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RESEARCH PAPER

Restrictive fertility policy and elderly suicides: evidence from China

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Abstract
This paper presents an empirical investigation of the hypothesis that exposure to the restrictive fertility policies of the Chinese "Later, Longer, Fewer" campaign in the 1970s contributes to the dynamics and patterns of elderly suicides in China in the period 2004–2017. We apply an identification strategy that exploits variation in exposure to this policy across birth cohorts that is based on the different timing of the implementation of the fertility policies across Chinese provinces. The results show that cohorts with a greater exposure to the restrictive fertility policy in the 1970s exhibit higher suicide rates during old ages.

Keywords: China; elderly suicides; fertility decline; fertility policy

JEL classification: J10; J11; J13; J14

1. Introduction

Suicide is one of the leading causes of death in China, especially among the elderly, with mortality from suicide being 2.75–7 times higher for the population aged 60 and older in comparison to the general population [see Figure 1(a) and Lien *et al.* (2018)]. While China's population is aging rapidly, the share of elderly committing suicide is sharply increasing, especially among women (see Figure 1(b) and (c)).[1] In fact, the dynamics of elderly suicides in China are exceptional in international comparison.[2]

[1]Although suicides display an overall decline, the rate of suicides among the elderly displayed an upward trend during the early 2000s, even when adjusting for the change in the age composition, see online Appendix Figure A1.

[2]According to data from the WHO [World Health Organization (2014)], the annual age-standardized suicide rate in China for 2012 was 7.8 per 100 000 population (7.1 for males and 8.7 for females), compared to an annual global age-standardized suicide rate of 11.4 per 100 000 population (15.0 for males and 8.0 for females). The elderly suicide rate for the population above 70 years of age in 2012

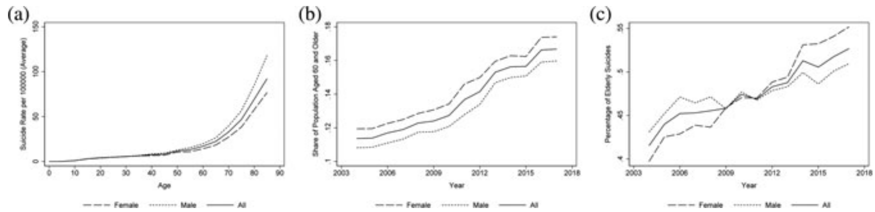

Figure 1. Population aging and suicides among the elderly in China, (a) suicides by age (2004–2017), (b) population share elderly, (c) percentage of suicides by elderly. *Source:* Chinese Disease Surveillance Points (DSP), see section 2 for details.

With unparalleled economic development on the one hand, and profound social changes and a rapidly aging population on the other, this observation has shifted the focus of recent research to the reasons for the suicide rates among the elderly in China. An increasing body of empirical research has isolated several salient risk factors, including, most prominently, economic living conditions, with suicides among elderly with low incomes, low education, and living in rural areas being significantly more frequent. These findings are consistent with an economic perspective that views suicide as the behavioral reaction to discounted future utility reaching a finite lower bound [Hamermesh and Soss (1974)]. Among the most frequently mentioned protective factors for elderly suicides are the family environment and companionship with children and kin. A candidate explanation for these patterns is that children and family structures are important factors for old age support in a country with limited public pension coverage. Empirical research has indeed shown that financial dependence and the lack of access to state pension schemes has a detrimental effect on life satisfaction and mental health of elderly [see, e.g., Abruquah *et al.* (2019)]. In this context, exposure to China's restrictive fertility policies in the past might have contributed to exacerbate this effect. The policy-induced decline in fertility not only accelerated population aging, but might also have contributed to mental health problems, particularly among women, and more precarious living conditions in old age for the most affected generations, who had fewer offspring to care for them, and a higher risk of no support in the case of premature death of their only child.[3] Besides affecting old age support within families, fertility policies might have direct effects on mental health, and hence on suicides among elderly. While earlier work on the one-child policy in China found no effect of having more children on mental and physical health and, if anything, an adverse impact on self-reported health of elderly parents [Islam and Smyth (2015)], recent work has documented a deterioration of mental well-being as consequence of the restrictive fertility policy in China [Chen and

was 51.5 (55.8 for males and 47.7 for females) in China. Corresponding numbers for other developed (G7) countries are: Canada (overall 9.8 and elderly 11.3), Germany (overall 9.2 and elderly 23.7), Italy (overall 4.7 and elderly 10.8), Japan (overall 18.5 and elderly 25.5), UK (overall 6.2 and elderly 6.3), USA (overall 12.1 and elderly 16.5). Suicide rates in emerging countries (BRICS) are: Brazil (overall 5.8 and elderly 9.8), Russia (overall 19.5 and elderly 32.1), India (overall 21.1 and elderly 20.9), China (overall 7.8 and elderly 51.5), South Africa (overall 3 and elderly 10.5). It should be noticed, however, that due to the lack of data, these numbers correspond to crude age-specific suicide rates among individuals aged 70+, not age-standardized numbers.

[3]See, e.g., Yu (2014) or the recent discussion in *The Conversation* (theconversation.com), June 30, 2021.

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Fang (2021)]. A specific channel that has been associated with mental health problems and suicides in this context is the lack of family companionship [Fang *et al.* (2021)]. In fact, the importance of children as providing a social network for their elderly parents and the role of social isolation as an important risk factor for depression have been emphasized repeatedly [see, e.g., Bures *et al.* (2009) for the differences of voluntary and involuntary childlessness for depressive symptoms]. Particularly in the context of the high suicide rates among the rural elderly in China, depressive symptoms and mental disorders have been shown to be amplified by an experience of being left-behind [Zhou *et al.* (2019)]. Although the exposure to restrictive fertility policy has been conjectured as a potentially important determinant of elderly suicides in China before, direct evidence for such a link is still missing.

In this paper, we investigate the hypothesis that the exposure to China's fertility policies is one of the factors contributing to the dynamics and patterns of elderly suicides in China. Starting in 1969, China implemented policies to control fertility, first in terms of a "Later, Longer, Fewer" policy and since 1979 in terms of the "One Child Policy." The hypothesis of this paper is motivated by the observation that China's "Later, Longer, Fewer" fertility policy strongly affected fertility, even more than the "One Child Policy." Figure 2 illustrates this by showing the average fertility dynamics in China in relation to the implementation dates of the "Later, Longer, Fewer" (LLF) fertility policies starting in 1969, and of the "One Child Policy" (OCP) starting in 1979 [see also Chen and Huang (2020) for causal evidence on this point]. Moreover, people who were affected by the LLF policy in 1970s are increasingly representing the majority of the elderly population above 60 years of age.

To investigate whether the exposure to the LLF policy influences suicides among the elderly, we apply an identification strategy that exploits variation in the exposure to the "Later, Longer, Fewer" (LLF) fertility policies across three different dimensions: time, space, and cohorts. The time dimension of the identification strategy makes use of variation in the timing of the policy implementation across Chinese provinces. The space dimension exploits the variation in the intensity of exposure across different geographical regions that is due to variation in pre-policy fertility rates. Finally, the

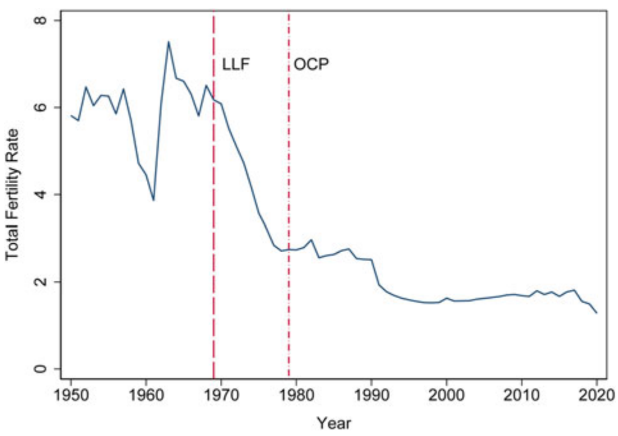

Figure 2. The dynamics of fertility and fertility policies in China. *Source:* Data from United Nations, Department of Economic and Social Affairs, Population Division (2022). World Population Prospects: The 2022 Revision, https://population.un.org/wpp/.

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cohort dimension captures variation in the exposure across birth cohorts as older cohorts were closer to completing their fecund period of life at the time when the policy was implemented than younger cohorts. Concretely, we consider cohorts born from 1930 to 1957 and construct a measure of policy exposure that varies across birth cohorts and region of residence depending on the timing of the establishment of the Family Planning Leading Group (FPLG) in a province. The establishment of these groups from 1969 to 1975 has been shown to have been closely related to the subsequent decline in fertility in the respective provinces. Spatial heterogeneity is measured in terms of the decline in fertility relative to the province-age-specific average fertility rates in 1969, prior to the establishment of the planning groups. As outcome, we consider suicide rates among elderly in the period 2004–2017, which corresponds to a period in which the respective birth cohorts are between 60 and 87 years old.

The findings show that a greater exposure to the fertility policy is associated with higher suicide rates. This finding emerges on top of sex-related differences, lower suicides in urban environments, and a declining time trend in suicides, all of which are replicated in our analysis. In addition, we find that the effect is mainly driven by individuals in urban areas, and by cohorts born in the period 1930–1957. Robustness checks using placebos in the policy implementation reveal that the effect is identified by the combination of variation in the policy implementation across cohorts, regions, and the effective time of implementation. We end by investigating whether the reduced form effect of policy exposure works through physical well-being or mental well-being, building on previous research that has found evidence for a policy effect using survey data from the China Health and Retirement Study. Our results based on the same survey data suggest that the policy-related effect on elderly suicides might reflect a deterioration in parental mental health, while we find little evidence for material and physical well-being as potential channel.

This paper contributes to the literature in several dimensions. Recent reviews of the evidence on causal and protective factors of suicides or suicide ideation by elderly find important influences of sex, age, and the urban/rural divide, as well as of physical and mental health and of the social, cultural, and family context [Dong *et al.* (2015); Li and Katikireddi (2019); Yu *et al.* (2021)]. Our findings replicate these patterns while adopting a cohort perspective and add evidence for a factor that has been conjectured repeatedly to play a role in this context, namely the exposure to restrictive fertility policies.

A stylized fact emerging from the existing literature is that the number of children seems to affect the prevalence of suicide ideation, especially among the elderly living in rural areas, and thereby contribute to the rural/urban divide in suicides [Wei *et al.* (2018)]. In a recent study that complements ours, Fang *et al.* (2021) focus on the role of family companionship for elderly suicides and exploit high-frequency variation in suicides at a highly disaggregate level using the Chinese Lunar New Year, when families traditionally have reunions, as a social experiment. Their results suggest that suicides decline significantly during the festivities related to the Chinese Lunar New Year, but increase significantly in the subsequent two months. Existing studies also repeatedly conjectured that, among recent developments such as economic reform and cultural change, also the restrictive fertility policies of the 1970s and 1980s may have contributed to the recent suicide dynamics among the elderly [see, e.g., Li *et al.* (2009); Wang *et al.* (2014); Liu *et al.* (2015)]. In light of the lack of causal evidence for an effect of fertility policies on elderly suicide rates, the present paper contributes novel evidence that is consistent with this long-standing conjecture. While consistent with a higher suicide rate among elderly in rural areas, our findings also show that the effect of policy exposure tends to mostly affect suicides in urban areas. The results thereby complement earlier studies at the population level that found that urbanization contributed to a decline of suicide rates on average [Sha *et al.* (2017)], while indicating that urbanization might have led to an increase of elderly suicide risk at the individual level [Zhou *et al.* (2019)].

With its focus on the long-term consequences of fertility policies on elderly suicides, our analysis contributes to earlier work on the effect of fertility policies on health in old age [e.g., Islam and Smyth (2015)] and complements recent findings of negative long-term consequences of China's fertility policies on parental mental health that uses a similar identification strategy [Chen and Fang (2021)].

The remainder of the paper is structured as follows. Section 2 presents the data and methodology, section 3 contains the results, section 4 provides a brief discussion of potential mechanisms, and section 5 concludes.

2. Empirical strategy

2.1. Background: the family planning campaign and its consequences

By the end of 1969, China's population exceeded 800 million people and the stagnation of economic growth was seen as a consequence of overpopulation. This lead to the consideration of measures of population control [Zhang (2017)]. In the early 1970s, family planning became a focus of national policies. The State Council released a document that required provinces to set up a Familiy Planning Leading Group (FPLG) that would effectively implement family planning policies [Chen and Fang (2021)]. The first implementations occurred in Guangdong in 1969 and in Shandong in 1970; the process of setting up FPLGs was completed with the last groups in Guizhou and Xinjiang in 1975 [for details, see Table A-1 in Chen and Huang (2020)].

Initially, the state propaganda prepared the family planning campaign with the motto "one child isn't too few, two are just fine, and three are too many" [Zhang (2017), p. 143]. In 1973, the State Council introduced the slogan "Later, Longer, and Fewer", which succinctly summarized the demographic goals of the family policy: aiming at marriages at a later age (25 years for women and 27 or 28 years for men in urban areas, and 23 years for women and 25 years for men in rural areas), longer intervals between births (at least three years between first and second child), and fewer children per couple [at most two children per couple in urban areas and three children per couple in rural areas; for details, see, e.g., Whyte *et al.* (2015); Zhang (2017)].

While officially being voluntary, the implementation of the family planning campaign in the 1970s made it mandatory through extensive monitoring at the micro level. In rural villages, urban work units, and neighborhoods, trained birth control staff introduced various birth-control techniques and meticulously kept track of women in fecund ages, leading to a sharp increase of IUD insertions, sterilizations, and abortions during the early 1970s [Whyte *et al.* (2015)]. Thus, while often considered as less coercive than the subsequent one-child policy implemented in 1979, the family planning campaign in the 1970s led to a dramatic decline in fertility rates already before 1978, from more than 6 to 2.7 as illustrated in Figure 2. This decline accounts for more than 70% of the overall fertility decline up to now [Whyte *et al.* (2015)]. However, the implementation was not entirely uniform across rural and urban areas, particularly during the later phases of the subsequent one-child policy. The enforcement was more lenient among the rural population, while the monitoring and control of urban residence was more strict [see, e.g., Zhang (2017)].

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The decline in fertility and the induced change in family structure had far-reaching implications. Rooted in Confucianism, old-age support in China heavily relies on family links, and the children's responsibility to care for their parents has constitutional status [Chen and Fang (2018)]. While the public pension system has been expanded over the past decades and covers a considerable fraction of the Chinese population, the low levels of pensions do not fully compensate for the loss of labor income after retirement [Cai *et al.* (2006)]. This implies an increasing burden on a smaller generation of offspring and, at the same time, a greater risk of financial constraints and poverty among the elderly affected by the restrictive family planning campaign. Recent evidence suggests that above and beyond the consequences for material well-being, the consequences in other dimensions might be even more far-reaching. In particular, fewer children provide fewer direct support in terms of personal contacts, caring, and companionship, with consequences for parents' subjective well-being and mental health [see, in particular, Chen and Huang (2020); Chen and Fang (2021)]. Overall, this implies that the exposure to the restrictive fertility policies induced considerable material, emotional, and psychological stress that might materialize decades later when individuals reach old age. By affecting important protective factors for suicide [Dong *et al.* (2015); Li and Katikireddi (2019)], this might give rise to an association with increasing suicides among the elderly that were more exposed to the policies.

2.2. Data on suicides

There are two sources of publicly available data on suicides in China. One is provided by the Ministry of Health's Vital Registration (MOH-VR) System, the other is provided by the Chinese Disease Surveillance Points (DSP) System of the Chinese Center for Disease Control and Prevention (CCDC). In the publicly available statistic, the MOH-VR system reports suicide information only at the aggregate (country) level, while the data from the DSP system include information about the region (east/central/west).

The DSP system was implemented in 1978 with a pilot study conducted in Beijing, to collect data on births, causes of death, and the incidence of infectious diseases. These data only became nationally representative in 1990, when the Chinese Academy of Preventive Medicine revised and expanded the system to 145 geographic locations across the country [Yang *et al.* (2005)]. By 2004, DSP had been expanded further to include 161 geographic locations across 31 provinces, covering 6% of the population [Liu *et al.* (2016)]. In 2013, the government merged the information of the MOH-VR and DSP into an integrated national mortality surveillance system, which comprises 605 surveillance points, covering 323.8 million people, 24.3% of the total population [Liu *et al.* (2016)]. Since this, the data from the two sources are identical [Sha *et al.* (2018)].

In the publicly available data set, the DSP system reports suicide rates (per 100,000 people) and the number of suicides based on the sample, by sex, quinquennial age group, living environment (rural/urban), and region (east/central/west). To obtain continuous observations across age and cohort, we assign the same suicide information to observations of ages within the same quinquennial age group.[4]

In the empirical analysis, we use data for the period 2004–2017. We focus on elderly suicides by restricting attention to suicides among individuals aged 60 and older.

[4]For example, we consider people aged 60–64 to reflect the same suicide rate without distinguishing suicide rates of, e.g., individuals aged 61 and 62.

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2.3. Measuring the exposure to fertility policies

The identification approach makes use of variation in the (latent) exposure to the fertility policies across birth cohorts, space, and time. In particular, the "LLF" policy has been implemented at different points in time in different provinces. Our measure of policy exposure makes use of the year of creation of the FPLG in a province as the starting point of fertility policies. This approach follows work by Chen and Huang (2020), who have documented that the so-constructed exposure measure indeed had an impact on fertility dynamics.[5]

Concretely, we construct the measure of policy exposure for birth cohort c in province p and urban/rural area u, as

$$PolExp_{p,u,c} = \sum_{a=15}^{49} \left\{ AFR_{p,u}(a) \cdot I\left[c + a > T_p\right] \right\}. \tag{1}$$

with T_p denoting the implementation year of the LLF policy by the FPLG in province p, $I[c + a > T_p]$ denoting a binary indicator function that equals 1 if the policy came into effect in the year when birth cohort c reached age a and 0 otherwise, and $\mathrm{AFR}_{p,u}(a)$ denoting the age-specific fertility rates of province p and urban/rural area u in 1969, prior to the fertility policies. The data on age-specific fertility rates in a province, distinguishing between urban and rural areas, are taken from Coale and Chen (1987). Since it is well known that suicide rates differ strongly between rural and urban areas [Dong *et al.* (2015); Li and Katikireddi (2019)], the analysis explicitly accounts for this rural–urban divide. In this sense, this measure extends the related measure of exposure to fertility policies used by Chen and Fang (2021); Chen and Huang (2020).[6]

Since DSP only reports suicide data at the level of regions and distinguishing between urban/rural areas, for each cohort c, we adjust the measure of fertility policy exposure at the level of province×urban location and derive the weighted average for each region×urban area using sample weights in Coale and Chen (1987). Concretely, for cohort c in region r and urban/rural area u, the modified formula of policy exposure is given by

$$PolExp_{r,u,c} = \frac{\sum_{p \in r} W_{p,u} \cdot PolExp_{p,u,c}}{\sum_{p \in r} W_{p,u}} \tag{2}$$

where $PolExp_{p,u,c}$ represents policy exposure at the level of province p and urban/rural area u for cohort c, and $W_{p,u}$ denote the population weights of a province p and urban/rural area u within a given region r. The age-specific fertility rate is only available for

[5] See Chen and Huang (2020, p. 993) for the specific implementation year in each province.

[6] The measure of exposure to fertility policies by Chen and Fang (2021); Chen and Huang (2020) is given by

$$PolExp_{p,c} = \sum_{a=15}^{49} \left\{ AFR_p(a) \cdot I\left[c + a > T_p\right] \right\}.$$

with T_p and $I\ [c + a > T_p]$ as described in the text, and $\mathrm{AFR}_p(a)$ denoting the age-specific fertility rate of province p in 1969, prior to the fertility policies. The strict inequality reflects the assumption that the policy has a visible effect on fertility roughly after one year, accounting for conception and pregnancy.

quinquennial age groups, so we assign the same value of fertility to all ages within a given quinquennial age group. Moreover, the same age-specific fertility rate is assigned to both sexes.[7]

The empirical analysis is based on data for the period 2004–2017 and focuses on suicides among the elderly aged 60 and older.[8] This implies that the youngest cohort in the sample was of age 60 in 2017, and hence was a member of the birth cohort born in 1957. We focus attention on cohorts that were actually affected by the LLF policies during their reproductive ages. As the policies were implemented across the country between 1969 and 1975, we drop cohorts born before 1930 since they were essentially not affected by the policy as they had already completed their reproductive life span when the policies were first implemented.[9] Therefore, the oldest age group considered in our sample, the cohort born in 1930, was aged 87 in 2017, the last year of observation in our data. Figure 3 plots the average exposure to the LLF policy in the estimation sample.

The summary statistics of the estimation sample are contained in Table 1.

2.4. Empirical methodology

Since an analysis of the role of family environment and companionship with children and kin is prevented by data availability, we investigate the hypothesis that exposure to the LLF policy affects suicide rates of the elderly by estimating the effect using a reduced-form approach. The empirical framework is given by

$$ESR_{r,u,c,s,a,t} = \alpha + \beta PolExp_{r,u,c} + I_s + Age_a + Time_t + Cohort_c + \epsilon_{r,u,c,s,a,t}. \qquad (3)$$

The dependent variable $ESR_{r,u,c,s,a,t}$ represents the elderly suicide rate in region r and urban/rural location u of members of birth cohort c and sex s at age a in year t. The explanatory variable of main interest is the exposure to the LLF policy, $PolExp_{r,u,c}$ as in (2), of cohort c in region r and urban/rural location u. Additional control variables include an indicator for sex s, I_s (distinguishing females and males), and a full set of dummies for quinquennial age groups, denoted by Age_a. Period trends are accounted for by a linear time trend or time dummies, $Time_t$, and $Cohort_c$ reflects dummies accounting for heterogeneity across birth cohorts (years).[10] In this setting, the exposure to policy exhibits the critical identifying variation across regions and birth cohorts. Since policy exposure and other environmental conditions might lead

[7]In contrast to previous studies of the impact of the LLF policy on other outcomes [Chen and Huang (2020); Chen and Fang (2021)], the lack of data availability prevents us from using a similar strategy of measuring policy exposure across provinces and cohorts. Instead, the analysis here makes use of coarser variation at the region and urban/rural level across cohorts. Unreported results of a replication of the results of Chen and Huang (2020) reveal that the variation in the policy exposure at this coarser level delivers results that are quantitatively and qualitatively very similar to their main results.

[8]Information on suicides is reported in quinquennial brackets and the oldest age group observed is aged 85 and above in the data, which we label as individuals aged 85–89 for consistency.

[9]Technically, the reproductive lifespan is defined as the time between menarche, typically around age 12, and menopause, which occurs typically between age 40 and 55. For instance, members of the cohort born in 1925 were already 44 years old in 1969, so their fertility was practically not affected by the policy.

[10]Note that, due to the semi-parametric specification, patterns across age groups, time, and cohorts are identified from non-linearities in the respective dimensions.

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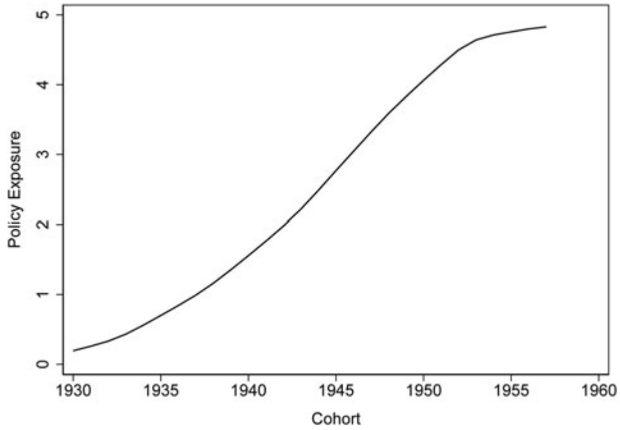

Figure 3. Average exposure to LLF policies across birth cohorts. *Notes:* This is calculated as the mean exposure of all regions based on equation (2) for each cohort.

to systematic unobserved heterogeneity across birth cohorts. Likewise, unobserved heterogeneity might affect individuals living in the same region. To account for both, the error term ϵ allows for two-way clustering at the region and cohort level, consistent with the usual practice in the literature that exploits comparable identification strategies.

The coefficient of interest is the effect of fertility polices on elderly suicide rate, β. Identification and consistent estimation of this coefficient requires exogeneity of the exposure to the LLF policy conditional on the controls in the empirical specification. In order to keep as much variation as possible, the baseline specification does not control for year and cohort dummies, or for region or urban/rural areas, but extended specifications also account for these factors. Moreover, in robustness checks below we show results for effect heterogeneity along these dimensions.

With the specification of the empirical model (3), the identifying variation in policy exposure as defined in (1) comes from three dimensions: birth cohorts, the implementation time of the policy, and space. With T_p denoting the year in which the policy was implemented in a particular province p, individuals belonging to a given birth cohort in a province in which the policy was implemented earlier experienced a greater exposure to this policy than members of the same birth cohort in a province in which the policy was implemented later. Moreover, with variation in fertility $AFR_{p,u}(a)$ across space (provinces and urban/rural areas), individuals belonging to a given birth cohort in a province/area with a relatively high fertility prior to the initial policy implementation in 1969 were more intensively affected by fertility policies than members of the same birth cohort in a province/area with low pre-implementation fertility. Finally, within a given province/area, $c + a$ reflects variation across cohorts, since later-born cohorts were younger when the policy was implemented and, as a consequence, their fertility was exposed to more restrictions by the policy than the fertility of older cohorts who were closer to completing their reproductive life span.

The validity of this approach of measuring the exposure to LLF policies has been documented previously. In particular, Chen and Huang (2020) show evidence for a

Table 1. Summary statistics

	Observations (1)	Mean	Standard Dev.	Minimum	Maximum
Suicide rate	3612	27.65993	22.20651	4.05	219.77
Policy exposure	3612	1.911843	1.620291	.0372508	6.688663
Male	3612	.5	.5000692	0	1
Observation year	3612	2011.256	3.960188	2004	2017
Age	3612	70.62791	6.814143	60	87
Birth cohort	3612	1940.628	6.814143	1930	1957

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causal effect of variation in policy exposure on fertility rates. Moreover, previous work found no significant correlation between the timing of the implementation of LLF policies by the FPLG and provincial characteristics prior to the implementation in 1969 [Chen and Huang (2020); Chen and Fang (2021)]. This suggests that the variation in policy exposure is plausibly exogenous in the present context. In addition, the reduced-form approach implicitly assumes that the exposure to LLF policies and the elderly suicide rates apply adequately to the resident population in a given province×urban location. The link between exposure, fertility, and suicide rates might be weakened by systematic internal migration patterns. However, internal migration in China was in fact heavily restricted through the *hukou* system. This system refers to the local registration status of a household, which determines access to food rations, housing, health care, pension benefits, or schools [Wang *et al.* (2017)]. As a consequence of this system, which was particularly strict until the late 1980s, most migrants move temporarily to urban centers for the purpose of study and work, but eventually go back to where their *hukou* is registered. Since the late 1980s, restrictions on internal migration have been relaxed, which allows for more work-related migration of young individuals from rural areas to urban areas. However, as a result of the *hukou* system, most work migrants still eventually return to their home towns. Thus, for a given age cohort, residing in a given location, the exposure to fertility policy was entirely determined by the timing of the policy implementation. Moreover, the shares of migrants are fairly small.[11] This implies that the location of elderly persons is unlikely to be severely affected by work-related migration. Internal migration is therefore unlikely to attenuate the link between exposure to the LLF policy and elderly suicides, supporting the implicit identification assumption.

3. Results

3.1. Main results

Table 2 presents the baseline results for the effect of exposure to the LLF policy on suicide rates among the elderly (aged 60 and older) in China during the period 2004–2017. The table contains results for different specifications of the empirical model (3). In particular, the baseline specification in column (1) includes controls for sex, a linear time trend and a full set of dummies for quinquennial age groups. Alternative specifications include year fixed effects instead of a linear time trend [column (2)], year fixed effects and a linear cohort trend [column (3)], or a full set of cohort dummies [column (4)].

The main finding that emerges regardless of the exact specification is that a greater exposure to the LLF policy is associated with a significantly higher suicide rate among the elderly. The coefficient estimate is similar across all specifications, suggesting that confounders such as non-linear trends or cohort patterns that differ systematically from age patterns are unlikely to drive the main finding. Quantitatively, the estimates

[11]Figure A2 in the online Appendix presents data on the shares of migrants, defined as respondents whose survey address is different from their *hukou*-address, from the 2005 One-percent Population Survey of China. This definition includes cases of temporary visits or travel, as well as migration within county or within city, and thus likely represents an upper bound. Even in urban areas, the shares of so-defined elderly migrants is at the order of less than 15 percent. This is corroborated by the similarity of the population pyramids of elderly resident population and *hukou* population in the literature [see, in particular Wang *et al.* (2017): Table 8].

Table 2. Policy exposure and elderly suicides: baseline results

	Dependent variable: suicide rate			
	(1) Suicide rate	(2) Suicide rate	(3) Suicide rate	(4) Suicide rate
Policy exposure	7.2573***	7.2883***	7.8664***	7.4684***
	(0.8106)	(0.8103)	(0.9060)	(0.8510)
Male=1	10.0919***	10.0919***	10.0919***	10.0919***
	(0.7295)	(0.7307)	(0.7308)	(0.7335)
Time trend	−3.2176***			
	(0.3433)			
Cohort trend			−1.1961***	
			(0.2393)	
Age groups	Yes	Yes	Yes	Yes
Year	No	Yes	Yes	Yes
Cohort	No	No	No	Yes
Observations	3612	3612	3612	3612
R^2	0.4197	0.4242	0.4293	0.4418

Note: OLS estimates. Standard errors allowing for clustering at region×cohort level in parentheses. Policy exposure refers to exposure to LLF policy, see text for details. Mean (std. dev.) of dependent variable: 27.66 (22.21); mean (std. dev.) of policy exposure: 1.91 (1.62). Age: full set of dummies for quinquennial age groups (reference group: 70–74); year: full set of year dummies (reference year: 2004); cohort: full set of cohort dummies (reference cohort: 1950). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

imply that the increased exposure to the fertility policy between cohorts born in 1930 and 1957 accounts for an increase in the suicide rate of about 35 per 100,000, which is sizable.[12]

In addition, the estimation results provide evidence for a significantly elevated suicide rate among elderly men, and a declining time trend in suicides, as well as systematic patterns across ages (with suicide rates increasing among older age groups) and cohorts (with suicide rates being significantly higher among older birth cohorts than among younger ones).[13]

3.2. Robustness

3.2.1. Accounting for regional differences and alternative clustering

Robustness checks reveal that the main results are robust to adding a full set of region dummies to account for systematic differences in elderly suicide patterns across East/

[12]The cohort born in 1930 exhibited almost no exposure, while the cohort 1955 exhibited an exposure of close to 5, see Figure 3. With a coefficient estimate of approximately 7, this implies a total increase of 35, which is larger than the mean in Figure 1(a), see also notes of Table 2. Alternatively, an increase in the policy exposure by one standard deviation (of 1.62) is associated with an increase in elderly suicides of 11.34, which corresponds to about one-half of a standard deviation in suicide rates (of 22.21).

[13]Online Appendix Figure A3 shows the coefficient estimates for the age group dummies and cohort dummies corresponding to column (4) of Table 2.

Central/West Chinese regions. In particular, accounting for region-specific differences delivers qualitatively and quantitatively very similar estimates for the coefficient of interest.[14] Additional replications with alternative assumptions about the clustering of standard errors also reveal the robustness of the main results.[15]

3.2.2. Effect heterogeneity

Next, we explore the possibility of heterogeneous effects across different dimensions. In particular, we first investigate whether the effect of policy exposure exhibits heterogeneity between suicides of men and women, across the urban/rural divide, across regions, and across different cohorts. Heterogeneity is identified by ways of interaction terms with policy exposure. Table 3 shows the corresponding results.

Column (1) of Table 3 shows the estimation results for an extended version of the baseline model that accounts for heterogeneity by sex; the specification includes age, time, and cohort controls [corresponding to column (4) of Table 2]. The estimates reveal a significantly positive effect of policy exposure as in the baseline, which is quantitatively even larger. In the present specification, this coefficient refers to the effect of policy exposure on suicide rates of women. While the results reveal that the suicide rate is higher on average among men, the effect of policy exposure is significantly larger for women than for men, as indicated by the negative coefficient for the interaction term.[16]

Column (2) of Table 3 presents the estimation results for an extended specification with age, time, and cohort controls that allows for heterogeneity in the effect of policy exposure by rural/urban areas. In this specification, the effect of policy exposure is positive but insignificant in rural areas (the reference). In addition, the results are consistent with a higher overall suicide rate among elderly in rural areas (represented by the negative effect of living in urban areas relative to the reference category rural). At the same time, the findings suggest that the effect of policy exposure tends to mostly affect suicides in urban areas (as reflected by the positive and significant interaction term between the indicator for urban and the policy exposure).[17] This suggests an important heterogeneity in the effect, which mainly materializes in urban areas. In light of the rural/urban divide in economic living conditions and in view of the significant decrease in suicide rates among the urban elderly relative to those of elderly in rural areas, these findings appear surprising at first sight. Existing work has argued that this relative decrease might have been due to the improvement in long-term care systems in urban areas and the lack of social support in rural areas [see, e.g., Li *et al.* (2009)]. In addition, the strictness of the LLM policies varied between rural and urban areas, see the discussion above. With our data, it is difficult

[14]See Table A1 in the online Appendix for details.

[15]In particular, the results are robust to 2-way clustering at region and cohort level, and for clustering at the level of regions and rural/urban areas using a wild cluster bootstrap, see Appendix Tables A2 and A3 for details.

[16]Table A4 in the online Appendix documents that quantitatively almost identical results are obtained for the more parsimonious specification with age controls and a linear time trend, but without year and cohort controls. Unreported results for separate regressions for suicides among women and men reveal that the explained variance in suicides increases slightly more for female suicides when accounting for policy exposure than for men.

[17]Unreported results reveal that the effect of policy exposure is insignificant in a specification that controls for urban/rural divide but that does not account for heterogeneous effects of policy exposure across rural and urban areas. See also the discussion below.

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Table 3. Heterogeneous effects by sex, area, and cohort

	Dependent variable: suicide rate				
	(1) Cohorts 1930-1957	(2) Cohorts 1930-1957	(3) Cohorts 1930-1957	(4) Cohorts 1930-1968	(5) Cohorts 1945-1968
Policy exposure	8.345***	−1.167	3.958***	4.305***	2.241***
	(0.893)	(1.407)	(1.082)	(0.397)	(0.136)
Male=1	13.445***	10.092***	10.092***	7.703***	3.033***
	(1.148)	(0.734)	(0.734)	(0.600)	(0.118)
Male=1 × Policy exposure	−1.754***				
	(0.310)				
Urban=1		−28.740***			
		(4.217)			
Urban=1 × Policy exposure		7.078***			
		(1.071)			
East			−24.130***		
			(2.796)		
West			−25.150***		
			(3.006)		
East × Policy exposure			5.297***		
			(0.860)		
West × Policy exposure			3.715***		
			(0.829)		

(*Continued*)

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Table 3. (*Continued.*)

	Dependent variable: suicide rate				
	(1) Cohorts 1930-1957	(2) Cohorts 1930-1957	(3) Cohorts 1930-1957	(4) Cohorts 1930-1968	(5) Cohorts 1945-1968
Age groups	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Cohort	Yes	Yes	Yes	Yes	Yes
Observations	3612	3612	3612	5460	1848
R^2	0.446	0.574	0.584	0.507	0.766

Note: OLS estimates. Standard errors allowing for clustering at region×cohort level in parentheses. Policy exposure refers to exposure to LLF policy, see text for details. Age: full set of dummies for quinquennial age groups (reference group: 70–74); year: full set of year dummies (reference year: 2004); cohort: full set of cohort dummies (reference cohort: 1950). Sample: cohorts born between 1930 and 1957[columns (1)–(3)]; cohorts born between 1930 and 1968 [column (4)]; cohorts born between 1945 and 1968 [column (5)]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

to associate this heterogeneity with one particular factor, in light of how many dimensions differ across rural and urban living environments. Thus, a possible interpretation is that in urban environments, lower fertility and the consequences in terms of a smaller family and reduced companionship with children and kin are potentially felt harder, with the consequence of significantly higher suicide rates among the elderly.[18] This interpretation is consistent with the findings of Chen and Fang (2021) that exposure to restrictive fertility policy had a negative influence on the mental health of the elderly and with earlier findings that suggest that mental health problems are more important as cause of suicide in urban areas than in rural areas, where family conflicts are relatively more important [see, e.g., Li *et al.* (2009); Dong *et al.* (2015), and the citations therein].

Column (3) of Table 3 shows the estimation results when allowing for heterogeneity in the policy effect by region. The estimated main effect of policy exposure is significant and positive for the Center as reference region. While on average, elderly suicide rates are lower in the East and West compared to the center, the significant positive interaction effects with policy exposure reveal that the policy effect is even more pronounced in the East and in the West regions than in the Center.

Columns (4) and (5) of Table 3 present results for an extended sample that includes younger cohorts than in the baseline sample. The baseline results were based on the elderly cohorts aged 60–87 during the period 2004–2017, who were born between 1930 and 1957, for which the variation in the implementation of the LLF policy across time and space implied substantial variation in the exposure to the policy in terms of fertility. To explore the robustness of the results, we extended this sample to cohorts that were younger than 60 during the observation period 2004–2017. To make the measure of policy exposure consistent, we focus on cohorts aged 49 and above who had already completed their reproductive life span. Thus, the younger cohorts comprise individuals born between 1945 (who were age 59 in 2004) and 1968 (who were age 49 in 2017). Despite the overlap in birth years from 1945 to 1957 in baseline and younger cohorts, the age constraint implies that the two sub-samples are exclusive to each other.

The estimation results in column (4) again reveal a significantly positive effect of policy exposure when including also the younger cohorts, with a slightly smaller coefficient estimate than in the baseline specification. The results in column (5) show that, in comparison, restricting the sample to younger cohorts implies an even smaller policy effect than for the baseline cohorts. However, the total effect of the policy exposure is still positive among the younger cohorts, and about a quarter of the size compared to the estimate for the baseline sample for cohorts for which there is more variation in policy exposure.[19]

In light of the considerable heterogeneity across agglomerations and regions reflected by positive interaction terms, we replicate the analysis in a fully flexible setting that

[18]Also notice that one would expect an attenuation of the heterogeneity in the effect of the policy in the presence of selective migration as elderly with rural *hukou* who move to live with their children in urban areas still enjoy lower levels of health and social welfare [see, e.g., Sha *et al.* (2018)].

[19]Alternative specifications allowing for distinct effects of policy exposure by birth cohort in the baseline sample reveal no evidence for significant heterogeneity in the effect across cohorts, but also suggest a greater effect among older cohorts, see online Appendix Figure A4. Additional unreported results also reveal that suicides in urban areas are more frequent among younger cohorts, whereas the effect of policy exposure for young cohorts in urban areas is diminished relative to older cohorts.

allows for heterogeneous treatment effects for each of the three regions and, within these regions, for rural and urban areas. The respective results are reported in Table 4. The first thing to notice is the heterogeneity in the means of elderly suicide rates across regions and urban/rural areas reported in the bottom part of the table. Suicides are substantially higher in rural than in urban areas, as discussed before. Also policy exposure differs, with higher exposure in rural areas than in urban areas due to the higher pre-policy fertility rates. This raises the question about systematic effect heterogeneity that could provide insights related to the mechanisms behind the positive average effect of policy exposure on elderly suicides. To explore this question, we estimate a fully interacted model, which in practice is equivalent to estimating the effect of policy exposure separately for each of the six region/urban–rural cells.

The results in Table 4 reveal a positive and significant effect of policy exposure on elderly suicides in all region/agglomeration cells. At the same time, the results reveal substantial heterogeneity in the effect. Overall, the effect is comparable in the Center and in the Eastern region, but substantially smaller in the Western region. Interestingly, there is no common pattern that would suggest systematically higher or lower effects in rural or urban areas across all regions. In fact, the coefficient estimate is larger in rural areas compared to urban areas in the Center and Eastern regions, whereas the estimate is larger in urban areas in the Western region. To account for heterogeneity in the variation across cells and make the estimates comparable across models, we also report beta-coefficients based on estimates for standardized variables that are comparable across samples at the bottom of the table.[20] When considering beta-coefficients, the effects are largest in the rural areas of the Center region, and similar in the Eastern region, where they are roughly comparable for rural and urban areas. They are smallest in the Western rural areas, and comparable in the urban areas in the Center and in the West. While consistent with the pattern of higher suicide rates during old ages among cohorts that experienced a greater exposure to the restrictive fertility policy in the 1970s, these results issue a note of caution about the quantitative interpretation of the average effects presented above. The effect heterogeneity likely leads to a bias in the average estimates.[21]

3.2.3. Placebo test: randomizing the intensity of policy exposure

To further investigate the robustness of the result, we analyze the source of the identifying variation for our main finding by comparing the estimates obtained from the baseline model to estimates obtained with an exposure measure $PolExp_{p,u,c}$ that is constructed for placebo policies. In particular, the variation in

$$PolExp_{p,u,c} = \sum_{a=15}^{49} \left\{ AFR_{p,u}(a) \cdot I\left[c + a > T_p\right] \right\}$$

comes from three dimensions: the timing of the implementation of the policy (the implementation year T_p), space [province×urban/rural divide, through $AFR_{p,u}(a)$],

[20]See also online Appendix Table A5.

[21]In particular, recent work by de Chaisemartin and D'Haultfoeuille (2020) suggests that the average treatment effect estimates are likely to be biased in a two-way fixed effects setting due to negative weights on some of the average treatment effects in the different region/agglomeration groups.

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Table 4. Heterogeneous effects by region and urban/rural area

	Dependent variable: suicide rate					
Sample:	Center/rural	Center/urban	East/rural	East/urban	West/rural	West/urban
	(1)	(2)	(3)	(4)	(5)	(6)
Policy exposure	19.738***	10.876**	16.105***	15.083***	2.942***	6.370***
	(4.715)	(4.052)	(3.402)	(2.843)	(0.464)	(1.121)
Male	Yes	Yes	Yes	Yes	Yes	Yes
Age groups	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Mean suicide rate	56.1	22.5	29.3	14.0	25.4	18.7
Mean policy exp.	2.70	1.42	2.28	0.88	2.83	1.36
Observations	602	602	602	602	602	602
R^2	0.906	0.846	0.852	0.812	0.867	0.802
Beta-coeff.	1.060***	0.891**	1.634***	1.888***	0.519***	0.742***
	(0.253)	(0.332)	(0.345)	(0.356)	(0.082)	(0.131)

Note: OLS estimates. Standard errors allowing for clustering at the cohort level in parentheses. Policy exposure refers to exposure to LLF policy, see text for details. Age: full set of dummies for quinquennial age groups (reference group: 70–74); year: full set of year dummies (reference year: 2004); cohort: full set of cohort dummies (reference cohort: 1950). Sample: region/urban–rural splits. Beta coefficients correspond to coefficient estimates based on measures of suicides rates and policy exposure that have been standardized on the respective sample. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

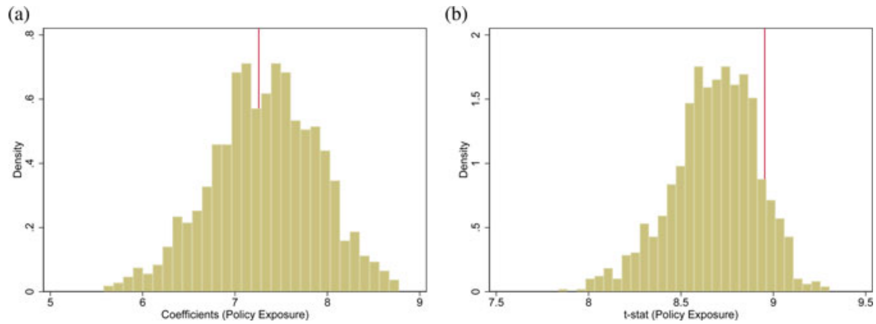

Figure 4. Placebo: random implementation of policy (time). (a) Coefficient estimates, (b) inference (t-statistics).
Note: Panel (a): coefficient estimates for β as in specification of column (1) of Table 2. Panel (b): t-values for estimates of β in panel (a), p-value > 0.5. Estimates based on a placebo data set of 1,000 iterations of randomized policy assignments over time, see text for details.

and birth cohorts (through c). To disentangle which dimension is crucial for the identification of the effect, we constructed alternative measures of policy exposure in which one of these three dimensions is replaced by a placebo based on randomization.

First, in order to generate a placebo regarding the time variation in exposure, we randomly assign policy implementation years to provinces. The FPLG implemented the LLF policy across all provinces between 1969 and 1975. In order to have a comparable pattern of implementation overall, provinces are randomly designated as implementing the fertility policy within each year, according to the actual distribution of implementation years without replacement. This implies that, for each year from 1969 to 1975, the number of provinces that had implemented the fertility policies in the placebo exercise is the same as the number of provinces that actually implemented the fertility policies in the observed data. Using the randomized policy implementation years, we compute policy exposure $PolExp_{p,u,c}$ as in (1). Then, we calculate the weighted average for each region×urban/rural area, $PolExp_{r,u,c}$, in the same way as for the observed data as in (2). We repeat the randomization for 1,000 times and replicate the estimation of the specification in column (1) of Table 2 for each placebo sample. Figure 4 shows the distribution of the resulting estimates of β and the corresponding distribution of t-statictics for the 1,000 estimated placebo treatment effects. The vertical line represents the location of the baseline estimates corresponding to column (1) of Table 2. The results suggest that the placebo estimates exhibit fairly similar estimation results as the baseline estimates. The likely explanation for this finding is that the variation in the timing alone is not what identifies the estimated effect in the baseline results; instead, the estimates rely on variation in the other components of the policy exposure variable $PolExp_{p,u,c}$ rather than time variation of policy implementation.[22] Very similar results are obtained when estimating the coefficient of interest with the specification with age, time, and cohort controls as in column (4) of Table 2 for each placebo sample.[23]

[22]Alternatively, averaging across provinces to obtain a measure for each region×urban/rural area might wash out relevant variation.

[23]See Figure A5 in the online Appendix.

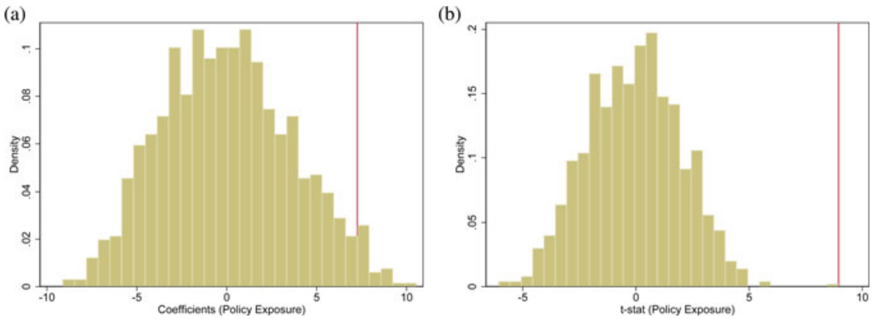

Figure 5. Placebo: random implementation of policy (space and cohorts), (a) coefficient estimates, (b) inference (t-statistics). *Note:* Panel (a): coefficient estimates for β as in specification of column (1) of Table 2. Panel (b): t-values for estimates of β in panel (a), p-value < 0.001. Estimates based on a placebo data set of 1,000 iterations of randomized policy assignments across both space and cohort, see text for details.

Second, we randomize the assignment of the policy implementation across space and cohorts. In particular, after calculating $PolExp_{p,u,c}$ based on the actual policy implementation year in each province as in (1), we randomly assign a combination of province×urban/rural area to each set of cohort-specific policy exposure measures, $PolExp_{p,u,c}$, and then, within each province×urban/rural area, randomize these measures across cohorts and then calculate weighted average $PolExp_{r,u,c}$ for each spatial entity (region×urban/rural area) as in (2). This randomization implies that the variation across time (policy implementation year) remains unchanged, while a placebo mapping is obtained across spatial entities (province×urban/rural area) and birth cohorts. We conduct this placebo construction for 1,000 times and then re-estimate for each placebo the effect with the empirical framework (3). Figure 5 plots the corresponding coefficient estimates and t-values. On average, the coefficient estimates are smaller compared to the baseline estimates, and often negative. Also the t-values are systematically smaller, but for some random draws, the coefficient estimates are still at the size of the baseline estimates, and significant with t-values exceeding the thresholds for conventional significance levels. This suggests that variation across space and cohorts is relevant for the identification of the effect of interest, but that randomization in these two dimensions is not enough to eliminate the effect in all cases.[24]

Taken together, the randomization of the individual components of the variation in the policy exposure measure affects the results, particularly when considering variation across space and cohorts. At the same time, these exercises suggest that it is not variation in one of these dimensions of variation alone that is responsible for the identification of the effect. To document that it is the combination of the different dimensions of identifying variation, we finally present results for a placebo that combines the randomization across the different dimensions as described before. In other words, we simultaneously implement a randomized assignment of the LLF policy across space, cohorts, and time.

[24]Figure A6 in the online Appendix shows the corresponding results for the specification with age, time, and cohort controls as in column (4) of Table 2. Similar results emerge when randomizing separately by cohort or space, see Figures A7 and A8 in the online Appendix.

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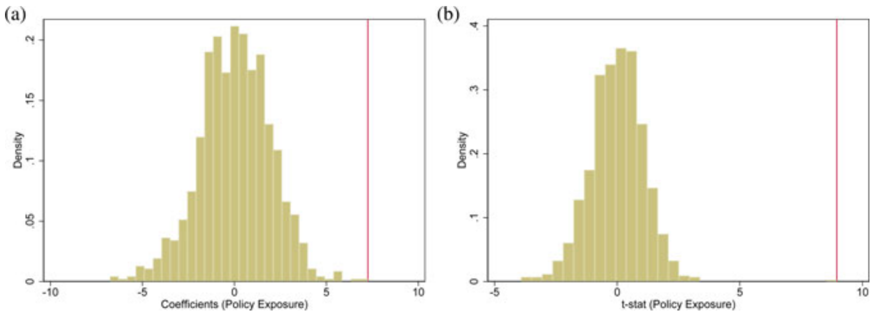

Figure 6. Placebo: random implementation of policy (space, cohort, and time). (a) Coefficient estimates, (b) inference (t-statistics). *Note:* Panel (a): coefficient estimates for β as in specification of column (1) of Table 2. Panel (b): t-values for estimates of β in panel (a), p-value < 0.003. Estimates based on a placebo data set of 1,000 iterations of randomized policy assignments across space, cohorts, and time, see text for details.

Specifically, we conduct a random assignment of policy implementation years for 1,000 times and compute the corresponding policy exposure measure $PolExp_{p,u,c}$ as in (1), and then randomly assign $PolExp_{p,u,c}$ across cohorts and spatial entities (province×urban/rural areas). For each of the 1,000 randomized samples, we then compute $PolExp_{r,u,c}$ as in (2) and regress the elderly suicide rate on the resulting weighted placebo policy exposure measure $PolExp_{r,u,c}$. Figure 6 shows the corresponding coefficient estimates and t-values in comparison to the baseline estimates. The results reveal that the distribution of estimates based on the placebo data are closely centered around zero. Similarly, the distribution of the corresponding t-values is closely centered around zero, leaving hardly any significant estimates, and with sizes substantially smaller than in the baseline results.[25] This suggests that the identification of the effect of policy exposure indeed draws on the combination of three dimensions of identifying variation across time, space, and cohorts.

4. Discussion: channels and policy relevance

The evidence for a reduced form effect of exposure to the restrictive LLF fertility policy raises the question about potential channels through which the effect works. Moreover, an understanding of the channels is important for the design of policies to reduce elderly suicides. In the following, we investigate two channels that have been considered in previous research: material/physical well-being and mental well-being. To investigate the channels of physical and mental well-being, we make use of the 2011 wave of the China Health and Retirement Study (CHARLS), a survey study that is based on a nationally representative sample of Chinese residents aged 45 and older. Our analysis builds on previous work by Chen and Fang (2021), who documented the effects of the LLF policy on fertility and the impacts on the physical and mental well-being of parents based on several waves of the CHARLS. Their results show that parental well-being is significantly reduced as a consequence of exposure to the policy, whereas the effects on physical well-being and financial support are not significant.

[25] Figure A9 in the online Appendix shows the corresponding results for the specification with age, time, and cohort controls as in column (4) of Table 2.

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In our analysis, we make use of the same survey information about physical and mental well-being as Chen and Fang (2021). In particular, as proxy measures for material and physical well-being, we use information in four categories: total household expenditures,[26] an indicator for being underweight,[27] the number of chronic health conditions,[28] and self-rated health status.[29] As proxy measures for mental well-being, we also use information in four categories: the number of monthly visits to parents, the number of monthly contacts with parents, a scale of depression symptoms developed by the Center for Epidemiologic Studies Depression Scale (CES-D Scale),[30] and a binary measure of depression that takes value 1 if the CES-D Scale measure exceeds a value of 10. While the former two measures capture social interactions that have been documented to improve parental mental well-being, the latter two measures constitute indicators for mental health problems. To incorporate this information with our previous analysis, we construct averages for each of these proxy measures for the same region-urban/rural-cohort-gender cells as in our main sample.[31]

Our analysis proceeds in two steps. In a first step, we investigate whether elderly suicide rates are related to the different dimensions of physical and mental well-being in OLS regressions. This provides a direct link between the results reported by Chen and Fang (2021) and our analysis of elderly suicides. In a second step, we investigate the channels through which exposure to the LLM policy affects elderly suicides by combining the two stages in a 2SLS estimation framework. The first stage uses policy exposure as instrument for the different measures of physical and mental well-being, whereas the second stage regresses elderly suicides on the instrumented well-being measures. This analysis is not intended to deliver causal estimates, but rather as an exploratory analysis that is informative about potential channels for the reduced form evidence.

The results for material and physical well-being reveal no systematic relation between these measures and elderly suicide rates in OLS regressions. Lower total household expenditures are related with higher suicide rates, but the other variables exhibit no significant effects, casting doubts on a channel that works through financial conditions or physical health. This finding is confirmed by the 2SLS estimates, which show no significant relationship between policy exposure and these measures of well-being on the first stage, and also no effect on the second stage.[32] The only

[26]Household expenditures are calculated as the sum of expenditures in 23 categories.

[27]The index is based on a body mass index (BMI) below 18.5; the BMI is computed as (weight in kilograms)/(body height in meters)^2.

[28]The CHARLS contains information 14 chronic conditions diagnosed by a doctor. These include hypertension, dyslipidemia, diabetes, cancer, lung disease, liver disease, heart problem, stroke, kidney disease, digestive disease, emotional problem, memory-related disease, arthritis, and asthma.

[29]The health status is measured on a range from 1 (excellent) to 5 (poor).

[30]The CES-D Scale is a brief questionnaire about depression symptoms that comprises ten questions, of which eight concern negative behaviors and two concern positive behaviors. For each question, responses range from 0 to 3, depending on the frequency of the respective behaviors (in days per week).

[31]Before averaging, we remove old people who cohabitate with their children, as in Chen and Fang (2021).

[32]See online Appendix Table A6 for details. The empirical specification controls for the same controls as in the analysis by Chen and Fang (2021), with the exception of ethnic minority (this information is not contained in the 2011 wave of the CHARLS) and using age of the main respondent (instead of the age gap between the main respondent and spouse) as control.

exception is the indicator of being underweight, which is affected positively by policy exposure on the first stage, and associated with higher suicide rates on the second stage.

In contrast, the results for mental well-being deliver evidence for a potential channel for the reduced form evidence. The OLS results document that more social contacts with children, in the form of monthly visits or monthly contacts, are associated with a significantly lower suicide rate. Likewise, higher scores on the depression scale are associated with greater suicide rates. When considering the 2SLS results, policy exposure is associated with a significant reduction in social contacts and a significant increase in depression symptoms on the first stage. In contrast to physical well-being, the first stage results for policy exposure are strong. The corresponding results on the second stage consistently reveal higher suicide rates as a result of reduced social contacts and increased depression symptoms, with the exception of the number of monthly contacts with parents, which only exhibits a weak and insignificant estimate on both stages.[33]

Taken together, these results complement and extend those reported by Chen and Fang (2021), who found little evidence for an effect of the restrictive fertility policy on financial and physical well-being, but a significant deterioration in the mental well-being of elderly parents. Our results suggest that the deterioration in parental mental health is reflected in elderly suicides, while we find little evidence for an effect that works exclusively or even primarily through material and physical well-being.[34] These results call for policies aiming at improving the mental well-being of elderly individuals that were exposed to restrictive fertility policies.

5. Concluding remarks

Overall, our results show that exposure to the "LLF" campaign to reduce fertility in the 1970s continues to have persistent effects on suicide patterns among the elderly still today. This finding emerges above and beyond the well-documented time and age trends in suicide rates, and beyond systematic heterogeneity in suicides across birth cohorts. The effect exhibits some heterogeneity across men and women and varies substantially across regions and rural and urban areas. While elderly suicide rates are generally lower in urban areas, the effect of exposure to the fertility policy is larger in urban areas in the Eastern and Western regions. Moreover, the findings suggest that younger cohorts are affected less by variation in policy exposure.

The results confirm earlier conjectures about a link between fertility, fertility policy, and suicide patterns among the elderly. Thereby, they contribute to a better understanding of the underlying causes for the notable suicide patterns among elderly in China. The findings can also rationalize recent evidence that documents a declining trend in the absolute number of elderly suicides since the late 1990s and a convergence of the rural/urban divide that began around the same time [see, e.g., Zhong *et al.* (2016); Sha *et al.* (2017)], since this timing coincides with the end of the reproductive life span of the last cohorts that were affected differentially by the fertility policies of the 1970s and 1980s. While there is an ongoing debate about the prospects for a rebound of elderly suicide rates in the literature [see, e.g., Parry (2014); Wang *et al.* (2014); Zhong *et al.* (2016); Sha *et al.* (2017)], our findings of a

[33]See online Appendix Table A7 for details.

[34]This is consistent with the results for a higher prevalence of being underweight, which might also be related to depression, a lack of personal care, or a lack of social interactions.

declining trend of suicides across cohorts, together with the fading out of the policy effects, particularly in urban areas, suggest that there might be opposing effects at work. It should also be noted, however, that for reasons of data availability, our analysis restricted attention to suicides among the cohorts affected by restrictive fertilities when the respective individuals are already older than 60 years. Hence, our analysis does not capture potential additional effects on suicides due to the exposure to restrictive family policies among the cohorts affected earlier in life because of the lack of data for some years. In this respect, the effects on the suicides among the elderly might in fact represent a lower bound of the overall effects of the exposure to restrictive fertility policies.

In terms of policy, the results indicate the need for increased attention to the mental health of elderly that have been particularly affected by restrictive fertility policies. Our findings complement earlier findings of considerable effects of the LLM policy on social interactions and mental well-being by Chen and Fang (2021) and suggest that policies that alleviate the psychological and social consequences of low fertility among the elderly might be most promising for reducing elderly suicides. More work is needed for a better understanding of the considerable unexplained variation in suicides.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/dem.2023.18.

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