, in 1998 for women aged 13 to 47 etc. To keep the age structure constant over a given period of time, there is a tradeoff between going far back and retaining older age brackets in the sample. We chose to restrict the analysis to the last 10 years before wave w. Thus we are

[15] Alternatively, we can collapse the data at the cluster*year*wave level. This sample has 909 observations (35 clusters and 23 years, some of them exist in two DHS waves). We also tried two-way clustering at the ethnicity*region level and at the woman level.

able to construct birth rates for women aged 15 to 40 in every year between 1990 and 2012. This allows us to include a couple of years before the extension without losing women in the second half of the reproductive life.[16] As a robustness test, we considered the last 15 years before wave w (restricting the sample to women aged 15 to 35 in year t) to look at a longer period with more pre-reform years (1985-2012).

4.2 Cohort specification

The cohort specification has a similar structure, except for being at the mother level. We look at completed fertility for different cohorts, instead of birth rates for different years:

$$\begin{aligned} NbBirth_i =& \gamma InitialNeeds_{c(i)} \times Exposure_{k(i)} + \beta_k + \alpha_{c(i)} \\ &+ \nu Z_i + \delta X_{c(i),pre} \times Exposure_{k(i)} + \epsilon_i \end{aligned} \tag{2}$$

The outcome variable is $NbBirth_i$, the number of children born to woman i in cluster c and cohort k. We restrict to women aged 44 to 50 in wave w. We chose this specific age range to mitigate censoring issues and to have observations for each cohort between 1950 and 1969. We flexibly control for cohort fixed effects β_k and cluster fixed effects $\alpha_{c(i)}$.

The time variable is $Exposure_k$, the exposure of cohort k to the extension. We define the exposure in three ways: not exposed vs. fully exposed, partially exposed or cohort-by-cohort.[17] Figure 3 explains the construction of the exposure variable. The graph shows the theoretical exposure of different cohorts to the extended pension system. The old cohorts are not exposed, because they already had all the children during the old system. The young cohorts are fully exposed, because they started having children in the new system. The cohorts in between are partially exposed because they already had some children, but not all of them. The precise shape builds upon (i) the descriptive statistics, showing a strong response in the late reproductive period, and (ii) some data constraints, requiring enough observations not exposed and fully exposed. We assume a linear exposure for cohorts born between 1955 and 1965. In the Pre-Post specification, we compare cohorts born before

[16] We cannot use the 1992 wave in this specification because regions are defined in 3 instead of 12 categories so that we have only 9 clusters.

[17] Our setting is similar to Bleakley (2010), who uses eradication campaigns to study the impact of malaria in childhood on economic outcomes. We draw upon his article to design the cohort specification.

1955 and cohorts born after 1965. In the Partial specification, we keep all cohorts and define $Exposure_k = max(min(10; k - 1955); 0)/10$. In the Cohort-by-cohort specification, we do not impose any arbitrary shape, and allow γ_k, the coefficient on $InitialNeeds_c$ to vary flexibly.

4.3 Identification assumptions

We define the treatment as "needs for informal old-age support". These needs vary across groups before the pension reform and they disappear after the reform. To interpret γ as the impact of removing the needs, or in other words, the impact of providing formal old-age support, we make two assumptions: (i) in the absence of the reform, fertility would have evolved in the same way in all groups, and (ii) the impact is homogenous between groups. We argue that both assumptions are likely to hold, provided that Whites and Khomas are excluded from the final analysis sample.

Starting with parallel trends, we provide supporting evidence in Table 4. When we estimate the linear annual trend in birth rate, before, during and after the extension, we find a flat trend before and after, and a sharp decline during the extension. In column 1, we show that Whites have specific pre-trends: the coefficient on $Before \times Whites$ is significantly negative, indicating that the decline in fertility started before the extension. In column 2, we exclude Whites from the sample and show that Blacks in the Khomas region have different pre-trends and post-trends: coefficients on $Before \times Khomas$ and $After \times Khomas$ are significantly negative, indicating that fertility declined throughout the whole period. If we kept Whites and Khomas in the sample, it would therefore be difficult to argue that, between 1992 and 1997, fertility would have evolved similarly in all groups in the absence of the reform.[18] In column 3, we exclude Whites and Khomas and test whether pre-trends and post-trends are correlated with initial needs. They are not: coefficients on the interaction terms $Before \times InitialNeeds$ and $After \times InitialNeeds$ are not significant and very close to zero. Trends were different only *during* the extension, between 1992 and 1997: fertility declined more in groups that had initially higher needs for old-age support.

Another threat to the parallel trend assumption is that a *global* convergence in fertility rates of sub-populations in Namibia could have taken place precisely during

[18] In a robustness test, we show that coefficients remain stable in size and significance if we include Khomas in the sample and allow for a Khomas-specific trend.

the 1992-97 period. We cannot rule out this hypothesis by looking at trends before 1992 and after 1997. However, we can test whether we observe a convergence when we partition the population in a different way. Figure A.4 in Appendix plots the evolution of rural vs. urban areas, poor vs. rich households and non-educated vs. educated mothers in the final analysis sample; the gap in fertility tends to increase between 1992 and 2000. Therefore, it seems unlikely that fertility rates in low coverage regions would have converged towards those in high coverage regions in the absence of the pension reform.

The second important assumption is the homogeneity of the treatment effect. de Chaisemartin and D'Haultfoeuille (2018) show that, in order to interpret the DD coefficient in a fuzzy design, we have to assume that the treatment effect is stable in time and homogenous between groups.[19] This assumption is plausible when groups are similar in terms of characteristics that may influence the strength of the old-security motive for fertility. Table A.2 in appendix displays the statistical significance of differences in the descriptive statistics shown in Table 3. Khomas is an outlier: comparisons with other regions yield very large t-stats in virtually all dimensions, even when the sample is restricted to the Black population (columns 10 and 11). In contrast, there is no significant imbalance between regions with high and low initial pensions in terms of poverty and urbanization (column 12).[20] An additional argument in favor of an homogenous effect of pensions in the final analysis sample is that regions with high and low initial coverage have the same fertility level once pensions are extended to all groups (cf. Figure 2).

Potential threats to validity are related to other changes provoked by the independence of Namibia in 1990. First, a new dominant ethnic group emerged: the Owambos. We show in a robustness test that our results are not driven by differential trends between the Owambos and other ethnic groups. Second, the AIDS epidemics burst during the 1990s, so we control directly for the prevalence of AIDS in the preferred specification. Third, there was a strong economic growth during the 2000s, but this cannot explain the evolution of fertility between 1992 and 1997.[21]

[19] de Chaisemartin and D'Haultfoeuille (2018) propose estimators that do not rely on the assumption of homogenous treatment effects. Unfortunately, we cannot implement them because we do not observe, within groups, treated and untreated individuals.

[20] Although insignificant, the difference in urbanization rates (9% vs. 27%) is large in magnitude and we discuss extensively the implications of this difference in the Results section.

[21] Other reforms were discussed but never really implemented – land redistribution (Werner and Odendaal 2010, p.40), compulsory primary schooling (Wikan 2008, p.9,23), decentralization of health system (Zere, Mandlhate, Mbeeli, Shangula, Mutirus, and Kapenambili 2007, p.1), and women's rights (LeBeau, Iipinge, and Conteh 2004, p.33,36). Other social grants were introduced

More generally, changes in structural drivers of fertility could threaten the validity of our design if they were correlated with both (i) the timing of the pension reform and (ii) the initial needs for old-age support; in other words, if, between 1992 and 1997, these changes affected disproportionately groups previously disadvantaged by the pension system. In the analysis, we are able to account for several potential confounders: wealth (poverty rate and financial inclusion), area of residence (urbanization rate and reliance on agriculture), cost of children (schooling and child labor rates), access to health (distance to hospital), child mortality rates and maternal education. We show below that, on these dimensions, the gap between groups previously disadvantaged and groups previously advantaged is not bridged during 1992-1997; if anything, it tends to widen. So these confounders cannot explain the convergence in fertility.

5 Results

5.1 Period analysis

Table 5 displays the estimates of the period specification. In the first column, we include no control. As explained above, the coefficient on $Post * Initial\ Needs$ identifies the impact of providing formal old-age support on fertility. A reduction by 12 percentage points in the annual probability of birth (p-value=0.02) corresponds to 3 children over 25 years of reproductive life. Given the distribution of the initial needs variable, this implies a difference of 0.45 children between groups below and above the median needs. We examine potential non-linear effects at the end of this section.

In the next columns, we add controls block by block. The DD coefficient increases up to 17 percentage points and becomes significant at the 1% level when we control for indicators of urbanization and wealth in columns 2 and 3. When we additionally control for access to health, cost of children, AIDS, child mortality and maternal education, the DD coefficient remains stable. The magnitude corresponds to a reduction by 0.65 additional children in group with high initial needs compared to groups with low initial needs. Controlling for urbanization and wealth tend to raise our coefficient of interest because groups with high and low initial needs have diverged on these dimensions, as shown by Figure A.5 in Appendix. In 1993-94,

for disabled people or war veterans, but their scope is much more limited than the old-age pensions: old-age pensions account for 87% of the total budget dedicated to social grants (Subbarao 1998).

regions with a high pension coverage were as poor as the others, although they were less rural. The large fraction of urban poor is typical of townships built under the apartheid regime. Township residents had a better access to pensions, especially because they were more likely to have a bank account. But their economic opportunities were limited by a series of rules designed to ensure the availability of cheap labor in cities. These places benefited relatively more from the end of the apartheid and became gradually much less rural and poor than the rest of the country. As a result, the gap between high and low initial pension regions widened over time. Since urbanization and wealth are strong drivers of low fertility, these confounding dynamics create an attenuation bias in the baseline specification.

We then look at heterogeneity by age to test whether the hint from the descriptive analysis is validated. Figure 4 shows that the interaction term is negative and significant only for women above age 25. Interestingly for those women, adding controls makes little difference. The stability of the estimates supports the assumption that older women are not affected by confounding factors. In contrast, younger women seem to be affected by the divergence in wealth and urbanization happening at the same time as the extension of pensions. For them, the difference-in-differences coefficient is slightly positive in the absence of controls and becomes null when we add controls. This means that fertility in early reproductive years decreased more in regions with lower initial needs for old-age support, because these regions experienced a faster structural transformation. This suggests that urbanization and wealth impact the onset of fertility in the opposite direction as the effect of pensions that we try to highlight.

In order to further examine the dynamics of the catch-up, we replace the term $Post_t \times InitialNeeds_c$ in Equation 1 by a sum of interactions between $InitialNeeds_c$ and an indicator for year t or $t-1$. Figure 5 plots the coefficient on each interaction term together with the 90% confidence intervals. The omitted category is 1991-92, the last period in which fertility decisions were made before the Pension Act. Graph (a) at the top uses the main sample and 10 years of retrospective data, while graph (b) at the bottom restricts the sample to women aged 15 to 35 and exploits 15 years of retrospective data. Both graphs show that, in pre-reform years, the correlation between initial needs for old-age support and fertility was the same as in 1991-92. Coefficients become more and more negative during the extension. After 1997-98, they fluctuate around $-0.2 p.p.$ in graph (a) and $-0.15 p.p.$ in graph (b); the magnitude is smaller in graph (b) because the older age group (36-40 y.o.), who responded the most strongly and quickly to the reform, is no longer in the sample. These patterns confirm that the differential decline in fertility precisely coincides with the extension of old-age pensions, providing additional support for the identification assumptions.

5.2 Cohort analysis

Table 6 displays the estimates of the cohort specification: the pre-post analysis is shown in Panel A and the partial analysis exploiting all cohorts is shown in Panel B. Both lead to the same conclusion. Younger cohorts, who were exposed to the extended pension system, have fewer children than older cohorts, and the decline is stronger in groups where older cohorts had higher needs for old-age support. The coefficient on $Exposure * Initial\ Needs$ is significant at conventional levels in both analyses without the controls. It increases and becomes significant at the 1% level when we control for the confounders. The magnitude ranges between 3.1 and 5.0 children, depending on the specification. They imply a differential decline by 0.5-0.75 children between "High" and "Low" initial needs groups. Compared to the period specification, estimates are slightly larger because they take into account adjustments made after age 40. However, the cohort analysis gives noisier estimates due to the smaller sample size.

In order to examine the effect cohort-by-cohort, without imposing any functional form on the exposure variable, we replace the term $Exposure_k \times InitialNeeds_c$ in Equation 2 by a sum of interactions between $InitialNeeds_c$ and an indicator for cohort k or $k-1$. We report the coefficients in Figure 6. The pattern is in line with the theoretical graph shown in Figure 3. First, for all cohorts born before 1955, we assumed that $Exposure = 0$. We find that the impact of initial needs on completed fertility is indeed the same for cohorts born in 1950-51 and 1952-53 as for the omitted category 1954-55. Second, for all cohorts born after 1965, we assumed that $Exposure = 1$. We find that since 1965, the coefficients on initial needs are always significantly lower than in 1954-55. Third, for the partially exposed cohorts between 1956 and 1964, fertility decreases gradually more where needs were initially higher, supporting the assumption of a progressive exposure. The catch-up in completed fertility was partial for women in their late thirties during extension, and complete for women in their early thirties. This is consistent with Chatterjee and Vogl (2018), who argue that only economic fluctuations occurring after the age of 30 influence completed fertility in developing countries. What happens earlier is compensated later in life.

5.3 Robustness

We conduct several robustness tests, varying the sample and the model specification. Coefficients are reported in Tables A.3 and A.4 in Appendix. They remain stable when we (i) restrict to Northern regions; (ii) restrict to rural households; (iii) allow for an Owambo-specific trend; (iv) include Khomas in the sample and control for a Khomas-specific trend; and (v) exploit only the regional variation in coverage, and not the ethnic variation in amount. Moreover, coefficients remain negative and significant if we exclude one region at a time. These tests rule out the possibility that our results are driven by differential dynamics between the North and the South, between rural and urban households, between different ethnic groups, or between one region and the rest of the country.

A remaining potential concern is migration. Many people moved during the 1990s, once restrictions put in place during the apartheid were lifted. In our sample, 28% of women surveyed in 2000 had migrated over the past 10 years. This could jeopardize the interpretation of our estimates as causal effects in case of differential migration within or between regions. Within regions, rural to urban movements play against finding a pension effect because the fraction of rural households started lower and decreased faster in regions with higher initial pension coverage (cf. Figure A.5 in Appendix). All else equal, rural-to-urban migration would therefore predict a divergence in fertility. Between regions, migration could potentially explain the catch-up observed in Figure 2 in two situations. First, if people with low fertility moved to low coverage regions, while people with high fertility moved to high coverage regions, this would generate a convergence. This is not the case in the data: migrants and natives have the same fertility.[22] Coherently, we still observe a catch-up when we exclude those who migrated in the 10 years preceding the survey, as shown by Figure A.6 in Appendix. The second situation is out-of-the-sample migration. If people with many children migrated away from low coverage regions to other countries or to the capital city, we would not observe them in 2000. This could rationalize the descriptive statistics, but not the econometric results, which are based on retrospective data. In the regressions, we observe a woman in wave 2000 and look at her birth history. The correlation between pension and fertility before 1992 is estimated on women who are, by construction, in the sample in 2000. When we exclude the migrants from the regressions, the difference-in-differences

[22] 6.17 children for migrants compared to 6.36 for natives in low initial coverage regions (p-value of the difference = 0.79) and 5.78 children for migrants compared to 5.79 for natives in high initial coverage regions (p-value of the difference = 0.99).

coefficients remain the same in magnitude but are imprecisely estimated since we lose many observations. All in all, the catch-up is not the result of population movements.

To further rule out composition effects, we consider an alternative specification for the period analysis which includes mother fixed effects. The baseline specification studies the evolution of the birth rate of different women, keeping the age structure constant. A specification with mother fixed effects examines the birth rate of the same woman, at different ages. This reduces power because only women older than 15 in 1992 contribute to identification; for the others, there is no within-mother variation in $Post_t$. Figure A.7 in Appendix reports the difference-in-differences coefficient, without and with controls, for different sub-samples. They are always negative, but only significant at 5% when we focus on women older than 25 in 1992. The older the sample of mothers, the higher the coefficient and also the higher the standard errors since we have fewer observations. This is consistent with our results without mother fixed effects.

5.4 Mechanisms

So far, all results point to the following conclusion: in presence of pensions, women have fewer kids during the late reproductive life. The relevant margin seems to be stopping rather than starting or spacing. Figure 7 provides evidence supporting this idea.

The graphs on the top show a strong convergence in self-reported fertility preferences between 1992 and 2000.[23] In regions with low initial pensions before the extension, the average ideal number of births was higher, and the fraction of women who wanted to stop having children was lower. This is no longer true after the extension. Both types of regions show very similar numbers during the post-period 2000-2013. These patterns support our interpretation of a change in desired fertility, and are inconsistent with stories emphasizing changes in ability to control births.

The graphs on the bottom plot the evolution of age at last birth and age at first birth. Contrary to self-reported preferences that can change from one day to the next, the average ages at last and first birth display some inertia. There is a stock of women for whom these ages are already determined and do not vary across waves. We therefore expect a more progressive convergence. This is what we find

[23] We can only look at preferences reported by women, because these questions were not asked to men in 1992.

for age at last birth: between 1992 and 2013, it gradually decreases in low coverage regions down to the level observed in high coverage regions, where it remained stable throughout the period. For age at first birth, we observe a different pattern. There is also a convergence, but the starting points are reverse: the age at first birth is higher in low coverage regions, meaning that women start having children later. Over time, the age is slightly decreasing, whereas in high coverage areas, there is a slight increase. This rules out the possibility that the decrease in total fertility in low coverage regions is driven by factors influencing the onset of births, like changes in female schooling or in marriage practices. The patterns in age at first birth are in line with our previous results on younger women suggesting that the onset of fertility was affected by confounding factors that go in the opposite direction.

All in all, the negative impact of social pensions on fertility operates through a reduction in how many children women want to have. This is the main take-away in terms of policy, although this is not enough to definitively nail down the old-age security motive. Besides the insurance mechanism, pensions can indeed influence fertility in two ways: through female labor supply and household income. Case and Deaton (1998) find that the extension of social pensions in South Africa had a substantial effect on household welfare, not only for the elderly but also for other members. One reason is that many families live with a pensioner and directly benefit from the transfer. Moreover, pensioners may have more time and resources to devote to their grand-children, which can potentially prompt women into working outside their homes, or even temporarily migrate for work.[24]

The female labor supply channel is unlikely to explain our results for two reasons. First, Jensen (2003) shows that, in the similar South African context, old-age pensions had no effect on labor supply, migration, or co-residence. Comparing men and women, above and below the age eligibility threshold, before and after the extension, he finds that the proportion of migrants, the earnings of members who live permanently in the household as well as the earnings of temporary migrants, and the number of members, are the same for households with and without old-age pension recipients. Second, in our sample, there is no catch-up in female labor force participation between high and low initial coverage regions. As shown by the left graph in Figure A.8 in Appendix, the gap has even widened between 1992 and 2000.

Turning to the income channel, it is unclear in which direction a rise in household earnings would influence fertility. On the one hand, the macro literature tends to

[24]A contributory pension system could also raise female labor force participation by making employment more attractive. But the Namibian system is non-contributory.

model children as normal goods and provides evidence from various settings that fertility is pro-cyclical (Chatterjee and Vogl 2018). This would predict a positive impact of pensions on fertility. On the other hand, the experimental literature on cash transfers has produced mixed results: positive, negative and insignificant impacts on fertility have been found (see Khan, Hazra, Kant, and Ali (2016) for a survey). The only consensus seems to be that unconditional transfers to young women delay first births. There is no evidence that cash transfers reduce completed fertility among older women, so the income effect alone fails to account for our results.[25] In addition, we can try to isolate the old-age security motive by restricting the analysis to households where no one is eligible for pensions. Although co-residence is potentially endogenous, the scope for this concern is limited because there is no differential change between high and low initial coverage regions. In our sample, roughly 20% and 40% of women live with a person older than 60 years old in high and low regions, respectively. These shares are stable in the 2000, 2006 and 2013 waves that we use in the retrospective analysis, and they evolved in a parallel way between 1992 and 2000. Table A.5 in Appendix estimates the period specification, without and with controls, for households living or not with an elderly. The difference-in-differences coefficient is negative and significant in the absence of an elderly, and larger than in the whole sample. When an elderly lives in the household, the coefficient is also negative, but smaller in magnitude and less precisely estimated. This suggests that, for them, the old-age security motive is partly offset by an income effect that raises fertility. In contexts where we expect pension benefits to be shared with younger generations, the reduction in fertility is therefore likely to be less strong.

5.5 Non-linearities

We take advantage of the distribution of the $InitialNeeds$ variable to investigate whether the relationship between needs for old-age support and fertility is linear. The distribution is trimodal, allowing us to create three categories: Low, Medium and High initial needs, consisting of groups with average needs of respectively 0.23 thousand rands (corresponding to an initial coverage between 50 and 65%), 0.32

[25] It is sometimes argued that cash transfers can reduce unwanted births by making modern contraceptives more affordable. This is not the case in our sample. The right graph in Figure A.8 in Appendix shows that the fractions of unwanted and unplanned births were the same in high and low coverage regions in 1992, so they fail to account for the initial differences in realized fertility. And they tend to increase over time especially in low coverage regions, which is inconsistent with the idea of an improved control.

thousand rands (coverage between 40 and 50%) and 0.43 thousand rands (coverage between 30 and 40%). We estimate a variant of Equations 1 and 2, where the continuous $InitialNeeds$ variable is replaced by the categorical variables, "Medium" being the omitted category. If the impact of providing old-age support was linear, we should get DD estimates of opposite sign on "High" and "Low" dummies with a magnitude roughly equal to $-/+0.017$ p.p. in the period specification and $-/+0.5$ children in the cohort specification.[26]

Instead, we find insignificant coefficients on the Low dummy, whereas coefficients on the High dummy are twice as large as expected and significant at 1% in the period analysis and at 5% in the partial cohort analysis (see column 9 in Table 5 and column 3 in Table 6). This suggests that the impact of pension on fertility is very strong at low initial levels of protection and disappears as protection improves. Improving coverage from roughly one third to one half and benefits from 1 to 1.2 times the poverty line (moving from "High" needs to "Medium" needs) reduces fertility by an order of magnitude of one child, while further raising the coverage and the benefits makes no detectable difference.

6 Discussion

Which lessons can be learnt from the Namibian experience? In this section, we relate the non-linear reduced-form estimates to the seminal model of fertility decisions developed by Cain (1983) and discuss the extent to which our findings can be extrapolated to other countries in Sub-Saharan Africa.

6.1 A Lexicographic Safety First model

The non-linear impact of pensions on fertility is consistent with the Lexicographic Safety First (hereafter LSF) model of fertility decisions. Drawing on the bounded rationality literature, Cain (1983) postulates that couples, instead of acting as global optimizers, follow a sequential process. First, they determine the minimum number of children necessary to be safe in old-age, the "target". Second, they may have additional children depending on the short-run costs and benefits, including for example female opportunity cost of time, schooling expenses and child labor.

[26] This is because the difference between "Low" and "Medium" needs (resp. "High" and "Medium" needs) is roughly equal to -0.1 (resp. +0.1) thousand rands; and the specifications with a continuous variable predict that an increase by 1 thousand rands reduces the annual probability of birth by 0.17 and completed fertility by around 5 children in the equalized system.

According to Cain, the main objective of couples is to prevent consumption from falling below subsistence level. If the elderly entirely depend on transfers from their children, their first-order concern is to ensure that enough children are born to avoid destitution in old-age.[27]

Ray (1998, Chap.9) formally defines the target as the smallest n such that $(1-s)^n \leq q$ where $(1-s)$ is the probability of child default and q is the highest level of destitution risk that parents are willing to take. Children may default on their parents for several reasons: premature mortality, migration, poverty or neglect. Child default is assumed to be independent across children and one child is enough to provide for the parents; hence $(1-s)^n$ is the probability of falling below subsistence level for parents with n children. As an example, if $s = 0.3$, couples need to have at least seven children to face a destitution risk lower than 10%; if $s = 0.5$, they only need four children. The second stage of the decision process is a standard cost-benefit analysis with increasing marginal costs and/or decreasing marginal benefits of young children. Denoting n^{SR} the optimal number of children based on this short-run analysis and n^{LR} the long-run target, couples choose to have $max(n^{SR}; n^{LR})$ children.

What happens if we introduce old-age pensions in this framework? Suppose that cash transfers to the elderly are larger than the subsistence level, but only paid with probability p. The new risk of destitution in old-age is $(1-s)^n \times (1-p)$. Figure A.9 in Appendix displays the optimal number of children for different values of the parameters. An increase in p lowers the target and reduces the elasticity of the target with respect to s. For instance, if $p = 0.5$, the new target for a destitution risk lower than 10% is five when $s = 0.3$ and three when $s = 0.5$. If pensions are almost certain, $n^{LR} = 0$ irrespective of s and fertility is only driven by n^{SR}. Therefore, the key prediction of this model is that the old-age security motive for fertility is quantitatively important as long as the levels of protection granted by the state (p) and by each child (s) are low.

6.2 External validity

We argue that our results shed light on the impact of potential extensions of social pension systems on fertility decline in Sub-Saharan Africa. As shown by Figure A.1 in Appendix, five countries currently have undeveloped systems: Mozambique,

[27]Note that the LSF model, mostly used in demography and sociology, can be nested inside a more general class of model featuring strongly diminishing marginal returns to consumption. We use the model to illustrate when the effect of pensions can be non-linear.

Nigeria, Uganda, Tanzania and Kenya. The characteristics of these systems are roughly similar to the "High" initial needs for old-age support in Namibia: pension coverage is below 25% and benefits are between 10 and 15% of average income. Fertility levels are also about the same as in the areas with high initial needs, at five to seven children per woman, and they share a similar pro-natalist culture. Therefore, we can use the Namibian non-linear estimates to predict that fertility would drop by around one child if these countries improved pension coverage to reach at least one half of the elderly and raised the benefits to at least the poverty line level.

In the vast majority of Sub-Saharan African countries today, there is no social pension system. We do not observe any group completely excluded from the system in Namibia, so we do not study changes at the extensive margin. If we want to predict what would happen after the *introduction* of a new system, one option is to rely on the LSF model, although the prediction will admittedly be speculative. The non-linear relationship suggests that the fertility response to the introduction of pensions would be larger than the one we estimated in Namibia. The response could be as high as a reduction by two children, from seven to five, as illustrated by the simulation with $s = 0.3$ and $n^{SR} = 5$ (see Figure A.9 in Appendix).[28]

In terms of timing, our results point to a rapid adjustment. If the response was concentrated in the early reproductive period, by postponing the age at first birth, we should wait for 20-25 years to observe a decrease in completed fertility. This is not the case: women respond by bringing the age at last birth forward. The decrease is likely to be observed among older cohorts immediately after the extension, and to be completed within a decade. There is a caveat, though. In the nineties in Namibia, the relatively strong rule of law, the decade-old pension system and the post-apartheid spirit contributed to make universal social pensions credible. In other contexts, fertility adjustments will not happen until people trust the system.

7 Conclusion

This paper provides quasi-experimental support for the old-age security motive for fertility. Exploiting variation in time, regions and ethnic groups in Namibia, we show that social pensions have a large negative impact on fertility: women stop

[28]This is our preferred simulation because it fits the one child difference between "High" and "Medium" groups and the absence of difference between "Medium" and "Low" groups in Namibia.

having children at a younger age. This has important policy implications. The majority of Sub-Saharan African governments target a reduction in fertility and have no pension system. Our results suggest that introducing or expanding social pensions could go a long way in fostering fertility decline. Including the fertility dimension in the cost-benefit analysis of pension systems is therefore crucial. On the benefits side, contributing to reach population targets is an argument in favor of social pensions. On the costs side, curbing population growth might affect the sustainability of the system.

Becoming aware of the fertility responses to old-age pensions opens the way for new insights into the efficiency of pension systems. What is the optimal mix of coverage and benefits to reach a given fertility level? Do people give birth to a socially efficient number of children when a formal system allows them to pool the risks of child default? Moreover, Namibia is an interesting context to explore the relationship between social pensions, gender norms related to caregiving, and gender preferences, in the same vein as Bau (2019). The presence of both patrilineal and matrilineal ethnic groups could allow researchers to test whether this relationship depends on inheritance rules. Finally, the extension of Namibian pensions can potentially be used to study other economic consequences of old-age pensions like multi-generational co-residence, transfers and labor supply. The impact on human capital would be particularly fascinating. In theory, the income effect and the quality-quantity trade-off would predict an increase in parental investments, while changes in incentives to bring up successful children would predict the opposite. Information on parental investments in the early nineties is needed to convincingly test which forces prevail in the Namibian context. Recent extensions of pension systems in other countries may also provide suitable empirical designs to study these questions.

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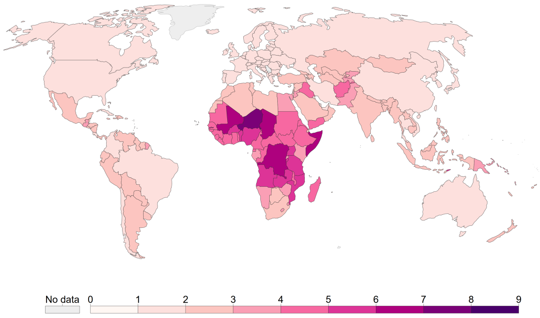
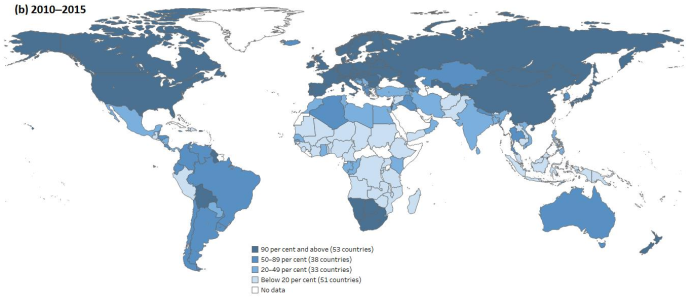
Figures and Tables

Figure 1: Fertility and pensions in the world

Current fertility rate

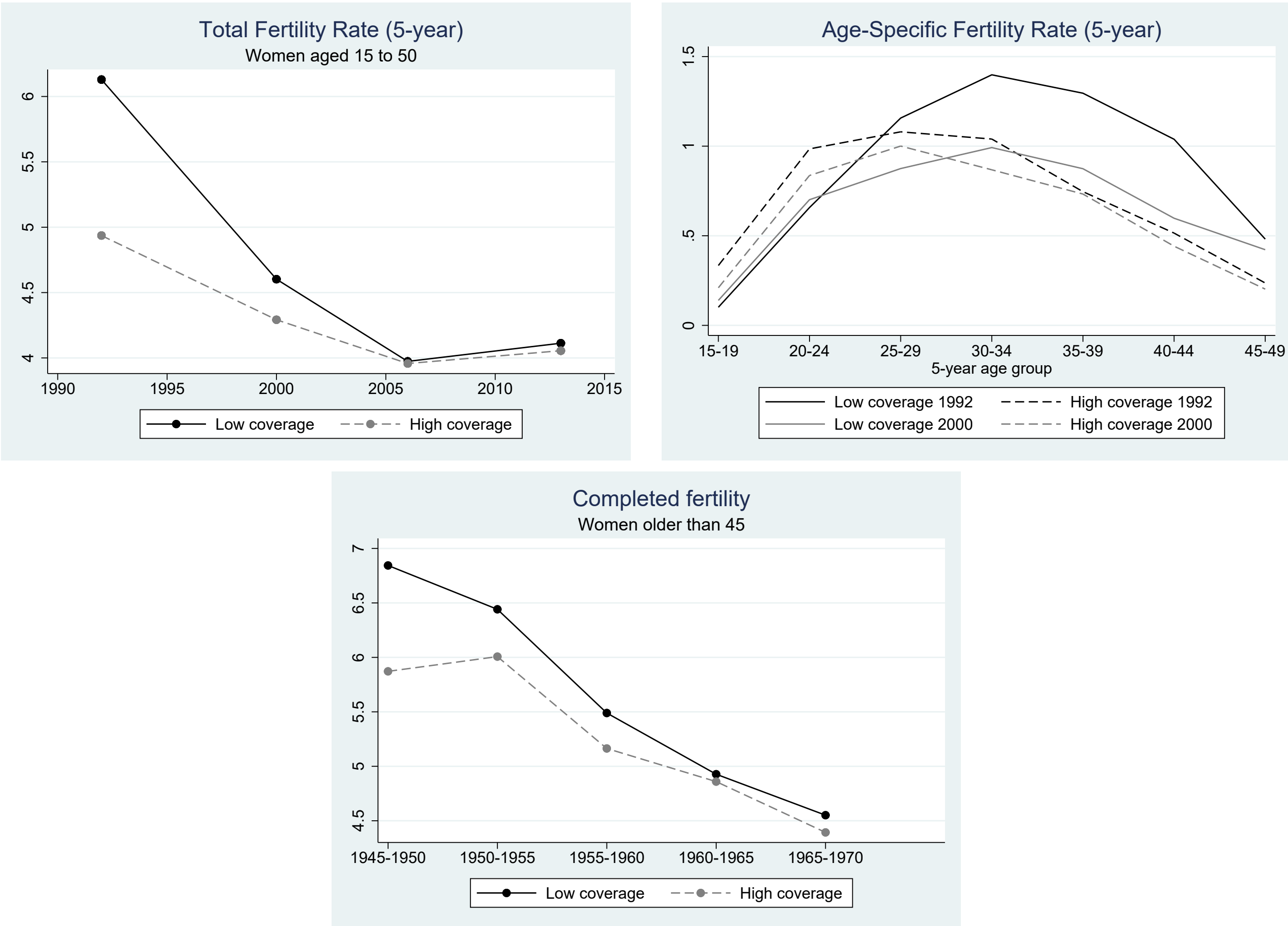


Current coverage of pensions



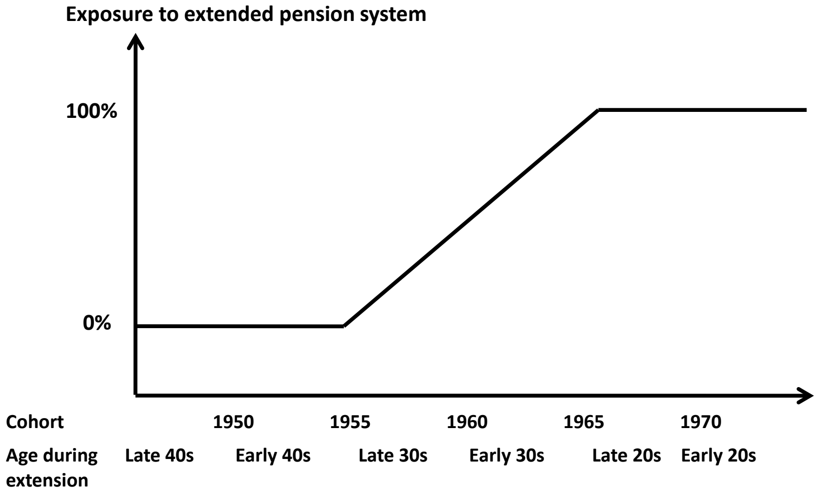
The map on the left shows the average number of births per woman in 2017, based on estimates from United Nations (2017). Link: https://ourworldindata.org/grapher/children-born-per-woman?year=2017
The map on the right shows the percentage of the population above statutory pensionable age receiving any type of old-age pension in 2010-15, based on estimates from International Labour Organization (2018). Link: http://www.social-protection.org/gimi/gess/RessourceDownload.action?ressource.ressourceId=54657

Figure 2: Evolution of fertility, by initial pension coverage



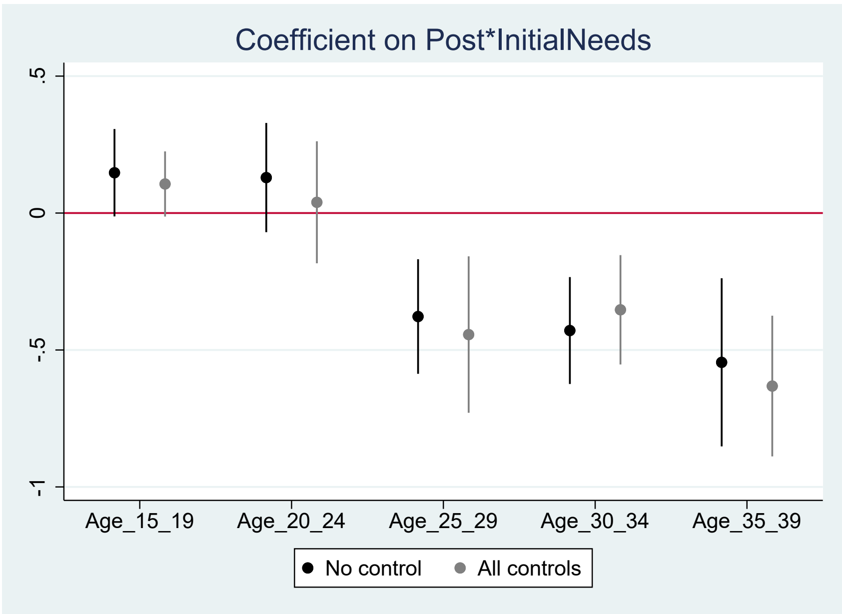
The graphs plot the evolution of several measures of fertility in regions with low (North, solid lines) and high (Northeast and South, dashed lines) initial pension coverage. The extension of pensions took place between 1992 and 1997. Data: DHS. Sample: non-White women aged 15 to 50 in the fertility rates graphs, older than 45 in the completed fertility graph.
The graph on the top left shows Total Fertility Rate, a synthetic rate measuring how many births a woman would have if she was subject to current age-specific fertility rates at all ages throughout her reproductive life. Own computation based on age-specific birth rates in the five years preceding each survey.
The graph on the top right shows Age-Specific Fertility Rates, before the extension (in 1992, black lines) and after the extension (in 2000, grey lines). The Age-Specific Fertility Rate measures how many births an average woman has during five-year periods. Own computation based on age-specific birth rates in the five years preceding each survey.
The graph on the bottom shows completed fertility, by mother's cohort. When the extension of pensions took place during the mid-nineties, women born in 1945 were around age 50 while women born in 1970 were around age 25. Completed fertility is the total number of births born to the average woman.

Figure 3: Theoretical exposure to extended pensions, by women's cohort



The figure shows the theoretical exposure of different cohorts to the new pension system. Depending on their age in the mid-nineties, they were always exposed, never exposed, or only during a fraction of their reproductive life. The exact formula is: $Exposure_k = max(min(10; k - 1955); 0)/10$.

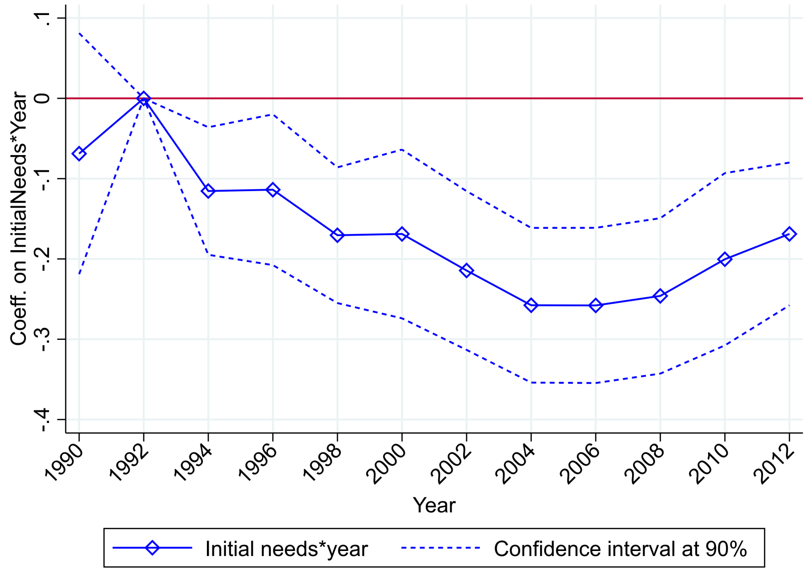
Figure 4: Period analysis – difference-in-differences coefficient, by age group



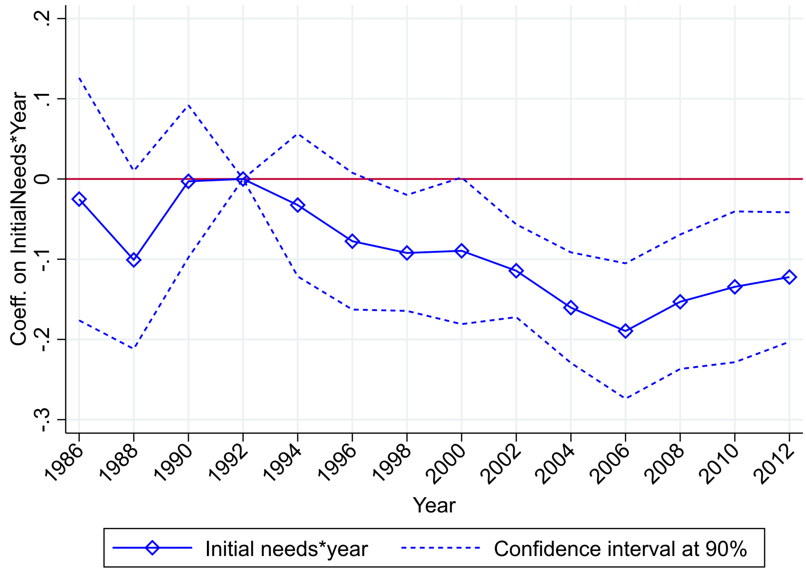
The graph plots γ, the coefficient on the interaction between $InitialNeeds_c$ and $Post_t$ in Equation 1, for different age groups, controlling or not for the full set of controls (urban, wealth, access to services, cost of children, AIDS, U5 mortality, maternal education). The outcome variable is the probability of birth. The vertical lines represent the confidence intervals at 95%.

Figure 5: Period analysis – impact of initial needs on probability of birth, by year

(a) Main sample: using 10 years of retrospective data

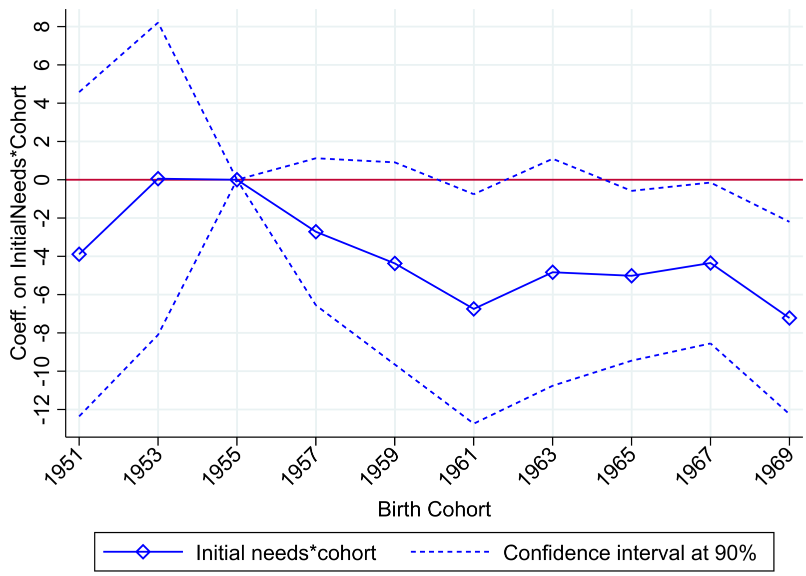


(b) Robustness: using 15 years of retrospective data



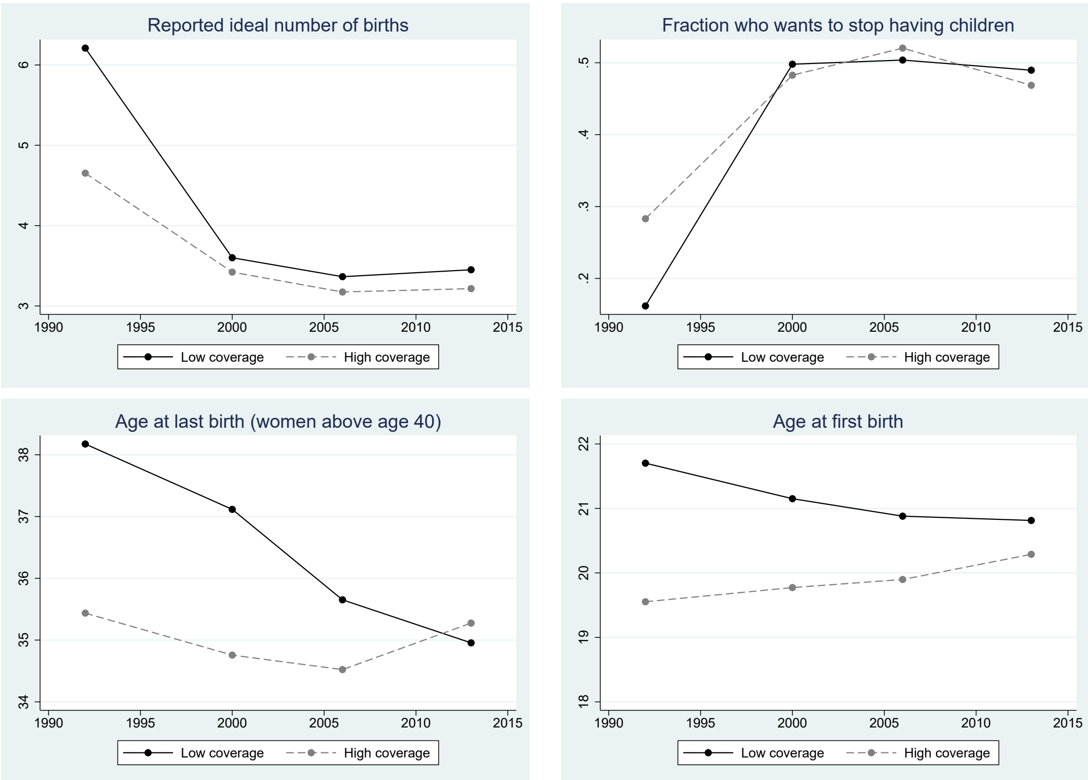
We replace the term $\gamma Post_t \times InitialNeeds_c$ by $\sum_{j=start}^{end} \gamma_{2j} \times \mathbf{I}\{2j-1 \leq Year_t \leq 2j\} \times InitialNeeds_c$ in Equation 1, controlling for year and cluster fixed effects and the full set of controls (urban, wealth, access to services, cost of children, AIDS, U5 mortality, maternal education). We pool together years t and $t-1$ to increase precision. The omitted category is 1991-1992, the last period in which fertility decisions were made before the Pension Act. The graph plots γ_t and the confidence intervals at 90%. Graph (a) uses the main sample, women aged 15 to 40 in year t, and 10 years of retrospective data covering the period 1990 ($start = 995$) to 2012 ($end = 1006$). Graph (b) uses 15 years of retrospective data covering the period 1985 ($start = 993$) to 2012 ($end = 1006$) and restricts the sample to women aged 15 to 35 in year t.

Figure 6: Cohort analysis – impact of initial needs on completed fertility, by cohort



We replace $\gamma Exposure_k \times InitialNeeds_c$ by $\sum_{j=975}^{984} \gamma_{2j+1} \times \mathbf{I}\{2j \leq Cohort_k \leq 2j+1\} \times InitialNeeds_c$ in Equation 2, controlling for cohort and cluster fixed effects and the full set of controls (urban, wealth, access to services, cost of children, AIDS, U5 mortality, maternal education). We pool together cohorts k and $k-1$ to increase precision. The omitted category is 1954-1955, the last cohort not exposed to the extended pension system. The graph plots γ_k and the confidence intervals at 90%.

Figure 7: Mechanism – change in stopping behaviors



The graphs show the evolution of ideal number children (top left), proportion of women who want to stop having children (top right), age at last birth (bottom left) and age at first birth (bottom right) in regions with low (North) and high (Northeast and South) initial coverage.

Table 1: Variation in pension benefits across ethnic groups

	Pension per month (1)	Ratio to poverty line (2)	Ratio to average income (3)	Fraction of population (4)
Before the extension				
White	R382	7.0	0.96	6%
Coloured	R192	3.5	0.48	6.5%
Baster	R150	2.8	0.38	1%
Tswana	R100	1.8	0.25	0.5%
Damara	R75	1.4	0.20	10%
Herero; Nama	R65	1.2	0.16	12%
Owambo; Kavango; Caprivi	R55	1	0.14	64%
After the extension				
All	R160	3	0.40	100%

The table shows the official amount of social pensions paid to different ethnic groups, in absolute terms and relative to the poverty line and the average income, before (1989) and after (1996) the Pension Act. Source: Devereux (2001).

Table 2: Variation in pension coverage across regions

		Fraction of recipients among eligible people		Fraction of population
		All	Blacks only	
		(1)	(2)	(3)
Before the extension				
South region	Hardap	78%	48%	4.7%
	Karas	76%	61%	4.4%
Central region	Erongo	72%	57%	3.8%
	*Khomas	66%	15%	11.9%
Northeast region	Caprivi	65%	65%	6.3%
	Omaheke	60%	52%	3.7%
	Otjozondjupa	48%	36%	7.2%
North region	Kunene	51%	45%	4.6%
	Oshikoto	49%	46%	9.1%
	Kavango	39%	38%	8.4%
	Ohangwena	36%	35%	12.8%
	Omusati	34%	34%	13.6%
	Oshana	31%	31%	9.5%
Namibia		49%	42%	100%
After the extension				
Namibia		88%	n/a	100%

The table shows the effective coverage of social pensions in different regions, before (1993-94) and after (1998) the outsourcing of pension delivery, for all ethnic groups and for Blacks only. *Khomas is the region of the capital city (Windhoek).
Source: all ethnic groups: Subbarao (1998); Blacks in 1993-94: own computation based on the assumption that the coverage for Whites was 100% (see Figure A.3 in Appendix for more details).

Table 3: Descriptive statistics before the extension of pensions

	All			Blacks only		
	High coverage	Low coverage	Khomas	High coverage	Low coverage	Khomas
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Individual characteristics						
Ethnicity						
% European/Afrikaans	0.15	0.00	0.39	-	-	-
% Damara	0.25	0.01	0.22	0.29	0.01	0.36
% Herero	0.18	0.00	0.09	0.21	0.00	0.15
% Owambo/Kavango/Caprivi	0.42	0.98	0.30	0.49	0.99	0.49
Education (higher than primary)	0.43	0.31	0.35	0.33	0.31	0.28
Panel B: Regional controls						
% poor	0.32	0.38	0.07	0.36	0.38	0.10
% living in urban areas	0.33	0.10	0.88	0.27	0.09	0.83
% econ. active in agriculture	0.44	0.59	0.05	0.50	0.60	0.07
% with bank/saving account	0.40	0.28	0.68	0.36	0.28	0.64
% 10-14 economically active	0.07	0.07	0.00	0.08	0.07	0.00
% 5-14 ever in school	0.86	0.93	0.94	0.84	0.93	0.93
% with hospital/clinic $>$ 1 hour-walk	0.51	0.50	0.15	0.56	0.50	0.14
Prevalence of HIV	0.10	0.09	0.07	0.10	0.09	0.07
Infant mortality rate	0.08	0.06	0.05	0.08	0.06	0.05
Observations NHIES	11204	10874	2906	9194	10841	1743
Observations DHS	2711	2149	561	2213	2137	499

The table displays the mean of important control variables, separately for regions with low (North) initial pension coverage, regions with high (Northeast, South and Erongo) initial pension coverage and the region of the capital city Khomas. In the first three columns, all ethnic groups are included. In the last three columns, only Blacks are included. The sample of interest for our analysis excludes Whites and Khomas, and corresponds to columns 4 and 5. Table A.2 in Appendix indicates the statistical significance of pairwise differences. The variables are constructed as follows: education is a dummy equal to 1 if the mother completed more than primary school in DHS 1992; urban areas are defined by the Local Authorities Act No23 1992; agricultural sector is defined according to the classification of activities in NHIES 1993-94; a household is classified as poor if they reported spending more than 60% of their budget on food in NHIES 1993-94; the schooling rate measures the share of 5-14 y.o. reporting that they ever attended school in NHIES 1993-94; the child labor rate measures the share of 10-14 y.o. reporting that they worked for a wage or family gain during the previous week in NHIES 1993-94; walking time to nearest hospotal/clinic is measured in minutes in NHIES 1993-94 and we created a dummy equal to 1 if the time is higher than 60 minutes; the prevalence of HIV is measured by the National HIV survey in 1994; the under-5 mortality rate is constructed from birth and death histories in DHS 1992.

Table 4: Fertility trends, before, during and after the extension

Sample	Include Whites and Khomas (1)	Exclude Whites, include Khomas (2)	Exclude Whites and Khomas (3)
Before extension (1990-1992)	-0.285	-0.200	-0.556
	(0.435)	(0.473)	(0.438)
During extension (1992-1997)	-0.769	-0.861	-0.211
	(0.104)	(0.114)	(0.311)
After extension (1997-2012)	-0.065	-0.017	-0.018
	(0.046)	(0.034)	(0.123)
Before × Whites	-0.003		
	(0.001)		
During × Whites	0.311		
	(0.163)		
After × Whites	0.025		
	(0.073)		
Before × Khomas		-0.002	
		(0.001)	
During × Khomas		0.405	
		(0.352)	
After × Khomas		-0.174	
		(0.048)	
Before × InitialNeeds			0.004
			(0.004)
During × InitialNeeds			-1.646
			(0.766)
After × InitialNeeds			-0.006
			(0.350)
Observations	180473	155774	141637
Clusters	51	38	35

We estimate the linear annual trend in birth rate separately for three periods: 1990-1992, 1992-1997, and 1997-2012. We multiply coefficients by 100 to improve the readability of the table. 1992 to 1997 is the period of transition from the old pension system, with unequal benefits and coverage, to the new system, with equalized replacement rate and universal coverage. $Whites$ is a dummy for people defined as White, Coloured or Baster during the apartheid regime. $Khomas$ is a dummy for the region of the capital city. $InitialNeeds$ is the difference between the poverty line and the expected annual pension for a given ethnic group in a given region before the extension, in thousand rands (average in the bottom half=0.28; average in the top half=0.43). Sample: women aged 15 to 40 in year t; Whites are excluded in columns 2 and 3, Khomas is excluded in column 3. Data: DHS. OLS regression. Standard errors are clustered at the region*ethnicity level. Coefficients on $Before \times Whites$, $Before \times Khomas$ and $After \times Khomas$ are significantly different from zero, indicating that Whites and Khomas have different pre- and post-trends. The sample of interest for our analysis excludes Whites and Khomas, and corresponds to column 3: only the coefficient on $During \times InitialNeeds$ is significantly different from zero.

Table 5: Period analysis – Birth rate

	DEPENDENT VARIABLE: PROBABILITY OF BIRTH								
	Baseline	+ urban	+ wealth	+ access to services	+ cost of children	+ AIDS	+ mortality	+indiv. controls	Non-linear effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*InitialNeeds	-0.123	-0.151	-0.171	-0.170	-0.160	-0.165	-0.177	-0.173	
	(0.051)	(0.045)	(0.051)	(0.048)	(0.045)	(0.047)	(0.045)	(0.043)	
Post*LowInitialNeeds									-0.008
									(0.016)
Post*HighInitialNeeds									-0.031
									(0.010)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	141637	141637	141637	141637	141637	141637	141637	141637	141637
Clusters	35	35	35	35	35	35	35	35	35
R^2	0.007	0.007	0.007	0.007	0.007	0.007	0.007	0.015	0.015

Data: DHS and NHIES. Level of observation: woman*year. Sample: women aged 15 to 40 in year t; White ethnic group and Khomas region are excluded. OLS regression. Standard errors are clustered at the region*ethnicity level. $InitialNeeds$ is the difference between the poverty line and the expected annual pension for a given ethnic group in a given region before the extension, in thousand rands (average in the bottom half=0.28; average in the top half=0.43). *Post* is a dummy for years after 1992 (date of the Pension Act). Full set of controls include: urban (share living in urban areas, share active in the agricultural sector), wealth (poor, savings account), access to services (distance to hospital), cost of children (schooling rate 5-14, share of 10-14 economically active), AIDS, under-5 mortality, indiv. time-invariant controls (maternal education). In column 9, the categories “Low”, “Medium” and “High” initial needs consist of groups with average needs of respectively 0.23 thousand rands (corresponding to a pension coverage between 50 and 65%), 0.32 thousand rands (coverage between 40 and 50%) and 0.43 thousand rands (coverage between 30 and 40%). Medium initial needs is the omitted category and the regression includes the full set of controls.

Table 6: Cohort analysis – Completed fertility

	DEP VAR: NUMBER OF BIRTHS		
	Baseline	Full set of controls	Non-linear effect
	(1)	(2)	(3)
Panel A			
Pre-post analysis (1950-55; 1965-69)			
Exposure*InitialNeeds	-3.67	-4.98	
	(1.76)	(1.78)	
Exposure*LowInitialNeeds			0.40
			(0.72)
Exposure*HighInitialNeeds			-0.41
			(0.57)
Cohort FE	Yes	Yes	Yes
Cluster FE	Yes	Yes	Yes
Observations	901	901	901
Clusters	26	26	26
R^2	0.214	0.294	0.293
Panel B			
Partial analysis (1950 to 1969)			
Exposure*InitialNeeds	-3.09	-4.58	
	(1.75)	(1.63)	
Exposure*LowInitialNeeds			-0.50
			(0.72)
Exposure*HighInitialNeeds			-1.07
			(0.51)
Cohort FE	Yes	Yes	Yes
Cluster FE	Yes	Yes	Yes
Observations	1798	1798	1798
Clusters	28	28	28
R^2	0.132	0.204	0.204

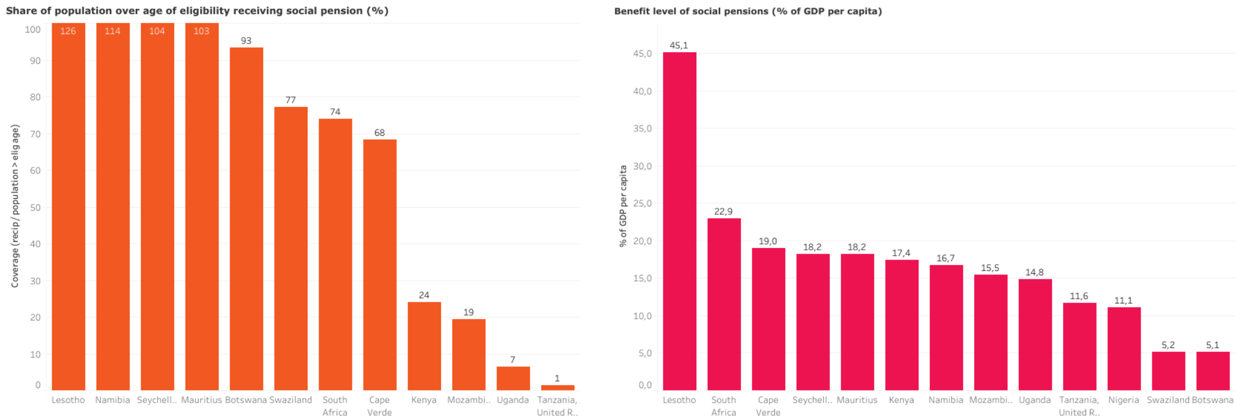
Data: DHS and NHIES. Level of observation: woman. Sample: women aged 44 to 50 in wave w; White ethnic group and Khomas region are excluded; cohorts 1950-55 and 1965-69 in Panel A; cohorts 1950 to 1969 in Panel B. OLS regression. Standard errors are clustered at the region*ethnicity level. $InitialNeeds$ is the difference between the poverty line and the expected annual pension for a given ethnic group in a given region before the extension, in thousand rands (average in the bottom half=0.28; average in the top half=0.43). $Exposure = max(min(10; k - 1955); 0)/10$ for cohort k and varies from 0 (before 1955) to 1 (after 1965). Full set of controls include: urban (share living in urban areas, share active in the agricultural sector), wealth (poor, savings account), access to services (distance to hosp.), cost of children (schooling rate 5-14, share of 10-14 economically active), AIDS, U5 mortality, indiv. time-invariant controls (maternal education). In column 3, the categories "Low", "Medium" and "High" initial needs consist of groups with average needs of respectively 0.23 thousand rands (corresponding to a pension coverage between 50 and 65%), 0.32 thousand rands (coverage between 40 and 50%) and 0.43 thousand rands (coverage between 30 and 40%). Medium initial needs is the omitted category and the regression includes the full set of controls.

Appendix: For Online Publication

Figure A.1: Current social pension systems in Sub-Saharan Africa

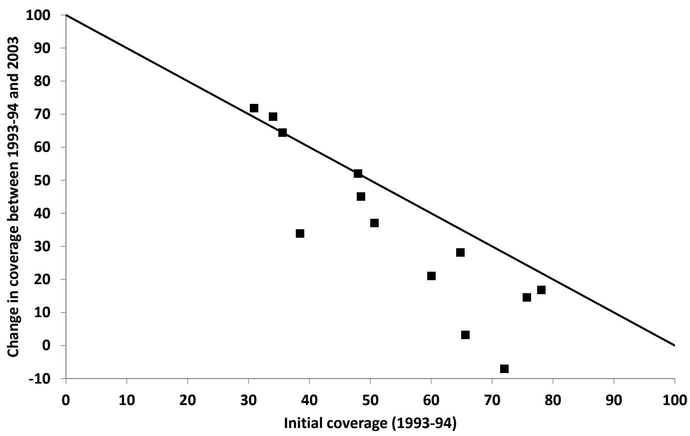
Coverage

Replacement rate



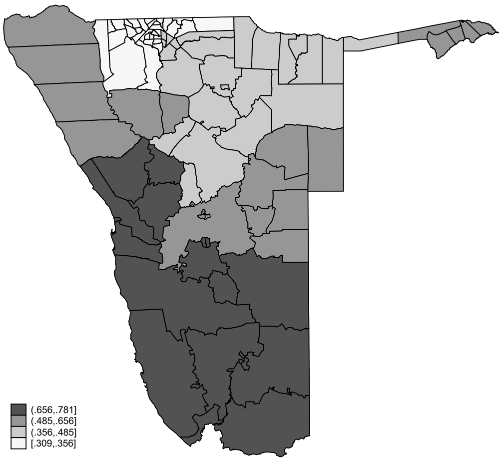
The graph on the left shows the share of population over age of eligibility receiving social pensions. The graph on the right shows the benefit level of social pensions as a percentage of GDP per capita. Countries that are not listed do not have a social pension system. Source: pension-watch.net, 2019

Figure A.2: Initial coverage predicts change in coverage by region

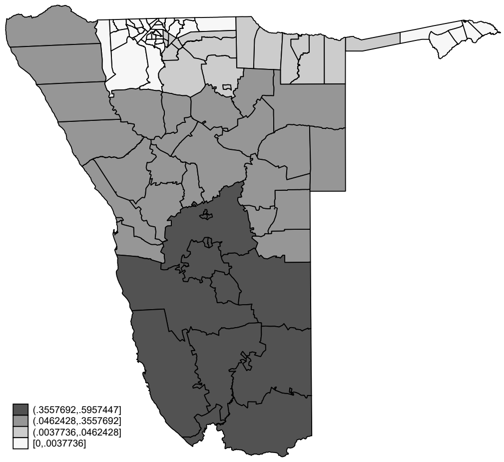


The graph plots the coverage in 1993-94 on the x-axis and the change in coverage between 1993-94 and 2003 on the y-axis. Each dot is a region. The line $y = 100 - x$ represents the attainment of universal coverage between the two periods. Source: own computation based on Subbarao (1998) and Levine, van den Berg, and Yu (2011)

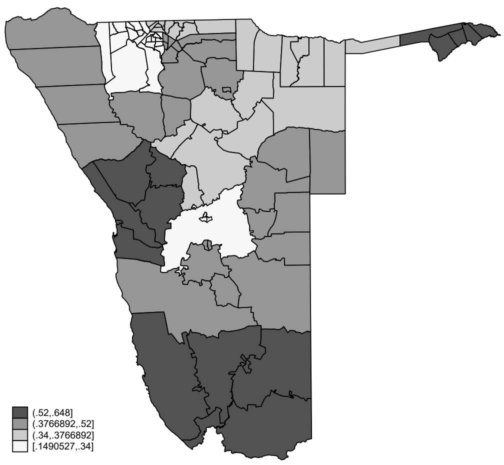
Figure A.3: Initial coverage partly reflects fraction of Whites in the population



Initial coverage (all)



Fraction of Whites

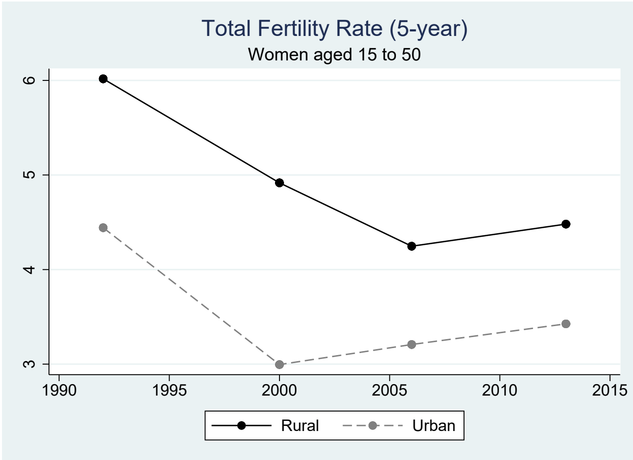


Initial coverage, excluding Whites

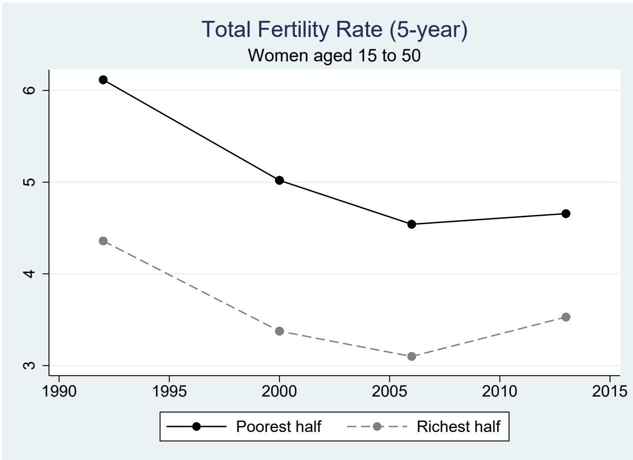
The map on the top left shows the initial coverage for the whole population, by region, computed by Subbarao (1998) using NHIES 1993-94. The map on the top right shows the fraction of Whites among people above age 60, by region, that we computed using NHIES 1993-94. The map on the bottom shows the estimated initial coverage for Blacks only, by region, that we computed under the assumption that the coverage for Whites is 100%.

Figure A.4: No *global* convergence in fertility between 1992 and 2000

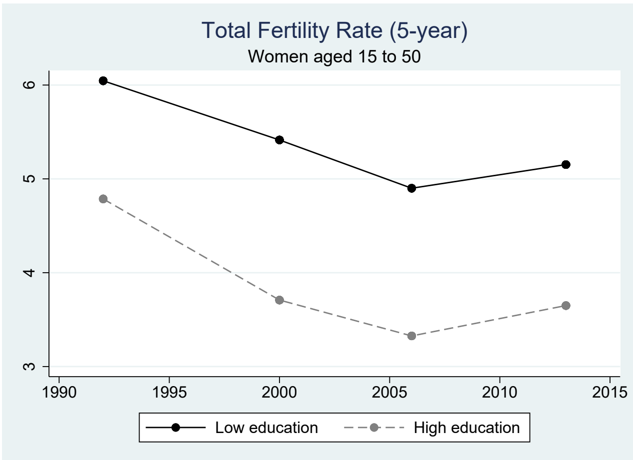
(a) Rural areas do not catch-up with urban areas



(b) Poor households do not catch-up with rich households

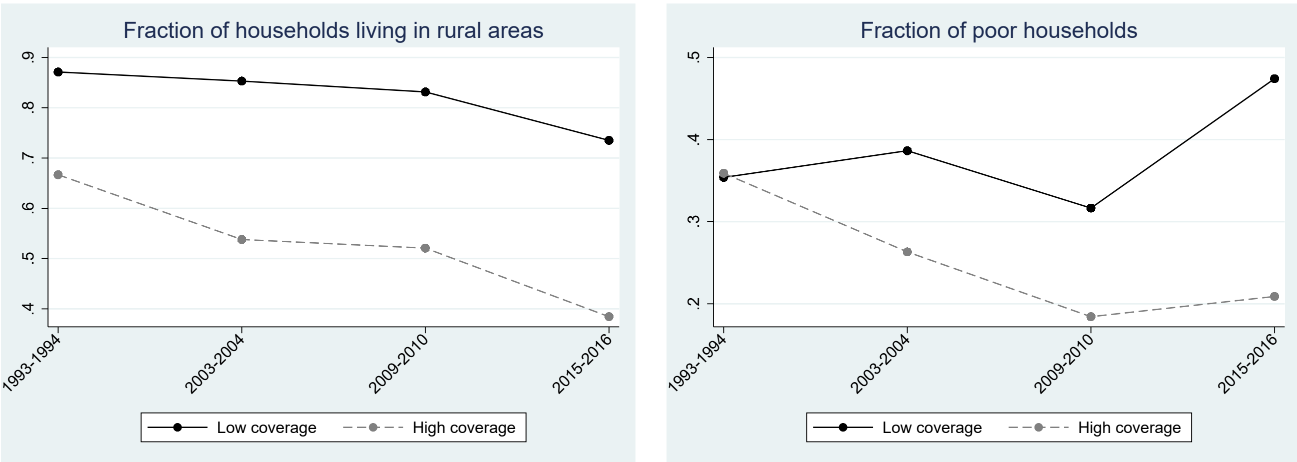


(c) Low educated mothers do not catch-up with high educated mothers



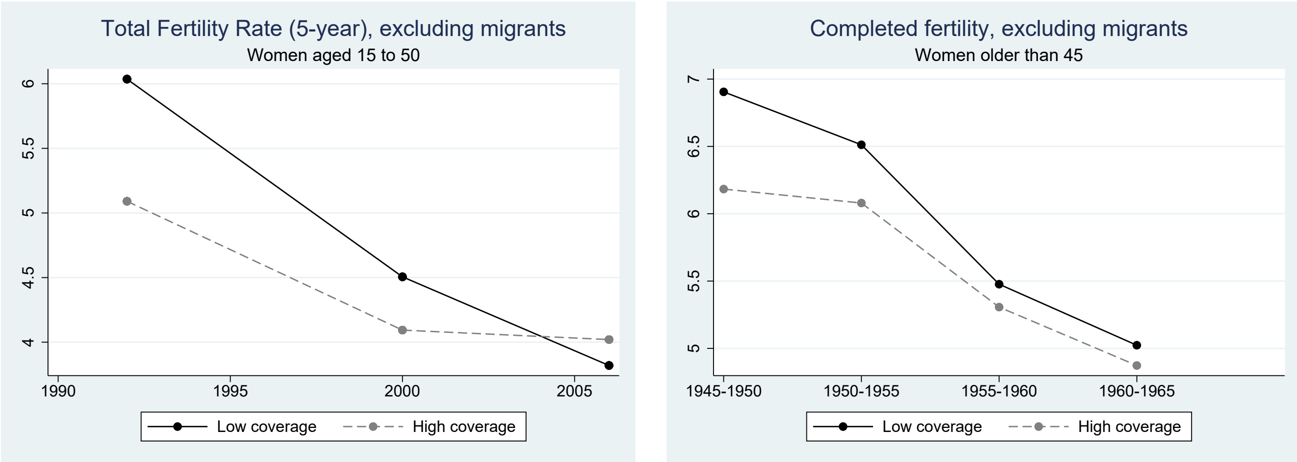
The graphs show the evolution of Total Fertility Rates in different sub-populations: (a) rural (in black) and urban (in grey) areas; (b) households with a wealth index below median (in black) and above median (in grey); (c) mothers with no or only primary education (in black) and mothers with secondary or tertiary education (in grey). The extension of pensions took place between 1992 and 1997. Source: own computation based on age-specific birth rates in the five years preceding each survey. Sample: women aged 15 to 50, White ethnic group and Khomas region are excluded. Data: DHS.

Figure A.5: Confounders: other drivers of fertility tend to diverge during the extension



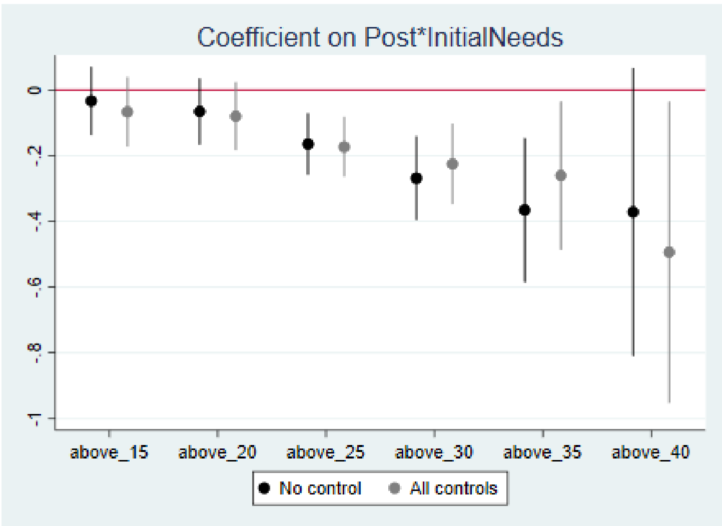
The graphs show the evolution of the fraction of rural (on the left) and poor (on the right) households in regions with low (North) and high (Northeast and South) initial pension coverage. The extension of pensions took place between 1992 and 1997. Sample: non-Whites. Data: NHIES.

Figure A.6: Robustness: catch-up in fertility is not driven by migration



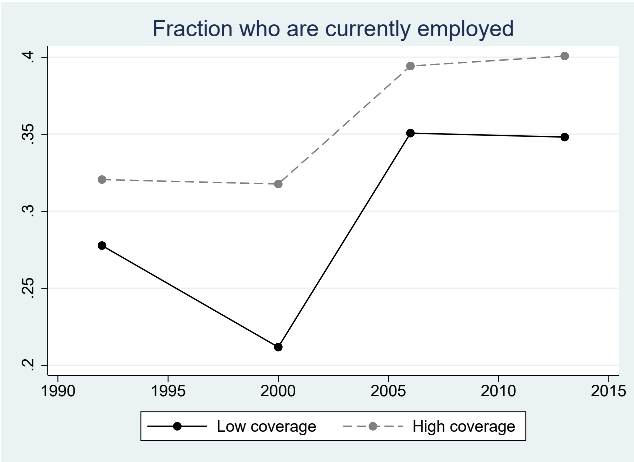
The graphs show the evolution of the 5-year total fertility rate (on the left) and completed fertility (on the right) in regions with low (North) and high (Northeast and South) initial pension coverage. We exclude people who migrated in the 10 years preceding the survey. Information on the migration status was not collected in the last DHS wave (2013). See Figure 2 for more details.

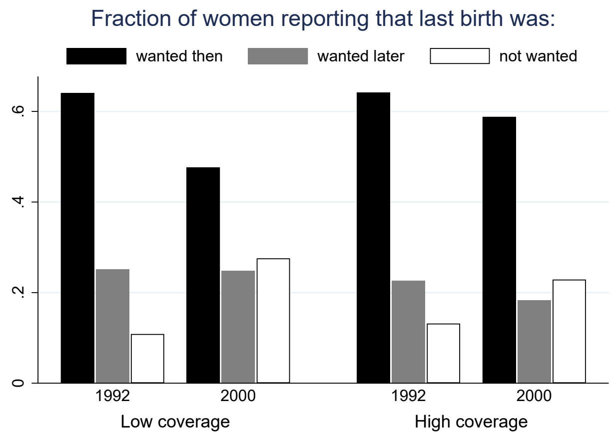
Figure A.7: Robustness: difference-in-differences coefficients with mother fixed effects



The graph plots γ, the coefficient on the interaction between $InitialNeeds_c$ and $Post_t$ in Equation 1 with mother fixed effects instead of cluster fixed effects, controlling or not for a restricted set of controls (urban and wealth), for women above age a in 1992. The vertical lines represent the confidence intervals at 95%.

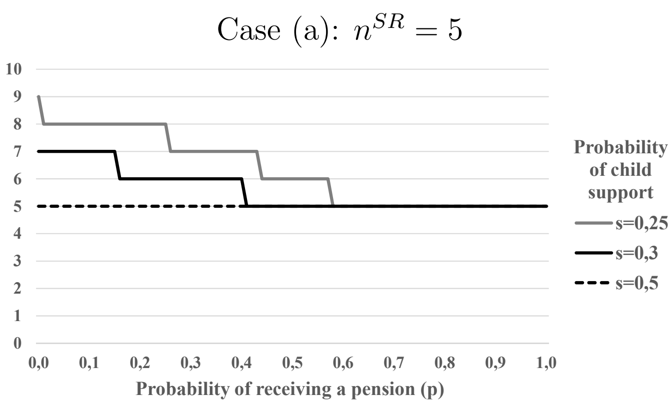
Figure A.8: Mechanisms: rule out changes in female labor and ability to control

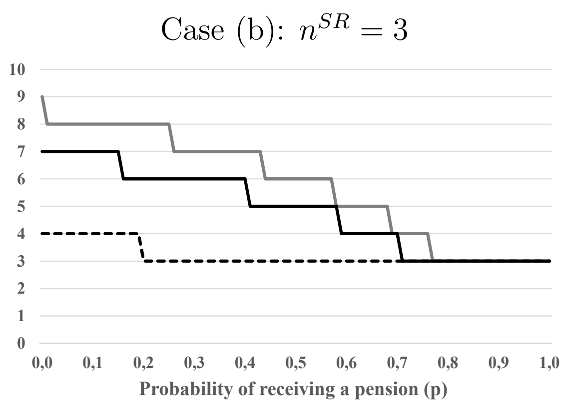




The graph on the left shows the evolution of the fraction of women currently employed in regions with low (North) and high (Northeast and South) initial pension coverage. Women are considered as unemployed if they answered that their occupation is "not working" or "self-employed in agriculture". We aggregated these two categories to make sure that answers are comparable across waves. Women working on fields without receiving any payment in kind or in cash were registered as "self-employed in agriculture" in 2006 and as "not working" in the other waves. Data: DHS.
The graph on the right shows the evolution of the fraction of births wanted then (black bars), wanted later (grey bars) and not wanted (white bars) in regions with low (North) and high (Northeast and South) initial pension coverage, before (1992) and after (2000) the extension of pensions. Data: DHS.

Figure A.9: Simulations: optimal number of children in the LSF model





The graphs plot $max(n^{SR}; n^{LR})$ as a function of p, the probability of receiving a pension. n^{LR} is the smallest n such that $(1-s)^n \times (1-p) \leq 0.1$ and $n^{SR} = 5$ (graph on the left) or $n^{SR} = 3$ (graph on the right).
Each line is drawn for a specific value of s, the probability of child support: $s = 0.25$ (grey solid line), $s = 0.3$ (black solid line) and $s = 0.5$ (black dashed line).

Table A.1: Sample selection: using propensity score matching to identify clusters in the common support

	PREDICTORS OF TREATMENT		
	Poverty rate (1)	Access to health services (2)	Share active in agriculture (3)
Panel A: list of clusters not in the common support and excluded from Panels B and C	Khomas-Herero	Khomas-Herero Khomas-Owambo	Khomas-Herero Khomas-Owambo Kavango-Owambo
Panel B: period analysis	DEP VAR: PROBABILITY OF BIRTH		
Post*LowInitialNeeds	0.004	0.002	0.001
	(0.019)	(0.019)	(0.016)
Post*HighInitialNeeds	-0.025	-0.022	-0.035
	(0.014)	(0.015)	(0.013)
Year FE	Yes	Yes	Yes
Cluster FE	Yes	Yes	Yes
Observations	153603	144834	130039
Clusters	37	36	35
R^2	0.015	0.014	0.013
Panel C: cohort analysis (partial)	DEP VAR: NUMBER OF BIRTHS		
Exposure*LowInitialNeeds	-0.53	-0.52	-0.58
	(0.81)	(0.71)	(0.72)
Exposure*HighInitialNeeds	-0.89	-1.08	-1.12
	(0.46)	(0.49)	(0.52)
Cohort FE	Yes	Yes	Yes
Cluster FE	Yes	Yes	Yes
Observations	1893	1825	1669
Clusters	30	29	28
R^2	0.226	0.202	0.208

In Panel A, we define the treatment as high initial pension coverage (Northeast, South and Erongo). We use the command pscore, comsup in stata to identify which clusters in the treatment/control groups are not in the common support. We use different indicators of socio-economic development as predictors of treatment: poverty rate (column 1), access to health services (column 2) and share active in agriculture (column 3). In Panels B and C, we estimate our main specifications (see notes below Tables 5 and 6) restricting the sample to clusters in the common support: we drop the Herero group in Khomas in column 1, the Herero and Owambo/Kavango/Caprivi groups in Khomas in column 2, the Herero and Owambo/Kavango/Caprivi groups in Khomas as well as the Owambo/Kavango/Caprivi group in Kavango in column 3. The results are the same as in Table 5 column 9 and Table 6 column 3 where we drop the Herero, Owambo/Kavango/Caprivi and Damara groups in Khomas.

Table A.2: Statistical significance of differences before the extension of pensions

	All						Blacks only					
	High coverage (1)	Low coverage (2)	Khomas (3)	t-stat 1 vs 3 (4)	t-stat 2 vs 3 (5)	t-stat 1 vs 2 (6)	High coverage (7)	Low coverage (8)	Khomas (9)	t-stat 7 vs 9 (10)	t-stat 8 vs 9 (11)	t-stat 7 vs 8 (12)
Panel A: Individual characteristics												
Ethnicity												
% European/Afrikaans	0.15	0.00	0.39	3.9	824.8	2.6	-	-	-	-	-	-
% Damara	0.25	0.01	0.22	-0.4	16.3	3.5	0.29	0.01	0.36	0.7	27.2	2.9
% Herero	0.18	0.00	0.09	-1.1	167.7	2.2	0.21	0.00	0.15	-0.6	274.1	2.2
% Owambo/Kavango/Caprivi	0.42	0.98	0.30	-0.8	-52.9	-3.6	0.49	0.99	0.49	-0.0	-39.3	-3.0
Education (at least primary)	0.43	0.31	0.35	-0.7	<0.1	1.0	0.33	0.31	0.28	-0.6	<0.1	0.3
Panel B: Regional controls												
% poor	0.32	0.38	0.07	-8.6	-4.2	-0.8	0.36	0.38	0.10	-9.4	-3.8	-0.3
% living in urban areas	0.33	0.10	0.88	6.1	12.9	2.3	0.27	0.09	0.83	7.2	12.2	1.8
% econ. active in agriculture	0.44	0.59	0.05	-5.0	-7.0	-1.5	0.50	0.60	0.07	-5.8	-6.7	-0.9
% with bank/saving account	0.40	0.28	0.68	7.6	6.8	1.8	0.36	0.28	0.64	8.6	6.1	1.3
% 10-14 economically active	0.07	0.07	0.00	-2.2	-5.4	0.2	0.08	0.07	0.00	-2.5	-5.8	0.5
% 5-14 ever in school	0.86	0.93	0.94	2.9	0.5	-2.1	0.84	0.93	0.93	3.0	0.3	-2.4
% with hospital/clinic > 1 hour-walk	0.51	0.50	0.15	-5.3	-4.8	0.1	0.56	0.50	0.14	-6.3	-5.0	0.6
Prevalence of HIV	0.10	0.09	0.07	-1.0	-1.3	0.1	0.10	0.09	0.07	-1.0	-1.3	0.2
Infant mortality rate	0.08	0.06	0.05	-2.1	<0.1	1.2	0.08	0.06	0.05	-2.6	<0.1	1.7
Observations NHIES	11204	10874	2906				9194	10841	1743			
Observations DHS	2711	2149	561				2213	2137	499			

The table describes important variables, separately for regions with low (North) initial pension coverage, regions with high (Northeast, South and Erongo) initial pension coverage and the region of the capital city Khomas. In columns 1 to 6, all ethnic groups are included. In columns 7 to 12, only Blacks are included. Means are displayed in columns 1-3 and 7-9 and t-stats are displayed in columns 4-6 and 10-12. Columns 4 and 10 compare the means between Khomas and high coverage regions; columns 5 and 11 compare the means between Khomas and low coverage regions; columns 6 and 12 compare the means between low and high coverage regions. The sample of interest for our analysis excludes Whites and Khomas, and corresponds to the means in columns 7 and 8 and to the t-stat in column 12. Data: DHS 1992 for education and infant mortality, NHIES 1993-94 for all other variables.

Table A.3: Period analysis: Robustness checks

	DEPENDENT VARIABLE: PROBABILITY OF BIRTH					
	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
	Baseline with controls	Northern regions only	Rural only	Owambo specific trend	Khomas specific trend	Pension coverage
Post*InitialNeeds	-0.173	-0.191	-0.228	-0.107	-0.202	
	(0.043)	(0.083)	(0.058)	(0.061)	(0.055)	
Post*IniLackCoverage						-0.118
						(0.014)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	141637	116695	83967	141637	155774	141637
Clusters	35	26	34	35	38	12
R^2	0.015	0.016	0.015	0.015	0.015	0.014

Data: DHS and NHIES. Level of observation: woman*year. Sample: women aged 15 to 40 in year t; White ethnic group and Khomas region are excluded, except in model (5) where Khomas is included; in model (2): only Northern regions; in model (3): only rural households. OLS regression. Standard errors are clustered at the region*ethnicity level. $InitialNeeds$ is the difference between the poverty line and the expected annual pension for a given ethnic group in a given region before the extension, in thousand rands (average in the bottom half=0.28; average in the top half=0.43). *IniLackCoverage* is equal to 1 minus the pension coverage in a given region before the extension (average in the bottom half=0.47; average in the top half=0.65). $Post$ is a dummy for years after 1992 (date of the Pension Act). All regressions control for urban (share living in urban areas, share active in the agricultural sector), wealth (poor, savings account), access to services (distance to hosp.), cost of children (schooling rate 5-14, share of 10-14 economically active), AIDS, U5 mortality, indiv. time-invariant controls (maternal education; ethnicity for model (6)). We add the interaction of $Post$ with a dummy for the Owambo ethnic group in model (4) and the interaction of $Post$ with a dummy for the Khomas region in model (5).

Table A.4: Cohort analysis, partial exposure specification: Robustness checks

	DEPENDENT VARIABLE: NUMBER OF BIRTHS					
	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
	Baseline with controls	Northern regions only	Rural only	Owambo specific trend	Khomas specific trend	Pension coverage
Exposure*InitialNeeds	-4.59	-3.13	-7.83	-5.23	-4.18	
	(1.63)	(1.10)	(1.80)	(1.62)	(1.67)	
Exposure*IniLackCoverage						-3.60
						(0.83)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1798	1479	1194	1798	1914	1798
Clusters	28	20	26	28	31	12
R^2	0.204	0.205	0.196	0.204	0.224	0.205

Data: DHS and NHIES. Level of observation: woman. Sample: women aged 44 to 50 in wave w; White ethnic group and Khomas region are excluded, except in model (5) where Khomas is included; in model (2): only Northern regions; in model (3): only rural households. OLS regression. Standard errors are clustered at the region*ethnicity level. $InitialNeeds$ is the difference between the poverty line and the expected annual pension for a given ethnic group in a given region before the extension, in thousand rands (average in the bottom half=0.28; average in the top half=0.43). $IniLackCoverage$ is equal to 1 minus the pension coverage in a given region before the extension (average in the bottom half=0.47; average in the top half=0.65). $Exposure = max(min(10; k - 1955); 0)/10$ for cohort k and varies from 0 (before 1955) to 1 (after 1965). All regressions control for urban (share living in urban areas, share active in the agricultural sector), wealth (poor, savings account), access to services (distance to hosp.), cost of children (schooling rate 5-14, share of 10-14 economically active), AIDS, U5 mortality, indiv. time-invariant controls (maternal education; ethnicity for model (6)). We add the interaction of $Exposure$ with a dummy for the Owambo ethnic group in model (4) and the interaction of $Exposure$ with a dummy for the Khomas region in model (5).

Table A.5: Separating income and insurance channels: heterogeneity by co-residence with an elderly

	DEP VAR: PROBABILITY OF BIRTH			
	Lives with no elderly		Lives with an elderly	
	No control	All controls	No control	All controls
	(1)	(2)	(3)	(4)
Post*InitialNeeds	-0.153	-0.237	-0.111	-0.094
	(0.069)	(0.056)	(0.129)	(0.040)
Year FE	Yes	Yes	Yes	Yes
Cluster FE	Yes	Yes	Yes	Yes
Observations	107302	107302	34335	34335
Clusters	35	35	29	29
R^2	0.008	0.016	0.007	0.016

Data: DHS and NHIES. Level of observation: woman*year. Sample: women aged 15 to 40 in year t; White ethnic group and Khomas region are excluded; the sample is split between households with zero and with at least one member older than 60. OLS regression. Standard errors are clustered at the region*ethnicity level. $InitialNeeds$ is the difference between the poverty line and the expected annual pension for a given ethnic group in a given region before the extension, in thousand rands (average in the bottom half=0.28; average in the top half=0.43). $Post$ is a dummy for years after 1992 (date of the Pension Act). Controls: urban (share living in urban areas, share active in the agricultural sector), wealth (poor, savings account), access to services (distance to hosp.), cost of children (schooling rate 5-14, share of 10-14 economically active), AIDS, under-5 mortality, indiv. time-invariant controls (maternal education).