Three Essays in Health Economics

by

Xiaolong Hou

(Under the Direction of Meghan Skira)

Abstract

This dissertation examines how family structure, labor market shocks, and historical legacies shape individual health behaviors and outcomes. In chapter 1, I study the mental health effects of becoming a grandparent using panel data from the Korean Longitudinal Study of Aging (KLoSA). Event study estimates show that the transition to grandparenthood reduces the likelihood of probable depression by 5.5 percentage points, a 35 percent decline relative to the baseline mean. Reduced loneliness emerges as the primary channel. To better understand the mental health effects, I examine potential mechanisms including labor market transitions, relationship satisfaction, and caregiving responsibilities. Using pre-birth geographic distance as a proxy for caregiving intensity, I find that grandparents who are less likely to provide intensive childcare experience larger mental health gains. This suggests that the psychological benefits of grandparenthood may be attenuated when accompanied by substantial caregiving demands. Chapter 2 examines how the restructuring of state-owned enterprises (SOEs) in late 1990s China affected smoking and alcohol use. The study compares SOE workers, who faced widespread layoffs and heightened job uncertainty, with government employees in stable positions. Using a difference-in-differences design, the analysis finds that exposure to SOE reform led to higher rates of smoking, increased cigarette consumption, and greater alcohol use. These effects are concentrated among less-educated workers and those in smaller firms, consistent with greater vulnerability to economic disruption. The findings highlight the behavioral costs of labor market shocks and the uneven burden placed on disadvantaged groups. Chapter 3 explores the long-term impact of historical institutional failure on present-day health behavior. Focusing on the legacy of the Tuskegee Syphilis Study, the analysis shows that counties closer to Tuskegee experienced slower reductions in the black–white gap in COVID-19 vaccination rates. The results suggest that longstanding medical mistrust continues to shape racial disparities in public health uptake, even in response to modern interventions.

Index words: Mental Health, Health Behaviors, Vaccination, Grandparenthood, Family dynamics, State-owned Enterprise Reform, Job Insecurity.

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Dedication

To my family —
my parents, my grandmother, my aunt, and uncle —
for their unwavering support and belief in me throughout this journey.

And to my grandfather, Shanxiu Hou,
who meant the world to me and passed away on September 24, 2023.
I miss you every day.
Your strength, wisdom, and warmth continue to guide me in everything I do.

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Chapter 1

The Grandparent Health Dividend: Transitioning to Grandparenthood and Its Impact on Mental Health

1.1 Introduction

The World Health Organization (WHO) estimates that suicide claimed over 700,000 lives globally in 2019, accounting for 1.3% of all deaths (World Health Organization, 2021). Recognizing this crisis, the WHO extended its Mental Health Action Plan to 2030, urging countries to improve mental health services, crisis interventions, and community support. Among Organization for Economic Co-operation and Development (OECD) countries, South Korea reported the highest suicide rate, with 24.6 deaths per 100,000 people in 2019, which is more than twice the OECD average of 11.2.[2] This issue is particularly severe among older adults, whose suicide rates far exceed those of younger groups.[3] As the share of South Koreans aged 65 and older surpassed 14% of the population in 2017 (K. W. Kim & Kim, 2020), understanding how demographic and social changes shape mental health in later life is critical for explaining elevated suicide risk in old age and for informing targeted policy responses.

Previous studies have identified depression, chronic illness, poverty, social isolation, poor self-rated health, loneliness, and limited social support as critical risk factors for suicidal ideation among older adults (Blázquez-Fernández et al., 2023; J.-I. Park et al., 2016). However, little is known about how the transition

[2] Data obtained from OECD Data Indicators, Suicide rates, available at OECD Suicide rates. Corresponding data are shown in Figure A.1

[3] According to data from the World Health Organization (2024), the suicide rate in South Korea in 2019 was approximately 68 and 73 per 100,000 among individuals aged 75–84 and 85 and older, respectively. These rates are more than twice as high as those observed among individuals under 45. The corresponding figure is reported in Figure A.2

to grandparenthood influences mental health risks and suicidal ideation in this population. South Korea has experienced a dramatic decline in fertility rates, from 5.21 in 1960 to 2.76 in 1980, reaching 0.84 in 2020, which is the lowest rate worldwide (K. Kim et al., 2023). Family planning policies from the 1960s and 1970s aimed at controlling population growth contributed to smaller family sizes (Choe & Park, 2006). Furthermore, economic pressures, high costs of child-rearing and education, and a shift toward individualism have further constrained fertility and led to delayed family formation (S. Kim et al., 2024; Seo, 2019). By 2018, the average age at first marriage had risen to 33.1 for men and 30.4 for women, with the average age of first childbirth for women reaching 31.9.[4] These trends delay the timing of grandparenthood and may prevent some individuals from experiencing it altogether. As a pivotal family role that reshapes daily routines, social interactions, and identity, the transition to grandparenthood may exert both protective and adverse effects on mental health in later life.

[4] Data obtained from the Korean Statistical Information Service (KOSIS), operated by Statistics Korea, based on national administrative records. Available at https://kosis.kr/eng/.

To examine the effect of becoming a first-time grandparent on mental health, I follow the event-study framework of Kleven et al. (2019) and use panel data from the Korean Longitudinal Study of Ageing (KLoSA). The analysis focuses on individuals aged 45 to 75 who transitioned into grandparenthood between 2006 and 2018. Exploiting variation in the timing of grandchild birth, I compare mental health outcomes between new grandparents and individuals who have not yet become grandparents. The results indicate a significant improvement in mental health following the birth of the first grandchild: the probability of probable depression declines by 5.5 percentage points, equivalent to a 35 percent reduction from the sample mean.[5] While grandfathers exhibit larger improvements than grandmothers, the gender difference is not statistically significant. An item-level analysis of the CES-D 10 further shows that the overall improvement is primarily driven by a 28 percent decline in reported feelings of loneliness among new grandparents.

[5] Each wave represents a two-year interval following the birth of the first grandchild.

These results are robust across various sensitivity checks. First, the event-study estimates show no evidence of differential pre-trends. The Honest DiD sensitivity analysis developed by Rambachan and Roth (2023) further confirms that the findings remain stable under modest deviations from the parallel trends assumption. Second, following the approach of Malisa (2024), I conduct a placebo test by randomly assigning grandparenthood timing to individuals who never became grandparents during the 12-year panel. The absence of significant effects in this test reinforces the validity of the main results. Third, to account for heterogeneous treatment effects across treatment cohorts, I apply the estimation method of L. Sun and Abraham (2021), which allows for variation in dynamic treatment effects and yields results consistent with the main specification. Finally, the findings remain robust when the sample is restricted to individuals observed in a balanced panel around the event time.

The impact of becoming a first-time grandparent differs by socioeconomic status. Grandparents with higher levels of education experience significantly greater improvements in mental health, while those with lower educational attainment do not exhibit comparable gains. This disparity may reflect differences in financial security and the capacity to participate in grandparenting without a substantial economic burden. Lower-educated grandparents are more likely to face financial constraints and competing role demands, whereas higher-educated grandparents often have greater flexibility and resources to provide support. Additionally, no significant differences are observed between paternal and maternal grandparents, suggesting a shift away from traditional patrilineal norms toward a more balanced bilateral kinship structure in contemporary South Korea.

I explore several mechanisms through which grandparenthood may influence mental health. First, becoming a grandparent does not significantly change health behaviors, including the likelihood of engaging in weekly physical activity, smoking, number of cigarettes smoked per day, alcohol consumption, or frequent drinking. This suggests that the observed mental health improvements are unlikely to result from adopting healthier lifestyles. Second, the birth of a grandchild affects labor market transitions differently by gender. Grandmothers are more likely to retire following the birth, whereas grandfathers are not. This gender difference may help account for the observed mental health improvements, as retirement is generally associated with null or negative effects on mental health (Nishimura et al., 2018). Third, using the geographic distance between adult children and their parents one wave prior to the birth of the first grandchild as a proxy for the likelihood of providing intensive childcare, I find that mental health improvements are concentrated among grandparents who are less likely to assume caregiving responsibilities. This pattern is consistent with prior research (Eibich & Zai, 2024) and suggests that the psychological benefits of grandparenthood may be diminished when accompanied by substantial caregiving demands.

Additionally, I examine whether this effect is driven by the first marriage of an adult child rather than by the birth of the first grandchild. Figure A.44 shows that less than 40 percent of first marriages occur in the same wave as the birth of the first grandchild. When I re-estimate the event study using the timing of the first marriage as the treatment, there is no significant impact on mental health. Moreover, the probability of cohabiting with children declines significantly following the first marriage, while there is no corresponding increase after the birth of a grandchild. These changes in cohabitation are associated with a shift in communication patterns, with less in-person interaction and more frequent phone contact, which may help mitigate intergenerational conflict over parenting styles. Finally, following the birth of a grandchild, satisfaction with child relationships improves significantly, which might also contribute to improvements in mental health.

This study contributes to the literature on the health impacts of becoming a grandparent, particularly in the context of an aging East Asian society. Although several studies have examined the health effects of transitioning to grandparenthood, most rely on cross-sectional variation or fixed effects models that identify correlations rather than causal effects (Lai et al., 2021; Yang, 2022; Zhang et al., 2022). Leimer and Van Ewijk (2022) use a two-way fixed effects model with data from the Survey of Health, Ageing and Retirement in Europe (SHARE) to estimate the health implications of grandparenthood in Western European countries. Their findings reveal a negative impact on subjective assessments of health, with minimal effects on physical health and cognitive function. However, their use of individuals who never become grandparents as a control group raises concerns about the validity of comparisons, as this approach may not fully account for time-varying unobserved differences between never-grandparents and new grandparents. These differences, such as changes in health trajectories or family involvement over time, may confound the estimated effects. Moreover, the impact of grandparenthood may change as individuals adapt to their new roles, suggesting that different phases of grandparenthood might yield varying health effects. The event-study design adopted in this paper addresses these concerns by leveraging variation in the timing of grandchild birth, allowing for a dynamic analysis of mental health trajectories. The institutional and cultural context of South Korea also provides new evidence that complements existing findings from Western settings, offering a broader perspective on the health implications of becoming a new grandparent.

This study also contributes to the literature on the health impact of providing care for grandchildren. Since unobserved factors that correlate with caregiving may also influence health outcomes, Ku et al. (2012) addresses the potential endogeneity of grandparent caregiving by using the number of grandchildren and the marital status of adult children as instrumental variables. However, these instruments may not satisfy the exclusion restriction, as I show here that the birth of a grandchild can directly affect the health outcomes of grandparents, not solely through caregiving. Furthermore, the impact of caregiving may vary based on the age of the grandchildren and the intensity of care provided, making it challenging to distinguish the general effects of becoming a grandparent from the specific effects of caregiving responsibilities (Sheppard & Monden, 2019). Although this study does not directly examine the impact of caregiving on mental health, the findings show that health benefits are concentrated among grandparents who are less likely to engage in intensive childcare. This finding is consistent with previous research showing that intensive caregiving can adversely affect mental health (Eibich & Zai, 2024), suggesting that the potential benefits of grandparenthood may be reduced when accompanied by intensive caregiving responsibilities.

Finally, this study contributes to a deeper understanding of how fertility events influence the mental health of older adults through changes in family relationships and living arrangements. The analysis shows that the birth of a grandchild often coincides with a decline in intergenerational cohabitation, but this shift does not appear to weaken family ties. Instead, communication increasingly takes place through remote channels such as phone calls, and new grandparents report greater satisfaction in their relationships with their adult children. These patterns suggest that adjusted living arrangements, combined with sustained communication, may help reduce intergenerational tensions and strengthen family bonds. More broadly, the results show that new births affect older family members by influencing their health outcomes and labor market behavior. While previous studies have examined the effects of grandchild birth on the labor supply of grandparents in the United States, Europe, and China (Backhaus & Barslund, 2021; Feng & Zhang, 2018; Frimmel et al., 2022; Malisa, 2024; Rupert & Zanella, 2018), this study contributes new evidence from South Korea, a rapidly aging society with distinct family structures and caregiving norms. These findings underscore the importance of considering how fertility-related policies may indirectly affect older adults, particularly in non-Western settings that remain underrepresented in the literature.

The paper is organized as follows: Section 1.2 discusses the conceptual framework for how becoming a new grandparent might impact health. Sections 1.3 and 1.4 outline the data and empirical strategy employed in the analysis. Section 1.5 presents the main results, supplemented by robustness checks, heterogeneity analysis, and an exploration of potential mechanisms. Finally, Section 1.6 concludes the paper.

1.2 Conceptual Framework

Role enhancement theory suggests that taking on additional roles can have positive effects on health (Sieber, 1974). The transition to grandparenthood often provides a sense of purpose and fulfillment, potentially leading to improved mental health. Research indicates that grandparenthood is frequently regarded as one of the most satisfying aspects of older age (Mahne & Motel-Klingebiel, 2012). Studies have shown psychological benefits associated with grandparenthood, including reduced symptoms of depression and improvements in cognitive abilities and physical strength (Arpino & Bordone, 2014; Ku et al., 2012; Xu, 2019). For example, Ahn and Choi (2019) estimate a fixed-effects instrumental variable model using the presence of a married child and a child aged 31–40 years as instruments, demonstrating that caregiving for grandchildren significantly enhances cognitive functioning in grandparents. Furthermore, positive correlations between grandparenthood and mental health have been observed, particularly among non-residential grandparents or those providing occasional caregiving in South Korea (Danielsbacka et al., 2022). Additionally, grandparenthood typically leads to increased social interactions, which are associated with a higher quality of life and better self-assessed health (Petrou & Kupek, 2008). Engaging in activities such as reading, playing games, and other cognitively stimulating interactions with grandchildren may help delay cognitive decline (Ku et al., 2012).

Generativity theory offers another perspective on the benefits of grandparenthood. This theory posits that grandparenthood provides older adults with opportunities to pass on knowledge, skills, and life experiences to their grandchildren, yielding psychological benefits (Erikson & Erikson, 1998). The sense of contributing to future generations can enhance self-esteem and life satisfaction. Research shows that many grandparents experience feelings of achievement and pride related to their grandparental role, which can positively influence their mental health (Cunningham-Burley, 1986; Kaufman & Elder Jr, 2003). These findings suggest that the generative aspects of grandparenthood play a crucial role in promoting health among older adults.

Despite the potential benefits, grandparenthood can also have negative impacts. Role strain theory indicates that new roles can lead to stress, particularly when individuals struggle to meet new responsibilities (Goode, 1960). For grandparents, increased demands on personal resources, such as time and finances, can cause stress, adversely affecting health, especially for those with significant childcare responsibilities. Role overload may lead to burnout and negative health outcomes. Furthermore, the transition to grandparenthood can sometimes strain relationships with adult children, particularly if disagreements about child-rearing practices arise. Poor relationships with adult children may result in reduced contact with grandchildren, potentially causing emotional distress for grandparents. These factors highlight the complex nature of the grandparental role and its varied impacts on individual health.

The effects of grandparenthood are often intertwined with other major life changes. The transition to grandparenthood frequently coincides with or triggers early retirement, especially among grandmothers (Frimmel et al., 2022; Rupert & Zanella, 2018). In South Korea, I find that grandmothers are more likely to retire after the birth of their first grandchild, while grandfathers are not. The health impacts of retirement have been extensively studied, with some research suggesting that retirement can reduce stress and improve mental health due to increased leisure time and decreased work-related pressures (Nishimura et al., 2018). However, other studies highlight potential negative consequences, including reduced physical activity, diminished social engagement, and loss of purpose, all of which can adversely affect health (Mazzonna & Peracchi, 2017). The health effects of retirement largely depend on its nature and context. For instance, early retirement associated with grandparenthood may lead to a loss of daily structure and work-related social interactions, which are crucial for maintaining mental and physical health (Frimmel et al., 2022). Furthermore, the economic strain resulting from unexpected early retirement can exacerbate stress levels and negatively impact health outcomes (Backhaus & Barslund, 2021). Using KLoSA, Oh (2024) finds an adverse but not significant impact of retirement on mental health in South Korea. These patterns help to understand gender differences in mental health responses to grandparenthood, particularly if grandmothers and grandfathers respond differently in their labor force participation following the transition.

The impact of grandparenthood on health and mental health is mediated by several factors. First, the quality of relationships with adult children plays a crucial role. Positive intergenerational support enhances health, while conflicts may lead to stress and negative outcomes. Second, the economic implications of grandparenting are significant; reduced labor supply or early retirement can affect financial stability, potentially causing stress. However, decreased work-related pressure and increased leisure time may positively influence health. Third, the extent of childcare responsibilities can vary, affecting the balance between benefits, such as increased life satisfaction and cognitive stimulation, and challenges, like role strain. The transition to grandparenthood can also reshape social interactions, with implications for mental health. On one hand, it may enhance social engagement and support, bolstering psychological well-being. On the other hand, if grandparenting duties constrain time for peer interactions, it could lead to social isolation and negative health outcomes (Danielsbacka

& Tanskanen, 2016). These diverse effects underscore the complexity of the mental health impact of grandparenthood in later life.

1.3 Data and Descriptive Statistics

This paper uses data from the Korean Longitudinal Study of Ageing (KLoSA), a sister dataset to the Health and Retirement Study (HRS) that tracks individuals aged 45 and older in South Korea. KLoSA is a multidisciplinary, nationally representative panel survey focused on aging in Korea. Initiated in 2006, it conducts biennial surveys, collecting extensive information on demographics, family structure, economic status, and health outcomes. The design of KLoSA offers two critical advantages for this study: first, its panel structure allows for tracking the same individuals over time; second, it provides detailed data on both family structure and health outcomes, enabling precise identification of grandparenthood transitions and subsequent health changes.

This analysis draws on data from 2006 to 2018, providing a 12-year window to examine the mental health impacts of transitioning to grandparenthood. The sample is restricted to individuals aged 45 to 75 who were parents but not yet grandparents at their first survey. Individuals who were always grandparents throughout their survey participation are excluded, as well as participants lacking information on the key outcome of mental health. To be included, individuals must appear in the survey at least twice: once before and once after becoming a grandparent. This criterion ensures the availability of pre- and post-grandparenthood data for each participant.

For the main analysis, the not-yet-grandparent group serves as the control, providing a basis for assessing the impact of grandparenthood. In robustness checks, I include individuals who never became grandparents during their survey participation as a control group, incorporating person-fixed effects to examine the within-person mental health impacts of becoming a new grandparent. The final sample used in the main analysis comprises 1,708 individuals and 11,004 individual-wave observations. Summary statistics are presented in Table 1.1.

1.3.1 Transitioning to grandparenthood

The survey collects information on the number of children reported by each respondent, allowing for the calculation of the total number of grandchildren. To obtain this total, I sum the number of children reported by the children of each respondent. Although the exact birth years of grandchildren are not pro-

Table 1.1: Summary statistics

	Mean	SD	Min	Max	Number
Age	52.92	5.03	45.00	72.00	1708
Male	0.48	0.50	0.00	1.00	1708
Education					
Less than high school	0.41	0.49	0.00	1.00	1708
High school	0.47	0.50	0.00	1.00	1708
College or more	0.12	0.33	0.00	1.00	1708
Marital status					
Married/partnered	0.93	0.25	0.00	1.00	1708
Separated/divorced/widowed	0.06	0.25	0.00	1.00	1708
Never married	0.00	0.03	0.00	1.00	1708
Rural	0.18	0.38	0.00	1.00	1708
Labor force status					
Employed	0.58	0.49	0.00	1.00	1675
Unemployed	0.03	0.17	0.00	1.00	1675
Not in the labor force	0.39	0.49	0.00	1.00	1675
Retired	0.10	0.30	0.00	1.00	1675
Number of children	2.23	0.67	0.00	5.00	1708
Number of sons	1.20	0.70	0.00	4.00	1708
First child is boy	0.54	0.50	0.00	1.00	1703
Child Gender Composition					
Mixed	0.57	0.50	0.00	1.00	1703
All Boys	0.29	0.45	0.00	1.00	1703
All Girls	0.14	0.35	0.00	1.00	1703
CES-D10 Score	5.19	4.09	0.00	30.00	1704
Probable depression (CES-D $\geq$ 10)	0.12	0.32	0.00	1.00	1708

Note: The CES-D score ranges from 0 to 30, with higher scores indicating greater depressive symptoms. Probable depression is defined as a CES-D score of 10 or higher. These statistics reflect the characteristics of individuals as they appeared in the survey.

vided, I can identify the transition to grandparenthood by observing changes in the number of grandchildren across successive survey waves.

I define a time-varying indicator for the transition to grandparenthood based on the calculated number of grandchildren in each wave. Let $N_{i,t}$ represent the number of grandchildren that individual i has at time t, calculated by summing the number of children reported by the children of each respondent up through wave t. The wave in which individual i first becomes a grandparent, denoted as GrandparentWave_i, is defined as follows:

$$\text{GrandparentWave}_i = \begin{cases} t & \text{if } N_{i,t-1} = 0 \text{ and } N_{i,t} > 0, \\ \text{undefined} & \text{if } N_{i,t} = 0 \text{ for all observed waves } t. \end{cases}$$

1.3.2 Outcomes

The key outcome of this study is mental health, measured using the 10-item Center for Epidemiological Studies Depression (CES-D10) scale. The CES-D10 is a widely validated screening tool for assessing depressive symptoms and has proven appropriate across various racial, gender, and age groups (Weissman et al., 1977). This brief tool evaluates depressive symptoms experienced over the past week, which include two positively phrased items (feeling pretty good, generally satisfied) and eight negatively phrased items (loss of interest, trouble concentrating, feeling depressed, feeling tired or low in energy, feeling afraid, trouble falling asleep, feeling alone, and difficulty getting going) (Irwin et al., 1999; Kohout et al., 1993). Each item is scored from 0 (rarely or less than once a day) to 3 (almost always or 5–7 days during the past week). The total score, reflecting the level of depressive symptoms, is calculated by summing all items after reversing the scores for the positively phrased items. Higher total scores indicate a greater severity of depressive symptoms. Consistent with previous research, I use a CES-D10 cutoff score of 10 to create a binary variable for probable depression (Andresen et al., 1994; Irwin et al., 1999).

As detailed in Appendix A.2, KLoSA rotates three CES-D10 items during the study period. To ensure that the main results are not affected by this variation, I conduct an additional analysis based solely on the seven items that are consistently present in each wave. Furthermore, I perform an item-level analysis of each CES-D10 item to examine the specific impact of becoming a new grandparent on individual symptoms, including loneliness, feelings of depression, and restlessness. A higher score on each item reflects an increased probability of depressive symptoms. Respondents also rate their satisfaction on a scale from 0 to 100, with increments of 10 (e.g., responses can include values such as 0, 10, 20, and so on up to 100). This satisfaction measure includes various aspects of life, including relationships with children, overall quality of life, and their health conditions. I standardize the measure of satisfaction to have a mean of zero and a standard deviation of one, adjusted by wave and gender.

Health behaviors are measured using binary variables for the probability of any weekly physical exercise, smoking, number of cigarettes smoked per day, and both alcohol use and frequent alcohol use.[6] Employment outcomes are captured through work status and retirement, including measures such as currently working, current retirement status, and hours worked per week on both extensive and intensive margins. Income measures include individual and household income, covering earnings from all sources, with all monetary values adjusted to 2010 levels for consistency. Social engagement is evaluated using two key indicators: weekly contact with children (categorized by in-person visits, phone calls, or either mode) and a binary variable indicating participation in any weekly social activities.[7]

[6] Frequent alcohol use is defined as drinking most days of the week or every day.

[7] Participation in social activities encompasses involvement in various groups, such as religious organizations, social clubs, sports clubs, arts or music groups, and volunteer organizations.

1.4 Empirical Framework

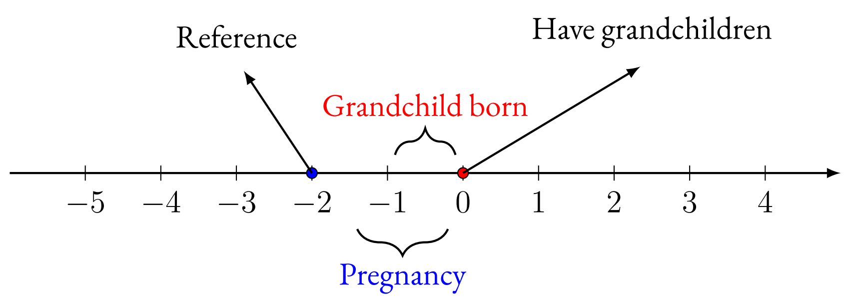
This study examines the causal impact of becoming a first-time grandparent on mental health. The ideal experimental design would randomly assign grandchildren to individuals and compare their mental health over time. However, such an approach is impractical. Instead, I use an event study framework that exploits the variation in the timing of grandparenthood to estimate its effect on mental health. This method has been used in the literature on parental child penalties (Kleven et al., 2019; Kuziemko et al., 2018), and recent studies have extended it to examine the labor market outcomes of grandparents (Malisa, 2024). While the transition to grandparenthood is not fully exogenous, the arrival of a first grandchild likely triggers a sharp change in mental health outcomes. This change can be considered unrelated to unobserved factors, which are expected to evolve gradually over time. The baseline specification is as follows:

$$Y_{it} = \sum_{\substack{j=-5 \\ j\neq -2}}^{j=4} \beta_j \cdot \mathbb{1}\{j = t - gc^i\} + \sum_k \gamma_k \cdot \mathbb{1}\{k = \text{age}_{it}\} + X_i'\delta + \lambda_t + \varepsilon_{it} \quad (1.1)$$

where Y_{it} denotes the mental health outcome for individual i in wave t. The event time j indexes the number of waves relative to the onset of grandparenthood, with gc^i denoting the wave when individual i became a grandparent. The specification includes wave fixed effects (λ_t) and a full set of age dummies to flexibly control for life-cycle trends. The vector X_i includes gender and education. Standard errors are clustered at the individual level. The coefficients β_j capture the dynamic effects of grandparenthood, relative to a normalized reference period $j = -2$. The estimation window spans five waves prior to and four waves after the transition, with each wave representing a two-year interval.

Due to data limitations, I am unable to identify the exact birth year of each grandchild. Instead, I determine grandparenthood status based on the number of grandchildren reported in each wave. As shown in Figure 1.1, event 0 represents the period when an individual is first identified as a grandparent. Thus, the new grandchild is born sometime between event time -1 and 0. Given the typical 10-month pregnancy, it is likely that the pregnancy occurred between wave -2 and -1. To capture potential anticipation effects during the pregnancy period, I normalize period -2 as the pre-treatment baseline. The results are robust to using event time -1 as the reference period.

Figure 1.1: Event study timeline for grandparenthood (waves)



Following Kuziemko et al. (2018), I present the average of the post-treatment coefficients alongside the event-study graphs to quantify the effect of becoming a grandparent. For the main analysis, I report three summary measures from the event study: (i) the average post-treatment effect, calculated as the mean of event-time coefficients from $t = 0$ to $t = 4$, with each period weighted equally at 20%; (ii) the slope of the pre-treatment trend; and (iii) the pre-trend-adjusted post-treatment average. The pre-trend adjustment is based on the following specification:

$$Y_{it} = \sum_{j=-1}^{j=4} \beta_j \cdot \mathbb{1}\{j = t - gc^i\} + \theta\zeta_{it} + \sum_k \gamma_k \cdot \mathbb{1}\{k = \text{age}_{it}\} + X_i'\delta + \lambda_t + \varepsilon_{it} \tag{1.2}$$

where the notation follows equation 1.1, and $\theta\zeta_{it}$ captures the projected pre-trend. This adjustment allows all post-treatment coefficients (including the year of pregnancy) to vary freely. The remaining pre-treatment periods ($j = -5, -4, -3, -1$) are constrained to lie on a linear trend, with $j = -2$ omitted as the reference period. This adjustment allows for flexible dynamics following the transition to grandparenthood, while accounting for any systematic trends in the outcome prior to the event. Importantly, the specification does not impose that pre-treatment trends continue after the event. Rather, it nets out projected pre-treatment variation to isolate the post-treatment response. If the pre-trend coefficient θ is not statistically significant, I rely on equation 1.1 for interpretation, as it does not include the pre-trend adjustment.

1.4.1 Identification assumptions and threats to validity

The identification of the causal effect of becoming a grandparent on mental health rests on two key assumptions: parallel trends and the absence of anticipatory behavior. First, the parallel trends assumption requires that, in the absence of treatment, mental health outcomes for treated and not-yet-treated individuals would have evolved similarly. I assess this by examining the lead coefficients from the event study. These coefficients are close to zero and statistically insignificant, indicating no differential trends in mental health before grandparenthood and supporting the validity of the parallel trends assumption. Second, to address potential anticipatory responses, I draw on two complementary tests. The event study reveals no systematic changes in mental health in the periods leading up to treatment, suggesting limited behavioral adjustments in anticipation of becoming a grandparent. Additionally, I conduct a placebo analysis, in which I randomly assign grandparenthood ages to individuals who never transitioned into grandparenthood during the study period. The placebo ages are sampled from the empirical distribution of actual transition ages. I find no significant effects in this falsification test, reinforcing the conclusion that the observed effects are unlikely to be driven by pre-treatment behavioral responses.

A separate concern involves reverse causality: adult children may time fertility based on the health or caregiving capacity of their parents. For example, older adults in better health may be more inclined to provide childcare, which can lower the perceived cost of raising children and encourage earlier family formation. In this case, observed changes in mental health around the birth of a grandchild could reflect selection rather than a causal effect. Several features of the South Korean context help mitigate this concern. First, co-residence between married adult children and their parents remains low and does not increase after grandchildren are born, as shown in Table 1.2. Second, approximately 10 percent of older adults in South Korea provide any childcare for grandchildren, as shown in Figure A.3, which indicates that most families do not rely on support from grandparents. In contrast, over half of grandparents in China and several European countries report providing some form of childcare.[8] In addition, if caregiving expectations were a primary driver of fertility timing, one would expect most births to occur after parents retire. However, Figure A.5 shows that in the majority of cases, retirement takes place several years after the birth of the first grandchild. Moreover, Figure A.44 indicates that more than 70 percent of births occur within three years of marriage. Together, these patterns suggest that fertility decisions are unlikely to be systematically delayed in anticipation of parental caregiving support, further reducing concerns about reverse causality.

[8] Figure A.4 shows that the female labor force participation rate in South Korea remains low compared to other OECD countries. Mothers are therefore more likely to serve as primary caregivers, rather than depend on informal support from grandparents.

Table 1.2: Probability of co-residing with married adult children

Married Adult Child	Coreside (%)	Not Coreside (%)	N
Married Daughter	2.78	97.22	9,811
Married Daughter with Children	2.74	97.26	9,155
Married Son	11.92	88.08	9,178
Married Son with Children	12.23	87.77	8,446

Note: This table reports the percentage of parents who co-reside with a married adult child based on data from the Korean Longitudinal Study of Aging (KLoSA) 2006 wave.

Another concern is that the staggered timing of treatment introduces potential bias in traditional event study models if treatment effects are heterogeneous. Recent studies have shown that event study models can yield biased estimates when treatments occur at different times, as different cohorts may receive inappropriate weights (Borusyak et al., 2024; Callaway & Sant'Anna, 2021; L. Sun & Abraham, 2021). To address this, I apply the L. Sun and Abraham (2021) estimator, which adjusts for variation in treatment timing and corrects potential biases in the event study estimates. The results remain robust, and a detailed discussion of the staggered timing analysis is provided in the robustness section.

A remaining concern involves the identification of causal effects in the absence of individual fixed effects in the main event-study specification. The baseline model compares new grandparents to those not yet treated. Including individual fixed effects would introduce multicollinearity with the full set of wave and age fixed effects. While it is possible to estimate a model with individual fixed effects by excluding wave and age fixed effects, this approach would not account for age-related changes in mental health or common time shocks. Age fixed effects are particularly important, as individuals necessarily age between the pre- and post-treatment periods. Without controlling for the life-cycle trend, estimates may conflate the effects of aging with the transition to grandparenthood.

Wave fixed effects capture common period shocks, including policy changes or macroeconomic shifts, that could affect all individuals regardless of grandparent status. As noted earlier, the ideal setting would involve randomly assigning grandparenthood status across individuals. In the absence of such variation, age and wave fixed effects play a crucial role in isolating the effect of grandparenthood from underlying trends.

To address this concern, I estimate an alternative specification using the estimator proposed by L. Sun and Abraham (2021), which incorporates individual fixed effects and compares treated individuals to the last-treated cohort. As an additional robustness check, I follow the approach in Malisa (2024) and normalize two pre-treatment periods, specifically $j = -5$ and $j = -2$.[9] Both approaches yield estimates that are consistent with the main specification. To further address the possibility that age or time trends in mental health may vary by gender or education, I interact age and wave fixed effects with these covariates. The resulting estimates, reported in Figure A.7, remain closely aligned with the baseline results.

[9] Results are shown in Figure A.6. The findings remain stable when normalizing to alternative combinations.

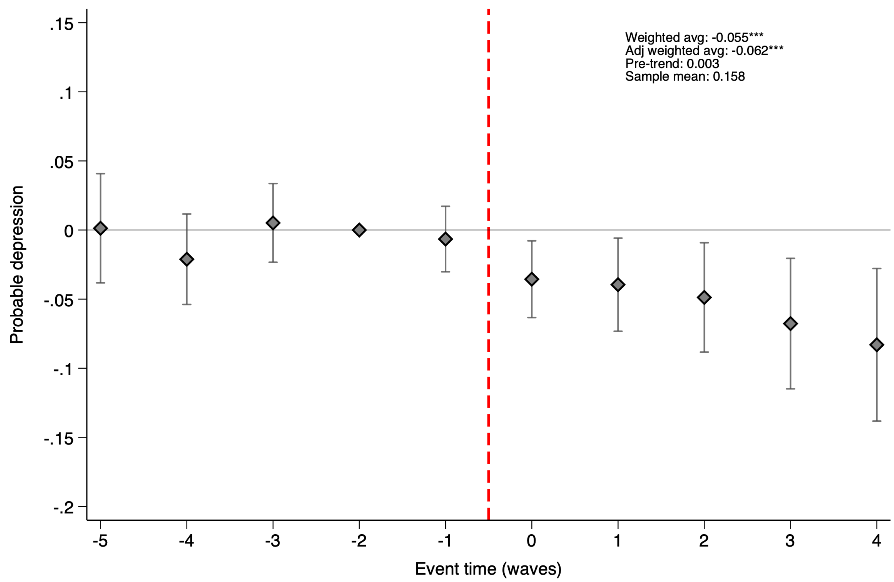
1.5 Results and Discussion

1.5.1 Main Results

Figure 1.2 illustrates a significant reduction in the probability of probable depression following the onset of grandparenthood. Relative to the reference period, new grandparents experience a decrease in the likelihood of probable depression ranging from 3.6 to 8.3 percentage points across post-grandparenthood periods, with all coefficients statistically significant. The transition to grandparenthood results in an average reduction in the probability of probable depression of 5.5 percentage points, representing a nearly 35% decrease from the mean. Additionally, event study estimates indicate that this beneficial impact of grandparenthood persists over time. These findings align with previous research indicating enhanced psychological health associated with grandparenthood (Arpino et al., 2018; Di Gessa et al., 2016).

Figure 1.3 highlights the differing mental health impacts of grandparenthood between grandfathers and grandmothers in response to this life transition. Compared to grandmothers, grandfathers experience a larger reduction in depressive symptoms, with an average decrease of 6.5 percentage points in the probability of probable depression after becoming a new grandparent. While grandmothers also show an improvement, this effect is not statistically significant. However, the average post-treatment effects between grandfathers and

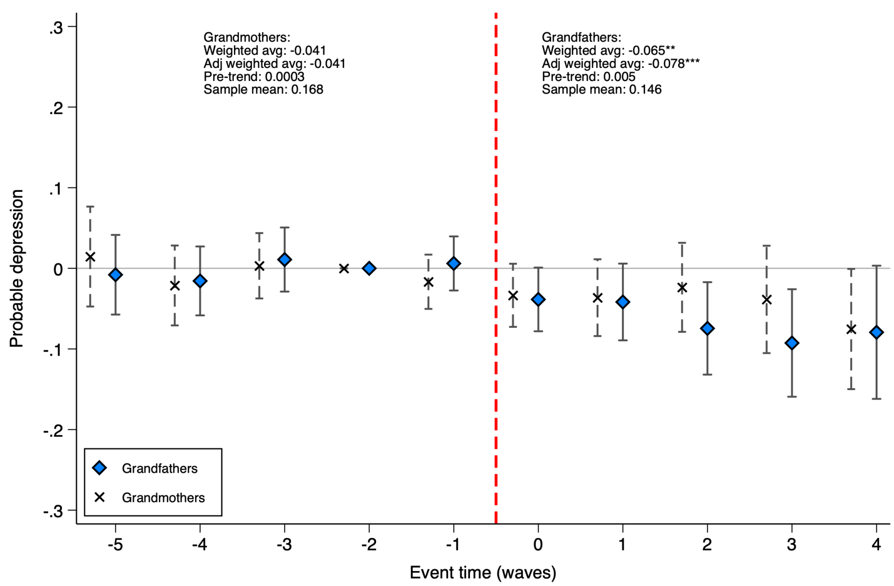
Figure 1.2: Effect of becoming a new grandparent on probable depression



Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. The sample includes individuals aged 45–75 who became first-time grandparents during the study period, observed at least once before and once after the transition. The outcome variable is a binary indicator for probable depression, where individuals with a CESD-10 score of 10 or higher are coded as 1, otherwise as 0. The reference period is the wave before pregnancy (event period $t = -2$). Event study models control for wave and age fixed effects, as well as individual characteristics (gender and education). Standard errors are clustered at the individual level. The figure reports the sample mean, the weighted average of post-treatment coefficients (event times 0 to 4), and a pre-trend-adjusted post-treatment mean.

grandmothers do not differ statistically. These gender-specific findings contrast with some European studies that report more substantial beneficial effects for grandmothers, particularly when their daughters have children (Di Gessa et al., 2020; Tanskanen et al., 2019). The mental health benefits observed for grandfathers in South Korea suggest that becoming a grandfather may enhance social status and provide a sense of purpose within this cultural context, consistent with observations made by Xu (2019) regarding the roles of older men in East Asian societies.

Figure 1.3: Mental health effects by gender



Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. The sample includes individuals aged 45–75 who became first-time grandparents during the study period, observed at least once before and once after the transition. The outcome variable is a binary indicator for probable depression, where individuals with a CESD-10 score of 10 or higher are coded as 1, otherwise as 0. The reference period is the wave before pregnancy (event period $t = -2$). Event study models control for wave and age fixed effects, as well as individual characteristics (gender and education). Standard errors are clustered at the individual level. Results are presented separately for grandfathers and grandmothers. The figure reports the sample mean, the weighted average of post-treatment coefficients (event times 0 to 4), and a pre-trend-adjusted post-treatment mean. Joint test of coefficient equality (SUR) for event time 0-4: $\chi^2(5) = 5.61$, Prob $> \chi^2 = 0.346$.

As discussed in Appendix A.2, I conduct a robust analysis based solely on the seven items consistently present in each wave of the CES-D scale. This approach ensures that the main results are not influenced by variations in the CES-D 10 items. The CES-D 7-item score ranges from 0 to 21. In line with previous research, a score of 8 or above identifies KLoSA respondents at high risk for probable depression (Levine, 2013). Figure A.38 presents the results from the CES-D 7-item score, which closely aligns with the main findings.

In addition to utilizing a binary variable for probable depression based on the CES-D 10 scale, I conduct an item-level analysis of each CES-D 10 item to gain a better understanding of the impact of becoming a new grandparent. Figure 1.4 reveals an improvement in sleep quality, with new grandparents being less likely to report experiencing restless sleep. More importantly, Figure A.40 indicates that becoming a new grandparent significantly decreases the probability of feeling lonely. After transitioning to grandparenthood, the average reduction in feelings of loneliness is nearly 30% from the mean. These findings align with prior research in the Chinese context, which shows that grandparenthood is associated with a reduction in loneliness (Tang et al., 2016; Zhang et al., 2022). However, while previous studies suggest that this reduction in loneliness primarily occurs through caregiving for grandchildren, this study demonstrates that simply becoming a new grandparent also contributes to a lower likelihood of feeling lonely.

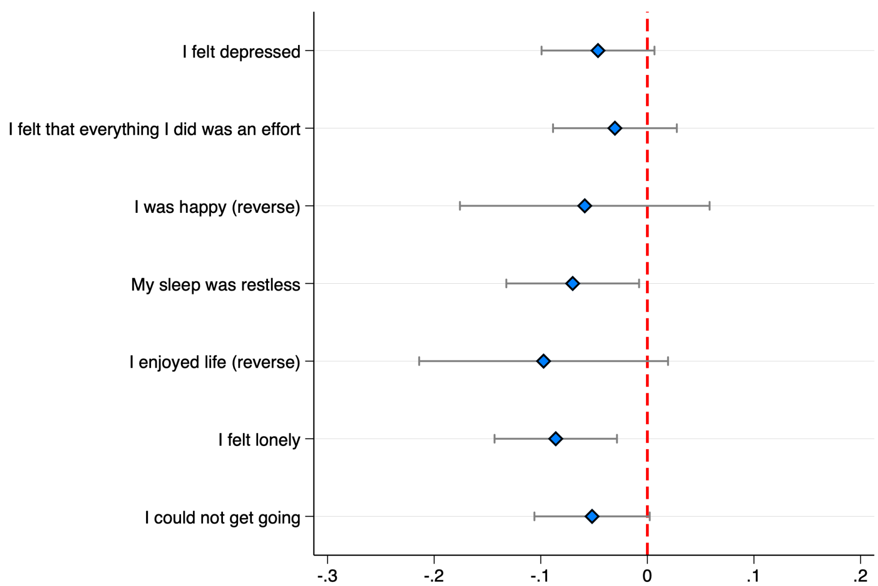
1.5.2 Robustness Checks

Parallel trend

The event study approach relies on the assumption of parallel trends, which posits that the treatment and control groups would have followed similar trajectories over time in the absence of treatment. This assumption is crucial for valid comparisons between the mental health outcomes of new grandparents (the treatment group) and those who have not yet become grandparents (the control group). The event study results reveal little to no evidence of differential pre-trends, supporting this assumption.

To further address concerns about parallel trends, I implement an alternative empirical strategy that imposes a linear trend on the pre-treatment period (from $j = -5$ to -2) while allowing post-treatment coefficients (including period -1) to vary freely. Importantly, this method does not assume that the pre-treatment trend continues into the post-treatment period; instead, it accounts for pre-treatment dynamics, ensuring that the post-treatment effects accurately reflect the true impact of becoming a grandparent without being

Figure 1.4: Event study estimates by CESD-10 depression item



Note: This figure reports the weighted average of post-treatment coefficients (event times 0 to 4) from the event study specification in Equation 1.1 for each CES-D-10 item that is consistently measured across survey waves.

influenced by prior trends and in nearly all my main analyses, netting out the projected pre-trend yields little difference in the conclusions, providing additional evidence for the robustness of the estimated effects.

Concerns may arise regarding unobserved time-varying confounding factors that could influence the results. To address these issues, I employ the Honest DiD sensitivity analysis developed by Rambachan and Roth (2023). This method relaxes the assumption of smoothness in pre-trends, permitting deviations from linearity up to a maximum of M, which indicates how much the slope of differential trends can vary between consecutive periods. The results, shown in Figure A.8, indicate significant effects for violations of parallel trends that are approximately linear (with $M \approx 0$). The critical threshold for maintaining a significant effect is $M \approx 0.02$, suggesting that we can reject the null hypothesis unless deviations from the linear extrapolation of pre-trend differences exceed 0.02 percentage points. This threshold represents more than six times the average change (0.003) in slope observed during the pre-treatment period, reinforcing the robustness of the estimated effects.

Placebo test

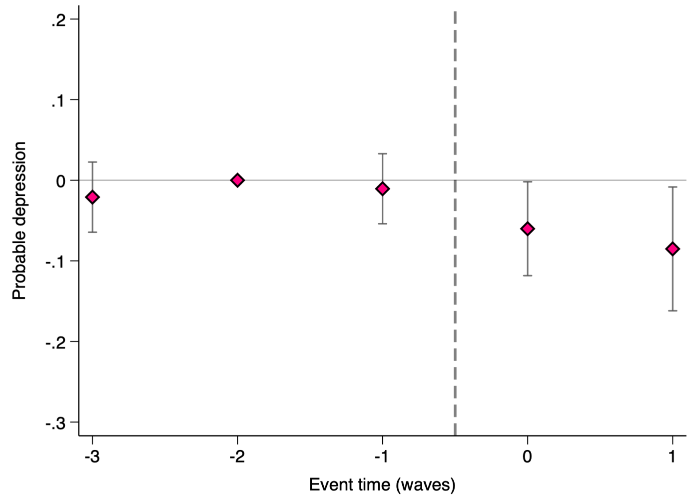
In event analysis, identifying short-term effects relies on the assumption of smooth outcome trends around the birth of grandchildren, supported by the absence of pre-existing trends in the event study estimates. However, assessing long-term dynamics introduces additional challenges that may require stronger identifying assumptions. Over extended periods, the reliability of pre-trends may weaken due to unobserved confounding, potentially violating the smoothness assumption (Kleven et al., 2019). To address this concern, I implement a placebo test following the approach of Malisa (2024), using individuals who never become grandparents during the survey period. A placebo age for becoming a grandparent is assigned to each individual based on the observed distribution of ages at which sons and daughters have their first child, estimated separately by gender.[10] Placebo ages are drawn from a log-normal distribution parameterized by this observed data. For individuals with both sons and daughters, the earlier of the two assigned ages is used as the placebo event time.

[10] The empirical age distributions are shown in Figure A.9.

The event study is re-estimated using these placebo event times, applying the same specification as equation 1.1 and the same sample restrictions as in the main analysis. The assignment procedure is repeated 5,000 times to construct bootstrapped standard errors. The results, presented in Figure A.10, reveal no statistically significant effects following the placebo event. Although the pre-trend pattern closely resembles that in the main analysis, the absence of any post-event change provides additional support for the interpretation that the observed effects are attributable to the transition to grandparenthood rather than to age-related or cohort-specific dynamics.

Event study with a balanced panel

Figure 1.5: Grandparenthood effects with balanced sample

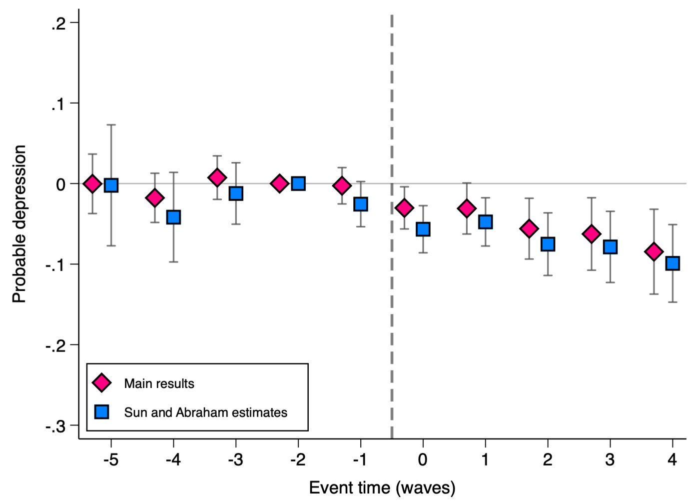


Note: This figure is based on a balanced event-time panel from the Korean Longitudinal Study of Aging (KLoSA), including individuals observed in all five event periods from $t = -3$ to $t = 1$. The reference period is $t = -2$. Estimates are based on the event study specification in Equation 1.1.

To address potential concerns about changes in sample composition throughout the event window, I construct a balanced panel consisting of individuals observed from event periods -3 to 1. This balanced sample retains approximately 35 percent of the original cohort, including only those individuals present in all five event periods. Given that this study only has seven waves, individuals in the balanced sample are observed to transition to grandparenthood during waves 4 to 6(from 2012 to 2016). The exclusion of individuals from the balanced sample primarily stems from insufficient pre-treatment data. As illustrated in Figure 1.5, the findings demonstrate a significant improvement in mental health, notably a reduction in the probability of probable depression.[11]

[11] Figure A.39 shows a similar pattern in mental health improvement using the CES-D 7 scale, along with a reduction in feelings of loneliness.

Figure 1.6: Grandparenthood effects using the method of L. Sun and Abraham (2021) with last-treated cohort as control group



Note: This figure presents results using the L. Sun and Abraham (2021) method. This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. The sample consists of individuals aged 45-75, including individuals observed becoming grandparents during the survey, using the last-treated cohort as the control group. The outcome variable is a binary indicator for probable depression, where individuals with a CES-D-10 score of 10 or higher are coded as 1 and others as 0. The reference period is the wave prior to pregnancy (event period $t = -2$). The event study includes controls for age, person, and wave fixed effects. Standard errors are clustered at the individual level.

Staggered treatment timing

Another concern in this analysis is the staggered timing of becoming a new grandparent across individuals, which can introduce biases stemming from treatment effect heterogeneity. When treatment is received at different times, standard event-study estimators may yield biased estimates due to improper weighting across cohorts. This variability complicates causal identification, particularly when treatment effects vary with time since the event. Recent research highlights that failure to account for such timing differences can lead to substantial bias in estimated effects (Borusyak et al., 2024; Callaway & Sant'Anna, 2021).

To address this concern, I implement the estimator proposed by L. Sun and Abraham (2021), which compares treated cohorts to a last-treated control group while allowing for cohort-specific treatment effects. The main specification defines the last-treated cohort based on survey timing, using individuals who transition to grandparenthood in the final observed wave as the control group. Figure 1.6 presents estimates from this approach.[12] These findings are further supported by an alternative specification using all never-treated individuals as the control group.[13]

I conduct robustness checks using varied control groups. Figure A.15 presents results that include never-grandparents as the control group, along with individual, age, and wave fixed effects. These findings are consistent with the main analysis, showing that becoming a grandparent reduces the probability of probable depression by 5.1 percentage points, indicating a within-person improvement in mental health.

1.5.3 Heterogeneity

By education

Previous literature underscores the role of education in moderating the mental health impact of grandparenthood, indicating that higher-educated individuals derive more substantial benefits from this transition due to their greater economic resources and enhanced social networks associated with higher educational attainment (Lai et al., 2021; Mahne & Huxhold, 2015). In this study, I classify educational levels into high (high school graduate or higher) and low (below high school) to examine differential impacts. Figure 1.7a demonstrates that the mental health effects of transitioning to grandparenthood vary significantly by educational attainment. Higher-educated grandparents experience substantial improvements in mental health following this transition, while those with lower education levels exhibit no significant changes. This pattern aligns with prior research on grandparenting and health outcomes (Arpino et al., 2018; Mahne & Huxhold, 2015), underscoring the role of education in moderating the relationship between grandparenthood and mental health.

I begin by examining whether higher economic resources help explain the differences observed between higher- and lower-educated grandparents. Wealth level is classified using the median net asset value as the cutoff. Among individuals in the upper quartile of net assets, 66.2% are higher-educated, compared to only 33.8% who are lower-educated. I use wealth level in the wave before

[12] Figure A.12 shows the distribution of ages at which individuals first become grandparents. Approximately 12 percent experience this transition at age 65 or older. As a robustness check, I define this group as a late-treated cohort and use them as the control group in the L. Sun and Abraham (2021) estimator. Figure A.13 reports results from this alternative specification, which includes age, wave, and individual fixed effects. The estimates remain consistent with those in the main analysis.

[13] Figure A.14 presents these results. The estimated effects remain robust to the choice of control group.

the birth of the first grandchild as a proxy for economic resources throughout the transition to grandparenthood. Figure A.16 shows no effect of becoming a new grandparent on the likelihood of staying in the upper wealth quartile. As illustrated in Figure 1.7b, event-study estimates by wealth level indicate that wealthier grandparents experience greater improvements in mental health following the transition to grandparenthood, consistent with the idea that financial resources can buffer the impact of stress (Silverstein & Marenco, 2001). The average mental health effects of grandparenthood, however, do not differ significantly between wealthier and less wealthy grandparents. [14]

[14] In addition to the potential role of financial resources, I examine several other mechanisms that may account for differences in mental health effects by education. I find no statistically significant differences between education groups in the probability of transferring money to or receiving money from adult children, participating in social activities weekly, or satisfaction with adult children. Lower-educated grandparents are slightly more likely to retire after the birth of a grandchild, and higher-educated grandparents have a modestly higher probability of planning to leave a bequest, but these differences are not statistically significant. Figures A.17a and A.17b in the appendix present these patterns.

Paternal versus maternal grandparents

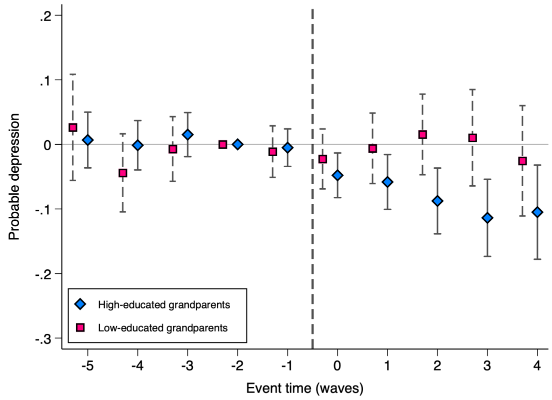
Maternal and paternal grandparents are defined as those related to the grandchild through the mother and father, respectively. Figure A.18 illustrates how the mental health effects of becoming a grandparent differ for maternal versus paternal grandparents. Although the overall impact on the probability of probable depression is similar for both groups, the timing and patterns of these effects vary. Maternal grandparents experience an immediate improvement in mental health following the birth of a grandchild through their daughter, though this benefit becomes insignificant over time.

Studies focused on European contexts often find greater mental health benefits for maternal grandparents, particularly grandmothers, due to closer relationships and more frequent contact with grandchildren (Danielsbacka et al., 2022). Western cultural norms generally emphasize the involvement of maternal grandparents in childcare and family support (Timonen & Arber, 2012). In Korea, however, the absence of a pronounced difference between maternal and paternal grandparents may reflect an evolving cultural context. Studies show that among Korean mothers who rely on kin-provided childcare, over 50% rely on maternal grandparents, while less than 40% rely on paternal grandparents (Lee & Bauer, 2013). This trend corresponds with a shift toward bilateral kinship interactions and increased engagement with both maternal and paternal relatives, contrasting with the traditional patrilineal emphasis in East Asia, where paternal grandmothers were more likely to remain closely involved (Lee & Bauer, 2013).

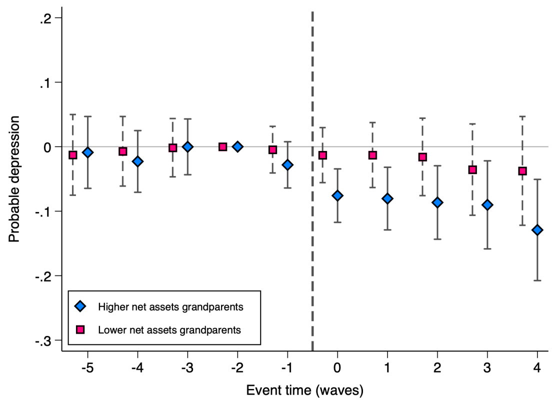
Gender of the first-born child

Following Rupert and Zanella (2018), I examine heterogeneous effects by the gender of the first-born child. As shown in Figure A.9, women in South Korea tend to become first-time parents at a younger age than men. Since the sex of the

Figure 1.7: Grandparenthood effects by education and wealth level



(a) By education



(b) By net asset value

Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. Panel (a) presents results by education level; Panel (b) presents results by net asset value (using the sample median before the birth of the first grandchild as the cutoff). Standard errors are clustered at the individual level. Joint test of coefficient equality (SUR) for event time 0-4: $\chi^2(5) = 10.95$, $p = 0.05$ for Panel (a); $\chi^2(5) = 5.50$, $p = 0.36$ for Panel (b).

first child is generally considered random, parents of first-born daughters tend to become grandparents earlier, as daughters typically marry and start families sooner than sons.[15] Figure A.20 illustrates the impact of becoming a grandparent on mental health by the gender of the first-born child. The trends observed closely align with those for maternal and paternal grandparents, as parents of first-born sons are more likely to become paternal grandparents, while parents of first-born daughters are more likely to become maternal grandparents.

[15] Figure A.19 shows the distribution of age at first-time grandparenthood by the gender of the first-born child, indicating that individuals with a first-born daughter tend to become grandparents at a younger age.

1.5.4 Mechanisms

Health behavior modifications

One channel through which becoming a grandparent might affect health is through changes in health behaviors. The transition to grandparenthood could potentially motivate individuals to adopt healthier lifestyles to live longer and remain actively involved in their grandchildren's lives. This motivation might lead new grandparents to exercise more, quit smoking, or reduce alcohol consumption. If these behavioral adjustments occur, they may contribute to the improved mental health observed in some grandparents.

To test this hypothesis, I examine the effects of becoming a grandparent on the likelihood of engaging in weekly physical exercise, probability of smoking, number of cigarettes smoked (on both extensive and intensive margins), probability of drinking alcohol, and probability of drinking alcohol frequently (on both extensive and intensive margins). Figures A.21 through A.25 show that no significant changes in these health behaviors are associated with the transition to grandparenthood, and effects do not differ by gender. These findings suggest that any health effects of grandparenthood likely operate through mechanisms other than these observable health behaviors.

Labor market transitions

The national pension system in South Korea mandates a minimum of ten years of contributions, allowing individuals to receive a standard pension at age 60 or to opt for early retirement at age 55 with reduced benefits (Oh, 2024). In 2013, a policy update introduced a gradual increase in the pension eligibility age, which will reach 65 by 2033. Studies suggest that becoming a grandparent can prompt early retirement or reduce work participation, especially among grandmothers (Malisa, 2024; Rupert & Zanella, 2018). To better understand this impact, I estimate the effects of grandparenthood on various labor supply measures, excluding individuals who reported never having worked in the first wave.[16]

[16] In KLoSA Wave 1, respondents who were neither currently employed nor seeking employment were asked about their work history, with one response option being, "Never had a job before (I have done a little work here and there but never had a clear job)." In our final sample of prospective grandparents, approximately 27% report never having held a clear job, 93% of whom are female. To better assess the impact of becoming a grandparent on employment, I exclude these respondents, which reduces the female sample by 42% and the male sample by 3.5%.

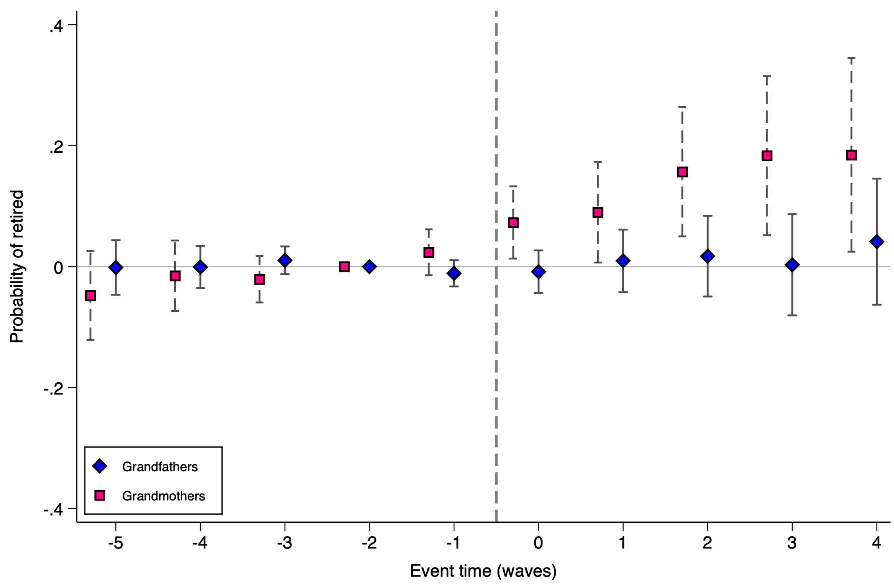
Figure 1.8 shows that grandmothers are more likely to retire following the birth of a grandchild, whereas no significant effects are observed among grandfathers, aligning with previous research (Rupert & Zanella, 2018). Additionally, Figure A.26 indicates that grandmothers are less likely to be employed after becoming grandparents, and Figure A.27 reveals a decrease in hours worked on the extensive margin, with no impact on weekly hours worked on the intensive margin. The literature on the health effects of retirement presents mixed findings. Some studies suggest that retirement reduces work-related stress and increases leisure time, potentially benefiting mental health (Johnston & Lee, 2009). However, most studies report either negative or neutral effects (Mazzonna & Peracchi, 2017). For example, Watson (2020) finds that retirement has a negative impact on mental health in China, likely due to the increased demands of caregiving for grandchildren. Using KLoSA data, Oh (2024) observes a negative but insignificant effect of retirement on mental health in South Korea. Since grandmothers are more likely to retire and reduce work following grandparenthood, and the impact of retirement on mental health is generally negative or insignificant, this may explain why mental health improvements are observed among grandfathers but not grandmothers.

Family dynamics

Figure A.29 shows a significant decrease of 21.7 percentage points in the probability of cohabiting with adult children following the birth of a grandchild, significant at the 1% level. However, this decline in cohabitation may not be solely attributable to the birth of a grandchild; other factors, such as the marriage of adult children, may also contribute to changes in living arrangements. Figure A.44 indicates that approximately 40% of first marriages and first births of grandchildren occur within the same survey wave, with only rare cases of childbirth occurring before marriage. About 35 percent of first grandchildren are born in the wave following the adult child's first marriage.

To isolate these effects, I apply the same empirical strategy, using the first marriage of the adult child as the treatment rather than the birth of a grandchild. Figure A.45 illustrates a significant decline in the probability of cohabitation following marriage. As living arrangements adjust, Figure A.46 shows that while weekly contact with the married child does change, the mode of contact shifts: parents become more likely to communicate by phone and less likely to meet in person. This pattern is consistent with findings in Figure A.30, where new grandparents similarly show an increased likelihood of phone communication and a reduced likelihood of in-person meetings with their children. Figures A.47 and A.48 report the effects of the first marriage of an adult child on parental

Figure 1.8: Grandparenthood effects on retirement



Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator for the probability of being retired. The reference period is the wave before pregnancy (event period $t = -2$). The event study controls for wave fixed effects, age fixed effects, and individual characteristics such as gender and education. Standard errors clustered at the individual level. Joint test of coefficient equality (SUR) for event time 0-4: $\chi^2(5)$ = 13.02, Prob $> \chi^2$ = 0.023.

mental health. While there are some improvements following marriage, these effects are not statistically significant, and any further gains may result from the subsequent birth of a grandchild. Thus, our main results are not driven by the marriage of adult children.

Figure A.31 shows notable improvements in satisfaction with relationships with adult children and with personal health following the transition to grandparenthood. Additionally, Figure A.32 indicates a significant increase in social engagement, with the probability of participating in weekly social activities rising by 5.4 percentage points, a 19.2% increase from the mean. This increased social involvement, alongside shifts in family dynamics, may reduce the potential for family conflicts and contribute overall to improvements in mental health.

Grandchild care

Caregiving for grandchildren represents another crucial channel through which becoming a new grandparent can impact health. Existing literature on the health effects of grandchild caregiving presents mixed results (Chan et al., 2023). While many studies find that non-intensive caregiving benefits the physical and mental health of grandparents, intensive caregiving often leads to health deterioration (F. Chen & Liu, 2012; Choi & Zhang, 2018). For co-resident grandparents, who are more likely to provide intensive care, studies frequently report negative health impacts (F. Chen et al., 2015). As shown in Figure A.3, grandparents in South Korea are relatively less likely to provide childcare. This pattern may be attributed to the low female labor force participation observed in Figure A.4, which results in mothers typically taking on primary caregiving duties. In KLOSA wave 1, less than 10% of grandparents reported caring for grandchildren under 10 years old in the past year. However, among those who did provide care, the average weekly commitment was 48 hours, indicating intensive involvement.

As shown in Table 1.3, grandparents in South Korea generally exhibit a low likelihood of providing care for grandchildren; however, those who cohabit or live very close are more likely to take on caregiving responsibilities. To explore whether caregiving moderates the mental health effects of grandparenthood, I use a binary indicator for living within one hour of a grandchild (including co-residence and living within 30 minutes) as a proxy for caregiving likelihood. Figure A.33 shows event study estimates using this proximity measure as the outcome. The results show that the birth of a grandchild does not significantly change the probability of living within this proximity, indicating that distance tends to remain stable post-birth.

Table 1.3: Probability of providing care for grandchildren by living distance

	All		Grandfathers		Grandmothers	
Living distance	No Care	Care	No Care	Care	No Care	Care
Cohabitation	84.89	15.11	90.30	9.70	80.79	19.21
Within 30 min	93.15	6.85	95.62	4.38	91.09	8.91
Within 1 hour	97.49	2.51	98.64	1.36	96.52	3.48
Within 2 hours	98.28	1.72	99.09	0.91	97.56	2.44
More than 2 hours	97.47	2.53	98.56	1.44	96.49	3.51
Total	95.98	4.02	97.62	2.38	94.57	5.43

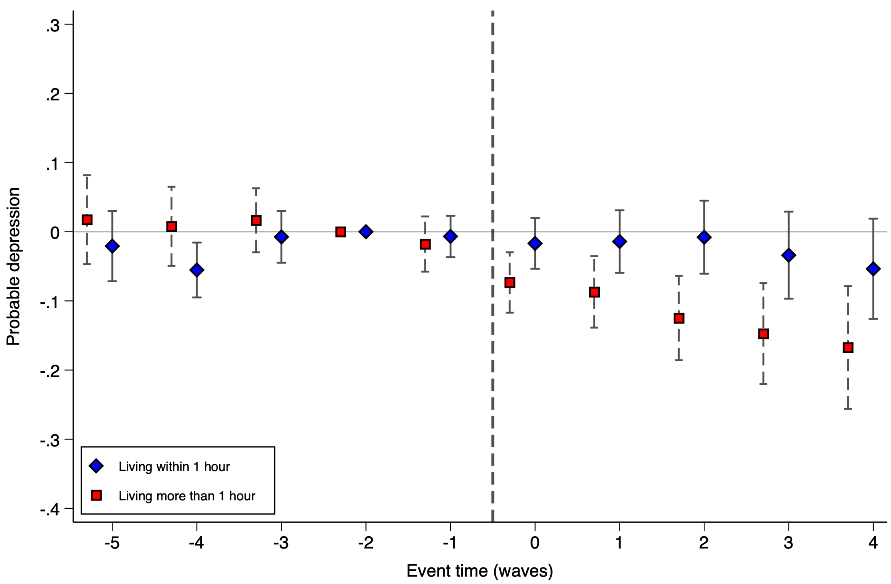
Note: Living distance categories are defined as follows: Cohabitation refers to living in the same household; within 30 minutes, within 1 hour, and within 2 hours indicate travel time by public transportation; more than 2 hours refers to a travel time greater than 2 hours by public transportation.

Using a binary indicator for living within one hour of a grandchild in the wave before the birth (event period -1), Figure 1.9 shows that grandparents who live farther from their grandchildren experience a stronger improvement in mental health compared to those living closer.[17] This effect may be partly driven by increased satisfaction in relationships with adult children following the birth of a grandchild, as shown in Figure A.35. Another possible factor is financial support from adult children: Figure A.36 suggests that grandparents are relatively more likely to receive monetary transfers from their children (and relatively less likely to provide financial support), despite some evidence of pre-trends. C. Park (2014) suggests that adult children in South Korea often provide financial support to their parents, motivated not only by altruism when parental income is low, but also as a substitute for in-person visits or as compensation for childcare provided by grandparents.

[17] As a robustness check, I also use living distance measured at event period -2, which precedes the likely onset of pregnancy. The results remain stable.

Figure A.34 further shows that grandparents living close to their grandchildren are relatively more likely to retire following the birth of a grandchild, potentially due to relatively higher caregiving demands compared to those experienced by grandparents living farther away. For grandparents living closer, Figure A.37 shows a decrease in weekly contact beginning around kindergarten age (4–5 years) for the grandchild, while no change in contact patterns is observed among those living farther away. These findings suggest that the relatively higher demands of caregiving among grandparents living closer to their grandchildren may help explain the differences in mental health outcomes between

Figure 1.9: Grandparenthood effects on probable depression by living distance



Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator for probable depression, where individuals with a CESD-10 score of 10 or higher are coded as 1, otherwise as 0. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

grandparents who live nearby and those who live farther away. The relatively higher likelihood of caregiving among those living nearby may place strain on grandparents' mental health, as suggested by F. Chen and Liu (2012). Additionally, role constraint theory indicates that extensive caregiving can impact time allocation and lead to role conflicts, potentially explaining the mental health disparities between grandparents based on proximity to their grandchildren.

1.6 Concluding Remarks

This paper studies how becoming a new grandparent affects mental health later in life. Using longitudinal data from South Korea and an event-study design, I find that the transition to grandparenthood leads to meaningful improvements in mental health. The Korean context, characterized by rapid population aging and the lowest fertility rate among OECD countries, offers a distinctive setting to examine intergenerational dynamics. The findings shed light on the intersection of fertility, family structure, and mental health, illustrating how major life transitions affect health outcomes in older adulthood.

The findings show that becoming a grandparent leads to a sustained reduction in the likelihood of probable depression, with an average decline of 5.5 percentage points over the post-treatment period. This effect is primarily driven by decreases in self-reported loneliness. Both grandmothers and grandfathers experience mental health improvements, with slightly larger gains observed among grandfathers, although the difference is not statistically significant. The results are robust across a range of specifications. The mental health effects of grandparenthood differ by education. Grandparents with more schooling experience clearer improvements, likely because they have more resources and greater flexibility in how they take on their new role. Those with less education see smaller effects. Interestingly, there is no significant difference between maternal and paternal grandparents. The result reflects an evolving cultural context in South Korea, where family dynamics are shifting away from traditional patrilineal norms toward a more balanced bilateral kinship model.

Several mechanisms may account for the observed mental health improvements. One is retirement. Grandmothers are more likely to retire after becoming grandparents, a pattern not observed among grandfathers, which may help explain gender differences in the effects. Caregiving intensity also matters. Grandparents who live near their grandchildren are more likely to provide childcare, reduce work hours, and exit the labor force, but they do not experience consistent gains in mental health. In contrast, those who are less directly involved report clearer improvements. A third factor is relationship quality.

Following the birth of a grandchild, grandparents report greater satisfaction in their relationships with adult children, suggesting that the transition may strengthen intergenerational ties. Finally, many grandparents stop living with their adult children during this period, but regular contact is maintained. This shift in living arrangements may ease household tension while preserving family connections.

These findings have direct relevance for family policy in aging societies. In South Korea, where elderly suicide rates are the highest among OECD countries,[18] grandparenthood appears to reduce loneliness and improve mental health among older adults. These effects suggest that fertility-related policies may generate broader intergenerational benefits. For example, South Korea introduced the Child Allowance in 2018, providing monthly cash benefits to families with young children.[19] Recent research finds that the policy improved household finances and modestly enhanced maternal health and life satisfaction (Kwon & Nam, 2022). The results in this paper point to an additional margin of impact involving improvements in the mental health of older family members, a dimension often overlooked in policy discussions. Consequently, evaluations that focus solely on direct recipients may understate the broader impact of fertility and family policy.

[18] OECD (2022), "Suicide Rates (Indicator)," OECD Health Statistics. Available at: https://data.oecd.org/healthstat/suicide-rates.htm.

[19] The program was established under the Child Benefit Act (Act No. 15539), enacted in March 2018 and implemented in September 2018. The English version of the law is available from the Korea Legislation Research Institute: https://elaw.klri.re.kr/eng_mobile/viewer.do?hseq=60064&key=38&type=part.

Chapter 2

Breaking the "Iron Rice Bowl": Health Behavior Effects of Chinese State-Owned Enterprise Reform.

2.1 Introduction

The restructuring of state-owned enterprises (SOE) in mid-1990s China marked one of the largest labor market transitions in recent developing country history (Naughton, 1995). Under the planned economy, jobs in the state sector came with lifetime tenure, stable pay, and extensive benefits, commonly referred to as the "iron rice bowl," and labor turnover remained exceptionally low (Berkowitz et al., 2017). Between 1995 and 2001, employment in the state sector declined by 40 percent, falling from 113 million to 67 million workers, while official urban unemployment rose above 10 percent by the late 1990s (Giles et al., 2006). In addition to massive layoffs, workers who remained employed faced rising job insecurity. Prior studies have examined how the transition affected precautionary savings (He et al., 2018), fertility (Zhao et al., 2024), depressive symptoms in later life (Luo et al., 2023), and intergenerational outcomes (Kong et al., 2019). However, less is known about health behaviors, which may respond sharply to economic stress and are critical for understanding the long-term health consequences of large-scale institutional change.

Tobacco use remains a major public health challenge in China. The country accounts for nearly one-third of global cigarette consumption, and smoking prevalence among adult men exceeds 50 percent (Z. Chen et al., 2015; Reitsma et al., 2021). Smoking contributes to more than one million premature deaths each year, a number projected to triple by 2050 in the absence of effective intervention (Z. Chen et al., 2015). While cultural norms and limited regulation have received substantial attention, the potential influence of job-related stress has not been fully examined. A large literature in public health and psychology documents a strong correlation between psychosocial stress and smoking behavior, including initiation, persistence, and relapse (Allen et al., 2019; Childs & De Wit, 2010; Slopen et al., 2013). These patterns raise the possibility that labor market instability may be an important contributor to smoking in a rapidly changing economic environment.

Extensive research has examined how macroeconomic conditions shape health-related behaviors. Some studies find that health behaviors improve during economic downturns, with evidence of reduced smoking, obesity, and heavy drinking during periods of rising unemployment (Ruhm, 2005; Ruhm & Black, 2002). Others document adverse effects of downturns, including increased mental distress and higher rates of smoking and alcohol use (Deb et al., 2011; Kaiser et al., 2018; Kivimäki et al., 2000; McInerney et al., 2013). While much of this work emphasizes unemployment, a growing literature highlights job insecurity among the employed as a distinct channel. Rising unemployment has been shown to reduce well-being even among those who remain employed, through increased fear of job loss and shifting social norms (Burgard et al., 2009; Witte, 1999). This distinction is critical, as stress generated by perceived insecurity may alter behavior independently of income loss or displacement.

Understanding how economic downturns affect health behaviors requires considering not only those who lose their jobs but also those who remain employed under increased stress. If job insecurity influences behaviors such as smoking or alcohol use, analyses that focus solely on job loss may substantially understate the broader behavioral costs of economic downturns. Estimating causal effects in this context is challenging. Macroeconomic shocks often influence multiple aspects of life simultaneously, including income, workload, and household dynamics, making it hard to isolate the impact of job-related stress (Johnston et al., 2020). Job insecurity is also endogenous. Individuals who smoke or drink may face higher dismissal risk or perceive greater insecurity if these behaviors signal poor health or low productivity.

Identifying the effects of job insecurity on health behaviors requires a source of exogenous variation in perceived employment stability. Ideally, the shock should affect some workers but not others, while leaving income and employment status unchanged. The restructuring of state-owned enterprises in China provides a natural experiment to examine how increased employment instability, arising from both job loss and heightened job insecurity, affects health behaviors. Beginning in 1995, the central government implemented reforms that eliminated lifetime job guarantees and introduced substantial uncertainty for SOE workers. Although not all SOE workers lost their jobs during the reform, it sharply increased perceived employment risk. In contrast, employees in government agencies, who held similar job protections before the reform, were largely unaffected. This institutional divergence generates plausibly exogenous variation in perceived employment risk, enabling the identification of behavioral responses to job insecurity, apart from job loss or wage reductions.

This study uses panel data from the China Health and Nutrition Survey (CHNS) to examine how job insecurity affects smoking and alcohol use between 1991 and 2000. The empirical strategy adopts a difference-in-differences approach, comparing individuals employed in SOEs before the reform to government employees who were not directly affected. The analysis focuses on prime-age workers in urban areas, where the reform was implemented most extensively. To account for potential confounding factors, I include individual, year, and province fixed effects, along with time-varying controls unlikely to be affected by the reform. The main estimated effects capture both job displacement and heightened perceived insecurity among workers who remained employed. To further disentangle these channels, I separately examine SOE workers who lost their jobs and those who retained employment during the reform to compare the effects of displacement and job insecurity. The findings suggest that the behavioral effects of job insecurity are at least as large as those associated with job loss.

In addition to the sector-based treatment definition, I construct two complementary measures of job insecurity: the probability of layoff and expected income loss. These proxies capture variation in perceived employment risk across provinces and years, and help validate the interpretation of the reform as a shift in perceived rather than realized job stability. Estimates based on these alternative measures closely mirror the main results and reinforce the robustness of the analysis. During the reform period, SOE workers were more likely to smoke, smoked more heavily, and were more likely to engage in excessive alcohol use. These patterns are consistent with stress-induced coping behaviors, in which individuals respond to heightened job insecurity by increasing tobacco and alcohol consumption. The effects are not uniform across individuals. Behavioral responses are larger among workers with lower levels of education and those employed in smaller SOEs, consistent with greater vulnerability to economic uncertainty.

This paper contributes to the literature on job insecurity and health by providing new evidence on how perceived employment risk influences health behaviors. Most existing studies focus on actual job loss or aggregate economic conditions, making it difficult to isolate the role of insecurity among the employed. A few papers examine perceived job security directly, including Caroli and Godard (2016), who use variation in employment protection to show effects on physical symptoms, and Reichert and Tauchmann (2017), who find that firm-level downsizing worsens mental health. Bratberg and Monstad (2015) study municipal workers in Norway and link financial shocks to reduced sickness absence under declining job protections. This study extends the literature by focusing on a large labor market reform in China, where workers faced a sharp increase in layoff risk without necessarily losing their jobs. The findings show that perceived insecurity alone can raise smoking and alcohol use, particularly among workers with lower education, those in smaller SOEs, and households with dual SOE employment. These patterns highlight the importance of individual and institutional vulnerability in shaping behavioral responses to insecurity.

Second, this paper adds to the literature on how macroeconomic downturns affect health behaviors. Prior studies using aggregate unemployment rates find that smoking and alcohol consumption tend to decline during recessions, as individuals reduce spending on these goods in response to falling incomes and tighter budget constraints, suggesting that any stress-induced increases in consumption are more than offset by income effects (Ruhm, 2005; Ruhm & Black, 2002). The SOE reform in China introduced massive layoffs concentrated in the state sector, leading to sharp increases in unemployment risk. Using individual-level data, this study finds that higher sectoral layoff risk is associated with increased smoking and alcohol use. Among those who remained employed and did not experience income loss, the findings reveal similar behavioral changes, highlighting the effects of job insecurity even in the absence of displacement.

Finally, this study contributes to a growing literature on economic transitions and health in developing countries. Much of the existing evidence on job insecurity is based on high-income contexts with strong social protections (D'Souza et al., 2003; Ferrie et al., 2005; László et al., 2010; Lau & Knardahl, 2008). In contrast, the state-owned enterprise reform in China took place in a weaker institutional setting, where workers had limited safety nets and little control over employment risk. This study shows that in such environments, rising job insecurity can lead to measurable changes in health behaviors, even in the absence of income loss. These findings help expand our understanding of how labor market reforms shape individual behavior in low- and middle-income countries.

The remainder of the paper is structured as follows. Section 2.2 examines the channels through which job insecurity may influence smoking and alcohol use. Section 2.3 provides institutional background on the SOE reform. Section 2.4 describes the data and key variables. The empirical strategy is outlined in Section 2.5. Section 2.6 presents the main findings, and Section 2.7 discusses robustness checks and heterogeneity. Section 2.8 concludes.

2.2 Mechanism

Job insecurity refers to the perceived threat of losing employment, independent of actual job loss. It reflects anticipatory stress and a lack of control over future job stability, including both fear of dismissal and concern over deteriorating work conditions (De Witte et al., 2016; Greenhalgh & Rosenblatt, 1984). This perception has psychological consequences that are distinct from those associated with realized unemployment. Prior studies show that perceived job insecurity can generate anxiety, emotional strain, and behavioral responses, even when income remains unchanged (Dekker & Schaufeli, 1995; Witte, 1999).

One mechanism linking job insecurity to health behaviors operates through psychological stress. Uncertainty in employment reduces perceived control over future outcomes and elevates anxiety. Empirical evidence shows that individuals exposed to sustained job insecurity report higher levels of psychological distress than securely employed counterparts (Burgard et al., 2009; Witte, 1999). Research in behavioral medicine documents that stress increases both the likelihood and intensity of smoking and drinking, as individuals use these behaviors to manage negative emotions (Brandon & Baker, 1991; Kassel et al., 2003). Experimental and survey evidence further suggests that stress amplifies the perceived reward from nicotine and alcohol, reinforcing usage under pressure (McKee et al., 2011).

A second mechanism involves consumption smoothing and precautionary saving motives. As the probability of job loss rises, households may reduce current spending to buffer against future income shocks (Weil, 1993). Since labor income represents the main resource for most households, perceived employment risk can lead to increased saving and lower demand for discretionary goods. Research using aggregate unemployment rates finds that labor market uncertainty raises private saving (Bande & Riveiro, 2013; Lugilde et al., 2018; Mody et al., 2012). Evidence from the SOE reform in China shows that heightened risk of displacement led to sharp increases in household saving rates among affected workers (He et al., 2018). These patterns suggest that job insecurity may reduce the consumption of goods such as cigarettes and alcohol.

Taken together, these two mechanisms point in opposite directions. Psychological stress may increase the demand for smoking and alcohol use, while consumption smoothing and precautionary saving motives may suppress it. The net effect depends on the relative strength of each channel. In high-income contexts, cigarettes are often treated as inferior goods, particularly among more educated individuals (Y. Cheng et al., 2005; Wasserman et al., 1991). In lower-income settings, these goods tend to behave as normal, with consumption falling when income declines (Kenkel et al., 2014). In 1990s urban China, Bishop et al. (2007) find that higher income reduces the probability of smoking but increases intensity among smokers, while higher education lowers both. These findings imply that job insecurity may simultaneously increase stress-related demand for smoking and decrease income-driven consumption. These offsetting forces underscore the importance of empirical analysis to determine the net behavioral response to job insecurity.

2.3 Institutional Background

2.3.1 Centrally planned economy and lifetime employment (1949–1978)

Between 1949 and 1978, China operated under a centrally planned economy in which the government controlled production and factor allocation, including labor. During this period, most urban jobs were assigned directly by government agencies. The Ministry of Labor and Personnel set annual employment quotas for local authorities, leaving individuals with little choice over where or how they worked (He et al., 2018). Once assigned, employees could not leave their positions, and dismissal was only allowed in cases of criminal misconduct (Meng, 2000). Despite relatively low wages, state-sector jobs provided guaranteed lifetime employment along with near-universal access to healthcare, education, housing, and pensions (L. Brandt & Rawski, 2008). Employment in the state sector was colloquially known as the "iron rice bowl," reflecting near-complete employment and income security.

2.3.2 Economic reforms and labor market conditions (late 1970s–early 1990s)

Economic liberalization began in the late 1970s with the launch of Deng Xiaoping's "open door" policy. Although lifelong employment policies were relaxed slightly during the 1980s, employment contracts in government agencies and SOEs rarely ended without cause. By the early 1990s, SOEs faced mounting competition from a growing private sector, compounded by inefficient resource allocation and weak performance incentives. Approximately half of all SOEs, particularly small and medium-sized firms, reported financial losses in 1995 and 1996 due to redundant labor and declining productivity (Meng, 2000). However, existing labor contract rules continued to restrict dismissals, resulting in limited job insecurity for workers during this period.

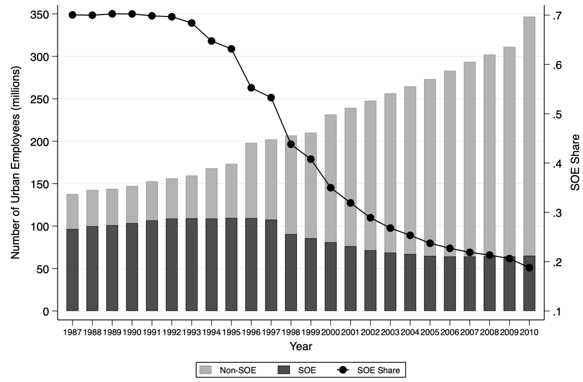
2.3.3 SOE restructuring and layoffs in the late 1990s

SOE reform began in 1995 to reduce fiscal pressure and improve efficiency. That year, a new labor policy permitted formal dismissal of workers, marking a departure from earlier employment guarantees. The reform adopted a strategy of "grabbing the large and letting go of the small," which retained large SOEs in strategic sectors under state control, while many small and medium-sized firms were privatized or closed (Hsieh & Song, 2015).[20] The reform expanded further in 1997 with the introduction of new labor laws, leading to widespread layoffs across larger firms.

Between 1995 and 2001, employment in state-owned enterprises fell from 113 million to 67 million, a reduction of nearly 40 percent (Giles et al., 2006). Figure 2.1 shows the number of workers in SOE and non-SOE sectors from 1987 to 2010, along with the SOE share of urban employment, which dropped sharply during the reform period. The restructuring was initiated by the central government and applied solely to the SOE sector, producing large employment shocks widely viewed as exogenous. At the time, formal safety nets were limited. In addition, a special assistance program for laid-off workers was criticized for low benefit coverage and minimal impact on reemployment (Giles et al., 2006). In this context, job insecurity likely caused substantial psychological stress not only among those dismissed but also among workers who remained employed and faced the risk of job loss.

[20] Large, medium, and small enterprise classifications are based on production capacity rather than employee count, as defined by the National Bureau of Statistics in 2002. A 2011 report from the Ministry of Commerce provides further detail: medium-sized firms typically have more than 100 but fewer than 300 employees, with assets between 50 million and 100 million RMB. For full classification criteria, see National Bureau of Statistics of China.

Figure 2.1: Trends in SOE and Non-SOE Employment, 1987–2010



Note: This figure displays the evolution of SOE and non-SOE employment at the national level between 1987 and 2010. The left axis shows the number of urban workers (in millions), and the right axis shows the share of SOE employment in the total urban workforce. The black line plots the SOE share of the total urban workforce. Data are from the *China Labour Statistical Yearbook*.

2.4 Data

2.4.1 The China Health and Nutrition Survey (CHNS)

This study uses data from the China Health and Nutrition Survey (CHNS), a longitudinal household survey conducted by the University of North Carolina at Chapel Hill and the Chinese Center for Disease Control and Prevention. The CHNS began in 1989 and has collected follow-up data every two to four years, with the most recent wave in 2015. The survey includes more than 4,000 households and approximately 15,000 individuals across nine provinces, covering regions with substantial variation in geography and economic development.

The CHNS uses a multistage, random cluster sampling design. In each province, counties were stratified by income level, and four counties were randomly selected: one low-income, two middle-income, and one high-income. In addition, the provincial capital and the lowest-income city were included in the sample.[21] As of 2011, the sampled provinces accounted for approximately 45 percent of the national population.

[21] A detailed description of the survey design is available at The China Health and Nutrition Survey.

Several features make the CHNS particularly well-suited to this study. First, the panel structure allows for comparisons of health outcomes across pre-, during-, and post-reform periods for the same individuals. Second, the survey includes household rosters, enabling analysis of spillover effects from job insecurity on other family members. Finally, the CHNS provides detailed information on labor market status, health behaviors, and individual demographics, facilitating an in-depth analysis of the relationship between job insecurity and smoking and alcohol use.

2.4.2 Measures of smoking and alcohol Use

The CHNS collects extensive information on tobacco and alcohol consumption. For smoking, respondents are asked whether they have ever smoked cigarettes. Those who answered "yes" were further questioned about their smoking history, including whether smoking now, the number of cigarettes smoked per day, duration of smoking, and age at smoking initiation. Three variables are constructed: (1) smoking status (never, former, or current smoker), (2) daily cigarette consumption among current smokers (intensive margin), and (3) daily cigarette consumption including non-smokers (extensive margin). Including both margins allows for a comprehensive analysis of smoking behavior and changes in sample composition.

For alcohol use, respondents report their drinking behavior over the past year. Three variables are constructed: an indicator for any alcohol use, an indicator for frequent drinking (defined as drinking at least four times per week), and an indicator for heavy drinking. Respondents who report alcohol consumption are also asked about weekly quantities of beer, wine, and liquor.[22] Following Li et al. (2011), the volume of pure alcohol is calculated based on beverage-specific alcohol content. One standard drink, as defined in the Dietary Guidelines for Americans 2015–2020, contains 14 grams of pure alcohol.[23] Alcohol consumption is categorized into four levels, based on thresholds used by J. Sun et al. (2022): none; light (up to 0.4 drinks per day for women and 0.9 for men); moderate (0.5 to 1 drink for women, 1 to 2 for men); and heavy (more than 1 drink for women, more than 2 for men).

[22] Questions include: "How many bottles of beer do you drink each week?" and "How many liang of wine/liquor do you drink each week?" One Chinese liang equals 50 grams.

[23] See full guidelines: Dietary Guidelines for Americans 2015–2020.

2.4.3 Treatment and control groups

Following He et al. (2018), individuals who reported their work unit as "state service/institute" or "state-owned enterprise" in the 1991 or 1993 CHNS waves are classified as the treatment group. Those who reported working in "government" units are classified as the control group. Before the SOE reform, both groups enjoyed similar levels of job security and employment benefits. However, only the SOE sector was affected by large-scale layoffs during the reform period, while government employment remained stable. This makes government employees a suitable control group for estimating the effects of perceived job insecurity.

2.4.4 Sample construction

The main difference-in-differences analysis uses CHNS data from 1991 to 2000, the period for which smoking and alcohol use information is available before and during the reform.[24] The sample includes men aged 16 to 55 and women aged 16 to 50. Age 16 is the legal minimum for employment in China. The upper bounds reflect the statutory retirement age: 60 for men and either 50 or 55 for women, depending on occupation (Feng et al., 2020). Since individuals nearing retirement are more likely to retire and may respond differently to job insecurity (G. H.-L. Cheng & Chan, 2008), the sample is restricted to prime-age workers to better capture behavioral responses to perceived employment risk during the reform. Given substantial disparities between rural and urban labor markets, the sample is limited to urban households.

[24] In additional event study analyses, data from 1991 to 2006 are used to assess whether the effects of the reform persist after layoff uncertainty subsides.

The SOE reform began in 1995 and reduced total state-sector employment from 113 million to 67 million by 2001, representing a 40 percent decline. The 1991 and 1993 CHNS waves define the pre-reform period; the 1997 and 2000 waves represent the reform period; and waves after 2000 capture the post-reform period. Individuals observed only in the pre-reform period or entering after the reform are excluded. The final sample also excludes respondents with missing data on smoking or alcohol use.

Table 2.1 reports summary statistics for the treatment and control groups using data from the 1991 and 1993 CHNS waves, which define the pre-reform period. The sample includes 852 individuals and 2,877 person-wave observations across the 1991–2000 waves. Compared to government employees, individuals working in the SOE sector are younger and have lower levels of education. Among men, SOE workers smoke more cigarettes per day, while government workers are more likely to be frequent drinkers. These imbalances highlight the importance of accounting for pre-reform differences through fixed effects and covariate controls in the empirical analysis.

Table 2.1: Descriptive statistics

	All			Men		
	Government	SOE sector	*p*-value	Government	SOE sector	*p*-value
Average age	34.61	33.18	0.001	35.82	33.76	0.001
Male	0.554	0.521	0.196	1.000	1.000	.
% currently employed	0.962	0.922	0.001	0.967	0.929	0.018
Education						
illiterate	0.050	0.140	0.000	0.039	0.127	0.000
primary school	0.082	0.205	0.000	0.077	0.225	0.000
middle school grad	0.341	0.444	0.000	0.327	0.459	0.000
high school grad	0.408	0.208	0.000	0.411	0.189	0.000
college and above	0.119	0.003	0.000	0.146	0.000	0.000
Marital status						
never married	0.150	0.172	0.247	0.145	0.200	0.041
married	0.841	0.823	0.360	0.851	0.794	0.037
divorced/widowed/separated	0.009	0.005	0.293	0.004	0.006	0.710
# of children under 18	0.972	1.014	0.271	0.974	0.944	0.584
# of sons under 18	0.481	0.564	0.009	0.492	0.525	0.447
# of daughters under 18	0.491	0.450	0.196	0.482	0.419	0.143
Current smoker	0.370	0.377	0.769	0.660	0.729	0.036
# of cigarettes smoked per day (0s excluded)	13.824	15.524	0.008	13.841	15.524	0.008
# of cigarettes smoked per day (0s included)	5.069	5.912	0.057	9.102	11.366	0.001
Current alcohol drinker	0.476	0.468	0.742	0.744	0.728	0.608
Frequent alcohol drinker	0.304	0.230	0.001	0.457	0.330	0.000
Heavy alcohol drinker	0.057	0.063	0.657	0.107	0.113	0.818
Observations	888	651	1539	492	339	831

Note: The *p*-values are reported for the null hypothesis that the means are equal across the samples, allowing for arbitrary correlation among observations within each province. Only the CHNS urban sample is included. The treatment group consists of individuals employed in the state-owned enterprise (SOE) sector, while the control group consists of those employed in the government sector. The sample includes men aged 16-55 and women aged 16-50.

2.5 Empirical Strategy

2.5.1 Difference-in-Differences

This study estimates the effects of SOE reform on health behaviors using a difference-in-differences (DiD) framework. The analysis compares outcomes over time between SOE workers, who were exposed to the reform, and government employees, whose employment remained stable. The baseline specification is:

$$Y_{ipt} = \alpha_i + \beta\left(\text{Reform}_t \times \text{SOE}_i\right) + \phi X_{ipt} + \delta S_{pt} + \tau_p + \gamma_t + \epsilon_{ipt} \quad (2.1)$$

Where Y_{ipt} denotes the outcome for individual i in province p and year t. The variable SOE_i is an indicator equal to one if the individual was employed in the state-owned sector before the reform. Reform_t equals one for the reform period from 1997 to 2000 and zero otherwise. The interaction term $\text{SOE}_i \times \text{Reform}_t$ captures the differential change in outcomes for SOE workers during the reform period. Following Caetano et al. (2022), I include time-varying covariates that are unlikely to be influenced by the reform and exclude those that may be affected, to avoid bias from conditioning on post-treatment variables. The vector X_{ipt} includes individual-level controls for age and education level.[25] The term S_{pt} captures province-level time-varying economic conditions such as GDP per capita. The specification includes individual fixed effects α_i, province fixed effects τ_p, and year fixed effects γ_t. Standard errors are clustered at the individual level. During the study period (1991–2000), no province in the sample had adopted comprehensive indoor smoke-free laws. According to Fu et al. (2024), more than 20 Chinese cities began introducing such legislation only after 2008.[26] The coefficient β represents the average effect of SOE reform on the outcome among SOE workers during the reform period.

[25] In a robustness check, I add marital status, the number of children under age 18, and the number of sons under age 18. These covariates may plausibly respond to the reform and are therefore excluded from the main specification. The results remain similar when they are included.

[26] The sample includes eight provinces: Liaoning, Jiangsu, Shandong, Henan, Hubei, Hunan, Guangxi, and Guizhou.

2.5.2 Parallel trends assumption

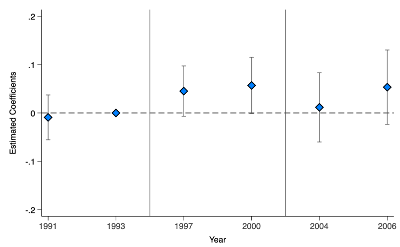
A key identification assumption in the difference-in-differences strategy is that outcomes for SOE and government workers would have followed similar trends in the absence of the reform. To assess the validity of this assumption, I estimate the following event-study specification:

$$Y_{ipt} = \alpha_i + \sum_{t=1991}^{2006} \beta_t \times SOE_i + \phi X_{ipt} + \delta S_{pt} + \tau_p + \gamma_t + \epsilon_{ipt} \qquad (2.2)$$

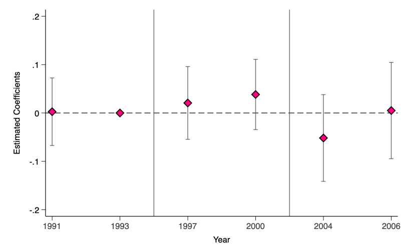
where the coefficients β_t capture differential trends in outcomes for SOE workers relative to government employees in each year. The year 1993 is omitted and serves as the reference period, so all coefficients are interpreted relative to that baseline. All other variables are defined as in equation 2.1. The specification includes individual fixed effects (α_i), year fixed effects (γ_t), province fixed effects (τ_p), and controls for individual and province-level covariates.

Figure 2.2 presents the estimated β_t coefficients from the event study model in Equation 2.2. Across all six outcomes (current smoking, cigarettes per day with and without non-smokers, any alcohol use, frequent drinking, and heavy drinking), the coefficient for 1991 is small and statistically indistinguishable from zero, suggesting no differential trends between SOE and government workers

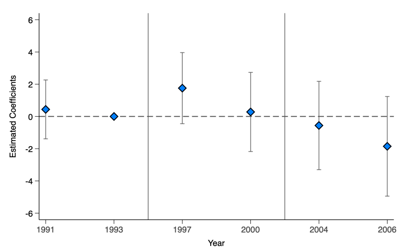
Figure 2.2: Event-study estimates



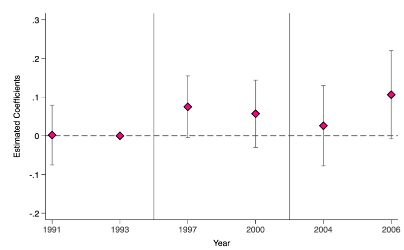
(a) Current smoking



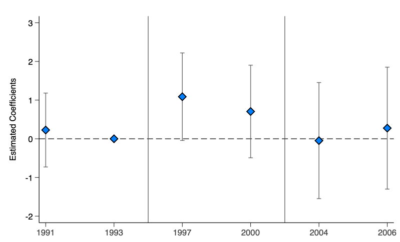
(b) Any alcohol use



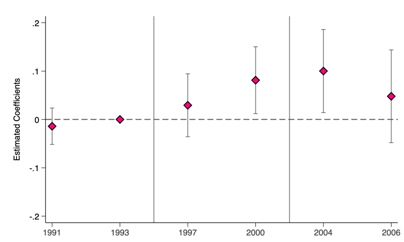
(c) Cigarettes per day (without os)



(d) Frequent drinking



(e) Cigarettes per day (with os)



(f) Heavy drinking

Notes: This figure reports event-study estimates from Equation 2.2, with 1993 as the reference year. All models include individual, year, and province fixed effects. Standard errors are clustered at the individual level. Vertical lines indicate the SOE reform period (1995–2002).

before the onset of reform. Although the pre-reform period is limited, the absence of lead effects does not indicate the presence of pre-trends. During the reform period, the coefficients diverge in directions consistent with increased perceived layoff risk, with smoking and alcohol use rising more among SOE workers. These patterns support the interpretation that the observed behavioral changes were a response to the reform. The event study results support the identifying assumption.[27]

[27] Appendix Figures B.1 present results for the male subsample. Findings are consistent with the full sample, showing no differential trends before the reform and divergence afterward.

2.5.3 Alternative measures

To complement the baseline difference-in-differences analysis, I explore alternative measures of job insecurity that capture variation in the intensity and timing of the SOE reform across provinces. This approach allows for a more nuanced analysis by accounting for heterogeneity in perceived employment risk. Following the methodology of Kong et al. (2019), I construct two province-year level proxies: the layoff rate and the expected income loss due to displacement. Both are derived from administrative statistics published in the China Labour Statistical Yearbook (1997–2006).[28]

[28] Each edition of the China Labour Statistical Yearbook reports statistics measured as of the end of the previous calendar year. For example, the 1997 yearbook reports values reflecting conditions at the end of 1996.

The layoff rate in province p and year t is defined as the ratio of laid-off workers to total state sector employment:

$$\text{LayoffRate}_{pt} = \frac{\text{Number of laid-off workers in state sector}_{pt}}{\text{Number of state sector workers}_{pt}} \tag{2.3}$$

To capture the financial severity of displacement, I construct a measure of expected income loss, defined as:

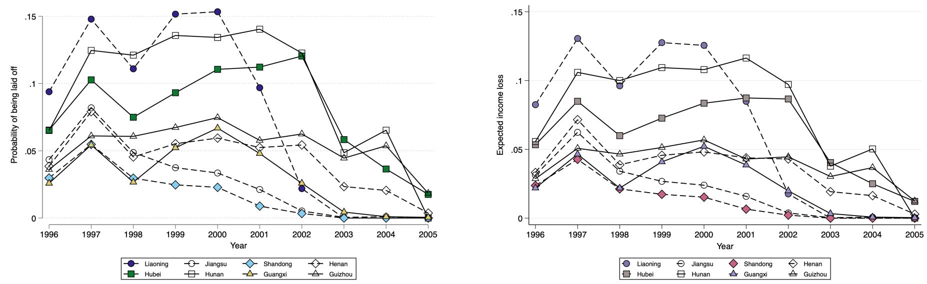
$$\text{FinancialLoss}_{pt} = 1 - \frac{\text{Average subsidy received by laid-off workers}_{pt}}{\text{Average wage of on-the-job SOE workers}_{pt}} \tag{2.4}$$

and multiply this by the layoff rate to obtain the expected income loss:

$$\text{ExpectedLoss}_{pt} = \text{LayoffRate}_{pt} \times \text{FinancialLoss}_{pt} \tag{2.5}$$

These measures capture perceived insecurity beyond observed job separations. Prior to 1995, layoff risk in the SOE sector was effectively zero due to lifetime job guarantees. Thus, I set the layoff rate and expected loss to zero for the pre-reform period. Figure 2.4 displays the variation in the probability of being laid off and the expected economic loss across provinces and years. Liaoning experienced a sharp rise in both the probability of layoff and expected income loss, while provinces such as Shandong remained relatively stable. In 2000, expected income loss reached 15.3% in Liaoning, compared to just 2.3% in Shandong. These marked spatial and temporal differences offer credible variation to assess the effects of perceived job insecurity.

Figure 2.3: Layoff probability and expected income loss by province, 1996-2005



(a) Probability of being laid off (b) Expected income loss

Note: This graph presents the probability of being laid off (a) and the expected income loss (b) for selected provinces in China from 1996 to 2005. The data is obtained from the China Labour Statistical Yearbook (1997–2006).

To incorporate these alternative measures into the empirical framework, I estimate the following difference-in-differences specification:

$$Y_{ipt} = \alpha_i + \beta_1(\mathrm{Z}_{pt} \times \mathrm{SOE}_i) + \beta_2\mathrm{Z}_{pt} + \phi X_{ipt} + \delta S_{pt} + \tau_p + \gamma_t + \epsilon_{ipt}, \quad (2.6)$$

where Z_{pt} is either the layoff rate or expected income loss in province p and year t. The coefficient β_1 captures the differential response of SOE workers to rising job insecurity, while β_2 estimates common effects across all workers. The model includes individual fixed effects (α_i), province fixed effects (τ_p), year fixed effects (γ_t), province-year economic controls (S_{pt}), and a set of time-varying individual characteristics (X_{ipt}). A positive and statistically significant β_1 would indicate that greater exposure to job insecurity is associated with increased smoking and alcohol use, consistent with the interpretation of these behaviors as stress responses. Estimating this alternative model thus serves as a robustness check and reinforces the interpretation of the SOE reform as a credible source of perceived employment risk.

2.6 Main Results

Table 2.2 reports the main DiD estimates of the effects of SOE reform on smoking and alcohol use. Panel A presents results for the full sample. Panel B restricts the analysis to men, given substantial gender differences in health behaviors in China. In the full sample, SOE workers experience a significant increase of 5.8 percentage points in the probability of current smoking relative to government employees. Cigarette consumption also rises significantly, increasing by approximately 1.93 cigarettes per day among smokers (intensive margin) and by 1.12 cigarettes per day on the extensive margin (including non-smokers). For alcohol outcomes, frequent drinking increases significantly by 6.6 percentage points, while heavy drinking increases marginally by 4.8 percentage points. The estimated 3.6 percentage point increase in the probability of any alcohol consumption remains statistically insignificant.

Results for the male subsample are consistently stronger. Male SOE workers show a significant 9.8 percentage point increase in the likelihood of current smoking compared to government counterparts. Cigarette use among male smokers significantly rises by about 1.94 cigarettes per day on the intensive margin, and increases by roughly 2.01 cigarettes per day on the extensive margin. Alcohol outcomes display substantial responses among men. Specifically, the probability of any drinking significantly rises by 10.4 percentage points, and frequent drinking significantly increases by 14.8 percentage points. Heavy drinking also increases by 8.9 percentage points, though this result is only marginally significant. These findings indicate robust increases in both smoking and alcohol consumption among men exposed to heightened job insecurity.

To examine geographic variation in reform intensity, Table B1 uses province-year differences in layoff rates and expected income losses. The interaction terms between these measures and SOE status indicate differential behavioral responses to varying levels of perceived job insecurity. Results consistently confirm the main findings: higher province-level layoff rates and expected income losses significantly raise both the probability of current smoking and daily cigarette consumption on the extensive margin for SOE workers. Alcohol-related behaviors show less robust responses; only heavy drinking demonstrates a marginally significant increase, while changes in frequent drinking and overall alcohol use remain imprecisely estimated.

For the male subsample, the estimated responses are even clearer and more consistently significant. Higher layoff rates and greater expected income losses significantly increase the probability of current smoking, frequent drinking, and heavy drinking among male SOE workers. These findings reinforce earlier evidence that men exhibit stronger behavioral responses to increased job insecurity. Effects on overall alcohol consumption show similarly positive directions, though with somewhat less precision. Taken together, these results highlight

Table 2.2: Effects of SOE reform on smoking and alcohol use

	(1) Current smoker	(2) Cigarettes (os excluded)	(3) Cigarettes (os included)	(4) Alcohol drinker	(5) Frequent drinker (os included)	(6) Heavy drinker (os included)
Panel A: All population						
Reform × SOE	0.058**	1.926**	1.118**	0.036	0.066**	0.048*
	(0.023)	(0.944)	(0.465)	(0.027)	(0.030)	(0.027)
Mean	0.365	15.649	5.552	0.465	0.255	0.108
Number of obs	2814	955	2800	2819	2855	2611
R-squared	0.813	0.668	0.776	0.680	0.490	0.508
Panel B: Men						
Reform × SOE	0.098**	1.943**	2.014**	0.104***	0.148***	0.089*
	(0.042)	(0.946)	(0.872)	(0.040)	(0.050)	(0.049)
Mean	0.668	15.646	10.225	0.730	0.388	0.191
Number of obs	1523	952	1510	1538	1556	1385
R-squared	0.659	0.669	0.671	0.541	0.439	0.495

Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. The reform period includes the 1997 and 2000 waves. All regressions follow the baseline specification in equation 2.1 and include individual, province, and year fixed effects. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

pronounced geographic heterogeneity in how labor market shocks influenced health behaviors, particularly among men.

Direct effects of provincial layoff rates and expected income losses, capturing average exposure across both SOE and government workers, remain small and statistically insignificant. This pattern further supports the interpretation that observed behavioral responses primarily result from increased perceived job insecurity among SOE workers. Taken together, these findings indicate that SOE reform significantly increased the probability of current smoking and cigarette consumption among SOE workers, with suggestive but less robust effects on alcohol consumption. The results for men are particularly pronounced, consistent with existing evidence on gendered coping behaviors in response to economic stress. These findings underscore the broader implications of job insecurity as a critical determinant of health behaviors, emphasizing the health consequences of labor market shocks in developing countries.

2.7 Discussion

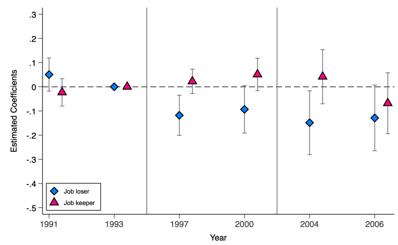
A large body of research has shown that job displacement during major layoffs can result in persistent earnings losses, though the underlying mechanisms vary across institutional contexts. In the United States, the dominant channel is reduced wages in post-displacement employment, whereas in European countries such as the United Kingdom and Germany, extended spells of unemployment account for most of the long-term income decline (Bande & Riveiro, 2013; Couch & Placzek, 2010; Hijzen et al., 2010). In the Chinese context, Tian et al. (2022) examine the SOE reform of the late 1990s and find substantial and lasting earnings losses among displaced workers, primarily driven by lower wages in subsequent employment rather than prolonged unemployment.

The baseline analysis defines the treatment group as individuals employed in state-owned enterprises prior to the reform. This group includes both workers who were laid off and those who remained employed during the reform, so the estimates reflect the combined effect of job displacement and job insecurity. Prior research has shown that even anticipated job loss can trigger psychological and behavioral responses comparable in magnitude to those associated with actual displacement (Witte, 1999). Distinguishing between these channels is crucial for understanding the broader implications of the SOE reform.

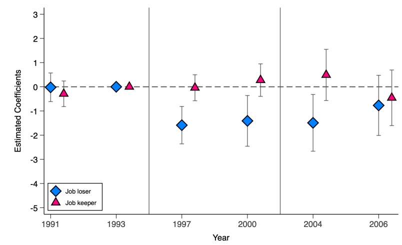
To isolate the effects of displacement from those of job insecurity, this study follows the classification in Tian et al. (2022) and defines displaced workers as those who transitioned from full-time SOE employment to self-employment, unemployment, or temporary contract work during the reform period. Although the data do not allow for a definitive separation between voluntary and involuntary exits, historical records indicate that roughly 40 percent of SOE employees were laid off between 1995 and 2002. In the sample used here, 45 percent of pre-reform SOE workers meet this displacement criterion, consistent with national estimates. Very few individuals moved from government to SOE employment during the reform period, and these cases are excluded from the analysis.

Figure B.2 displays descriptive trends in employment, work hours, and income for three groups: government workers, SOE workers who remained employed throughout the reform, and those who experienced displacement. Labor market trajectories diverge sharply following the onset of the reform. Displaced SOE workers experienced steep and persistent declines in employment and hours worked, with effects that extended well beyond the reform period. In contrast, outcomes for non-displaced SOE workers remained broadly parallel to those of government employees, with only modest post-reform declines.

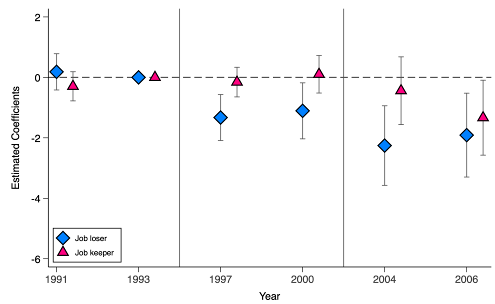
Figure 2.4: Event-study estimates on labor market outcomes



(a) Employment



(b) Hours worked (with os)



(c) Log (annual income)

Note: This figure presents event study estimates from Equation 2.2, with 1993 normalized as the reference year. The outcome variable is the logarithm of annual individual income, where zero values are replaced with one before transformation. All monetary values are adjusted to 2015 RMB. The coefficients capture differential trends in labor market outcomes between SOE and government workers. Vertical bars represent 95% confidence intervals. Grey lines indicate the SOE reform period (1995–2002). All regressions include individual, year, and province fixed effects. Standard errors are clustered at the individual level.

Figure 2.4 presents event study estimates that compare SOE workers to government employees, disaggregated by displacement status. The results show sharp and persistent labor market penalties for displaced workers. Relative to government employees, displaced SOE workers experienced significant declines in employment, hours worked, and earnings starting in the reform period and continuing thereafter. By contrast, estimates for non-displaced SOE workers are statistically indistinguishable from zero across all three outcomes. These findings highlight that the reform affected different groups through distinct mechanisms. For displaced workers, the effects reflect both direct income loss and the psychological stress of unemployment. For those who remained employed, behavioral responses are more likely driven by heightened job insecurity stemming from the sudden and substantial increase in layoff risk.

Table 2.3: Effects on smoking and alcohol use by laid-off experience

	(1) Current smoker	(2) Cigarettes (os excluded)	(3) Cigarettes (os included)	(4) Alcohol drinker	(5) Frequent drinker (os included)	(6) Heavy drinker (os included)
Panel A: Job keeper						
Reform × SOE	0.059**	2.519**	1.578***	0.039	0.081**	0.054
	(0.029)	(1.108)	(0.610)	(0.035)	(0.038)	(0.036)
Mean	0.369	15.704	5.624	0.468	0.261	0.107
Number of obs	2253	771	2240	2262	2286	2089
R-squared	0.810	0.680	0.781	0.675	0.494	0.516
Panel B: Job loser						
Reform × SOE	0.064**	1.252	0.803	0.033	0.046	0.042
	(0.028)	(1.275)	(0.566)	(0.033)	(0.037)	(0.034)
Mean	0.347	15.158	5.056	0.456	0.261	0.100
Number of obs	2186	695	2171	2192	2221	2023
R-squared	0.804	0.697	0.773	0.681	0.496	0.503

Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. The reform period includes the 1997 and 2000 waves. All regressions follow the baseline specification in equation 2.1 and include individual, province, and year fixed effects. Panel A restricts the sample to SOE workers who remained employed during the reform, while Panel B reports results for those who were laid off. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3 presents health behavior estimates separately for displaced workers and those who remained employed during the SOE reform. Among displaced workers, smoking prevalence increases significantly, consistent with prior evidence that involuntary job loss can trigger stress-related health behaviors (Tian et al., 2018). The effects on cigarette consumption and drinking behaviors are positive but not statistically significant. One possible explanation is the presence of offsetting forces: psychological distress may increase the demand for coping behaviors, while income loss may constrain consumption. These muted responses suggest that displacement alone does not account for the full extent of behavioral change observed in the baseline results.

By contrast, job keepers exhibit stronger and more consistent shifts in behavior. Smoking prevalence rises by approximately six percentage points, accompanied by significant increases in cigarette consumption across both the extensive and intensive margins. The probability of frequent drinking also increases, though effects on heavy drinking are smaller and not statistically significant. These changes occur despite stable income and employment, as shown in the labor market analysis. The findings suggest that elevated job insecurity, rather than income loss, drove the observed changes in behavior. In the context of widespread layoffs, even retained SOE workers faced an abrupt increase in perceived dismissal risk. These results align with prior evidence that anticipated job loss can independently affect health behavior (Witte, 1999), and underscore the role of employment uncertainty as a key mechanism.

Table 2.4: Effects on smoking and alcohol use by spousal employment status

	(1)	(2)	(3)	(4)	(5)	(6)
	Current smoker	Cigarettes (os excluded)	Cigarettes (os included)	Alcohol drinker	Frequent drinker (os included)	Heavy drinker (os included)
Panel A: Men whose wives also work in the SOE sector						
Reform × SOE	0.123**	2.574**	2.786**	0.137***	0.122*	0.057
	(0.050)	(1.207)	(1.153)	(0.051)	(0.063)	(0.059)
Mean	0.639	15.168	9.457	0.708	0.399	0.181
Number of obs	1090	648	1082	1102	1113	985
R-squared	0.677	0.687	0.685	0.546	0.435	0.490
Panel B: Men whose wives do not work in the SOE sector						
Reform ×SOE	0.085	0.343	1.262	0.082	0.173**	0.073
	(0.059)	(1.252)	(1.133)	(0.056)	(0.067)	(0.071)
Mean	0.659	15.383	9.820	0.717	0.389	0.172
Number of obs	1052	640	1038	1057	1068	943
R-squared	0.676	0.660	0.699	0.538	0.465	0.465

Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. All regressions follow the baseline specification in Equation 2.1 and include individual, province, and year fixed effects. Panel A includes male SOE workers whose wives are employed in the SOE sector. Panel B includes male SOE workers whose wives are not employed in the SOE sector. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4 examines household-level exposure by stratifying male SOE workers based on whether their wives were also employed in the SOE sector. Behavioral responses are markedly stronger when both spouses are employed in the sector. Among these men, smoking prevalence increases by 12.3 percentage points, accompanied by significant rises in cigarette consumption on both extensive and intensive margins. Alcohol use also rises sharply, with a 13.7 percentage point increase in the probability of any drinking. Frequent drinking increases marginally, while the effect on heavy drinking is positive but not precisely estimated.

In contrast, when only the husband is employed in the SOE sector, the estimated increases in smoking and general alcohol use are smaller and not statistically distinguishable from zero. Frequent drinking rises significantly, but the effects on cigarette consumption and heavy drinking lack precision. These patterns suggest that perceived employment risk extends beyond the individual, with joint exposure amplifying uncertainty and intensifying coping behaviors. The findings align with prior evidence on joint income risk and household-level responses to shocks (Blundell et al., 2016; Gallipoli & Turner, 2009), highlighting the salience of shared economic vulnerability in shaping men's health behaviors during the reform.

Table 2.5: Effects on smoking and alcohol use by educational attainment

	(1)	(2)	(3)	(4)	(5)	(6)
	Current smoker	Cigarettes (0s excluded)	Cigarettes (0s included)	Alcohol drinker	Frequent drinker (0s included)	Heavy drinker (0s included)
Panel A: Below high school education						
Reform × SOE	0.052*	3.074**	1.425**	0.039	0.039	0.082**
	(0.028)	(1.232)	(0.587)	(0.034)	(0.039)	(0.036)
Mean	0.380	16.606	6.185	0.445	0.228	0.117
Number of obs	1601	573	1601	1613	1629	1508
R-squared	0.830	0.643	0.789	0.709	0.487	0.524
Panel B: High school education and above						
Reform × SOE	0.017	-0.573	-0.098	-0.003	0.090	0.002
	(0.042)	(1.873)	(0.818)	(0.051)	(0.056)	(0.047)
Mean	0.348	14.196	4.748	0.492	0.293	0.095
Number of obs	1152	362	1137	1147	1162	1048
R-squared	0.801	0.700	0.758	0.645	0.490	0.492

Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. All regressions follow the baseline specification in equation 2.1 and include individual, province, and year fixed effects. Panel A restricts the sample to individuals with less than a high school education, while Panel B includes those with at least a high school degree. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5 reports heterogeneous effects by educational attainment. The behavioral response to SOE exposure is concentrated among less-educated men. For workers without a high school degree, smoking prevalence increases by 5.2

percentage points, and both the extensive and intensive measures of cigarette consumption rise significantly. Heavy drinking also increases by 8.2 percentage points. In contrast, estimates among more educated workers are small and statistically indistinguishable from zero across all outcomes. These patterns are consistent with evidence from other settings showing that lower-SES individuals face greater psychological strain during periods of economic instability, in part due to more limited reemployment options and weaker financial buffers (Y. Cheng et al., 2005; Sverke et al., 2002). In the Chinese labor market, where educational attainment is closely tied to occupational mobility, these disparities likely reflect heightened perceived insecurity among the less educated.

Table 2.6: Effects on smoking and alcohol use by firm size

	(1) Current smoker	(2) Cigarettes (os excluded)	(3) Cigarettes (os included)	(4) Alcohol drinker	(5) Frequent drinker (os included)	(6) Heavy drinker (os included)
Panel A: Small to medium-sized firms						
Reform × SOE	0.065**	1.947*	1.491***	0.068**	0.087**	0.058*
	(0.028)	(1.099)	(0.567)	(0.033)	(0.037)	(0.034)
Mean	0.365	15.420	5.451	0.465	0.267	0.103
Number of obs	2294	777	2282	2299	2327	2123
R-squared	0.811	0.681	0.778	0.664	0.500	0.497
Panel B: Large firms						
Reform × SOE	0.044	1.540	0.442	-0.015	0.041	0.024
	(0.031)	(1.457)	(0.662)	(0.036)	(0.041)	(0.037)
Mean	0.351	15.378	5.190	0.458	0.255	0.103
Number of obs	2079	666	2064	2089	2114	1929
R-squared	0.803	0.695	0.777	0.693	0.492	0.521

Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. All regressions follow the baseline specification in equation 2.1 and include individual, province, and year fixed effects. Panel A restricts the sample to workers in small firms, while Panel B presents results for workers in larger firms. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6 examines heterogeneity in behavioral responses to SOE reform by firm size. The reform prioritized the privatization and restructuring of smaller state-owned enterprises, consistent with the policy directive to grasp the large and let go of the small (Hsieh & Song, 2015). As a result, job insecurity was more pronounced among workers in small and medium-sized firms, where layoffs were concentrated at the onset of the reform. The classification of firm size follows the National Bureau of Statistics definition, with the threshold for small and medium-sized enterprises set at 300 employees.[29]

Among individuals employed in smaller SOEs, the probability of smoking rises by 6.5 percentage points, and cigarette consumption increases significantly on the extensive margin and marginally on the intensive margin. Alcohol use also rises significantly. The probability of drinking increases by 6.8 percentage points, while frequent drinking rises by 8.7 points. Heavy drinking increases by 5.8 points, although this estimate is marginally significant. By contrast, estimates among workers in larger SOEs are smaller in magnitude and lack statistical significance across all outcomes. These patterns underscore the protective role of institutional insulation within larger firms and suggest that perceived employment risk was more salient and consequential among workers in smaller SOEs. Prior research similarly documents that perceived layoff risk varies across firms and strongly predicts stress-related health behaviors (Kivimäki et al., 2001; Moore et al., 2004).

[29] See classification standards published by the National Bureau of Statistics of China, available at: stats.gov.cn.

As a robustness check, I estimate several alternative specifications. First, I include province-by-year fixed effects to account for region-specific shocks that vary over time. Second, I add province-specific linear trends to capture gradual changes in outcomes across provinces. Third, I restrict the sample to individuals aged 20 to 45 to focus on the prime working-age population. This adjustment addresses concerns that individuals nearing retirement may respond differently to employment-related risk. In China, the statutory retirement age is 60 for men, 50 for women in blue-collar jobs, and 55 for women in professional or technical roles. Older individuals may be more likely to exit the labor force rather than adjust smoking or drinking behavior in response to employment uncertainty (G. H.-L. Cheng & Chan, 2008). The results, reported in Table B2, Table B3, and Table B4, are consistent with the baseline findings and support the main conclusion that exposure to SOE reform increased smoking and alcohol use.

2.8 Conclusion

This paper examines how large-scale economic restructuring affects health behaviors, using the SOE reform in late 1990s China as a natural experiment. The reform led to widespread job displacement and heightened employment uncertainty among SOE workers, while government employees remained largely insulated. Also, this context provides a setting in which both job loss and the threat of future displacement can be evaluated. By comparing SOE workers to government employees, the analysis identifies the effects of SOE reform on smoking and alcohol use.

Using a difference-in-differences strategy, the findings show that SOE workers became more likely to smoke and engage in excessive drinking during the reform period. Although some estimates are imprecise, the direction and magnitude of effects are consistent across outcomes. The behavioral response is especially pronounced among men, particularly those with spouses also employed in the SOE sector, suggesting that household-level exposure intensified stress. Heterogeneity analysis shows stronger effects among workers with less education and those employed in smaller firms, groups more vulnerable to uncertainty and displacement.

The results highlight that the impact of SOE reform extended beyond job loss. Workers who remained employed also adjusted their behavior in response to perceived risk, indicating that job insecurity played a central role. Supplementary analyses using province-year variation in layoff rates and expected income loss offer alternative measures of exposure to reform, reinforcing the interpretation that more intense reform pressure led to greater adverse behavioral responses. In addition, comparing job keepers to job losers suggests that the stress of anticipating job loss may be at least as detrimental, in behavioral terms, as unemployment itself.

This study contributes to several strands of literature. It provides new evidence on the behavioral costs of economic restructuring, identifying smoking and alcohol use as key channels. It also adds to the understanding of labor market uncertainty by demonstrating that employment transitions influence health behaviors even without direct income loss. Finally, the findings underscore the uneven burden of reform, with the most pronounced effects observed among socioeconomically disadvantaged groups.

In settings with limited formal protection, structural economic reform can generate substantial welfare costs even for those who remain employed. These results suggest that policy responses should not focus solely on displaced workers. Interventions aimed at stabilizing expectations, reducing uncertainty, or expanding access to mental health support during periods of large-scale reform may be essential in mitigating the broader public health consequences of labor market transitions.

Chapter 3

The Lasting Impact of the Tuskegee Syphilis Study: COVID-19 Vaccination Hesitation among African Americans with Yang Jiao, Leilei Shen and Zhuo Chen [30]

3.1 Introduction

Starting in 1932, the United States Public Health Service enlisted 600 African American men from Tuskegee, Alabama, to participate in the Tuskegee Study of Untreated Syphilis in the Negro Male (henceforth the Tuskegee Study). Treatment was withheld even though penicillin was proven effective and became the standard of care by 1940. In addition, the men were actively discouraged from seeking medical advice from doctors outside the study (A. M. Brandt, 1978).[31] The experiment ended in 1972 when a whistle-blower, Peter Buxtun, leaked information about the project to the New York Times.[32] The news release of the Tuskegee Study sparked a nationwide outcry, prompting the National Association for the Advancement of Colored People to file a class-action lawsuit against the United States Public Health Service.[33] The Tuskegee Syphilis Study "became a symbol of their [African American's] mistreatment by the medical

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[31] A. M. Brandt (1978) detailed a thorough summary of the Tuskegee study and racism in medical research.

[32] The paper published the story on the front page on November 16^{th}, 1972, and "dropped a bomb into the laps." Only 74 study participants were alive then; 128 patients had died of syphilis or its complications, 40 of their wives had been afflicted, and 19 of their children had had congenital syphilis. The digitized version of the article was retrieved from the New York Times Archives, Aide Questioned Syphilis Study.

[33] Survivors of the study later reported that the doctors diagnosed them with "bad blood," and they thought they were being treated when in fact they were only given the placebo.

establishment, a metaphor for deceit, conspiracy, malpractice, and neglect, if not outright genocide" (Corbie-Smith et al., 1999, p. 542).

The COVID-19 pandemic has disproportionately affected African Americans, both in terms of their economic and health outcomes.[34] The negative impact on this particular population has raised concerns among policymakers and medical professionals regarding vaccination hesitancy within African American communities. Studies have extensively explored the racial disparity in vaccination rates and the factors contributing to it (Ndugga et al., 2021; Nguyen et al., 2021).[35] However, there is a lack of quantitative research at sub-national levels within the United States that examines the underlying causes of these variations, despite the significance of potential racial differences in COVID-19 vaccinations. Furthermore, there is a scarcity of research investigating the influence of historical events on African American vaccination practices. To address these gaps, our study focuses on the long-term effects of the Tuskegee Study on vaccine uptake among the Black population during the COVID-19 pandemic. By exploring the impact of historical injustices, our findings provide new insights into how such events can contribute to suboptimal health behaviors and emphasize the government's responsibility to advance equity in public health.

The present study evaluates a causal relationship between African Americans' hesitation to receive the COVID-19 vaccine and their exposure to the Tuskegee Study. Building on the theory of social identification (Ashforth & Mael, 1989; Bhattacharya et al., 1995), we gauge the exposure to the Tuskegee Study by assessing the geographic proximity to Macon County, Alabama, the site of the Tuskegee Study. African Americans who live in close proximity to Tuskegee may be more knowledgeable about the event and, therefore, may have a higher level of medical mistrust and a lower level of vaccination uptake.[36] Empirically, we utilize a three-way fixed effect design, and the coefficient of interest is the interaction between a time indicator, the percentage of the Black population, and the exposure to the Tuskegee study based on the proximity to Macon County. In all specifications, we compare the change in vaccination rates across counties with large proportions of the Black population and those near Tuskegee and those with low proportions of the Black population but far from Tuskegee.

Our findings show that, while the overall vaccination gap between communities with low and high proportions of Black residents gradually closes over time, the vaccination rate — for both those who are fully immunized and those who have received at least one dose of the vaccine — increases more slowly in counties with a higher proportion of African Americans. Additionally, the rate of increase is substantially correlated with exposure to the Tuskegee Study,

[34] When compared with non-Hispanic Whites in the United States, African Americans are three times more likely to get COVID-19 and up to six times more likely to die from it (Yancy, 2020). A more recent study by Aburto et al. (2022) found that life expectancy fell more for Black men (3.6 years) compared with White men (1.5 years). Black Americans saw increases in cardiovascular diseases and "deaths of despair" over this period. These changes dramatically increase the already large gap in life expectancy between Black and White people.

[35] According to a National Association for the Advancement of Colored People study in November 2021, only 14% of the Black survey respondents trust the vaccine's safety, and only 18% trust the vaccine's safety and plan to get vaccinated. The full report can be retrieved here: Coronavirus Vaccine Hesitancy in Black and Latinx Communities.

[36] Proximity between agents, as described by (Tabellini, 2010, p. 680), "could refer to geography, but also to social or economic dimensions such as religion, ethnicity, and class."

measured by the distance between the county and Tuskegee, indicating that the gap closes more quickly due to less mistrust when there is less exposure to the Tuskegee Study. Our findings indicate that COVID-19 vaccine hesitancy is more common in locations with higher percentages of Black residents.

There are two potential threats to our identification. The first comes from the reporting of COVID-19 vaccination rates because the Centers for Disease Control and Prevention (CDC) released COVID-19 vaccination rates for all races. In all specifications, we add both the interaction between week dummies and the share of the White population, and the interaction between week dummies and the population share of Hispanics to control for differentiated trends in vaccination growth for other race groups. Another challenge to our identification is the likelihood that vaccine spillover effects are substantially stronger for persons living in the same community (Bruhin et al., 2020), especially among people of various races. We divide counties into distinct racially separated residential regions and compare the estimated results of separate regressions for high- and low-segregated areas to exploit the existence and extent of such spillover effects.[37] The estimates for high- and low-segregated areas, on the other hand, are very similar, showing that spillover effects are not responsible for our findings.

[37] The idea behind such an experiment is that if our results are predominantly driven by spillover effects between people, the impacts in less segregated areas (with more social interaction) should be more significant than in highly segregated areas.

In addition, we perform a series of robustness tests. First, we utilize the intensity of news coverage regarding the Tuskegee Study in different counties to establish an alternative measure of Tuskegee exposure. The results corroborate that in counties with higher levels of exposure to Tuskegee news reporting, the vaccination rate is indeed lower. Second, these findings apply exclusively to African Americans and do not extend to other racial groups. Moreover, the results hardly exhibit any variation, and in fact, they become more pronounced in size when we restrict our sample to the Southern regions. Third, given that our treatment variables (the Black population share and distance to Macon, Alabama) remain constant over time, there is a potential for changes in demographic composition due to migration. The migration during the pandemic may also have an impact on our results. To address this concern, we have conducted two tests. In the first test, we replace the Black population percentage in 2010 with the percentage between 1990 and 2005, resulting in small changes in the results. Our second test involves excluding counties with the highest in and out-of-state migration during the pandemic, and our findings remained consistent and robust.

Additionally, our findings are robust to estimation on a nationwide sample and the baseline coefficients for vaccination rate based on geographic proximity to Macon County are larger than 96.7% of placebo tests when substituting proximity to other falsely assigned counties. We have run another robustness test by analyzing the effects of the Tulsa Massacre, another incident that specifically targeted African Americans. The estimates show that the Tulsa Massacre has no impact on vaccination rates. Finally, in the spirit of Altonji et al. (2005), we provide a measure to gauge the strength of the likely bias arising from unobservables. Our findings indicate that it is unlikely that unobserved heterogeneities alone provide a complete explanation.

We have also performed several heterogeneity analyses. Several findings emerge. First of all, counties characterized by higher proportions of young African Americans exhibit not only lower vaccination rates but also encounter slower progress and face a greater challenge in closing the racial vaccination gap. Political ideology is also important, and the impact of the Tuskegee Study is stronger among Republican-leaning counties. Education plays a crucial role, with a more pronounced effect observed in counties where a higher proportion of residents have attained higher levels of education. Lastly, vaccination eligibility is crucial because the vaccination gap narrowed after vaccine eligibility expanded.

This article builds on and contributes to two strands of literature in economics and public health. First, our research adds to the body of research on medical mistrust and health consequences. Alsan and Wanamaker (2018) find that the disclosure of the Tuskegee experiment led to an increase in medical mistrust and mortality, a decrease in the life expectancy of Black men, and declines in both outpatient and inpatient visits for older Black men. In a subsequent paper using a randomized controlled trial, Alsan et al. (2019) find that Black men are more likely to get a flu vaccine when paired with a Black doctor. Lowes and Montero (2021) find that greater exposure to colonial medical campaigns that resulted in deaths and severe side effects reduces vaccination rates and trust in medicine in Central Africa.

Moreover, our paper is also related to a broader literature on the historical origin of (mis)trust. We contribute to the literature on mistrust in several ways. First, we show that the mistrust from suffering historical wrongdoing could have an intergenerational impact (Algan & Cahuc, 2010; Knack & Keefer, 1997; Nunn & Wantchekon, 2011). The aftermaths of the Tuskegee Study include vaccine hesitation among African Americans to this day. Second, we find that the mistrust could have a spatial pattern, as the closing of the gap in vaccine takeup rates correlates to the distance of the county to Tuskegee. Third, we show that historical traumas unrelated to health, such as the Tulsa Race Massacre, while with a lasting impact, have little bearing on future generations' health-seeking behavior (Dupas & Miguel, 2017).

3.2 Data and Estimation Strategy

3.2.1 Data Source

Vaccination Coverage

The vaccination coverage data are retrieved from the CDC COVID-19 Vaccine Tracker.[38] Weekly vaccine rates for each county between January and December 2021 are used.[39] Table C1 summarizes the vaccination rate for 52 weeks over the study period and Figure C.1 shows the percentage of each state's population aged 18 and older who are fully vaccinated in mid-May, early October, and mid-December, individually. As expected, more people get vaccinated over time, but there is a wide variation in reported vaccination rates by county across the United States.

[38] The link to CDC COVID-19 Vaccinations in the United States can be found here: COVID-19 Vaccination Tracker.

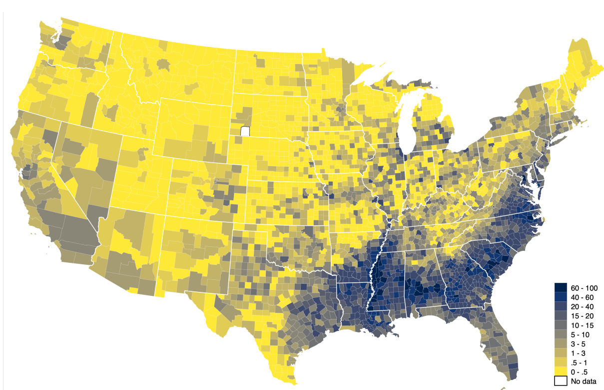
[39] Counties in Alaska and Hawaii are excluded from the analysis due to missing values. Additionally, CDC vaccine coverage data does not include vaccine coverage rates for some counties in certain weeks. Therefore, our baseline analysis is based on an unbalanced panel.

Racial Composition and County-level Variables

The fractions of populations of different races aged 18 years and older at the county level are computed based on data from the 2010 Census.[40] Figure 3.1 shows the distribution of the Black population aged 18 years and older at the county level where darker-shaded areas represent a higher Black population share. The pronounced clustering of the Black population in the Southern United States is readily evident. The Southern region exhibits a distinctive pattern of Black population distribution, which has deep-rooted historical underpinnings and continues to shape the demographic landscape.

[40] We classify people into non-Hispanic White, non-Hispanic Black, Hispanic/Latino, non-Hispanic Asian, and others. In addition, we calculate the racial composition by age groups and gender.

Figure 3.1: Share of Black population, 2010



Data Source: U.S. Census Bureau. 2010 Census Redistricting Data.
Note: Darker shaded areas represent counties with higher Black population shares.

Data regarding county-level demographic and socioeconomic information is obtained from 2008-2012 American Community Survey (ACS) 5-year estimates. Data on county-level unemployment rates are obtained from the U.S. Bureau of Labor Statistics. Additionally, we collect information about primary care physicians and nurses based on data from the 2019 Area Health Resources Files. The U.S. Census Bureau's County Business Patterns include information on pharmacy locations. We also utilize data from Bazzi et al. (2020), which quantifies the extent of time each county spent on the frontier during westward expansion in the 18th and 19th centuries.

Vaccine Hesitancy

Based on the Household Pulse Survey (HPS), a rapid-response survey of persons aged 18 and up conducted by the U.S. Census Bureau in collaboration with seven other federal statistical agencies, we calculate vaccination hesitancy for each county. The study collects detailed information about household experiences during the COVID-19 pandemic.

3.2.2 Summary Statistics

Table 3.1: Balance table

	Close to Tuskegee with high Black share	Far from Tuskegee with low Black share	*p*-value
Panel A: Baseline			
% Black aged 18 years and older	0.268	0.002	0.000
% White aged 18 years and older	0.655	0.885	0.000
% Hispanic aged 18 years and older	0.049	0.069	0.001
% Asian aged 18 years and older	0.011	0.004	0.000
% Population (25+) with no high school diploma	0.196	0.122	0.000
% Population (25+) with high school diploma	0.346	0.352	0.082
% Population (25+) with some college	0.276	0.331	0.000
% Population (25+) with college degree and above	0.182	0.194	0.001
Unemployment rate (2020)	7.168	5.571	0.000
Panel B: Supply side			
# of pharmacies per 100k population	19.42	7.71	0.000
# of primary care physicians per 100k population	43.07	43.16	0.963
# of physician assistants with NPI per 100k population	23.48	37.90	0.000
# of advanced practice registered nurses per 100k population	93.59	64.98	0.000
# of nurse practitioners per 100k population	78.25	54.37	0.000
Observations	787	718	1505

Note: A low (or high) share county is one where the proportion of Black individuals is in the bottom 30^{th} (or top 30^{th}) percentiles of the distribution. If the distance is in the bottom 50^{th} percentile of the distribution, a county is considered "close"; otherwise, it is considered a "far" county. The *p*-value is for the test of the null hypothesis that the means across the samples are the same.

Table 3.1 compares counties that are close to Tuskegee and have a high percentage of the Black population with those counties that are far from Tuskegee and have a low percentage of the Black population. A county is defined as having a low (or high) share of Black individuals if the proportion of Black individuals falls within the bottom 30^{th} (or top 30^{th}) percentile of the distribution. Furthermore, if a county's distance to Tuskegee falls within the bottom 50^{th} percentile of the distribution, it is classified as "close"; otherwise, it is considered a "far" county. The results suggest that these two groups of counties differ significantly in almost all characteristics, including the demographics of residents, the educational attainment of residents, local economic and labor market conditions, and access to medical resources. It is not surprising that counties with a lower proportion of Black residents and located far from Tuskegee tend to be more wealthy, have a more educated population, and have a low unemployment rate.

3.2.3 Empirical Specifications

The main estimation in our study is to explore the dynamic impact of Black population share, the impact of the Tuskegee Study, and their interaction on a county's vaccination rate, using the following regression model:

$$\begin{aligned} Y_{ct} = \alpha + \beta_1\, PB_c \times Dist_c \times Week_t + \beta_2\, PB_c \times Week_t \\ + \beta_3\, Dist_c \times Week_t + \gamma X_{ct} + \tau_c + \tau_t + \epsilon_{ct} \end{aligned} \tag{3.1}$$

where Y_{ct} represents the vaccination rate of people at least 18 years old for county c at week t, and PB_c measures the share of Blacks in county c. The exposure to the Tuskegee Study is assessed by $Dist_c$, the geographic distance between county c and Macon, Alabama, where the Tuskegee Study was conducted. As described by Tabellini (2010), "distance between agents could refer to geography, but also to social or economic dimensions such as religion, ethnicity, and class." As a result, being closer to Macon County meant being more exposed to the Tuskegee Study revelation. The interaction term, $PB_c \times Dist_c \times Week_t$, which demonstrates the heterogeneous effect of Tuskegee exposure to communities with varying percentages of Black people, is of particular relevance.

The term X_{ct} includes a linear trend of the share of the White population, a linear trend of the share of Hispanics, a linear trend of the share of high school graduates, and a linear trend of the unemployment rate. These allow us to control for a linear trend of county specific factors affecting its population vaccination rate. In alternative specifications, we also control for the non-linear trend and the results are barely changed.

County fixed effect τ_c controls for time-invariant county-specific factors, such as the stock of medical facilities, the number of primary care physicians, and the number of drug stores; while week fixed effect τ_t controls for a common economic/social impact which affects all counties at the same time. In the analyses, all regressions are weighted by the total population age 18 and older at the county level, and the standard errors are clustered by state.

3.3 Empirical Results

3.3.1 Baseline Results

We begin our study by showing the weekly trend of vaccination rate for those who have received at least one dose of vaccine in Figure 3.2. In Panel (a), counties are categorized into two groups based on the proportion of Black residents, while in Panel (b), counties are classified according to their proximity to Tuskegee, similar to the grouping shown in Table 3.1. The bottom panel depicts the interplay of the Black population and distance. Only the vaccination patterns for these two groups are displayed in Panel (c), where the Black line represents the former and the red line, respectively, represents the latter. The pattern is striking. Counties close to Tuskegee with significant Black populations initially had lower vaccination rates but gradually caught up to the latter.[41]

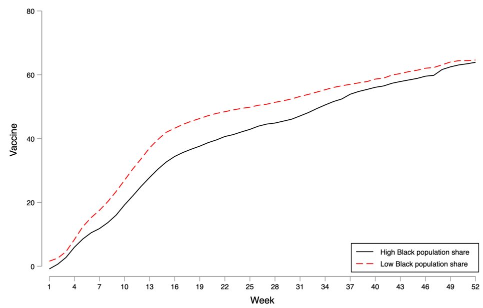
It is essential to highlight that the key specification in Equation 3.1 focuses on comparing counties in a way that goes beyond just looking at differences between places with a high or low Black population or the distinction between those in proximity to or far from Tuskegee. The county and time fixed effects have addressed these comparisons. Our analysis is instead focused on examining the time variation in vaccination rates between counties with high percentages of the Black population and those with low percentages, with a particular emphasis on the influence of the Tuskegee Study.

The first two columns of Table 3.2 report our baseline results. For both the fully vaccinated rate and the rate for at least one vaccine dose, the vaccination rate increases more slowly in counties with a higher Black population. However, the growth rate correlates with the distance to Tuskegee. More specifically, for two counties with the same proportion of Black people, the one further away from Tuskegee has a higher growth rate. In other words, if the proportion of Black people in the population rises from 1^{st} to 99^{th} percentiles, the vaccination rate falls by 67.38 percentage; however, the distance to Tuskegee is associated with a gradual reduction of the difference.[42]

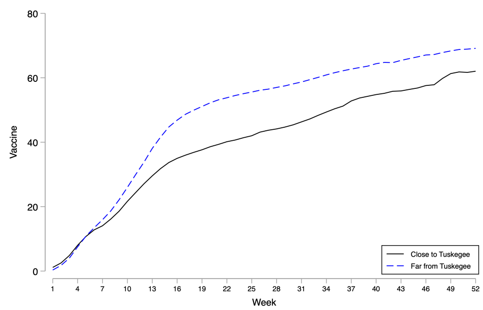
[41] We have also depicted the trend for each of the six groups that were present in every possible combination of the categories between Panel (a) and Panel (b). The pattern suggests that vaccination uptake is inversely related to the distance from Tuskegee. The higher the vaccination rate, the further a county is from Tuskegee, regardless of its racial composition. Furthermore, within the "near" and "far" distance groups, counties with a higher proportion of Black people had lower vaccination rates at first but eventually caught up with others. The results are available upon request.

[42] Black people make up 0.00001 percent of the population in the bottom 1^{st} percentile and 56.15 percent of the population in the top 99^{th} percentile.

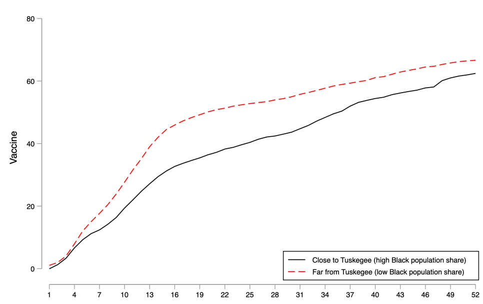
Figure 3.2: Vaccination coverage between week 1 and week 52, 2021



(a) Share of Black population



(b) Distance to Tuskegee



(c) Interaction of distance and Black population share

Note: The vaccination rate is calculated as the percentage of the population over age 18 who received at least one dose. A low (or high) share county is defined as one where the proportion of Black individuals is in the bottom or top 30th percentile, respectively. Counties are classified as "close" if their distance to Tuskegee is below the median, and "far" otherwise.

The difference is narrowed by 16 percentage for every 100 miles traveled. Furthermore, we conduct two alternative estimations to account for the within-county variation over time, as presented in Table 3.2. In the first approach, we incorporate county-by-month fixed effects, enabling us to control for unobserved factors that are specific to each county and month but remain constant over time. Additionally, we introduce a county-by-week linear trend, which captures the linear changes in vaccination coverage at the county level over time. The results from both sets of regressions align with our primary specification, affirming the consistency of our findings.

Table 3.2: Impact of Tuskegee Study on vaccination rate

	Baseline results		County-month fixed effect		County × week linear trend	
	At least one dose	Fully vaccinated	At least one dose	Fully vaccinated	At least one dose	Fully vaccinated
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Black × distance × week	0.160*	0.164**	0.158*	0.151**	0.204***	0.206***
	(0.087)	(0.077)	(0.085)	(0.067)	(0.069)	(0.055)
Share of Black × week	-1.200***	-1.442***	-0.979***	-1.386***	-0.576	-0.825***
	(0.383)	(0.398)	(0.354)	(0.342)	(0.406)	(0.301)
Distance × week	-0.006	-0.005	-0.001	-0.003	0.009**	0.009**
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	134,259	143,666	133,971	143,533	133,971	143,533
R-squared	0.966	0.970	0.999	0.999	0.999	0.999
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes	No	No
Share of Hispanic × week	Yes	Yes	Yes	Yes	No	No
Share of HS grads and above × week	Yes	Yes	Yes	Yes	No	No
County unemployment rate × week	Yes	Yes	Yes	Yes	No	No
County-month FE	No	No	Yes	Yes	Yes	Yes
County × week linear trend	No	No	No	No	Yes	Yes

Note: The vaccination rate is calculated for the percentage of the population over 18 years old who received at least one vaccination. The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * p < 0.01; ** p < 0.05; *** p < 0.01.

The implications of our estimated results are twofold. To begin with, it improves our knowledge of geographical heterogeneity in vaccination rates.[43] Education, income and wealth gap (McLaughlin et al., 2022), and access to health facilities (Murthy et al., 2021), are among the most important factors, according to recent studies. Our findings reveal that the Tuskegee effect is significant and sizable, implying that the difference in COVID-19 vaccination rates between counties is suggested to be more of a function of culture than of traditional economic factors. Second, our findings enable us to better understand the vaccination disparity between Black and White people. According to a recent Kaiser Family Foundation (KFF) report, Blacks and Hispanics have been less likely than their White and Asian counterparts to receive a COVID-19 vaccine throughout the rollout. However, these disparities have narrowed over time

[43] Up to May 2022, at least 257 million people or 78% of the population have received at least one dose. While vaccination coverage has increased, it remains uneven across the country. In five of the six New England states, for example, more than 60% of residents are at least partially vaccinated. In the South, however, Mississippi, Alabama, Arkansas, Georgia, Louisiana, and Tennessee have the lowest rates of residents who have had at least one vaccination in the country.

and reversed for Hispanics.[44] Our findings also lend credence to the notion that the vaccination rate gap between communities with varying percentages of Black populations, particularly those located far from Tuskegee, becomes smaller. Consider two hypothetical communities, one entirely made up of only White people and the other entirely made up of Black people. The distance needed to close the racial disparity between these two counties is 750 miles, assuming that vaccination rates grow in a linear fashion.[45]

3.3.2 Results by Cohorts and Gender

Given that the subjects in the Tuskegee Study were Black men aged 25 and above in 1932, the influence of this study is expected to be more pronounced within the older Black male demographic. We first ran separate regressions for age groups, ranging from 18 to 24, 25 to 34, 35 to 44, 45 to 54, 55 to 64, and 65 and older. We then run separate regressions for men and women in various age groups.

The results are displayed in Table 3.3. We have three main findings. First, counties with a higher share of the young Black population have a lower vaccination rate. This is consistent with the study by Baack et al. (2021), which was published in the CDC's Morbidity and Mortality Weekly Report and demonstrates that non-Hispanic Black adults have the lowest reported vaccination coverage and intent to get vaccinated, along with those with less education, no health insurance, and lower family incomes. Second, when older Black persons are included in calculating the proportion of Black people in the population, the vaccination gap between high and low-density areas narrow.

As shown in column (1) of Table 3.3, the Tuskegee Study's effect is smaller for younger people, implying that a highly Black-concentrated county must converge to a county with a low Black share if it is located 894 miles away from Tuskegee. In comparison, column (6) indicates that convergence occurs when a county with a larger senior Black population is roughly 451 miles away from Tuskegee.

Third, the vaccination rate is lower in counties with more Black men, and the convergence distance is likewise greater than in counties with more Black women. There are a couple of possible explanations, including Black males tend to have lower educational attainment and worse market outcomes than Black females. Furthermore, males have a more elastic need for health services (Cubbin et al., 2000) and are thus more readily deterred from visiting physicians and pharmaceutical websites.

[44] The link to the full description of the KFF report is Latest Data on COVID-19 Vaccinations by Race/Ethnicity.

[45] The first-order derivative with regard to the share of Black people yields the distance between two counties with the same percentage of Black people. Therefore, it requires $((1.2/0.16)\times 100)$ miles to close the gap. In terms of magnitude, there are around 1205 counties with a distance greater than 750 miles, making up 38.8% of all counties (3,107 counties).

Table 3.3: Tuskegee impact by gender and age

	18 - 24 (1)	25 - 34 (2)	35 - 44 (3)	45 - 54 (4)	55 - 64 (5)	65 and older (6)
Panel A: Full population						
Share of Black× distance × week	0.923*	0.846**	0.918**	0.833**	1.079**	1.131**
	(0.502)	(0.390)	(0.373)	(0.387)	(0.504)	(0.471)
Share of Black × week	-8.261***	-7.796***	-8.794***	-6.785***	-7.455***	-5.108**
	(1.913)	(1.995)	(2.352)	(2.028)	(2.370)	(2.464)
Distance × week	-0.004	-0.006	-0.007	-0.004	-0.002	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	143,666	143,666	143,666	143,666	143,666	143,666
R-squared	0.970	0.971	0.971	0.970	0.970	0.970
Panel B: Males						
Share of Black × distance × week	1.817*	1.645*	1.855**	1.712*	2.298*	2.703**
	(1.046)	(0.872)	(0.850)	(0.866)	(1.175)	(1.266)
Share of Black × week	-17.403***	-15.192***	-18.547***	-15.008***	-16.153***	-13.122*
	(4.073)	(4.057)	(5.200)	(4.524)	(5.370)	(6.689)
Distance × week	-0.004	-0.005	-0.007	-0.005	-0.002	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Observations	143,666	143,666	143,666	143,666	143,666	143,666
R-squared	0.970	0.970	0.971	0.970	0.970	0.970
Panel C: Females						
Share of Black × distance × week	1.854*	1.668**	1.755***	1.572**	2.001**	1.910**
	(0.934)	(0.671)	(0.653)	(0.683)	(0.874)	(0.743)
Share of Black × week	-14.873***	-13.736***	-15.227***	-11.906***	-13.668***	-8.383**
	(3.496)	(3.563)	(4.026)	(3.561)	(4.194)	(3.862)
Distance × week	-0.003	-0.005	-0.006	-0.003	-0.002	0.002
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Observations	143,666	143,666	143,666	143,666	143,666	143,666
R-squared	0.970	0.971	0.971	0.970	0.970	0.970
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes	Yes	Yes
Share of Hispanic × week	Yes	Yes	Yes	Yes	Yes	Yes
Share of HS grads and above × week	Yes	Yes	Yes	Yes	Yes	Yes
County unemployment rate × week	Yes	Yes	Yes	Yes	Yes	Yes

Note: Each column presents the coefficient estimates from a separate regression. The outcome is the percentage of adults aged 18 years and older fully vaccinated. For each column, the Black share is calculated by dividing the number of Black individuals in a specific age group by the total population of the county. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * $p < 0.01$; ** $p < 0.05$; *** $p < 0.01$.

3.4 Robustness Checks

3.4.1 Non-linearity and Racial Residential Segregation

Because the vaccination data includes all races, a few assumptions are necessary in order to determine that changes in the full vaccination rate are predominantly driven by changes in the Black vaccination rate. First, the rate of variation in Whites and other races should have remained constant or followed a consistent pattern over time. This problem is addressed by controlling the share of other races and their interactions with time. We also allow non-linear growth rates for other races. The results for the nonlinear trend are shown in Table C2. The point estimates remain nearly unchanged, and our conclusions are similarly corroborated by the estimations.

Additionally, there should be no cross-group spillover, meaning that Blacks' vaccination behavior is unrelated to Whites' in the same area. To determine the size of the spillover effect, we divide counties into high- and low-racially segregated residential regions, assuming that high-racially segregated areas have less interaction across races. We then examine the results of separate regressions for high- and low-segregated areas. If a county is on the top (bottom) 25^{th} of the distribution, it is considered high (low) segregated.[46] The findings of the racial segregation classification are presented in Table C3.[47] The results for the least racially segregated regions are displayed in columns (1) and (4), whereas the results for the most segregated regions are displayed in columns (2) and (5).

The minimal differences observed among regions with diverse levels of racial segregation provide strong assurance that our findings remain unaffected by any potential racial spillover effects. A more rigorous test indicates that the top and bottom racially divided regions do not statistically differ from one another.

[46] The information of racial segregation can be found here: residential segregation – Black/White. Index of dissimilarity where higher values indicate greater residential segregation between Black and White county residents.

[47] Because around 800 counties do not have a segregation index, we discard them and re-estimate the regression using Equation 3.1. After deleting counties with missing segregation index, the point estimates are extremely near to the baseline values, supporting the sample's representation.

3.4.2 Tuskegee Study's Impact on Other Races

One possibility that could undermine our causal relationship between the Tuskegee Study and Black vaccination practice is that public exposure to the Tuskegee Study could affect people of all races. We re-estimate the regression in Equation 3.1 by substituting the proportion of the Black population with the population of other racial groups. This is done to investigate whether the impact of the Tuskegee Study is similar for individuals of different racial backgrounds. The theory behind this test is that the Tuskegee study should only impact the COVID-19 vaccination among African Americans while having little or no effect on people of other races.

The results are presented in Table 3.4. This table is split into two parts: the left segment displays results for individuals from various racial backgrounds, while the right segment showcases findings for individuals of Black ethnicity. Furthermore, the table has been divided into three panels, each reporting results specific to Whites, Hispanics, and Asians.

There are several noteworthy discoveries to highlight. To start, as expected, the coefficients for the White community are both statistically insignificant and of a relatively small magnitude. This implies that vaccination patterns among White individuals appear to remain relatively consistent across different levels of White population density, and the influence of the Tuskegee Study does not seem to be prominent. Second, Hispanics and Asians are concentrated in specific geographic areas that differ significantly from the geographical distribution of Black individuals. In Figure C.2, we present the percentages of Hispanics and Asians along with based on 2010 Census Data for each county. Black populations are primarily concentrated in the Southern regions; Hispanics are primarily located in the southwestern states, and Asians are concentrated on the eastern and western coasts. To eliminate the possibility that the outcomes are influenced by geographic separation or the choice of residence for Hispanic and Asian populations, we conduct our analysis by excluding states with the highest proportion of Hispanics (Asians). The findings show no significant impact on the vaccination behavior of Hispanic and Asian communities, reinforcing the notion that the Tuskegee Study has minimal influence on these groups.

3.4.3 Alternative Measurement of Tuskegee Exposure

Our major estimate of Tuskegee Study exposure is based on a county's geographic distance from Macon County, assuming that people who lived closer to the study's victims were more significantly influenced by the news coverage. We are still curious to see how our results hold up against a different measure of Tuskegee Study exposure.

To develop an alternative metric, we carefully examine regional variations in any news or report containing the Tuskegee Study in the newspapers circulated between 1972 and 1973 and create an index that captures varied levels of Tuskegee news exposure across areas. In particular, we search all pages of all papers in the database for mentions of "Tuskegee Syphilis Study" or 'Tuskegee Syphilis Experiment," resulting in about 27,012,238 pages of newspapers from about 2,000 different publications.[48] Albright et al. (2021) constructed a similar metric to analyze the gradient impact of the Tulsa Massacre on Black communities across the United States.

[48] The historical distribution of news was retrieved here: Newpaper. The source does not offer comprehensive or representative coverage of all newspapers, but the selection does give an idea of how quickly the news of the Tuskegee Study traveled across the nation.

Table 3.4: Robustness checks (other races)

	White/Hispanic/Asian Americans		African Americans	
	At least one dose	Fully vaccinated	At least one dose	Fully vaccinated
	(1)	(2)	(3)	(4)
Panel A: The Tuskegee impact on Whites				
Population share by race × distance × week	-0.004	-0.005	0.160*	0.164**
	(0.014)	(0.016)	(0.087)	(0.077)
Population share by race × week	-0.440	-0.377	-1.200***	-1.442***
	(0.305)	(0.293)	(0.383)	(0.398)
Distance × week	0.007	0.010	-0.006	-0.005
	(0.012)	(0.013)	(0.004)	(0.005)
Observations	134,259	143,666	134,259	143,666
R-squared	0.965	0.969	0.966	0.970
County FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Share of White × week	No	No	Yes	Yes
Share of Black × week	Yes	Yes	No	No
Share of Hispanic × week	Yes	Yes	Yes	Yes
Share of HS grads and above × week	Yes	Yes	Yes	Yes
County unemployment rate × week	Yes	Yes	Yes	Yes
Panel B: The Tuskegee impact on Hispanics (exclude Arizona, California, Colorado, New Mexico, and Texas)				
Population share by race × distance × week	-0.030	-0.059	0.200**	0.197***
	(0.063)	(0.072)	(0.085)	(0.069)
Population share by race × week	0.787	0.941	-1.278***	-1.409***
	(0.550)	(0.634)	(0.426)	(0.511)
Distance × week	0.010	0.016*	0.000	0.003
	(0.008)	(0.009)	(0.004)	(0.004)
Observations	126,132	134,382	126,132	134,382
R-squared	0.963	0.968	0.964	0.969
County FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes
Share of Black × week	Yes	Yes	No	No
Share of Hispanic × week	No	No	Yes	Yes
Share of HS grads and above × week	Yes	Yes	Yes	Yes
County unemployment rate × week	Yes	Yes	Yes	Yes
Panel C: The Tuskegee impact on Asians (exclude California, Connecticut, Washington, D.C, Maryland, and New Jersey)				
Population share by race × distance × week	0.029	0.019	0.207**	0.214***
	(0.094)	(0.102)	(0.087)	(0.071)
Population share by race × week	0.697	1.275	-1.276***	-1.493***
	(1.317)	(1.537)	(0.447)	(0.522)
Distance × week	0.001	0.004	-0.003	-0.001
	(0.006)	(0.006)	(0.005)	(0.005)
Observations	129,527	138,934	129,527	138,934
R-squared	0.962	0.967	0.964	0.969
County FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes
Share of Black × week	Yes	Yes	No	No
Share of Hispanic × week	No	No	Yes	Yes
Share of HS grads and above × week	Yes	Yes	Yes	Yes
County unemployment rate × week	Yes	Yes	Yes	Yes

Note: The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. Hispanic and Asian Americans are highly concentrated in a few states in the U.S. In Panel B, we remove the top 5 most concentrated states for Hispanics and in Panel C, we remove the top 5 most concentrated states for Asians. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * $p < 0.01$; ** $p < 0.05$; *** $p < 0.01$.

Since information regarding news coverage is obtained at the state level, we adjusted it to the county level by multiplying the percentage of state news that featured the Tuskegee Study between 1970 and 1972 by the proportion of the county population (as per the 1970 Census) among the total articles published. Greater newspaper coverage would mean that the disclosure of the Tuskegee Study would have been more extensively and clearly communicated among the public. In particular, our measure is constructed based on the following formula.

$$TK_c = \frac{Pop_{cs,1970}}{Pop_{s,1970}} \times News_{s,1972-73}, \tag{3.2}$$

where $News_{s,1972-73}$ is the ratio obtained by dividing the number of news articles about Tuskegee that were published in the state of s between 1972 and 1973 and contained the keywords "Tuskegee Syphilis Study" or "Tuskegee Syphilis Experiment" by the total number of articles published in that state.[49] To map the state exposure to each county within a state, we multiply it by $\frac{Pop_{cs,1970}}{Pop_{s,1970}}$, the share of county c's population in the state s.[50] As shown in Figure C.3, there is vast variation in exposure to the Tuskegee study across regions.

We replace the distance to Tuskegee with the news exposure in the main specification of Equation 3.1 and report the results in Panel A of Table 3.5. It is worth noting that the newspapers' political inclination appeared to be in line with their readers' political ideologies and preferences (Gentzkow & Shapiro, 2006; Iyengar & Hahn, 2009). Despite the fact that the county fixed effect accounts for such a time-invariant influence, political ideology and preferences may evolve at different rates throughout time. To capture time-varying trends, we interact with a county's majority political vote with a time dummy.[51]

As demonstrated in Panel A of Table 3.5, when a county is exposed to more Tuskegee news, the vaccination rate is lower. Specifically, a one percentage point increase in the presence of Tuskegee news relative to all news reported in a county results in an 8.83% reduction in the vaccination rate for at least one dose and a 9.31% reduction for those fully vaccinated. However, exposure to Tuskegee news does not appear to have differing effects on counties with varying proportions of Black residents. The newspaper measure is based on the assumption that Black residents and individuals of other racial backgrounds of a county have equal access to newspapers and are, therefore, equally exposed to shocks when the Tuskegee Study is made public. While it is true that newspapers have limited reach among Blacks, particularly among those who are poor, we lack nationwide data to compare the readership between Blacks and people of other races during 1970. We admit that newspaper coverage may have exaggerated the actual influence of the Tuskegee Study.

[49] The results have barely altered despite the addition of other ratios that were created by gradually extending the 1972–1980 era.

[50] The data source for calculating such population share is from County Intercensal 1970-1972.

[51] We divide the votes for the Democratic (Republican) party by the total votes in that county to get county-level data on support for the Democratic (Republican) party in the 2020 presidential election from the MIT Election Data and Science Lab. Figure C.4 shows the county level and vote share results of the 2020 U.S. Presidential Election. The darker the blue, the more Democratic a county voted, and the darker the red, the more Republican a county voted. If a county receives more than half of the Democratic vote, it is classified as Democratic; otherwise, it is coded as Republican.

Table 3.5: Robustness checks for other concerns

	At least one dose (1)	Fully vaccinated (2)
Panel A: Alternative measurement (newspaper exposure)		
Share of Black × Tuskegee news × week	41.983	42.415
	(54.367)	(49.439)
Share of Black × week	-0.854**	-1.244***
	(0.382)	(0.362)
Tuskegee news × week	-8.883***	-9.308***
	(1.319)	(2.421)
Observations	133,968	143,345
R-squared	0.965	0.969
Panel B: South region		
Share of Black × distance × week	0.190**	0.230**
	(0.089)	(0.082)
Share of Black × week	-0.161	-0.515
	(0.582)	(0.737)
Distance × week	0.023	0.011
	(0.022)	(0.029)
Observations	52,950	55,338
R-squared	0.963	0.964
Panel C: Tulsa impact		
Share of Black × distance × week	0.060	0.050
	(0.038)	(0.039)
Share of Black × week	-0.756	-0.963
	(0.487)	(0.611)
Distance × week	0.019***	0.020***
	(0.005)	(0.005)
Observations	134,259	143,666
R-squared	0.967	0.971
County FE	Yes	Yes
Week FE	Yes	Yes
Share of White × week	Yes	Yes
Share of Hispanic × week	Yes	Yes
Share of HS grads and above × week	Yes	Yes
County unemployment rate × week	Yes	Yes

Note: The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * $p < 0.01$; ** $p < 0.05$; *** $p < 0.01$.

3.4.4 Is It Exclusively in the South?

One could argue that our findings are predominantly influenced by unique characteristics of the Black communities in the Southern region, such as their tendency to exhibit lower trust in modern medicine and their reluctance towards receiving vaccinations. In such a scenario, we would expect to observe a statistically insignificant coefficient, as the vaccination rate should not vary based on the proximity to Tuskegee if these specific factors are the primary drivers. To investigate it, we restrict our analysis to counties in the Southern region. As shown in Panel B of Table 3.5, our major findings remain consistent and robust. Moreover, the influence of the distance to Tuskegee, conditional on the proportion of the Black population, becomes even more pronounced.

3.4.5 Impact of Tulsa Massacre

We perform another robustness test and investigate the impact of another historical event, the Tulsa Massacre, that targeted African Americans. Both the Tuskegee Study and the Tulsa Massacre were two significant events in the history of the Black community in the United States. Although each event had distinct repercussions on the community, they had varying impacts on health and trust. In this study, we focused on the Tulsa Massacre due to several reasons.

Firstly, the Tulsa Massacre took place in 1921 within Tulsa, Oklahoma's Greenwood District, often referred to as "Black Wall Street." It involved a violent assault perpetrated by a White mob on the thriving Black community, destroying over 1,200 homes and the loss of hundreds of Black lives. The consequences of the Tulsa Massacre were profoundly devastating for the Black community, leading to the destruction of property, businesses, and the loss of lives. The trauma inflicted by this event lingered for generations (Albright et al., 2021). However, it is worth noting that the economic aftermath of the Tulsa Massacre overshadowed its impact on public health trust, indicating that its influence on Black vaccination decision-making might be relatively limited.

Furthermore, it is important to highlight that the perpetrators of the Tulsa Massacre were predominantly White mobs, whereas the Tuskegee Study was conducted by various government entities such as the U.S. Public Health Service, a division of the Department of Health and Human Services (HHS), the National Institutes of Health (NIH), and the Centers for Disease Control and Prevention (CDC). These very agencies are currently involved in the COVID-19 vaccination program. Consequently, considering the historical trauma inflicted by the Tuskegee Study, there is a possibility that the Black community may harbor distrust towards these government agencies, which could potentially impact their willingness to receive the COVID-19 vaccine. By contrast, the Tulsa Massacre may not have had a similar effect, making it a plausible comparison.

To do so, we modify the estimation function in Equation 3.1 by substituting the distance to Tulsa, Oklahoma, for the distance to Macon, Alabama, as in our baseline and maintaining the same sets of control variables. The results in Panel C of Table 3.5 reveal that distance to Tulsa has no impact on reducing or exacerbating the vaccination gap between high and low Black population density.

3.4.6 Permutation Tests

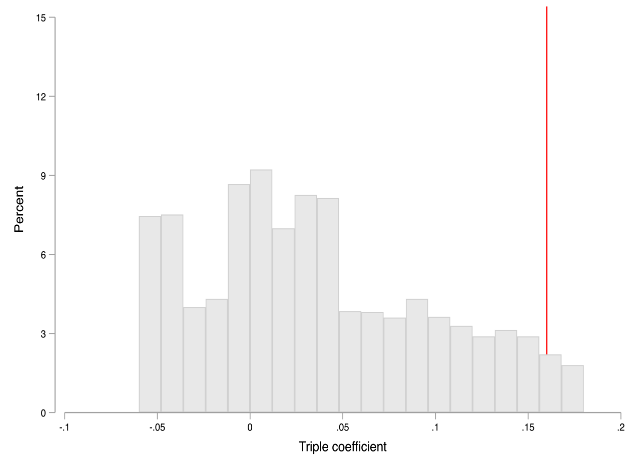
We further conduct a set of placebo regressions, substituting the baseline proximity measure (proximity to Macon County, Alabama) with the proximity to the geographic centroid of all counties (except Macon), to test if the baseline results are unique. We perform these permutation tests to see if the results hold when we examine the distance between a county and a location that has been artificially altered—the geographic centroid of each county. As noted in Alsan and Wanamaker (2018), these regressions serve as control experiments to see if we find the same vaccination effects as a function of the gradient to other U.S. locations. Another advantage of doing this is to avoid bias in selecting a particular group of locations that might be responsible for our results. In each of these tests, the distribution of the estimated values is shown in Figure 3.3, and the vertical line is the estimated triple coefficient from Table 3.2. For at least one dose (full immunization), the main estimate (proximity to Macon County) is higher than 96.8% (96.7%) of the placebo estimates.

3.4.7 Using Selection on Observables to Assess the Bias from Unobservables

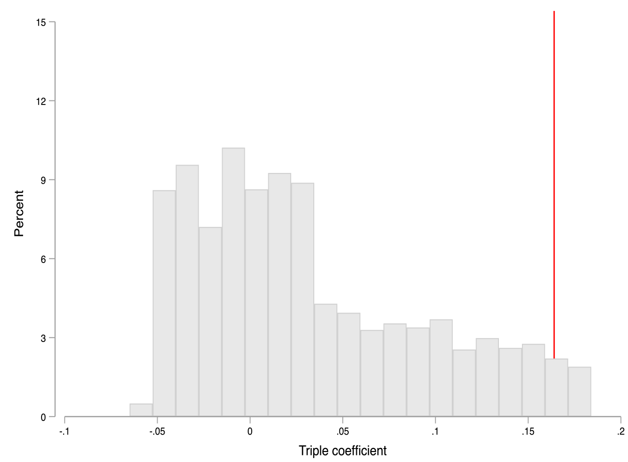
Despite our attempts to control for a variety of observable factors, the estimates may still be biased by unobservable factors correlated with a location choice and subsequent vaccination behavior. In this section, we assess the likelihood that the estimates are biased by unobservables. The strategy that we use exploits the insights from (Altonji et al., 2005; Nunn & Wantchekon, 2011) to evaluate how much greater the influence of unobservable factors would need to be about observable factors to fully account for the relationship between the distance to Tuskegee, the percentage of the Black population, and the vaccination rate.

We take into account two sets of restricted covariates: one with no controls and the other with a minimal set of individual controls that include the percent-

Figure 3.3: Permutation tests



(a) At least one dose



(b) Fully vaccinated

Note: This figure shows the distribution of placebo β_1 coefficients from permutation tests. Each estimate is based on assigning "treatment" distance using a random county and re-estimating Equation (1). Panel (a) reports results for the probability of receiving at least one dose; Panel (b) reports results for being fully vaccinated. The vertical line represents the baseline estimate of β_1 using the true distance to Macon County, as reported in Table 3.2.

age of the White population and the share of the Hispanic population in 2010. We also consider several sets of full controls, including the initial set of controls from Equation 3.1, a second set that includes the nonlinear trend for the share of Whites and Hispanics, and a third set that further includes additional influencing factors for vaccine supply. As displayed in Table 3.6, our findings indicate that the impact of unobservable factors would need to be at least 20.7 times greater than observable factors for at least one dose, and 18.2 times greater for full vaccination, to fully explain our estimations. It is unlikely that unobserved heterogeneities alone can completely account for our findings.

Table 3.6: Using selection on observables to assess the bias from unobservables

		At least one dose		Fully vaccinated	
Controls in the restricted set	Controls in the full set	Ratio	Standard error	Ratio	Standard error
None	Full set of controls from equation (1)	21.861	2.165	21.097	1.898
None	Full set of controls from equation (1), and non-linear trend	20.704	1.989	20.677	1.865
Share of White and share of Hispanic	Full set of controls from equation (1)	40.151	7.048	29.275	3.677
Share of White and share of Hispanic	Full set of controls from equation (1) and non-linear trend	37.043	6.152	28.553	3.588
Share of White and share of Hispanic	Full set of controls from equation (1), non-linear trend, and supply-side factors	22.603	2.675	18.219	1.684

Note: In the table, each cell represents a ratio based on the coefficients from two regressions for share of Black $\times$ distance $\times$ week. One includes a "restricted set" of covariates for the control variables and its coefficient is called β^R. The other regression includes a "full set" of controls and its coefficient is called β^F. The sample sizes are the same in both regressions and county and week fixed effects are included. Here is how we calculate the ratio: $\beta^F/(\beta^R - \beta^F)$. In Table 3.2, the full set of controls from Equation (1) is described.

3.4.8 Testing the Migration Impact

We recognize that our arguments are based on the assumption of a static model, where both the percentage of the Black population and exposure to the Tuskegee Study are time-invariant. As a result, a potential issue arises regarding changes in demographic composition as a result of migration. Nevertheless, we have conducted two tests to mitigate such concerns. We first perform a test to determine whether population migration, which alters the demographic structure and composition of counties over time, could potentially influence our findings. This is because we calculate the proportions of different racial groups aged 18 years and above at the county level using data from the 2010 Census as the foundation for our baseline estimation. To address this issue, we substitute the percentage of the Black population in 2010 with the percentage of the Black population between 1990 and 2005 and report the results in Table C4. We find that the results remain consistent and unaffected by this adjustment.

Next, we examine migration patterns within individual states and between different states by analyzing the 2021 American Community Survey (ACS) Data. We calculate the proportion of individuals relocating from different counties for all counties. We then identify the counties that fall within the top 5 and 10 percentiles of this distribution.[52] It is worth mentioning that it is not feasible for us to determine the migration direction or whether a county experiences a net influx or exodus of people. To ensure that our findings are not primarily influenced by migration, we have excluded counties with the highest percentile of residents relocating from different counties. The results are displayed in Table C5. Compared to the baseline findings, excluding counties that had the highest migration rates before the pandemic had minimal impact on our results, and the point estimates are very similar to those of the baseline.

[52] A mover, in this context, refers to individuals who have changed their place of residence within the past year. Figure C.5 illustrates these migration patterns.

3.5 Mechanism

3.5.1 Tuskegee Impact on Blacks' Vaccine Hesitancy

Relative to other races, African American men have worse health outcomes in the United States (Do et al., 2008; Hayward & Heron, 1999; Williams & Mohammed, 2009) and the COVID-19 pandemic has only made things worse.[53] But socioeconomic factors, such as lower labor income and education attainment, weaker labor market attachment, and lack of health insurance, do not fully account for these gaps (Brunello et al., 2016; Cutler et al., 2008) and a few empirical studies indicate that mistrust of healthcare institutions with historical roots also contributes to these inequities (Alsan & Wanamaker, 2018).

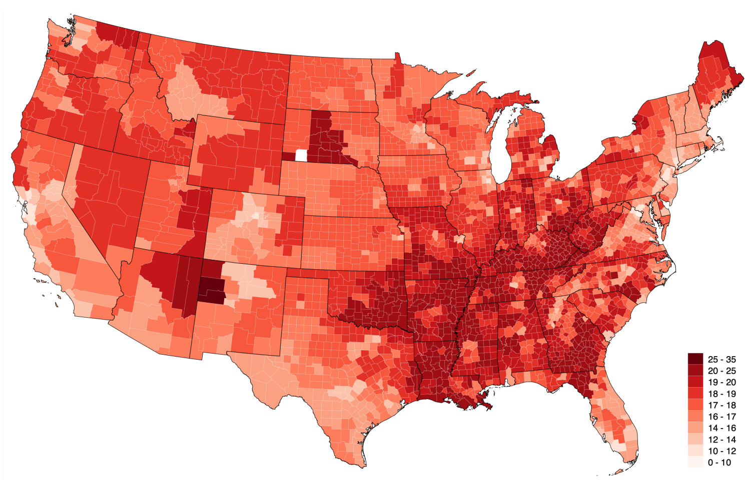
Alsan and Wanamaker (2018) discuss the disclosure of the Tuskegee Study fuels mistrust in modern medicine and public health among African Americans, especially males. As noted in the theory of social identification (Ashforth & Mael, 1989; Bhattacharya et al., 1995) and studies of empathy demonstrate that individuals are more responsive to injustices perpetrated against their group and more empathetic to individuals in closer "proximity" to themselves. Therefore, when much of the truth behind Tuskegee was revealed, mistrust among African Americans toward the medical profession and public health spiked.

[53] In fact, the U.S. Department of Health and Human Services documented racial/ethnic health disparities almost 35 years ago. As noted in Arias et al. (2021), African people live fewer years, on average, than White people. They are also more likely to die from treatable conditions; more likely to die during or after pregnancy and to suffer serious pregnancy-related complications, and more likely to lose children in infancy.

Public health is concerned with promoting health, preventing disease, and enhancing the quality of life at the population level. Moreover, key public health models and frameworks underscore the fact that social conditions are powerful determinants of health within a given population (Kass, 2001). As outlined in the Public Health Code of Ethics, the "effectiveness of public health policies, practices, and actions depends upon public trust gained through decisions based on the highest ethical, scientific, and professional standards". Regarding COVID-19 disparities, a lack of trust in public health systems demands major attention as a result of persisting racism and socioeconomic inequity (Best et al., 2021).

To evaluate whether the lower vaccination rate among Black Americans we observe is driven by medical mistrust, we investigate racial differences in vaccine hesitancy throughout the study. We use survey data from the Household Pulse Survey (HPS) regarding the individual intention to vaccinate. In particular, we use the following HPS survey question, "once a vaccine to prevent COVID-19 is available to you, would you get a vaccine?". The responses are recorded in one of the five options, "definitely get a vaccine"; "probably get a vaccine"; "unsure"; "probably not get a vaccine"; or "definitely not get a vaccine".

Figure 3.4: County-level hesitancy of COVID-19 vaccine



Data Source: Household Pulse Survey.
Note: Darker shaded areas represent a higher resistance to receiving a vaccine.

A dummy variable is constructed to represent an individual's reluctance to receive a vaccine, which includes their responses of "definitely not" and "probably not". The county-level hesitation rate is computed in three steps using the approach described in Beleche et al. (2021). First, we use HPS responses to estimate hesitation rates at the state level. Then, combining the Census Bureau's 2019 American Community Survey 1-year Public Use Microdata Sample, we projected values to anticipate hesitation rates in more granular areas. Lastly, we use a PUMA-to-county crosswalk from the Missouri Census Data Center to create county-level estimates. In Figure 3.4, we plot the measure of hesitation. The vaccine hesitancy is positively correlated with the proportion of Blacks, which depicts the geographical distribution of Black population share by county in 2010 based on data from the American Community Survey.

We established two measures for vaccine hesitancy, specifically categorized as "strongly hesitant" and "hesitant." We partitioned the time frame into pre and post-April for two key reasons. Firstly, beginning in April 2021, the general population in most states gained widespread access to vaccinations, potentially impacting their views on vaccine acceptance. Secondly, the Health and Politics Survey (HPS) altered the way they measured vaccine resistance in their survey.[54] The findings in Table 3.7 indicate that the likelihood of being both strongly reluctant and reluctant to receive vaccination increases as the percentage of the Black population grows. Nevertheless, this effect is attenuated by the proximity to Tuskegee. However, the statistical significance is only observed for the reluctance not the strong reluctance. Our results lend support to the findings in Khan et al. (2021), which noted that Black people and people of other minorities are “unlikely or very unlikely” to take the COVID-19 vaccine.

[54] For the initial three months, the HPS survey did not include an "unsure" category in the question related to vaccine probability. However, in the April survey, the HPS survey introduced a modification by adding a new "unsure" category to the scales.

3.5.2 Ruling out Supply-Side Factors

It is worth noting that the research findings may also be subject to influence from supply-side factors, such as the availability of vaccines, the presence of medical professionals, and access to vaccination services, among others. To mitigate the effects of these factors, we incorporate the number of medical doctors for a county and its interaction with the time trend, as well as the number of pharmacy sites for a county and its interaction with the time trend (both pharmacy sites and doctor/nurses are normalized by the county population) in the regression.

Table C6 shows the findings after accounting for supply-side issues. When the point estimates for individuals who received at least one dose of vaccination are compared with the baseline, the point estimates are little modified. Similarly, increasing supply-side factors have a negligible impact on the rate of being completely vaccinated.

3.6 Heterogeneity

Our aforementioned analysis documents a strong correlation between the Tuskegee Study and heterogeneity in vaccination behavior in regions with varying proportions of Black residents. To gain a further understanding of our findings,

Table 3.7: Impact of Tuskegee Study on vaccination hesitancy

	Strongly hesitant		Hesitant	
	Jan. - Mar.	April - Sept.	Jan. - Mar.	April - Sept.
	(1)	(2)	(3)	(4)
Share of Black × distance × week	-0.009	-0.001	-0.030**	-0.013**
	(0.006)	(0.003)	(0.014)	(0.005)
Share of Black × week	1.712***	0.315***	1.551***	0.513***
	(0.332)	(0.088)	(0.451)	(0.118)
Distance × week	-0.000	-0.001***	-0.002	-0.002**
	(0.001)	(0.000)	(0.001)	(0.001)
Observations	12,428	24,856	12,428	24,856
R-squared	0.968	0.974	0.982	0.985
County FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes
Share of Hispanic × week	Yes	Yes	Yes	Yes
Share of Asian × week	Yes	Yes	Yes	Yes

Note: To measure vaccine hesitancy, we use the following HPS survey question, "once a vaccine to prevent COVID-19 is available to you, would you get a vaccine?". Five responses are recorded, including "definitely get a vaccine"; "probably get a vaccine"; "unsure"; "probably not get a vaccine"; "definitely not get a vaccine." Strongly hesitant is defined as definitely not, and Hesitant is defined as definitely not and probably not. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * $p < 0.01$; ** $p < 0.05$; *** $p < 0.01$.

we examine several types of heterogeneity by varying vaccine eligibility, political ideology, education, and income levels to shed light on the relevance of these forces.

3.6.1 Political Ideology and Time Variation

Vaccination is an effective tool for reducing the impact of COVID-19, but systemic inequities pose a serious threat to progress. Despite the greater availability of vaccines, racial discrepancies in vaccination persist. Vaccination uptake is substantially influenced by political ideology, and Republican voter support in the 2020 presidential election has a negative impact on vaccination take-up (Agarwal et al., 2021).

We categorize counties into Democratic (or Republican) led counties based on the Democratic (or Republican) Party's majority voter share in the 2020 presidential election and report the results in Panel A of Table 3.8. The Tuskegee Study's influence is greater in Republican-dominated counties than in Democratic counties because many of the former are counties in the South that are closer to Macon, Alabama. Democratic counties had a wider vaccination gap between counties with high and low proportions of the Black population, which can be attributed to the higher Black population in Democratic counties.[55]

We assume that time has a linear effect on the vaccination rate in our main specification. Separating periods based on vaccine eligibility, though, might be intriguing. As shown in Panel B of Table 3.8, we divide time based on the date that the White House announces that individuals 16 years of age and older are eligible for vaccinations, which is before and after April 25, 2021.[56] The findings reveal that, before vaccine availability to the general public, there was a significant vaccination gap between counties with varying proportions of Black people. However, after the vaccine became eligible, the vaccination gap was largely closed. Tuskegee, on the other hand, plays a vital role in closing the immunization gap between regions.

3.6.2 Education and Income

In Table 3.9, we examine whether the impact of the Tuskegee Study on vaccination varies by education and income level. Panel A compares the results for counties with lower and higher-educated residents. There is a notable distinction between counties in the upper and lower 50^{th} percentiles of the education distribution. The gap in vaccination rates between a higher-educated Black community and a White community with a comparable higher-educated Black population reduces more rapidly as the distance from Tuskegee increases. The

[55] In our data, the average percentage of Black people is 19.4% in Democratic areas and only 8% in Republican areas.

[56] As demonstrated in Figure C.6, the distribution and availability of the COVID-19 vaccine for adults 16 and older vary by state. To alleviate the possibility of endogeneity resulting from vaccine adoption and vaccination behavior across states, we are utilizing the announcement date rather than the state distribution of vaccines.

Table 3.8: Heterogeneity by political ideology and eligibility timeline

Panel A: Political ideology	At least one dose			Fully vaccinated		
	Democratic	Republican	Cross-Model Difference (*p*)	Democratic	Republican	Cross-Model Difference (*p*)
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Black × distance × week	0.167*	0.197*	0.057	0.178**	0.222**	0.004
	(0.089)	(0.114)		(0.079)	(0.101)	
Share of Black × week	-1.749***	-0.725	0.161	-2.145***	-0.609	0.404
	(0.466)	(0.503)		(0.493)	(0.494)	
Distance × week	-0.013***	-0.006	0.112	-0.014**	-0.003	0.040
	(0.005)	(0.004)		(0.006)	(0.004)	
Observations	23,012	111,143		23,909	119,653	
R-squared	0.975	0.961		0.979	0.963	
Panel B: Vaccine eligibility	At least one dose			Fully vaccinated		
	Before April 25	After April 25	Cross-Model Difference (*p*)	Before April 25	After April 25	Cross-Model Difference (*p*)
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Black × distance × week	0.345*	0.043	0.017	0.224	0.042	0.079
	(0.195)	(0.052)		(0.138)	(0.040)	
Share of Black × week	-3.660***	0.315	0.008	-2.551***	-0.145	0.0003
	(0.804)	(0.300)		(0.664)	(0.313)	
Distance × week	0.001	-0.007*	0.038	-0.001	-0.007*	0.035
	(0.011)	(0.004)		(0.006)	(0.004)	
Observations	41,340	92,918		42,296	101,370	
R-squared	0.960	0.974		0.953	0.976	
County FE	Yes	Yes		Yes	Yes	
Week FE	Yes	Yes		Yes	Yes	
Share of White × week	Yes	Yes		Yes	Yes	
Share of Hispanic × week	Yes	Yes		Yes	Yes	
Share of HS grads and above × week	Yes	Yes		Yes	Yes	
County unemployment rate × week	Yes	Yes		Yes	Yes	

Note: The White House announced that all people aged 16 and older are eligible for the COVID-19 vaccine on April 19, 2021. The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * p < 0.01; ** p < 0.05; *** p < 0.01.

findings align with the June 2021 report from the Centers for Disease Control and Prevention, which indicated that younger adults, non-Hispanic Black adults, individuals with lower educational attainment, no health insurance, and lower household incomes exhibited the lowest vaccination coverage rates and expressed less willingness to get vaccinated.[57]

[57] The report can be found here: COVID-19 Vaccination Coverage and Intent Among Adults Aged 18–39 Years — United States, March–May 2021.

Table 3.9: Heterogeneity by education and income level

	At least one dose			Fully vaccinated		
	Bottom 50% (1)	Top 50% (2)	Cross-Model Difference (*p*) (3)	Bottom 50% (4)	Top 50% (5)	Cross-Model Difference (*p*) (6)
Panel A: Education level						
Share of Black × distance × week	0.105	0.216*	0.003	0.104	0.223**	0.001
	(0.071)	(0.110)		(0.064)	(0.103)	
Share of Black × week	-1.097**	-1.289***	0.007	-1.236**	-1.807***	0.000
	(0.466)	(0.449)		(0.542)	(0.390)	
Distance × week	-0.010**	-0.006	0.091	-0.015**	-0.004	0.014
	(0.005)	(0.004)		(0.006)	(0.004)	
Observations	65,222	69,037		68,103	75,563	
R-squared	0.966	0.968		0.972	0.973	
Panel B: Income level						
Share of Black × distance × week	0.144**	0.159	0.138	0.131**	0.173*	0.151
	(0.064)	(0.103)		(0.057)	(0.097)	
Share of Black × week	-0.579	-1.525***	0.023	-0.522	-2.041***	0.017
	(0.397)	(0.473)		(0.421)	(0.455)	
Distance × week	0.001	-0.006	0.058	0.002	-0.006	0.005
	(0.005)	(0.004)		(0.006)	(0.004)	
Observations	68,164	66,095		72,590	71,076	
R-squared	0.958	0.972		0.962	0.976	
County FE	Yes	Yes		Yes	Yes	
Week FE	Yes	Yes		Yes	Yes	
Share of White × week	Yes	Yes		Yes	Yes	
Share of Hispanic × week	Yes	Yes		Yes	Yes	
Share of HS grads and above × week	Yes	Yes		Yes	Yes	
County unemployment rate × week	Yes	Yes		Yes	Yes	

Note: The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. The level of education is determined based on the share of high school graduates and above at the county level. The median income at the county level determines the income level. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * $p < 0.01$; ** $p < 0.05$; *** $p < 0.01$.

Khairat and his research team, as discussed in their 2022 study (Khairat et al., 2022), delved into several factors and reasons contributing to the low uptake of COVID-19 vaccines in highly hesitant communities in the United States. They identified a low vaccination rate within these communities associated with factors such as lack of a high school education, and concerns regarding vaccine availability and distribution. The primary driver of vaccine hesitancy among these communities was a lack of trust in COVID-19 vaccines, followed by apprehensions about vaccine side effects and a lack of trust in the government. Additionally, individuals with lower levels of education tend to possess limited knowledge about vaccines and their distribution, which makes them more likely to express doubts about the effectiveness of the vaccine (Borga et al., 2022). Furthermore, individuals with lower educational levels often have limited access to transportation, which can hinder their ability to get vaccinated.

However, the impact of Tuskegee on reducing the vaccination disparity does not exhibit statistical significance in counties falling within both the top and bottom 50^{th} of the income distribution. One plausible explanation for this is that the COVID-19 vaccine is available to the general public at no cost, which should make access to it equally accessible in both affluent and economically disadvantaged counties.

3.7 Concluding Remarks

As of June 21, 2022, more than 1 million individuals have died due to the COVID-19 pandemic in the U.S. alone,[58] and more than 14.9 million deaths worldwide.[59] The rate of vaccination has stagnated and fallen from the high in February 2021 (Diesel et al., 2021), despite the availability of the three types of vaccines in the United States and widespread immunization programs. Furthermore, there are variances in the vaccination rate by race across regions and throughout time. Among the many elements that could lead to vaccination resistance and refusal are political affiliation, cultural norms, the perception of COVID-19's threat, trust in the vaccine itself, and faith in governmental institutions (Bagasra et al., 2021; Bian et al., 2022).

By investigating the impact of a historical event and its lasting effects on the mistrust of the healthcare system among African Americans, our study adds to the existing literature on the factors contributing to the racial disparities in the vaccination rate. Our findings demonstrate that over time, the vaccination rate for Black communities converges with that of their White counterparts in the same county. The degree of convergence varies by region. In counties closer to Tuskegee, the racial gap in vaccination rates is closing at a slower pace, likely because of the scars from the Tuskegee Study. Our study points to a spatial distribution of the intergenerational impact of historical trauma, and the need to rebuild the trust among African Americans who continue to harbor fears of the racist past of the United States.

[58] The U.S. cases and the death toll was collected by Johns Hopkins University Coronavirus Resource Center.

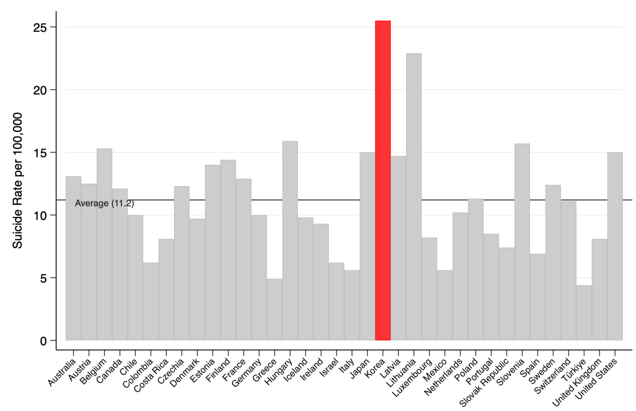
[59] New estimates from the World Health Organization show that the full death toll associated directly or indirectly with the COVID-19 pandemic (described as "excess mortality") was approximately 14.9 million (range 13.3 million to 16.6 million) globally. Data source: Department of Economics and Social Affairs, United Nations.

Appendix A

The Grandparent Health Dividend: Transitioning to Grandparenthood and Its Impact on Mental Health

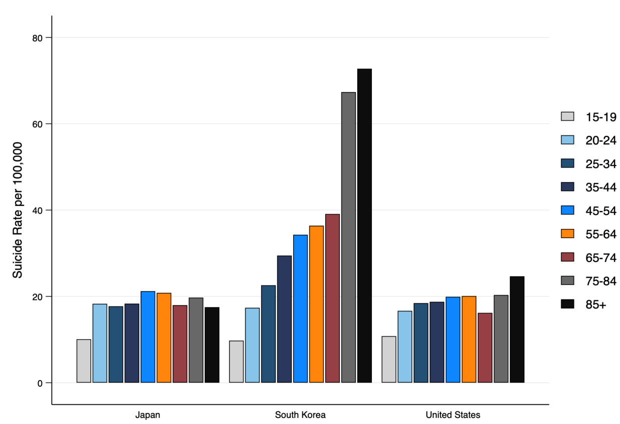
A.1 Supplemental Tables and Figures

Figure A.1: Suicide rates across OECD countries in 2019



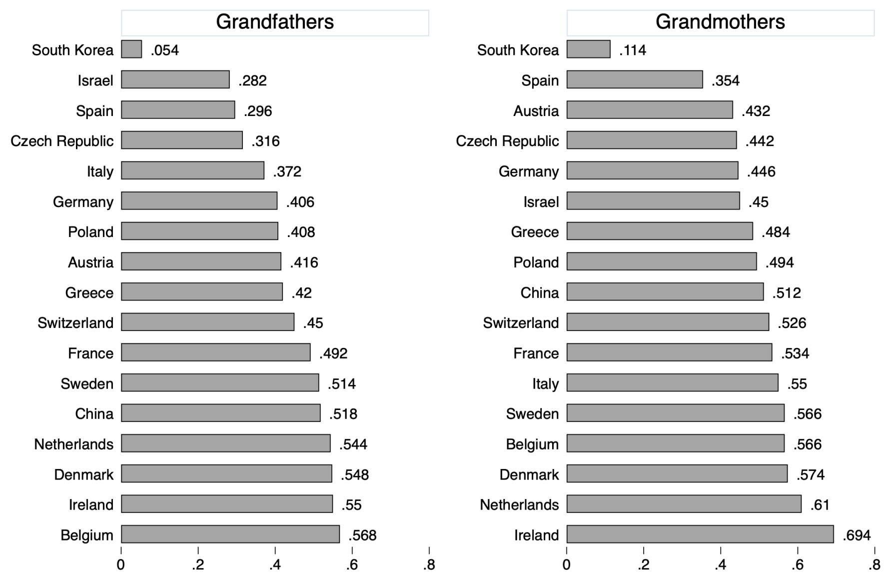
Note: This figure displays the suicide rate per 100,000 population across OECD countries in 2019. Data obtained from OECD Data Indicators.

Figure A.2: Suicide rates by age group in 2019



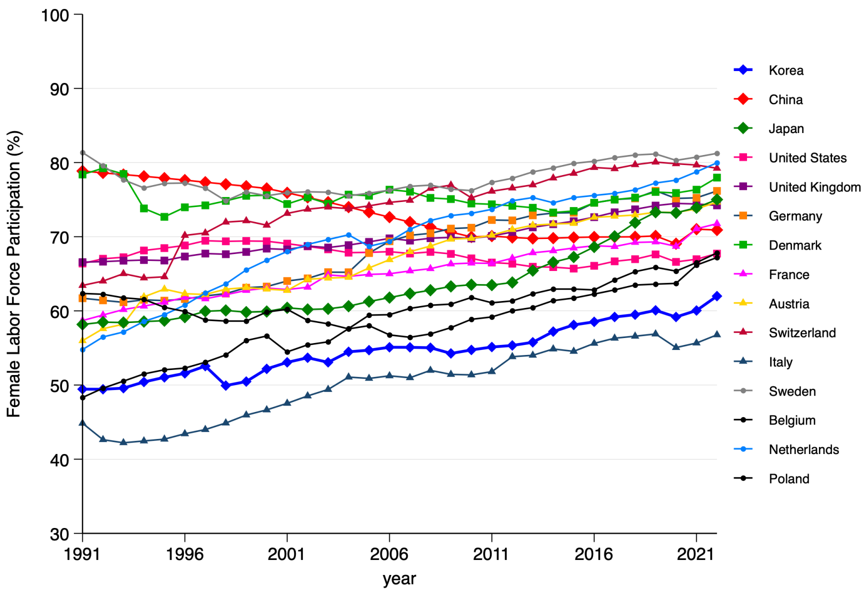
Note: This figure reports age-specific suicide rates per 100,000 population in 2019 for the United States, Japan, and South Korea. Age groups are defined as 15–19, 20–24, 25–34, 35–44, 45–54, 55–64, 65–74, 75–84, and 85 and above. Data are obtained from the World Health Organization (2024).

Figure A.3: Probability of providing care for grandchildren



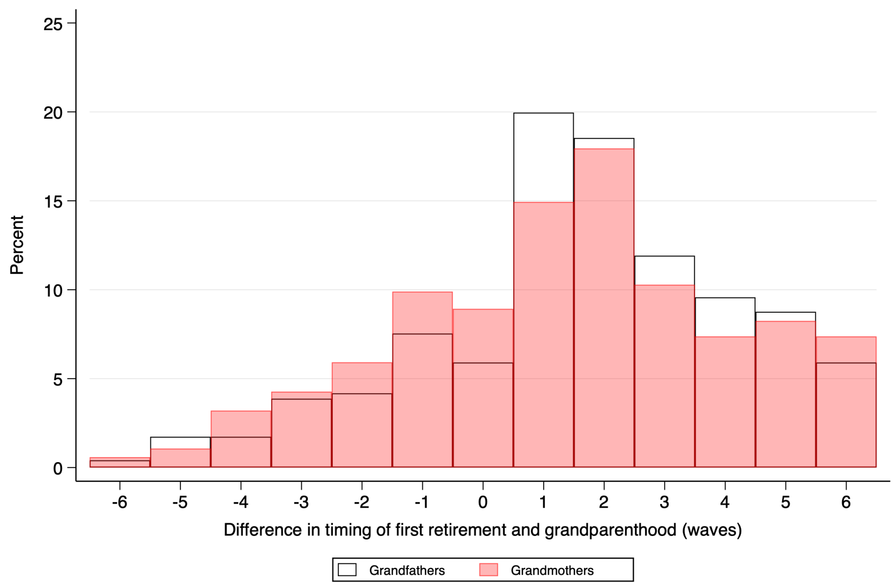
Note: This figure presents the probability of providing caregiving for grandchildren in the last 12 months for each country, differentiated by gender. Data for European countries are sourced from SHARE wave 2, data for China are obtained from CHARLS wave 2, and data for Korea are drawn from KLoSA wave 1. For SHARE and CHARLS, the survey asks respondents who had at least one grandchild whether they looked after grandchildren without parents being present in the 12 months preceding the interview. For KLoSA, the survey asks respondents with grandchildren whether they provided care for a grandchild aged under 10 in the last year.

Figure A.4: Female labor force participation rate



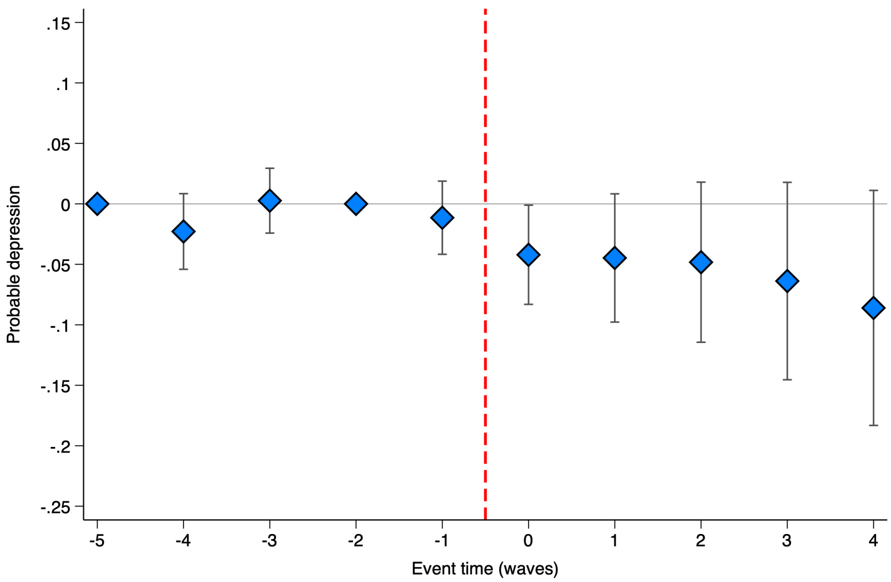
Note: This figure presents the female labor force participation rate for individuals aged 15-64 in selected countries. Data are obtained from World Bank.

Figure A.5: Distribution of the timing difference between retirement and becoming a grandparent



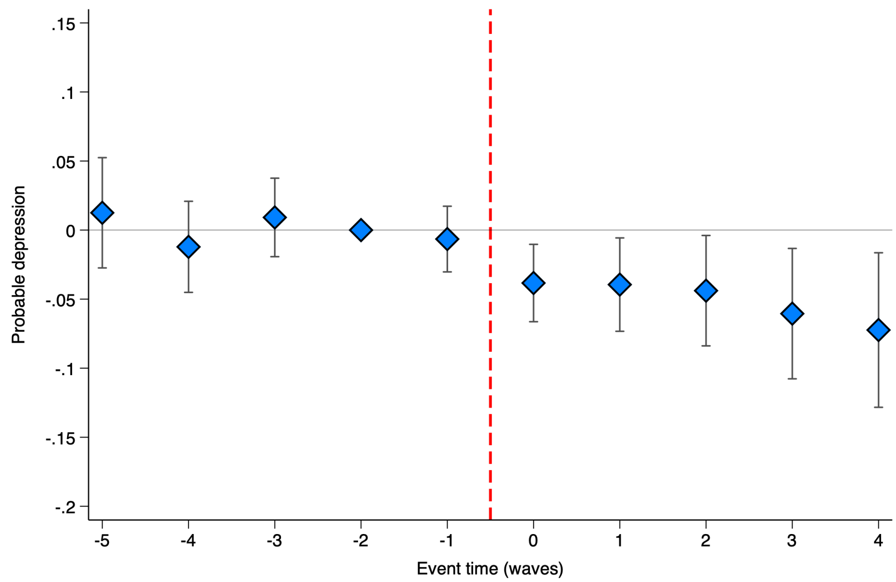
Note: This figure shows the distribution of the difference in event time (measured in survey waves) between the first observed retirement and the birth of the first grandchild. The difference is calculated as: retirement wave minus the first wave in which an individual became a grandparent. For individuals who did not retire during the survey period, the retirement wave is assigned as the wave following their last observed interview. Positive values should be interpreted as lower bounds, since the actual retirement date likely occurred after the survey ended.

Figure A.6: Mental health effects of grandparenthood (omitting two pre-treatment periods)



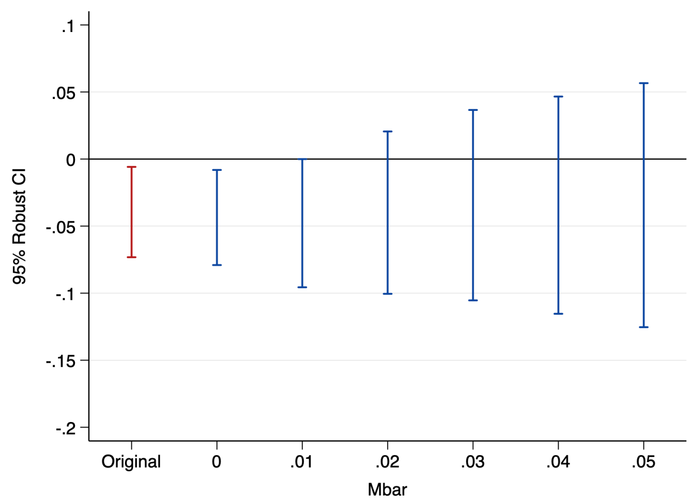
Note: This figure reports event-study estimates of the effect of becoming a grandparent on probable depression. The analysis uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006 to 2018. The sample includes individuals aged 45–75 who became first-time grandparents during the study period and are observed at least once before and once after the transition. The outcome is a binary indicator for probable depression, defined as having a CESD-10 score of 10 or higher. The model omits two pre-treatment periods ($j = -5$ and $j = -2$) for normalization and controls for wave and age fixed effects, along with gender and education. Standard errors are clustered at the individual level.

Figure A.7: Mental health effects of grandparenthood (interacting fixed effects with gender and education)



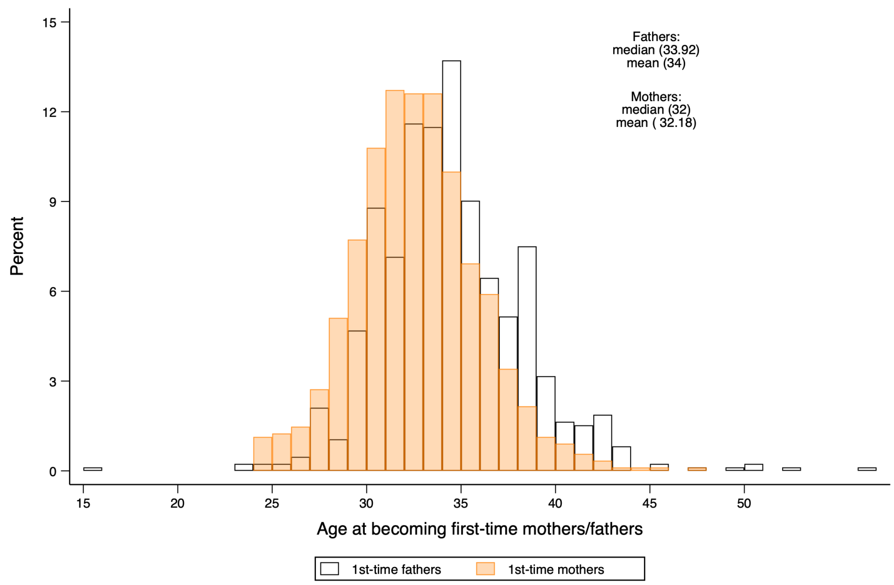
Note: This figure reports event-study estimates of the effect of becoming a grandparent on probable depression. The analysis uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006 to 2018. The sample includes individuals aged 45–75 who became first-time grandparents during the study period and are observed at least once before and once after the transition. The outcome is a binary indicator for probable depression, defined as having a CESD-10 score of 10 or higher. The model interacts wave and age fixed effects with gender and education to capture group-specific trends. Standard errors are clustered at the individual level.

Figure A.8: Honest DiD sensitivity analysis for grandparenthood effects



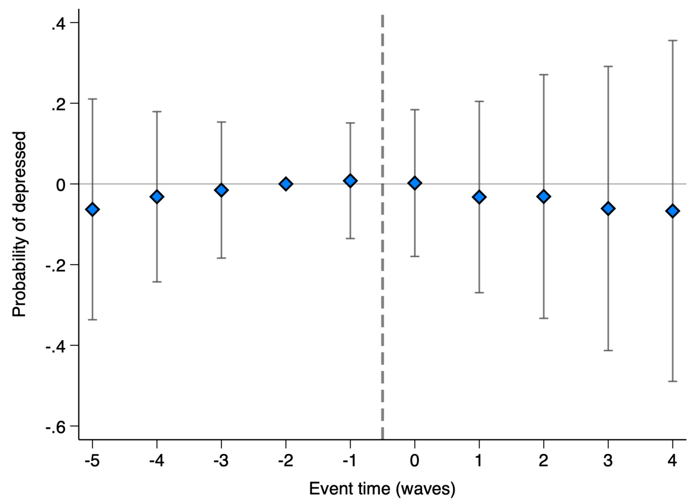
Note: This figure presents results from the Honest DiD sensitivity analysis (Rambachan & Roth, 2023) for the impact of transitioning to grandparenthood on mental health. The y-axis represents 95% robust confidence intervals for the treatment effect. The x-axis represents M, the maximum allowed deviation from parallel trends between consecutive periods. "Original" shows the baseline estimate without accounting for potential violations of parallel trends. As M increases, confidence intervals widen, allowing for greater non-parallel pre-trends. Results remain statistically significant (confidence intervals exclude zero) up to M = 0.02, four times the observed pre-trend slope of 0.003.

Figure A.9: Age at becoming first-time parents



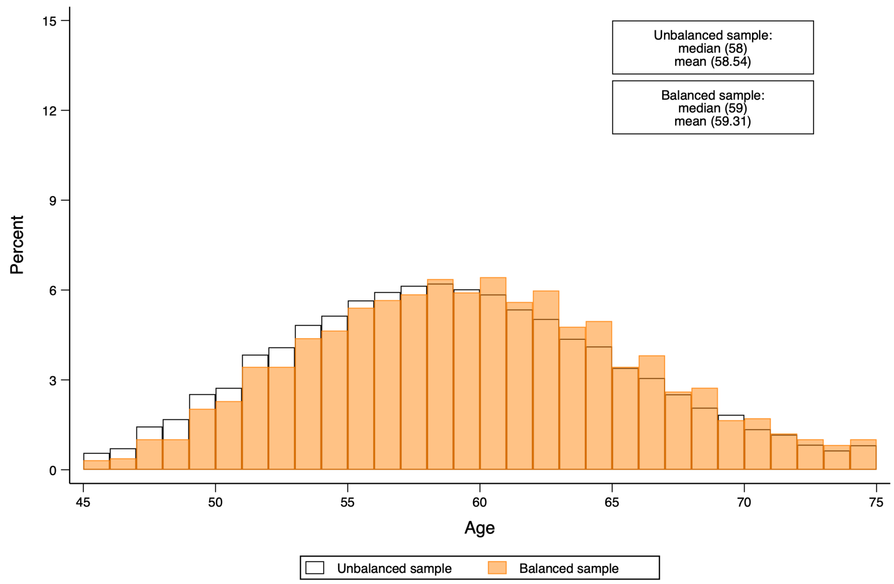
Notes: This table plots the age at which individuals become first-time mothers or fathers. Data are sourced from the Korean Longitudinal Study of Ageing (KLoSA), covering waves from 2006 to 2018. The exact birth year of each child is not observed in the data; hence, we determine the age at which an individual becomes a new parent based on the wave during which they are first recorded as a parent. This means an individual might have become a new parent between the previous wave and the current wave. Since each wave spans two years, the reported average and mean ages are likely to be one or two years older than the actual age at which individuals became parents. Therefore, the information presented here should be interpreted with caution when compared to other studies. Additionally, this age distribution is used in the placebo test, where a random age of childbirth is assigned to individuals who have never become grandparents to determine an assigned wave of becoming a new grandparent.

Figure A.10: Placebo test: grandparenthood effects on mental health



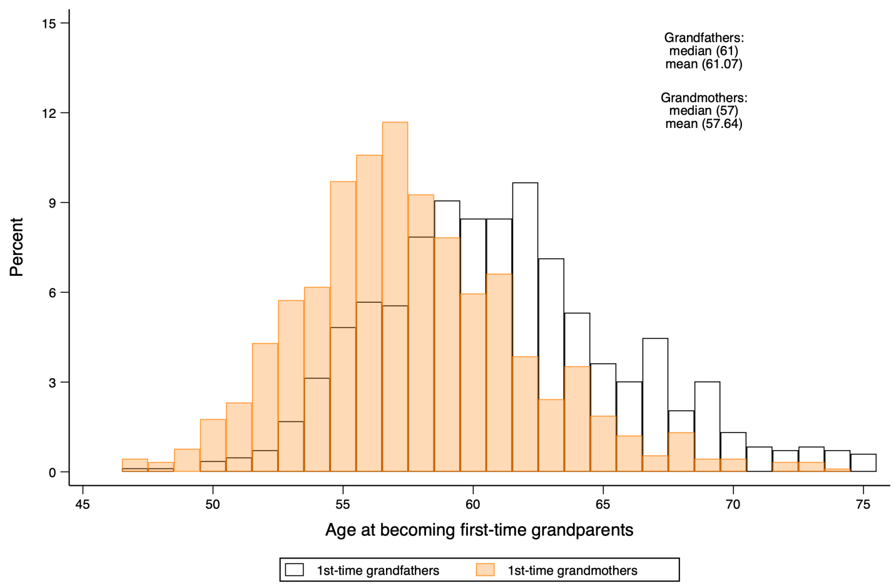
Notes: Data are from the Korean Longitudinal Study of Aging (KLoSA), which is conducted every two years. This placebo test uses a sample of individuals aged 45-75 who never became grandparents during the survey period. A placebo age of becoming a grandparent is assigned to each individual based on the age of their oldest child potentially becoming a parent, drawn from a log-normal distribution with parameters derived from the observed ages of first-time parents in the main sample categorized by gender. For individuals with children of both genders, the earlier of the two assigned ages is used as the placebo event time. The outcome variable is the probability of probable depression, based on a binary indicator from the CESD-10 survey (cutoff score of 10). The wave before the placebo event (event period $t = -2$) is normalized for reference. The event study controls for wave fixed effects, age fixed effects, and individual characteristics such as gender and education. Standard errors are bootstrapped with 5,000 replications.

Figure A.11: Age distribution between balanced and unbalanced samples



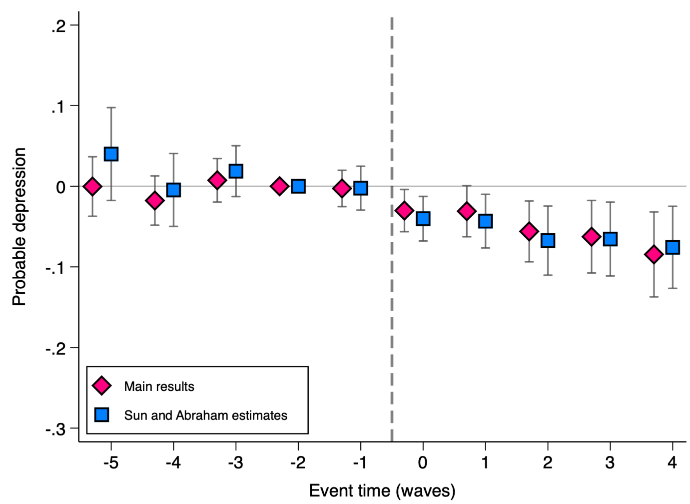
Notes: This figure presents the age distribution of individuals who become new grandparents during the survey period, separated into balanced and unbalanced samples. Data are from the Korean Longitudinal Study of Ageing (KLoSA), covering waves from 2006 to 2018.

Figure A.12: Age at becoming first-time grandparents



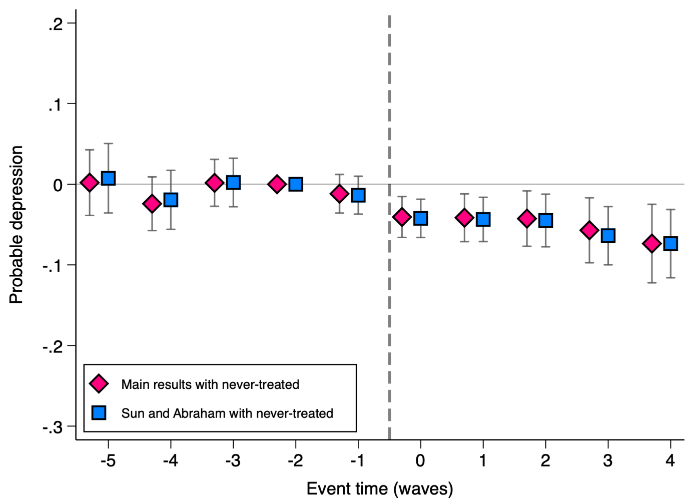
Note: This figure shows the distribution of ages at which individuals first become grandparents, presented separately for grandfathers and grandmothers. Data are drawn from the Korean Longitudinal Study of Aging (KLoSA), covering waves from 2006 to 2018. Because the exact birth year of grandchildren is not observed, the reported age corresponds to the survey wave in which individuals are first identified as grandparents. As each wave spans two years, the reported average age may overstate the actual transition age by one to two years. Caution is advised when comparing these estimates to those from studies with precise birth timing.

Figure A.13: Grandparenthood effects using the method of L. Sun and Abraham (2021) with late-treated cohort as control group



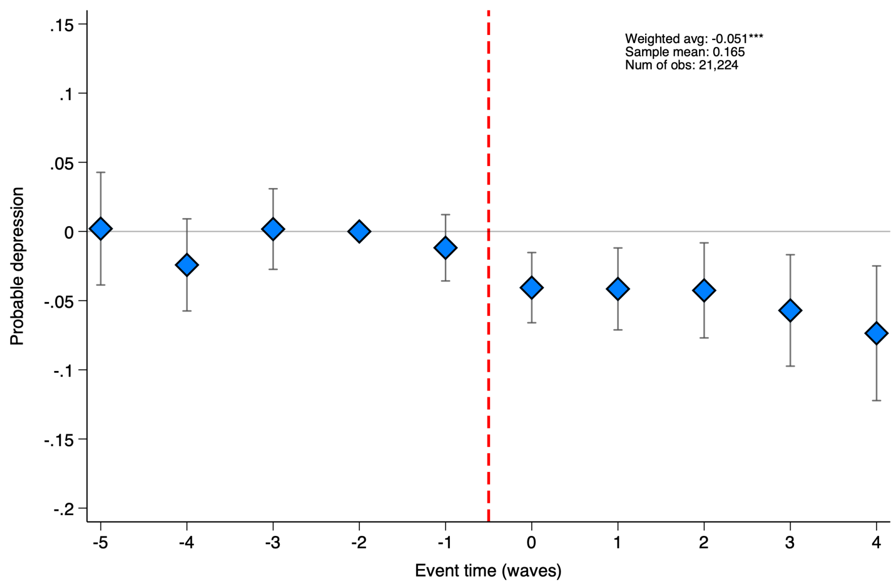
Note: This figure reports estimates using the method of L. Sun and Abraham (2021), applied to data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006 to 2018. The sample includes individuals aged 45 to 75 who are observed transitioning into grandparenthood during the study period. Individuals who become grandparents at age 65 or older are treated as the last-treated cohort and serve as the control group. The outcome is a binary indicator for probable depression, coded as 1 if the CES-D-10 score is 10 or higher and 0 otherwise. The reference period is the wave prior to pregnancy (event time $t = -2$). The specification includes age, wave, and individual fixed effects. Standard errors are clustered at the individual level.

Figure A.14: Grandparenthood effects using the method of L. Sun and Abraham (2021) with never grandparents as control group



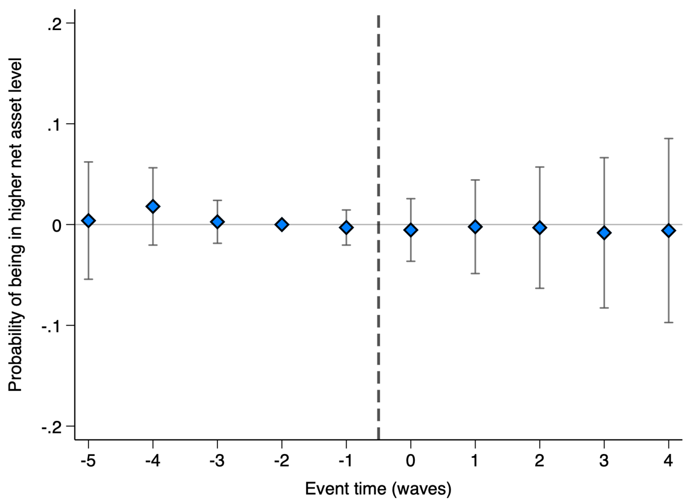
Note: This figure presents results using the L. Sun and Abraham (2021) method. This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. The sample consists of individuals aged 45-75, including individuals observed becoming grandparents during the survey, using the never-treated cohort as the control group. The outcome variable is a binary indicator for probable depression, where individuals with a CESD-10 score of 10 or higher are coded as 1 and others as 0. The reference period is the wave before pregnancy (event period $t = -2$). The event study includes controls for age, person, and wave fixed effects. Standard errors are clustered at the individual level.

Figure A.15: Mental health effects of grandparenthood (never grandparent as control)



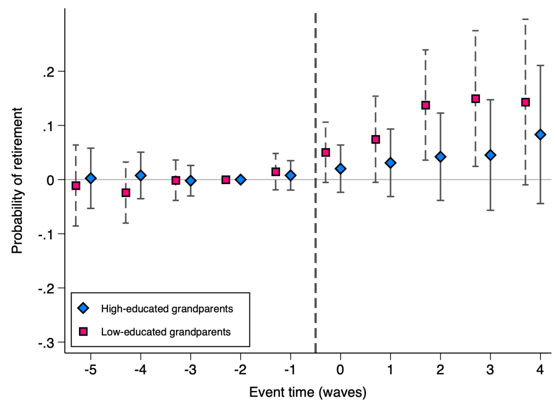
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006 to 2018. The sample includes individuals aged 45 to 75 who became first-time grandparents during the study period, observed at least once before and once after the transition. Individuals who never became grandparents are used as the control group. The outcome is a binary indicator for probable depression, coded as 1 if the CES-D-10 score is 10 or higher and 0 otherwise. The reference period is the wave prior to pregnancy (event time $t = -2$). The specification includes individual, wave, and age fixed effects. Standard errors are clustered at the individual level. The figure reports the sample mean, the weighted average of post-treatment coefficients (event times 0 to 4).

Figure A.16: Probability of staying in higher net asset level

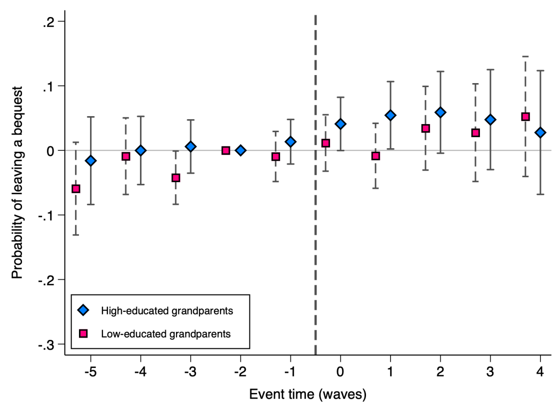


Notes: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial survey conducted between 2006 and 2018. The sample consists of individuals aged 45–75 who became new grandparents during the study period, each observed at least once before and after the transition. The outcome is net asset level, with the sample median used as the cutoff. The reference period is the wave prior to pregnancy (event period $t = -2$). Estimates are based on event study models controlling for wave and age fixed effects, as well as gender and education. Standard errors are clustered at the individual level.

Figure A.17: Retirement and bequest patterns by education



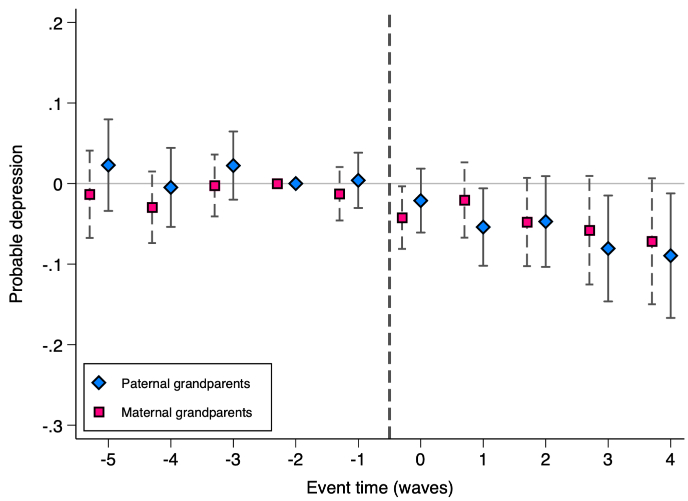
(a) Probability of retirement



(b) Probability of leaving a bequest

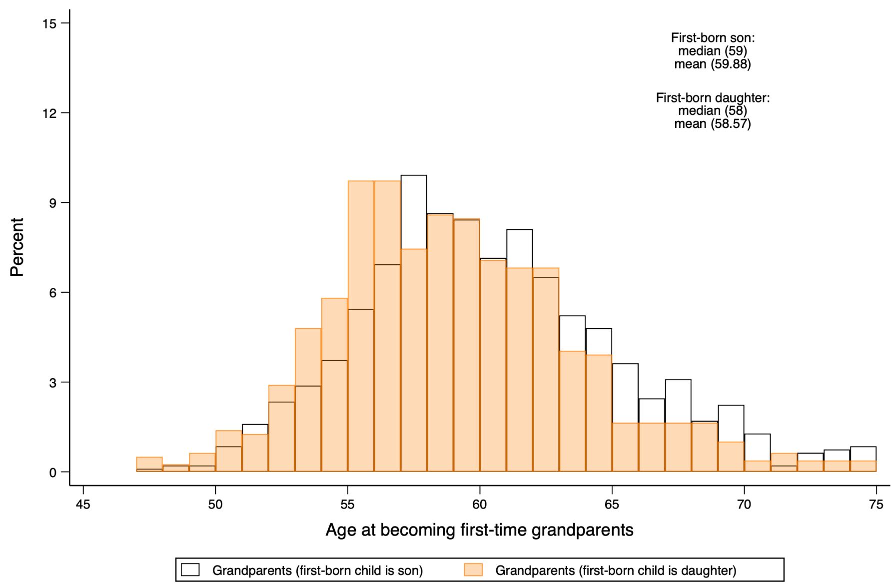
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006 to 2018. Estimates are based on the event study specification in Equation 1.1. Panel (a) shows changes in the probability of retirement following the birth of a first grandchild; Panel (b) shows changes in the probability of reporting an intention to leave a bequest. Each panel reports results separately by education level. Standard errors are clustered at the individual level.

Figure A.18: Grandparenthood effects by paternal and maternal grandparents



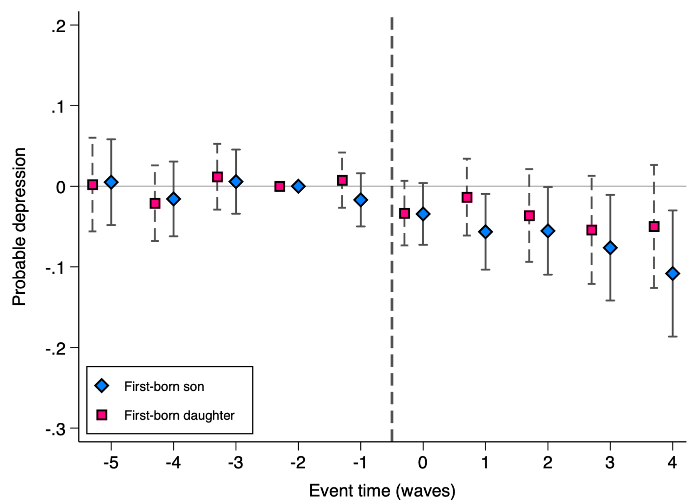
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. The sample includes individuals aged 45–75 who became first-time grandparents during the study period, observed at least once before and once after the transition. The outcome variable is a binary indicator for probable depression, where individuals with a CESD-10 score of 10 or higher are coded as 1, otherwise as 0. The reference period is the wave before pregnancy (event period $t = -2$). Estimates are based on event study models controlling for wave and age fixed effects, as well as gender and education. Standard errors are clustered at the individual level. Results are presented separately for paternal and maternal grandparents.

Figure A.19: Age at becoming first-time grandparents (by gender of first child)



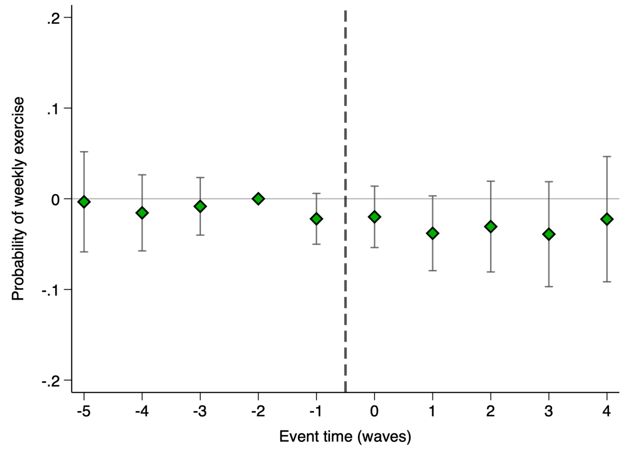
Note: This figure shows the distribution of ages at which individuals first become grandparents, presented separately by the gender of the first-born child. Data are drawn from the Korean Longitudinal Study of Aging (KLoSA), covering waves from 2006 to 2018. Because the exact birth year of grandchildren is not observed, the reported age corresponds to the survey wave in which individuals are first identified as grandparents. As each wave spans two years, the reported average age may overstate the actual transition age by one to two years. Caution is advised when comparing these estimates to those from studies with precise birth timing.

Figure A.20: Grandparenthood effects by gender of first child

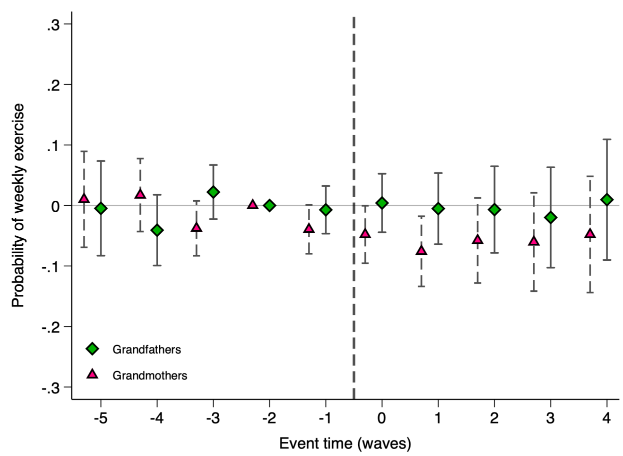


Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. The sample includes individuals aged 45–75 who became first-time grandparents during the study period, observed at least once before and once after the transition. The outcome variable is a binary indicator for probable depression, where individuals with a CESD-10 score of 10 or higher are coded as 1, otherwise as 0. The reference period is the wave before pregnancy (event period $t = -2$). Estimates are based on event study models controlling for wave and age fixed effects, as well as gender and education. Standard errors are clustered at the individual level. Results are presented separately by the gender of the first-born child.

Figure A.21: Grandparenthood effects on probability of weekly exercise



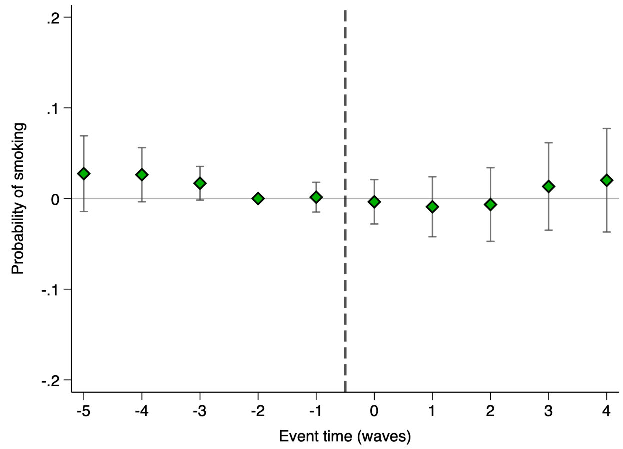
(a) Probability of weekly exercise



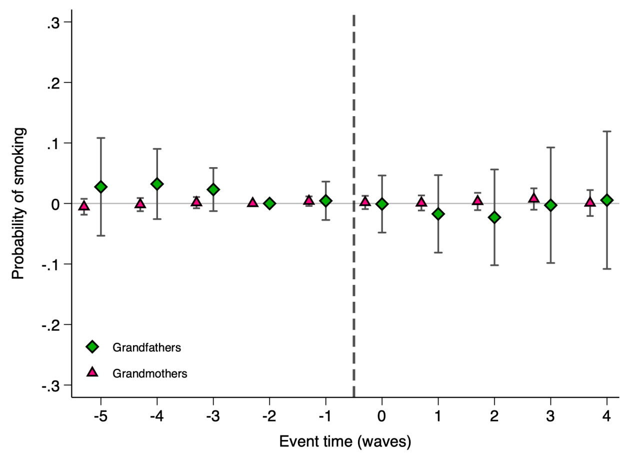
(b) Probability of weekly exercise by gender

Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator of the probability of weekly physical exercise. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

Figure A.22: Grandparenthood effects on the probability of current smoking



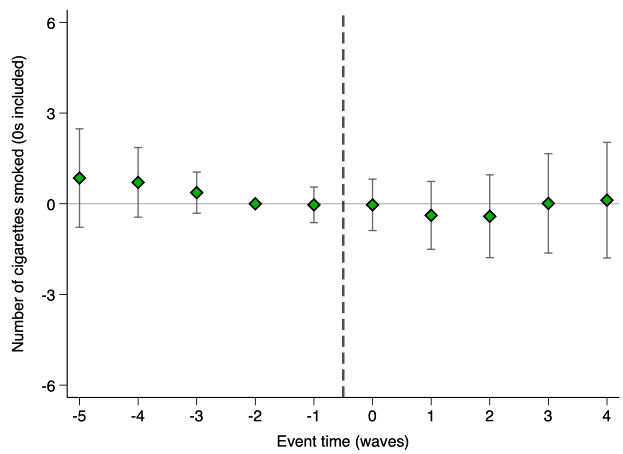
(a) Probability of current smoking



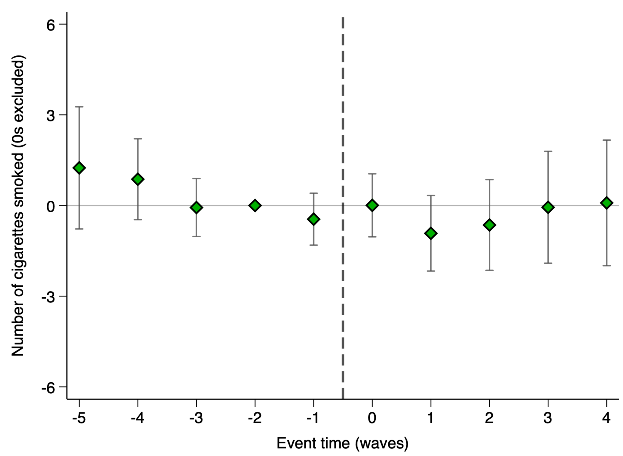
(b) Probability of current smoking by gender

Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator of the probability of current smoking. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

Figure A.23: Grandparenthood effects on number of cigarettes smoked per day



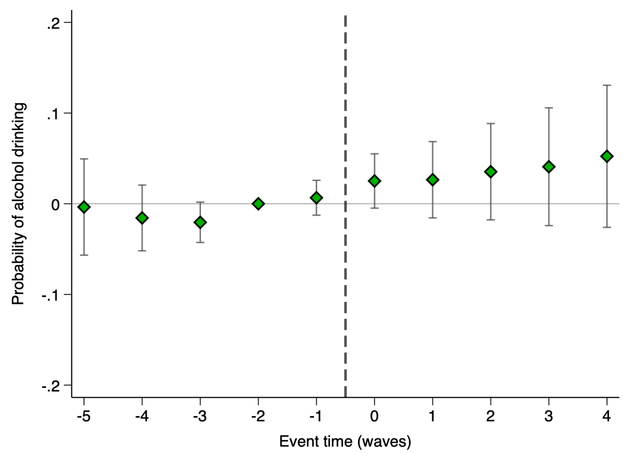
(a) Number of cigarettes smoked (0 included)



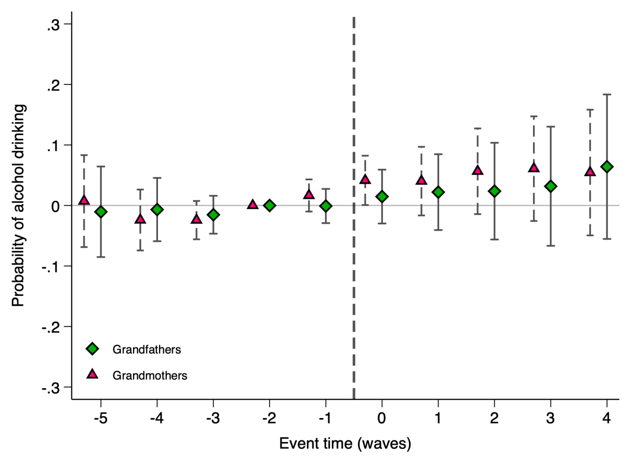
(b) Number of cigarettes smoked (0 excluded)

Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1.The outcome variable is a binary indicator of the probability of current smoking. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

Figure A.24: Grandparenthood effects on the probability of alcohol drinking



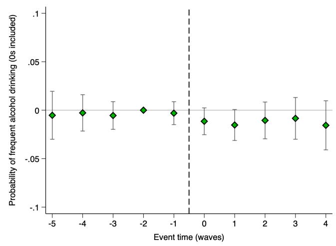
(a) Probability of alcohol drinking



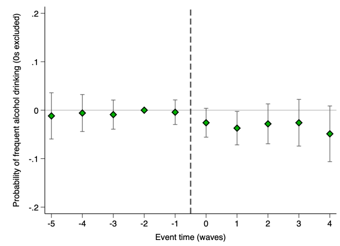
(b) By gender

Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator of the probability of alcohol drinking. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

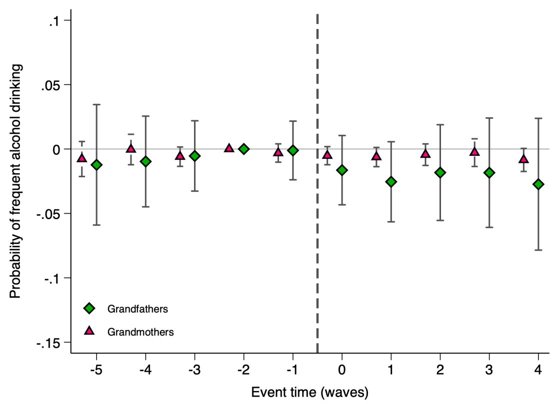
Figure A.25: Grandparenthood effects on the probability of frequent alcohol drinking



(a) 0s included



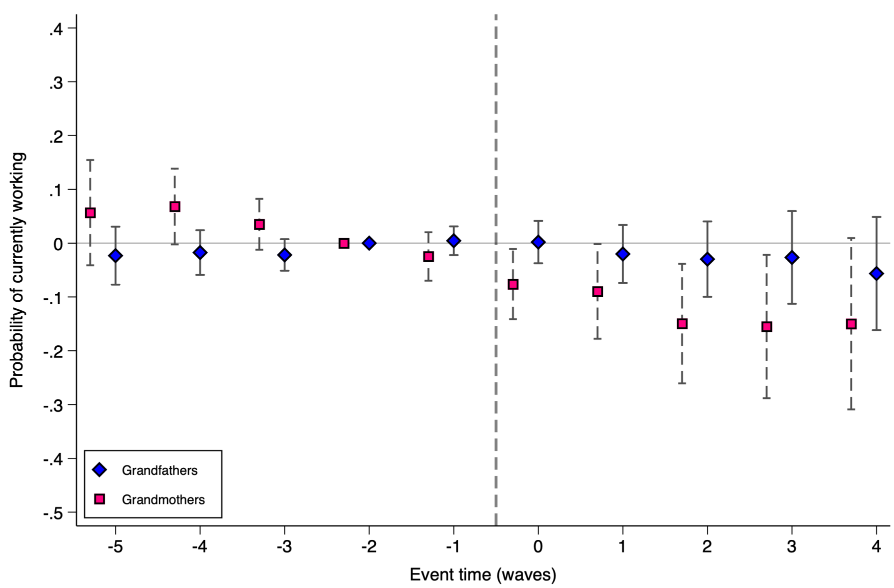
(b) 0s excluded



(c) By gender

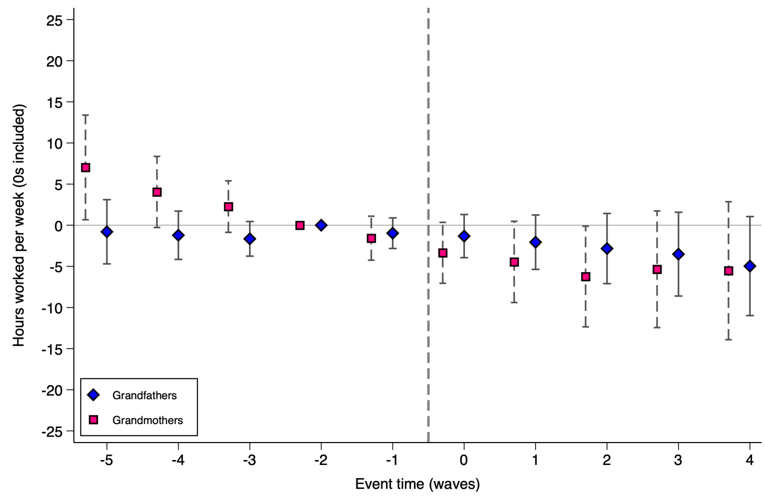
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator of the probability of frequent alcohol drinking. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

Figure A.26: Grandparenthood effects on currently working

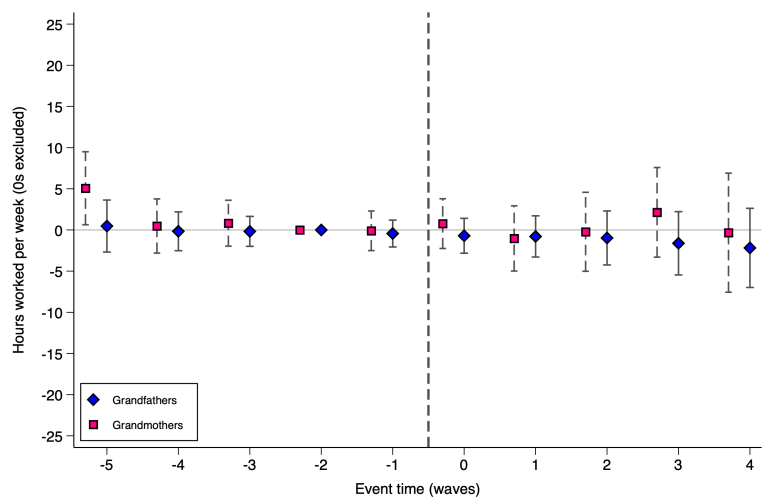


Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator of the probability of currently working. The reference period is the wave before pregnancy (event period $t = -2$). The event study controls for wave fixed effects, age fixed effects, and individual characteristics such as gender and education. Standard errors clustered at the individual level. Joint test of coefficient equality (SUR) for event time 0-4: $\chi^2(5)$ = 8.18, Prob $> \chi^2$ = 0.15.

Figure A.27: Grandparenthood effects on hours worked per week



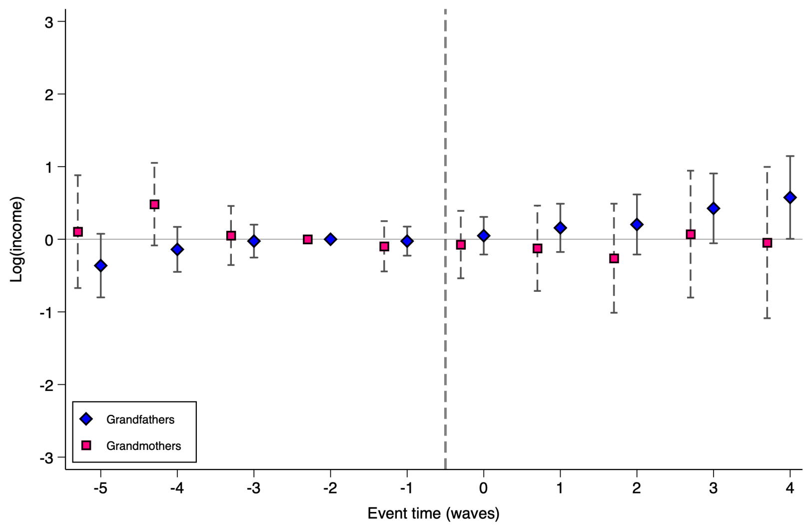
(a) Hours worked per week (0 included)



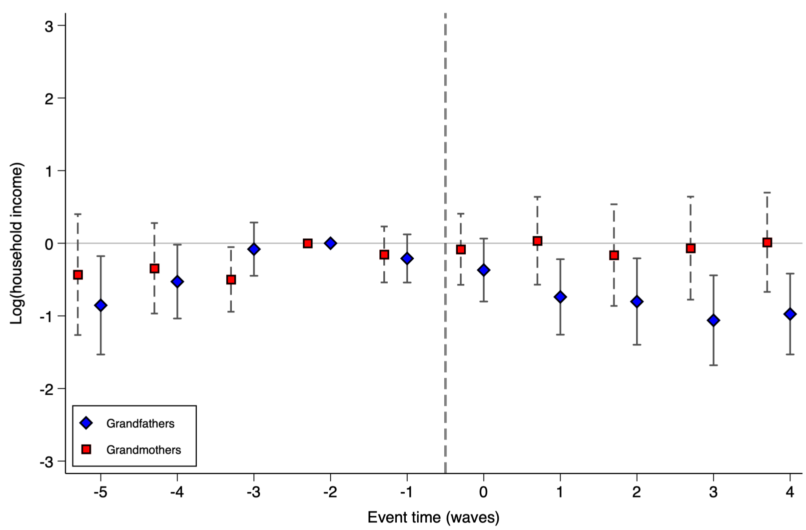
(b) Hours worked per week (0 excluded)

Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is the number of hours worked per week (both the intensive and extensive margins). The reference period is the wave before pregnancy (event period $t = -2$). Standard errors clustered at the individual level.

Figure A.28: Grandparenthood effects on income



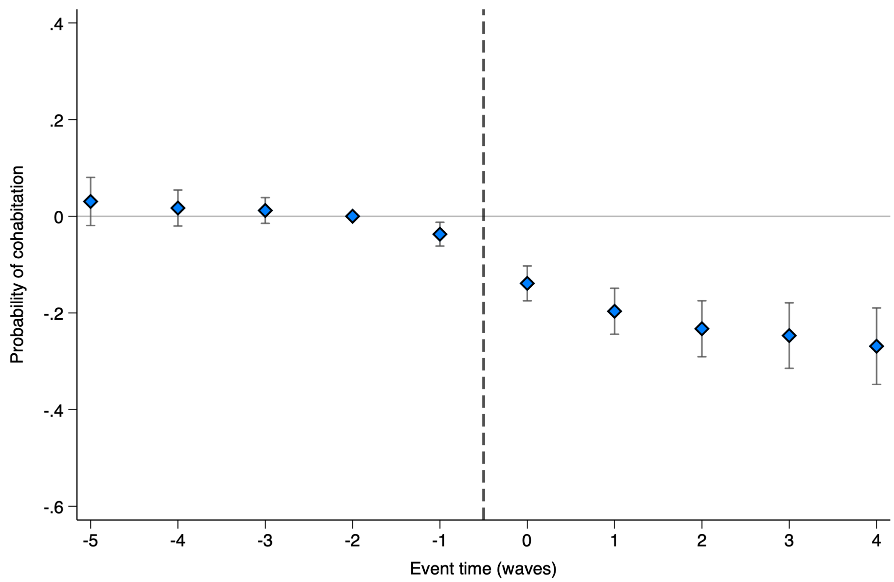
(a) Log (income)



(b) Log (household income)

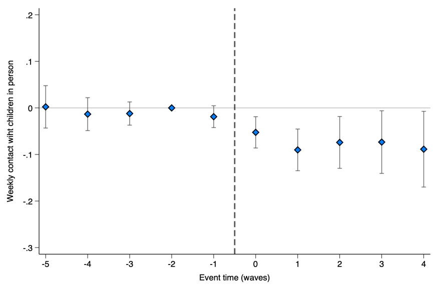
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable includes the log-transformed values of individual and household income, with income defined as total earnings from all sources. A log transformation is applied by adding 1 to any zero values. All monetary figures are adjusted to 2010 values. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

Figure A.29: Grandparenthood effects on probability of cohabitation

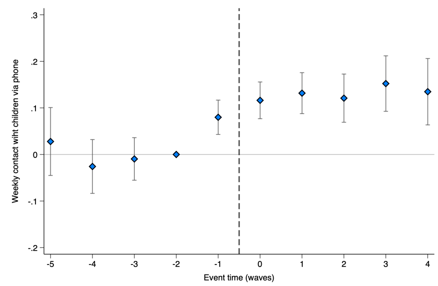


Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator for the probability of cohabiting with children. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors clustered at the individual level.

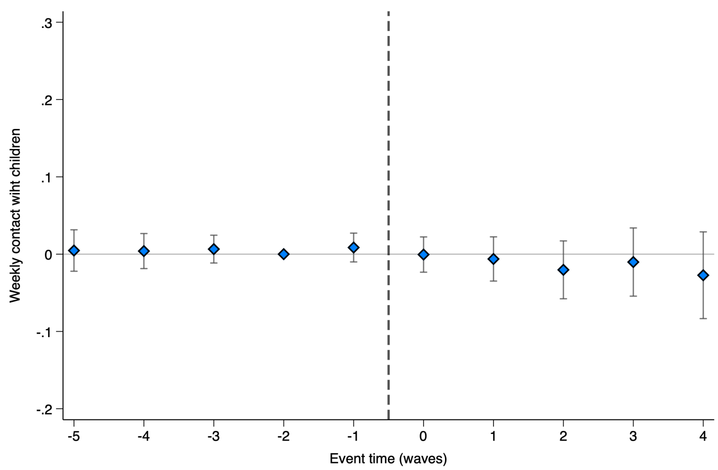
Figure A.30: Grandparenthood effects on the weekly contact with children



(a) Weekly contact in person



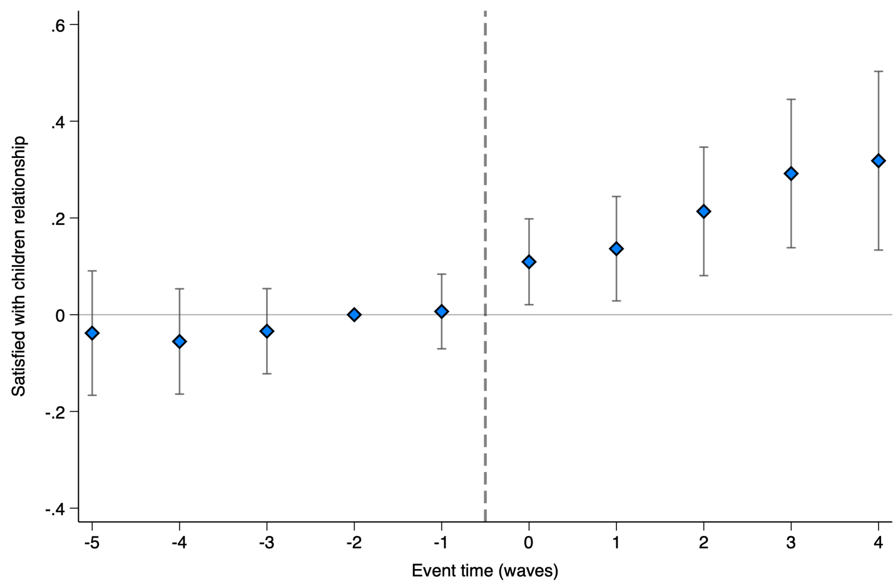
(b) Weekly contact via phone



(c) Weekly contact either in person or via phone

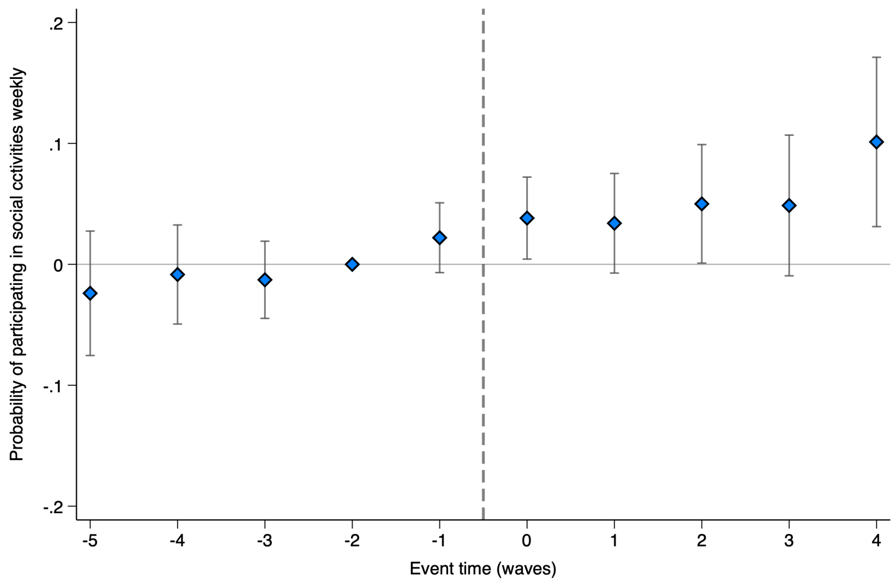
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variables are the probabilities of contacting children in person, by phone, or through any means. The reference period is the wave before pregnancy (event period $t = -2$). The event study controls for wave fixed effects, age fixed effects, and individual characteristics such as gender and education. Standard errors clustered at the individual level.

Figure A.31: Grandparenthood effects on satisfaction in relationships with children



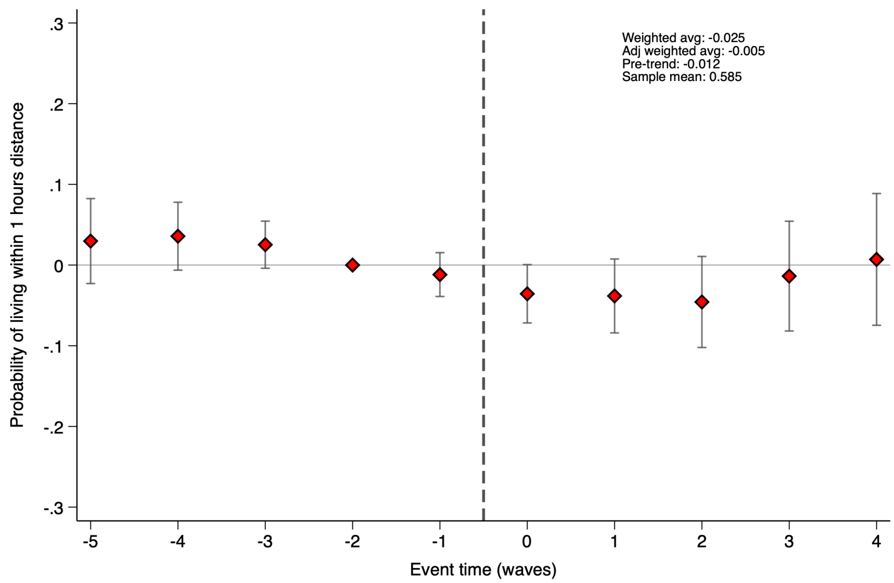
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is satisfaction in relationships with children, rated on a 0–100 scale in increments of 10. This measure is standardized to have a mean of zero and a standard deviation of one, adjusted by wave and gender. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors clustered at the individual level.

Figure A.32: Grandparenthood effects on weekly social activity participation



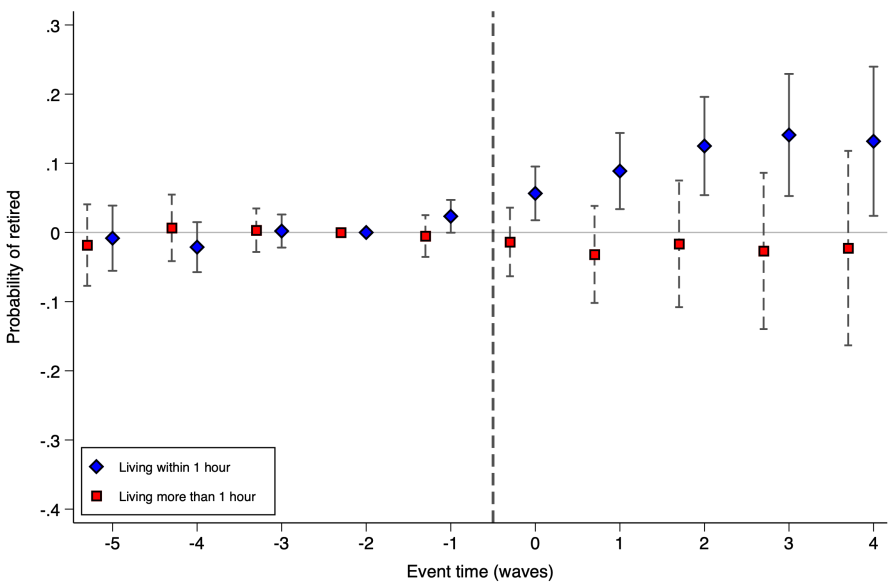
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator for the probability of currently working and the probability of weekly participation in social activities. These social activities include social clubs, leisure, culture, or sports groups, alumni associations, hometown communities, family councils, volunteer groups, political parties, NGOs, interest groups, or other activities. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors clustered at the individual level.

Figure A.33: Grandparenthood effects on probability of living within 1 hour distance



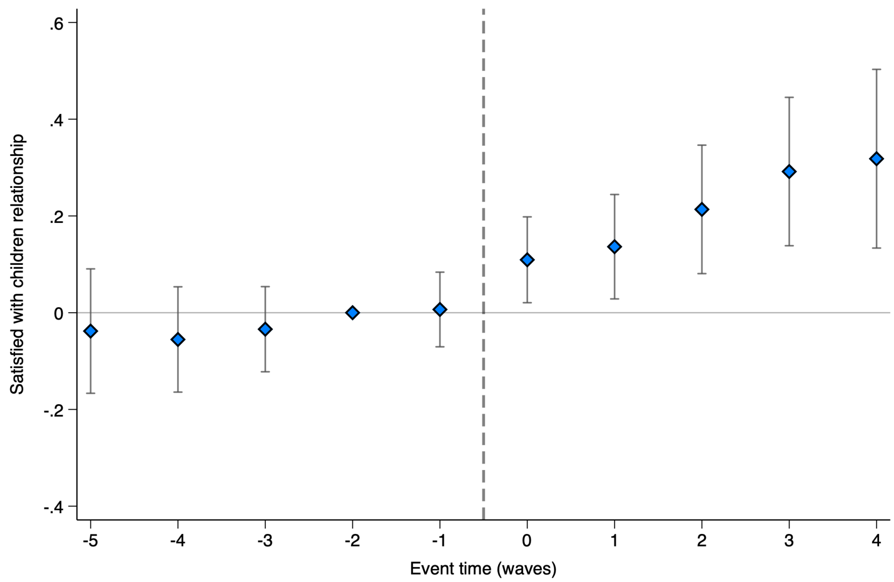
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variables include binary indicators for the probability of living within one hour of distance from the grandchild, which includes cohabitation, living within 30 minutes, and living within one hour. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors clustered at the individual level. The figure also presents the sample mean, the weighted average of post-treatment coefficients (event times 0 to 4), and the pre-trend-adjusted post-treatment average.

Figure A.34: Grandparenthood effects on the probability of retirement by living distance



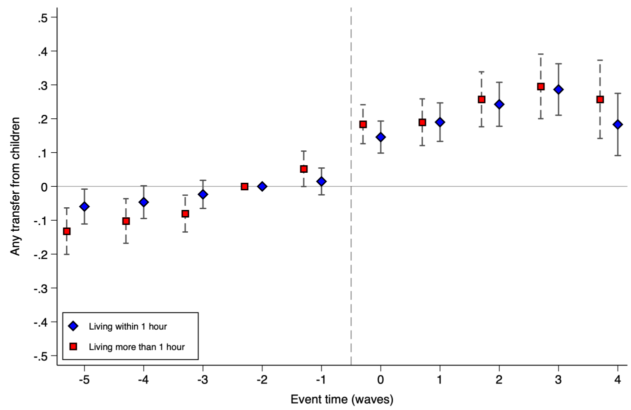
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variables include binary indicators for the probability of retired. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

Figure A.35: Grandparenthood effects on satisfaction by living distance

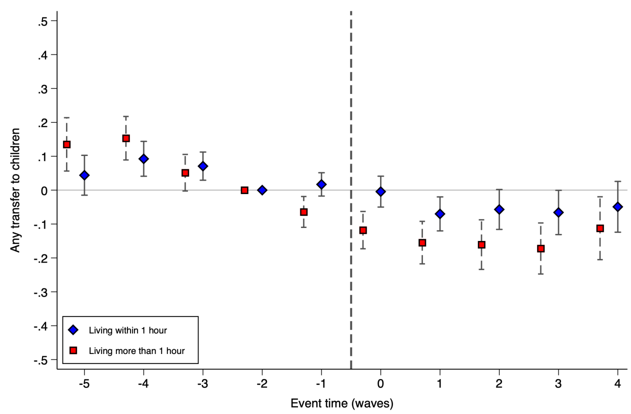


Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is satisfaction in relationships with children, rated on a 0–100 scale in increments of 10. This measure is standardized to have a mean of zero and a standard deviation of one, adjusted by wave and gender. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors clustered at the individual level.

Figure A.36: Grandparenthood effects on financial support by living distance



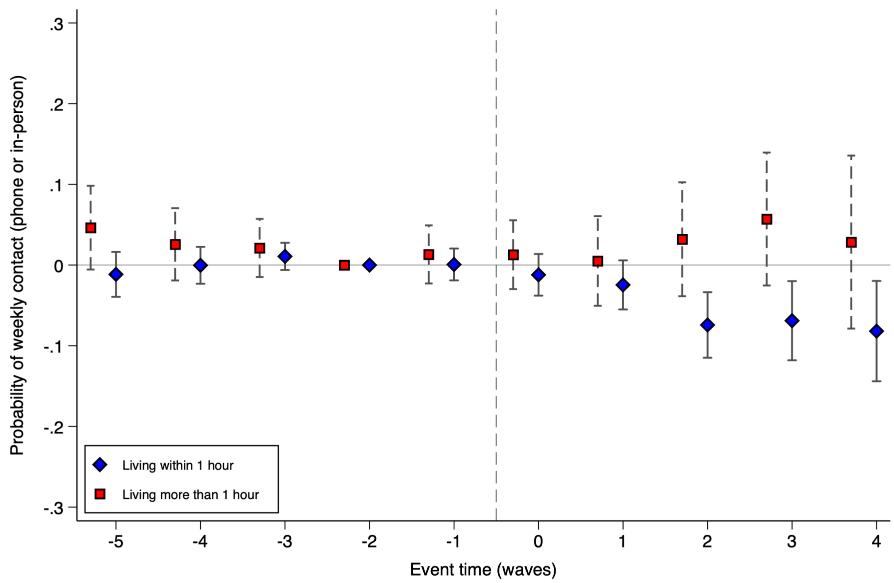
(a) Any transfer from children



(b) Any transfer to children

Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variables include binary indicators for the probability of transferring money from an adult child and the probability of transferring money to an adult child. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

Figure A.37: Grandparenthood effects on weekly contact with children by living distance



Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variables are the probabilities of contacting children either in person or by phone. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors are clustered at the individual level.

A.2 Measures of Mental Health

In this study, I assess mental health using the 10-item Center for Epidemiological Studies Depression (CES-D-10) scale, developed by (Radloff, 1977). The CES-D-10 is a widely validated screening tool used to measure depressive symptoms and has been shown to be appropriate across different racial, gender, and age groups (Weissman et al., 1977). The scale includes two positively phrased items (e.g., feeling pretty good, generally satisfied) and eight negatively phrased items (e.g., loss of interest, trouble concentrating, feeling depressed). Respondents rate each item on a scale from 0 (rarely or less than once per week) to 3 (almost always or 5–7 days per week). The positive items are reverse-scored, and the total score, which ranges from 0 to 30, reflects the respondent's overall level of depressive symptoms. Higher scores indicate a greater likelihood of exhibiting symptoms associated with probable depression. In line with previous research, I use a CES-D-10 cutoff score of 10 to generate a binary measure indicating the likelihood of depressive symptoms (Andresen et al., 1994; Irwin et al., 1999). Individuals with scores of 10 or higher are classified as having a higher probability of depressive symptoms, while those scoring below 10 are less likely to be depressed.

Table A1: CES-D-10 items across KLoSA waves (2006-2018)

2006-2018	I felt depressed I felt that everything I did was an effort I was happy (reverse) My sleep was restless I enjoyed life (reverse) I felt lonely I could not get "going"
2006-2012	I was bothered by things that usually don't bother me I had trouble keeping mind on what I was doing I felt fearful
2014-2018	People were unfriendly I felt sad I felt that people disliked me

*Note:*Items are rated on a scale from 0 to 3: 0 represents very rarely or less than once a day, 1 represents sometimes or 1–2 days during the past week, 2 represents often or 3–4 days during the past week, and 3 represents almost always or 5–7 days during the past week. Positively phrased items are reverse-scored. The total score, ranging from 0 to 30, indicates the level of depressive symptoms, with higher scores reflecting greater distress.

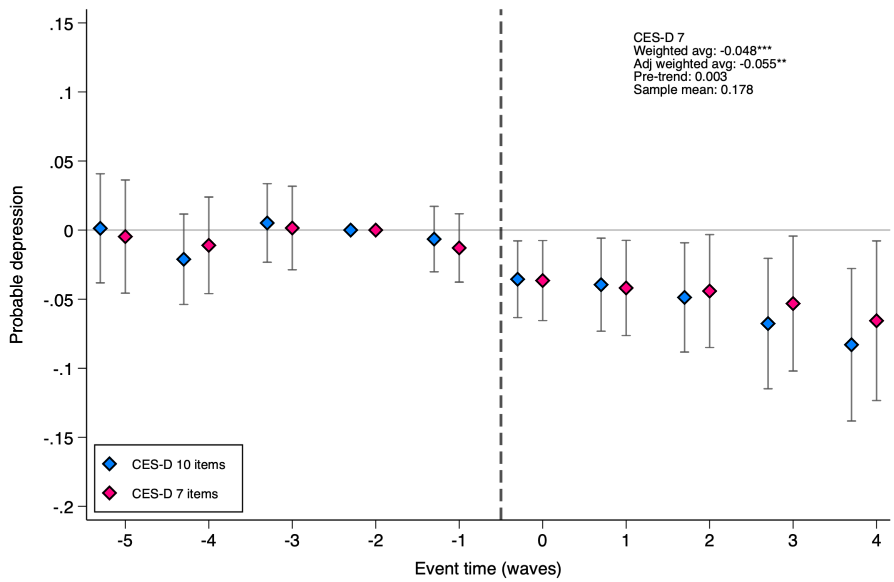
The analysis uses data from the Korean Longitudinal Study of Aging (KLoSA) covering the period from 2006 to 2018. The CES-D-10 scale, which includes ten items as shown in Table A1, is used to assess depressive symptoms. However, only seven of the ten items are consistently included in all survey waves, with three items rotating across different waves. To ensure that the main results are not driven by this variation, I conduct a secondary analysis based solely on the seven items that appear in every wave. The scores for these seven items are summed, and a cutoff score of 8 is applied to assess the likelihood of depressive symptoms (Levine, 2013). This approach maintains consistency across survey waves and minimizes potential biases introduced by changes in the survey structure. Additionally, I conduct an item-level analysis of each CES-D10 question to examine the specific impact of becoming a new grandparent on individual symptoms, such as loneliness, feeling depressed, or restlessness. This analysis allows for a more detailed understanding of how the transition to grandparenthood influences particular aspects of mental health, offering a more nuanced view of the effects on specific symptoms.

To better understand the variation in the CES-D10 questions and its potential impact on the results, I compare how classifications based on the full 10-item CES-D scale align with those from the reduced 7-item scale, as shown in Table A2. The main analysis uses the 10-item CES-D with a cutoff score of 10, while the alternative analysis applies the 7-item version with a cutoff score of 8. The table indicates that 91.08% of individuals identified as probable depressed by the 10-item CES-D are similarly classified by the 7-item scale. Additionally, 95.93% of those not classified as probable depressed by the 10-item CES-D are also not classified by the 7-item scale. This consistency between the two measures reinforces the robustness of the results and minimizes concerns about potential biases due to variation in the CES-D-10 questions.

Table A2: Comparison of CES-D (10 items) and CES-D (7 items) measures

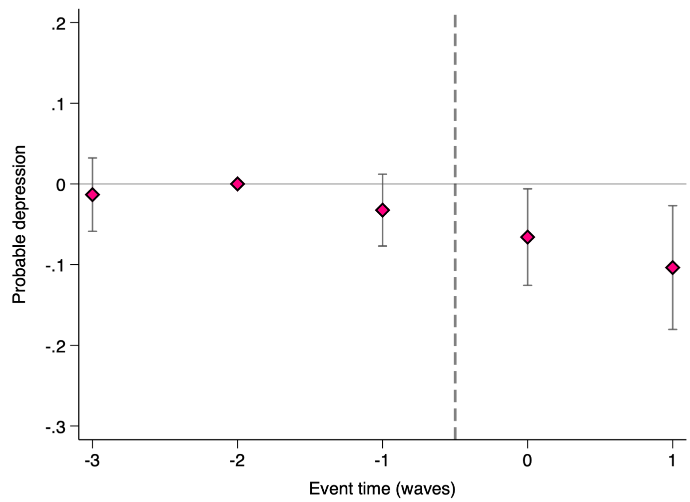
Probable depression	Probable depression (7 items)	
(10 items)	No	Yes
No	95.93%	4.07%
Yes	8.92%	91.08%
Total	82.14%	17.86%

Figure A.38: Mental health effects using CES-D-7 items



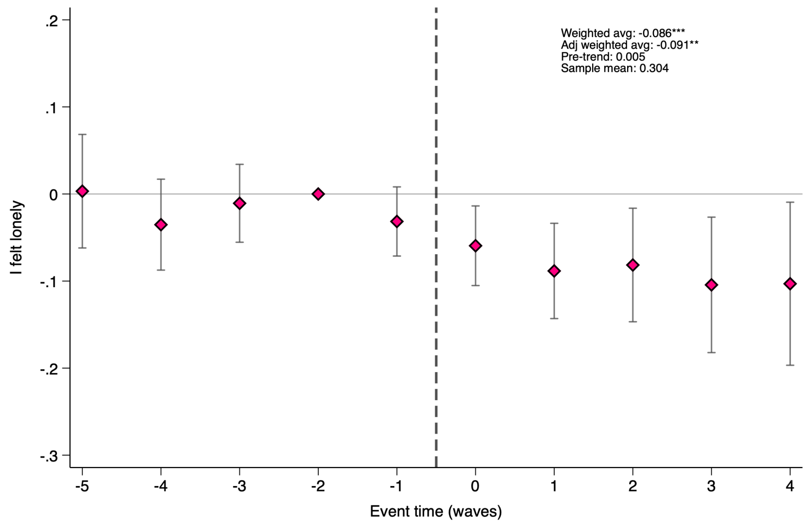
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. The sample includes individuals aged 45–75 who became first-time grandparents during the study period, observed at least once before and once after the transition. The outcome variable is a binary indicator for probable depression, where individuals with a CES-D score of 8 or higher (based on the 7 items consistently included across all waves) are coded as 1 and otherwise coded as 0. The reference period is the wave before pregnancy (event period $t = -2$). Event study models control for wave and age fixed effects, as well as individual characteristics (gender and education). Standard errors are clustered at the individual level. The figure reports the sample mean, the weighted average of post-treatment coefficients (event times 0 to 4), and a pre-trend-adjusted post-treatment mean.

Figure A.39: Grandparenthood effects with balanced sample (CES-D-7 items)

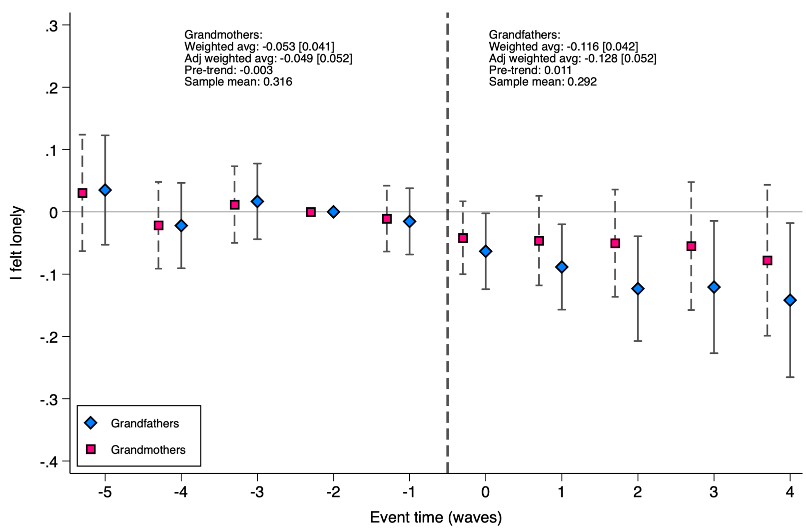


Note: This figure is based on a balanced event-time panel from the Korean Longitudinal Study of Aging (KLoSA), including individuals observed in all five event periods from $t = -3$ to $t = 1$. The reference period is $t = -2$. The outcome variable is a binary indicator for probable depression, where individuals with a CES-D score of 8 or higher (based on the 7 items consistently included across all waves) are coded as 1 and otherwise coded as 0. Estimates are based on the event study specification in Equation 1.1.

Figure A.40: Grandparenthood effects on loneliness



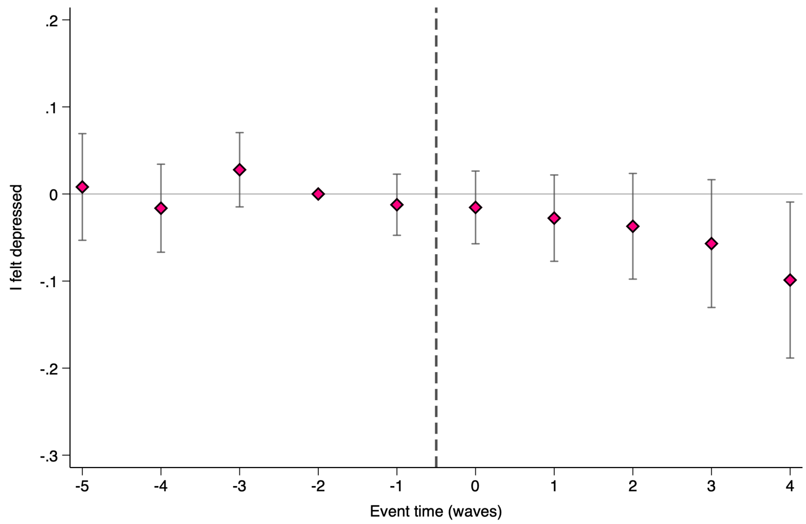
(a) All individuals



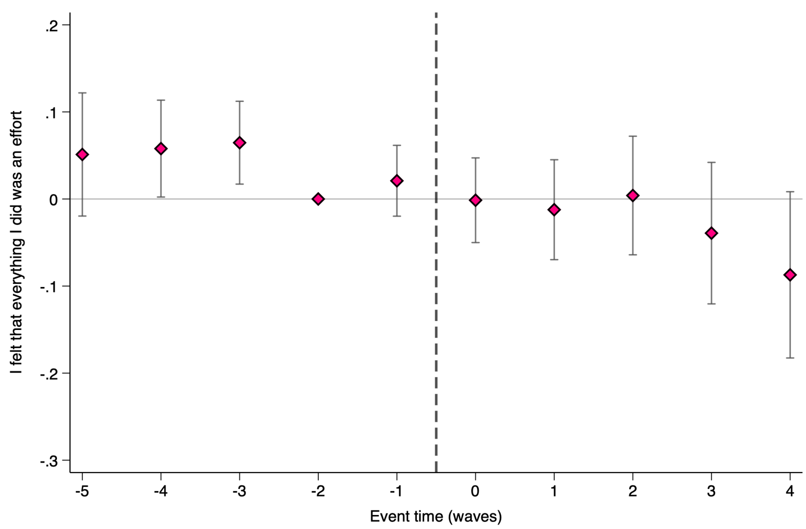
(b) By gender

Note: This figure reports event study estimates for the CESD-10 item "I felt lonely," which ranges from 0 (rarely or none of the time) to 3 (most or all of the time). Estimates are based on the event study specification in Equation 1.1. Panel (a) shows results for the full sample; Panel (b) presents results separately by gender.

Figure A.41: Effects on individual CES-D items (part 1)



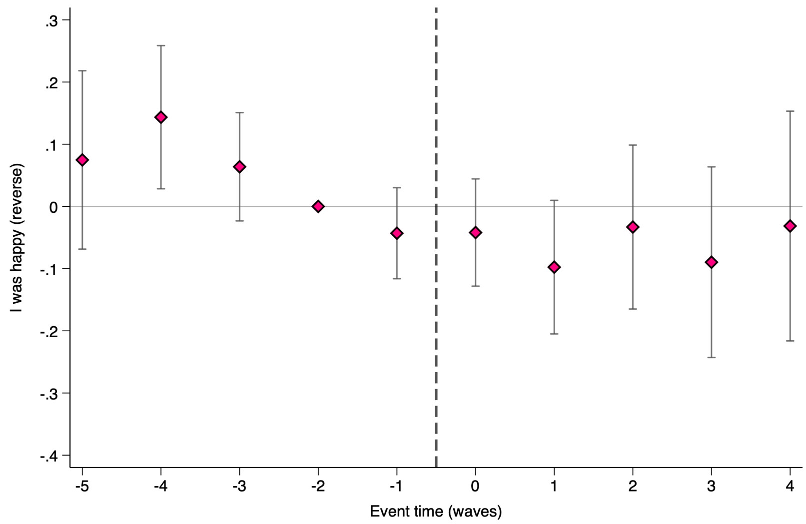
(a) I felt depressed



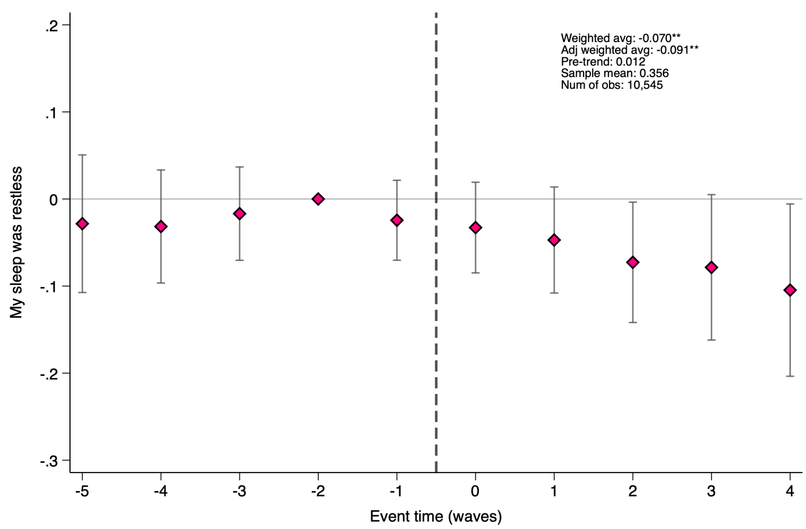
(b) I felt that everything I did was an effort

Note: This figure presents event study estimates for two CES-D items: *I felt depressed* and *I felt that everything I did was an effort*. Each item is rated on a scale from 0 (rarely or none of the time) to 3 (most or all of the time). Estimates are based on the event study specification in Equation 1.1.

Figure A.42: Effects on individual CES-D items (part 2)



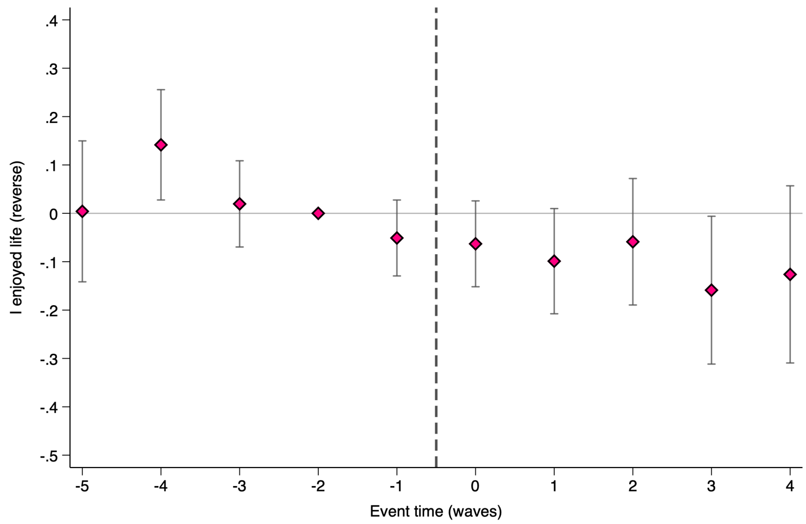
(a) I was happy (reverse-coded)



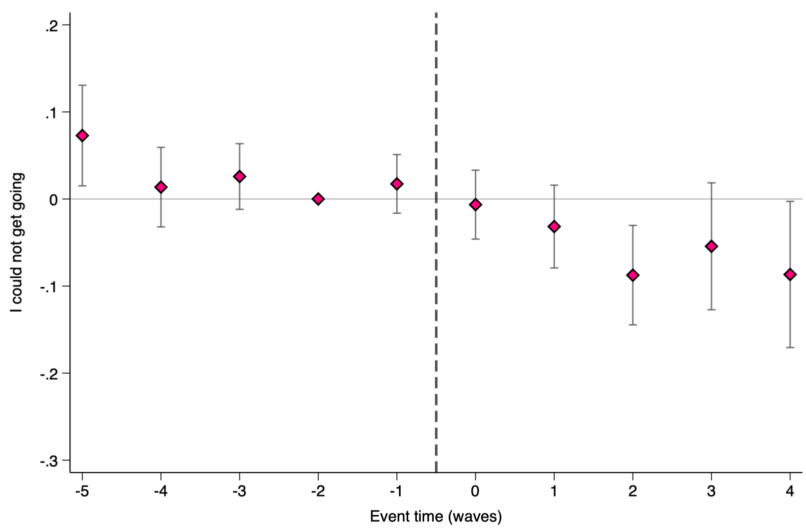
(b) My sleep was restless

Note: This figure presents event study estimates for two CES-D items: *I was happy (reverse-coded)* and *My sleep was restless*. Each item is rated on a scale from 0 (rarely or none of the time) to 3 (most or all of the time). Estimates are based on the event study specification in Equation 1.1.

Figure A.43: Effects on individual CES-D items (part 3)



(a) I enjoyed life (reverse-coded)

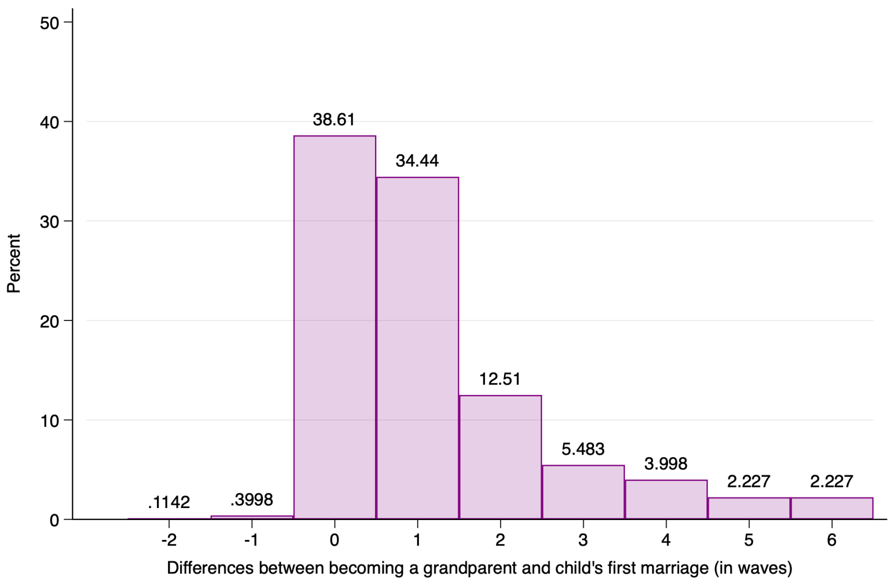


(b) I could not get going

Note: This figure presents event study estimates for two CES-D items: *I enjoyed life (reverse-coded)* and *I could not get going*. Each item is rated on a scale from 0 (rarely or none of the time) to 3 (most or all of the time). Estimates are based on the event study specification in Equation 1.1.

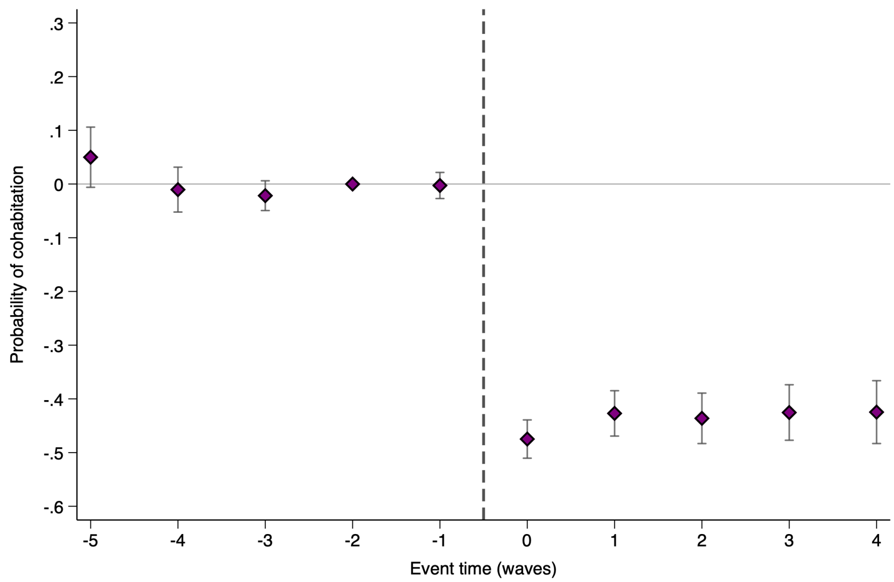
A.3 Impact of Children's Marriage on Mental Health

Figure A.44: Difference between the first marriage of an adult child and the birth of the first grandchild



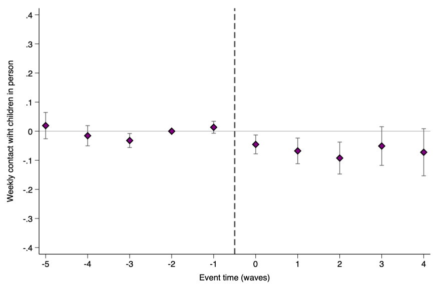
Note: This figure shows the timing difference between the first marriage of an adult child and the birth of the first grandchild, illustrating how the sequence of these events may shape grandparental roles and involvement.

Figure A.45: Effects of first marriage of adult child on cohabitation

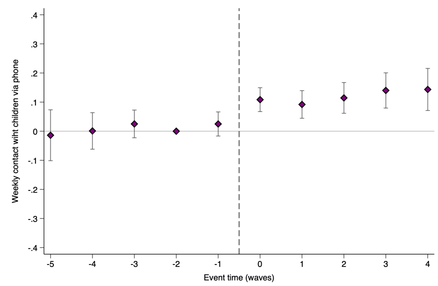


Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator for the probability of cohabiting with children. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors clustered at the individual level.

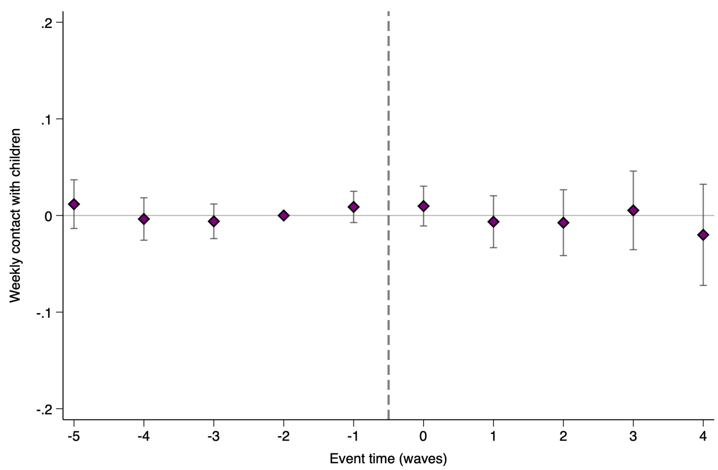
Figure A.46: Effects of the first marriage of an adult child on the weekly contact



(a) Weekly contact in person



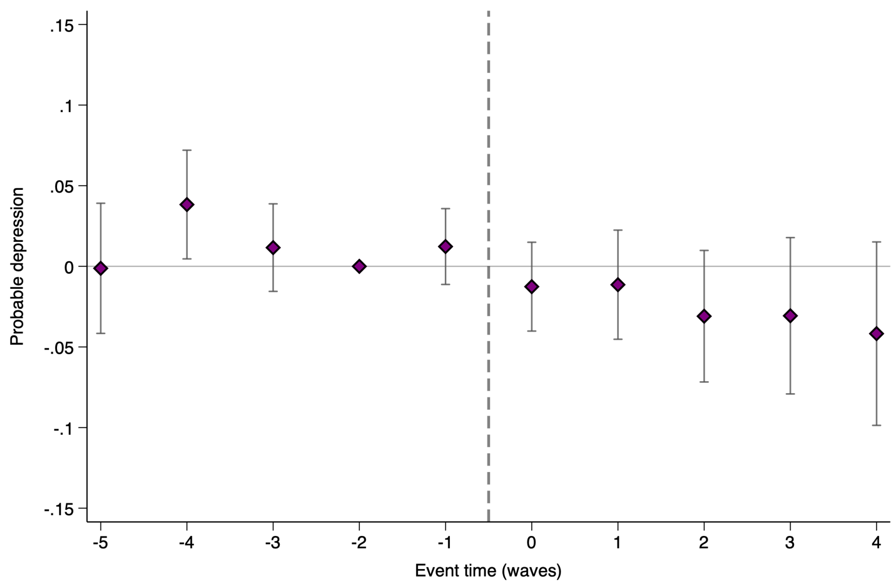
(b) Weekly contact via phone



(c) Weekly contact either in person or via phone

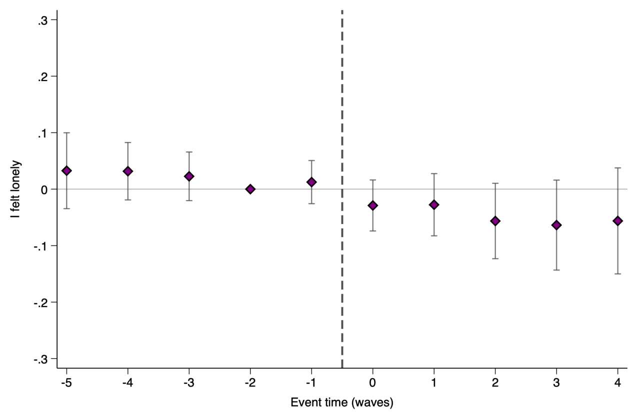
Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variables are the probabilities of contacting children in person, by phone, or through any means. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors clustered at the individual level.

Figure A.47: Effects of first marriage of adult child on probable depression

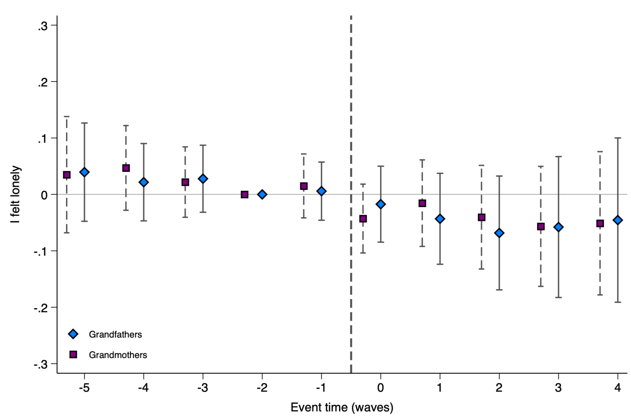


Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variable is a binary indicator for probable depression, where individuals with a CESD-10 score of 10 or higher are coded as 1, and others as 0. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors, clustered at the individual level.

Figure A.48: Effects of first marriage of adult child on loneliness



(a) Loneliness



(b) Loneliness by gender

Note: This figure uses data from the Korean Longitudinal Study of Aging (KLoSA), a biennial panel from 2006–2018. Estimates are based on the event study specification in Equation 1.1. The outcome variables are individual CES-D items: *I felt lonely*. Items are rated on a scale from 0 to 3, where 0 represents very rarely or less than once a day, 1 represents sometimes or 1–2 days during the past week, 2 represents often or 3–4 days during the past week, and 3 represents almost always or 5–7 days during the past week. The reference period is the wave before pregnancy (event period $t = -2$). Standard errors, clustered at the individual level.

Appendix B

Breaking the "Iron Rice Bowl": Health Behavior Effects of Chinese State-Owned Enterprise Reform.

Table B1: Heterogeneous effects of job insecurity

	(1) Current smoker	(2) Cigarettes (0s excluded)	(3) Cigarettes (0s included)	(4) Alcohol drinker	(5) Frequent drinker (0s included)	(6) Heavy drinker (0s included)
Panel A: All population						
LayoffRate × SOE	0.568**	13.740	9.984**	0.204	0.364	0.538*
	(0.230)	(10.202)	(4.963)	(0.276)	(0.308)	(0.280)
LayoffRate	-0.625**	-16.002	-10.238*	-0.031	-0.218	-0.112
	(0.316)	(12.357)	(5.912)	(0.341)	(0.403)	(0.328)
Mean	0.365	15.649	5.552	0.465	0.255	0.108
Number of obs	2814	955	2800	2819	2855	2611
R-squared	0.813	0.667	0.776	0.679	0.489	0.508
E(Loss) × SOE	0.699**	17.862	12.548**	0.252	0.441	0.660*
	(0.283)	(12.631)	(6.197)	(0.343)	(0.382)	(0.345)
E(Loss)	-0.740*	-16.733	-11.785*	-0.021	-0.235	-0.185
	(0.384)	(14.916)	(7.129)	(0.411)	(0.484)	(0.390)
Mean	0.365	15.649	5.552	0.465	0.255	0.108
Number of obs	2814	955	2800	2819	2855	2611
R-squared	0.813	0.667	0.776	0.679	0.489	0.508
Panel B: Men						
LayoffRate × SOE	0.988**	14.047	18.080*	0.918**	1.110**	1.047**
	(0.436)	(10.238)	(9.738)	(0.417)	(0.527)	(0.521)
LayoffRate	-1.068*	-16.368	-17.644*	-0.006	-0.317	-0.169
	(0.544)	(12.421)	(10.630)	(0.496)	(0.657)	(0.572)
Mean	0.668	15.646	10.225	0.730	0.388	0.191
Number of obs	1523	952	1510	1538	1556	1385
R-squared	0.660	0.668	0.671	0.540	0.437	0.496
E(Loss) × SOE	1.211**	18.252	22.808*	1.164**	1.392**	1.300**
	(0.538)	(12.677)	(12.192)	(0.519)	(0.654)	(0.647)
E(Loss)	-1.297*	-17.215	-20.508	-0.026	-0.379	-0.272
	(0.663)	(15.003)	(12.877)	(0.599)	(0.792)	(0.682)
Mean	0.668	15.646	10.225	0.730	0.388	0.191
Number of obs	1523	952	1510	1538	1556	1385
R-squared	0.660	0.668	0.671	0.540	0.437	0.496

Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. The reform period includes the 1997 and 2000 waves. All specifications follow the baseline model in equation 2.6 and include individual, province, and year fixed effects. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Robustness to province-by-year fixed effects

	(1) Current smoker	(2) Cigarettes (os excluded)	(3) Cigarettes (os included)	(4) Alcohol drinker	(5) Frequent drinker (os included)	(6) Heavy drinker (os included)
Panel A: All population						
Reform × SOE	0.061**	1.927**	1.174**	0.022	0.050	0.051*
	(0.024)	(0.970)	(0.485)	(0.029)	(0.032)	(0.029)
Mean	0.365	15.649	5.552	0.465	0.255	0.108
Number of obs	2814	955	2800	2819	2855	2611
R-squared	0.814	0.681	0.778	0.689	0.501	0.521
Panel B: Men						
Reform × SOE	0.096**	1.935**	1.980**	0.108**	0.140***	0.111**
	(0.045)	(0.972)	(0.913)	(0.042)	(0.052)	(0.051)
Mean	0.668	15.646	10.225	0.730	0.388	0.191
Number of obs	1523	952	1510	1538	1556	1385
R-squared	0.665	0.681	0.677	0.556	0.456	0.522

Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. The reform period includes the 1997 and 2000 waves. All regressions include individual and province-by-year fixed effects. Standard errors clustered at the individual level. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Robustness to province-specific linear trends

	(1) Current smoker	(2) Cigarettes (0s excluded)	(3) Cigarettes (0s included)	(4) Alcohol drinker	(5) Frequent drinker (0s included)	(6) Heavy drinker (0s included)
Panel A: All population						
Reform × SOE	0.055**	2.086**	1.125**	0.020	0.053*	0.045
	(0.024)	(0.972)	(0.483)	(0.028)	(0.031)	(0.029)
Mean	0.365	15.649	5.552	0.465	0.255	0.108
Number of obs	2814	955	2800	2819	2855	2611
R-squared	0.813	0.671	0.777	0.684	0.494	0.515
Panel B: Men						
Reform × SOE	0.085*	2.123**	1.933**	0.100**	0.146***	0.094*
	(0.045)	(0.974)	(0.910)	(0.042)	(0.051)	(0.051)
Mean	0.668	15.646	10.225	0.730	0.388	0.191
Number of obs	1523	952	1510	1538	1556	1385
R-squared	0.661	0.672	0.674	0.549	0.447	0.509

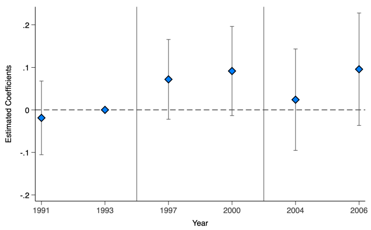
Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. The reform period includes the 1997 and 2000 waves. All regressions include individual, year, and province fixed effects, as well as province-specific linear time trends. Standard errors are clustered at the individual level. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Robustness to alternative age restrictions

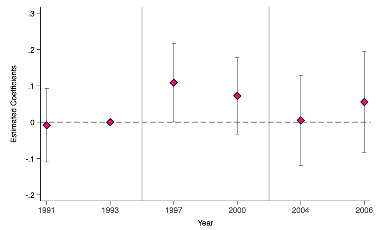
	(1) Current smoker	(2) Cigarettes (os excluded)	(3) Cigarettes (os included)	(4) Alcohol drinker	(5) Frequent drinker (os included)	(6) Heavy drinker (os included)
Panel A: All population						
Reform × SOE	0.071***	1.225	1.199**	0.014	0.042	0.044
	(0.025)	(1.179)	(0.513)	(0.034)	(0.036)	(0.032)
Mean	0.351	15.456	5.224	0.436	0.249	0.097
Number of obs	1935	623	1918	1936	1962	1791
R-squared	0.838	0.676	0.781	0.673	0.485	0.524
Panel B: Men						
Reform × SOE	0.139***	1.245	2.322**	0.130**	0.152**	0.069
	(0.048)	(1.183)	(1.037)	(0.052)	(0.063)	(0.063)
Mean	0.699	15.450	10.502	0.731	0.401	0.188
Number of obs	960	620	944	966	978	853
R-squared	0.676	0.677	0.664	0.533	0.428	0.515

Note: Each column reports estimates from separate DiD regressions using CHNS data from 1991 to 2000. Outcomes are listed in the column headers. The reform period includes the 1997 and 2000 waves. All regressions follow the baseline specification in equation 2.1 and include individual, province, and year fixed effects. The sample is restricted to individuals aged 20 to 45, representing the prime working-age population. Standard errors are clustered at the individual level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

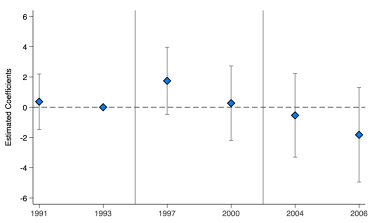
Figure B.1: Event-Study estimates (male sample)



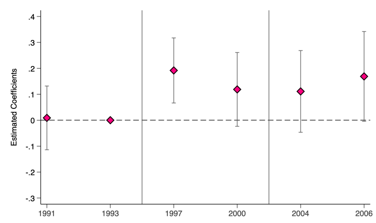
(a) Current smoking



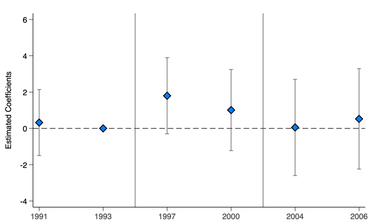
(b) Any alcohol use



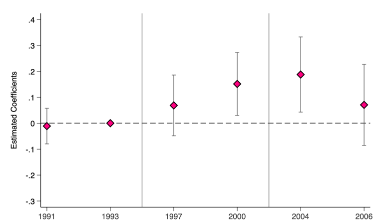
(c) Cigarettes per day (without os)



(d) Frequent drinking



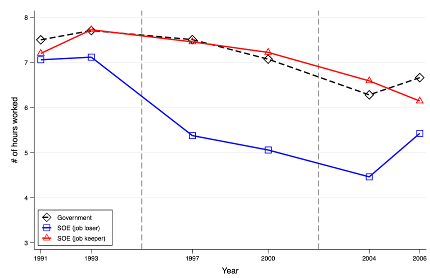
(e) Cigarettes per day (with os)



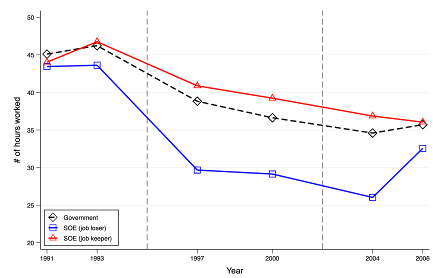
(f) Heavy drinking

Notes: This figure reports event study estimates from Equation 2.2 for the male subsample, using 1993 as the reference year. All models include individual, year, and province fixed effects. Standard errors are clustered at the individual level. Vertical lines mark the SOE reform period from 1995 to 2002.

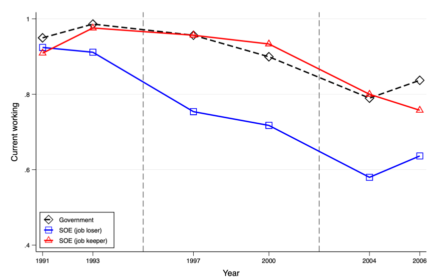
Figure B.2: Trends in labor market outcomes



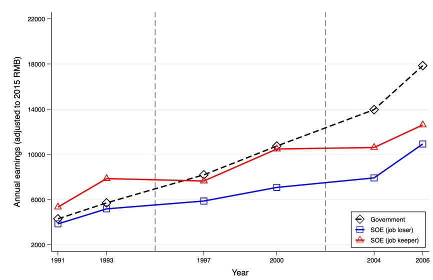
(a) Work hours per day (with os)



(b) Work hours per week (with os)



(c) Employment



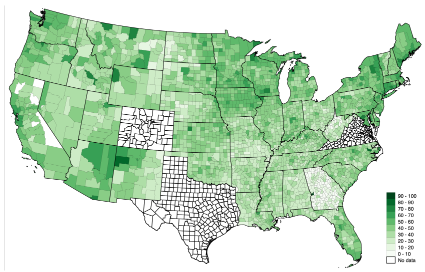
(d) Annual income

Note: The above figures show average labor market outcomes for SOE workers and Government workers, using data from the China Nutrition and Health Survey (CHNS) 1991-2006. The solid line represents the average outcomes for SOE workers and the dashed line corresponds to the average outcomes for government workers. All monetary values are adjusted to 2015 RMB.

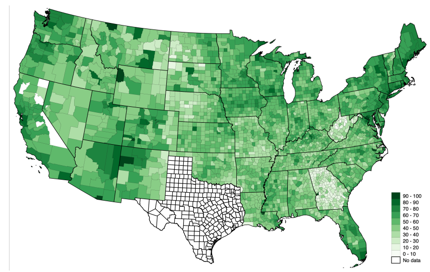
Appendix C

The Lasting Impact of the Tuskegee Syphilis Study: COVID-19 Vaccination Hesitation among African Americans with Yang Jiao, Leilei Shen and Zhuo Chen

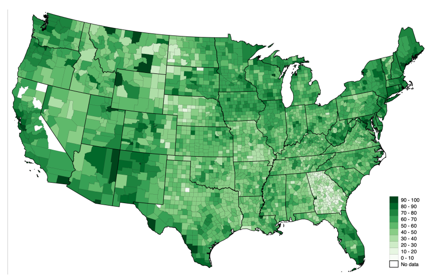
Figure C.1: COVID-19 vaccine rates by county and over time



(a) May 17–23, 2021



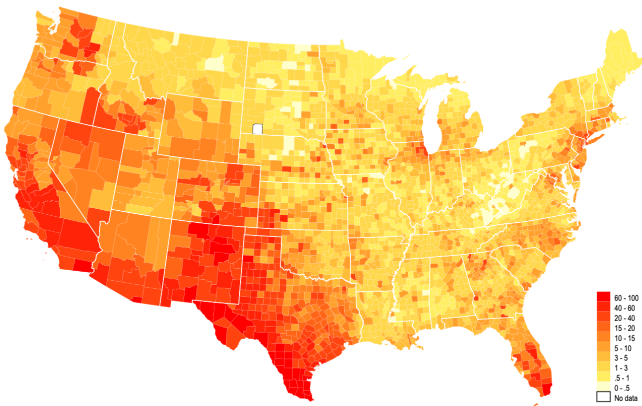
(b) October 4–10, 2021



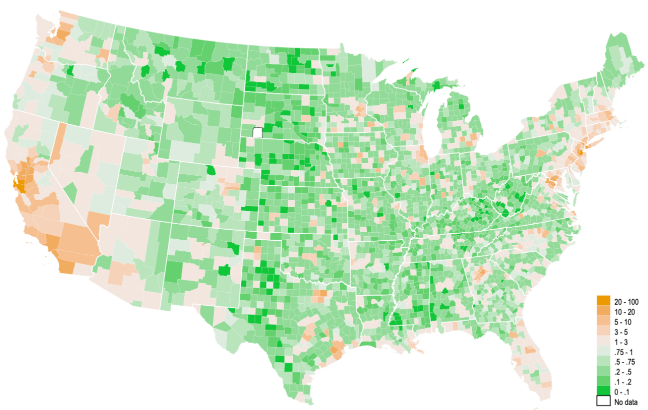
(c) December 13–19, 2021

Data source: U.S. Centers for Disease Control and Prevention.
Note: Darker shaded areas represent counties with higher vaccination coverage. The CDC caps reported county-level vaccination coverage at 95 percent in the COVID Data Tracker.

Figure C.2: Geographic distribution of Hispanic and Asian population



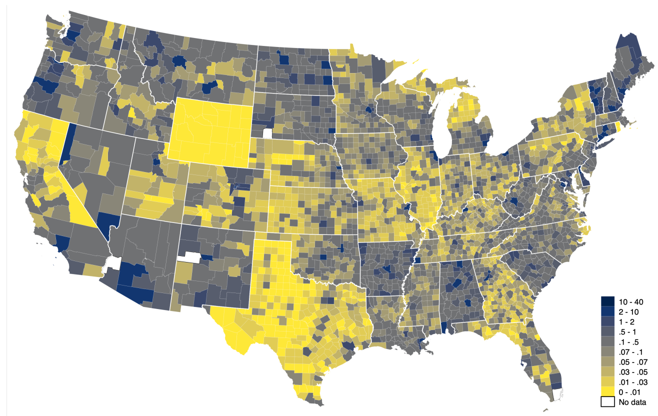
(a) Share of Hispanic population, 2010



(b) Share of Asian population, 2010

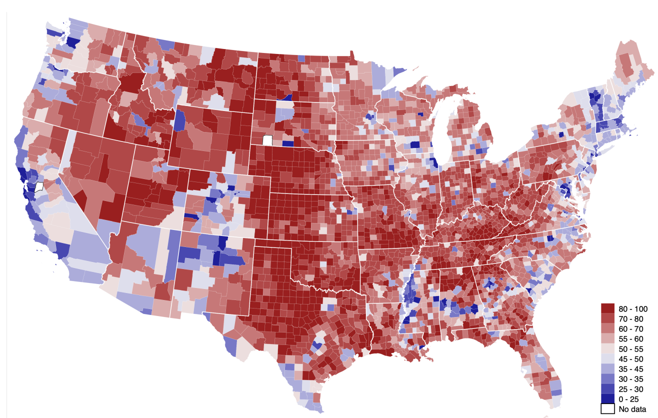
Data source: U.S. Census Bureau, 2010 Census Redistricting Data.

Figure C.3: County exposure to Tuskegee news



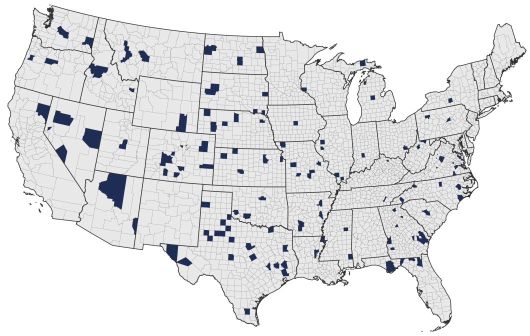
*Note:*Exposure to Tuskegee news is computed based on Equation 2.

Figure C.4: 2020 U.S. Presidential Election map by county & vote share

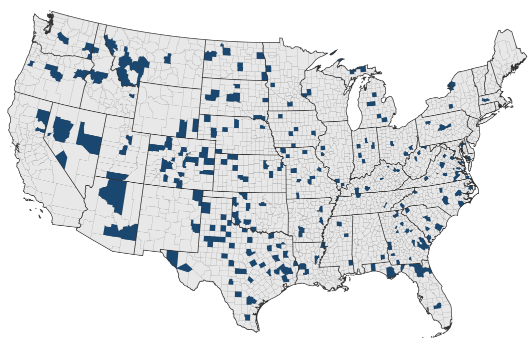


Note: This figure shows the county-level and vote share results of the 2020 U.S. Presidential Election. The darker the blue, the more Democratic a county voted, and the darker the red, the more Republican a county voted.

Figure C.5: Geographical mobility in the past year for current residence



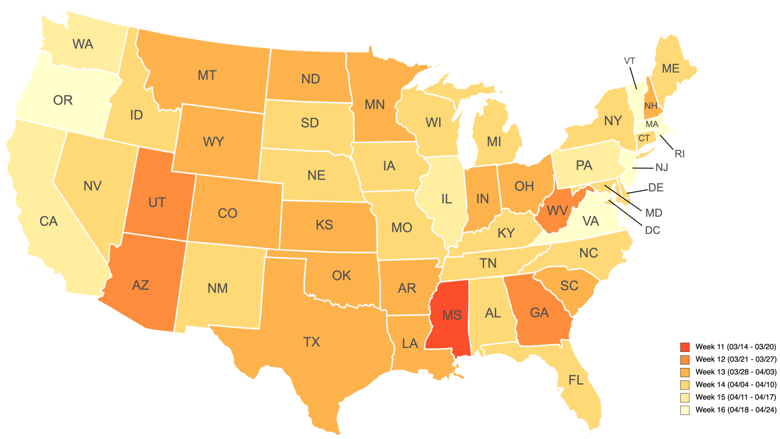
(a) Top 5% of counties by resident mobility in the past year



(b) Top 10% of counties by resident mobility in the past year

Data source: 2021 American Community Survey (ACS).
Note: Darker shaded areas represent counties with higher levels of resident mobility in the past year.

Figure C.6: Earliest date of vaccine eligibility for adults age 16 and older by state



Note: Information of COVID-19 vaccination rates and distribution plans by state is collected from vaccine distribution plan.

Table C1: Summary statistics - vaccine coverage

Variable	Mean	Std. Dev.	Min.	Max.	N
Percentage of population age 18 and older receiving at least one dose					
01/30/2021(week 4)	7.69	4.86	0.00	46.80	2643
02/27/2021(week 8)	17.29	7.34	0.00	73.50	2628
03/27/2021(week 12)	30.22	10.04	0.70	95.00	2549
04/24/2021(week 16)	40.33	12.99	0.90	95.00	2437
05/29/2021(week 21)	45.72	14.73	3.20	95.00	2388
06/26/2021(week 25)	48.40	14.80	4.10	95.00	2312
07/31/2021(week 30)	51.36	15.16	5.60	95.00	2350
08/28/2021(week 34)	54.94	15.51	7.30	95.00	2437
09/25/2021(week 38)	58.06	15.21	7.70	95.00	2619
10/30/2021(week 43)	60.65	15.04	8.00	95.00	2966
11/27/2021(week 47)	62.58	15.21	11.20	95.00	2960
12/31/2021(week 52)	65.63	14.09	11.90	95.00	2900
Percentage of population age 18 and older fully vaccinated					
01/30/2021(week 4)	1.68	1.40	0.00	25.40	2643
02/27/2021(week 8)	8.53	4.28	0.00	56.50	2643
03/27/2021(week 12)	18.40	7.04	0.50	81.30	2643
04/24/2021(week 16)	30.89	10.26	0.60	95.00	2645
05/29/2021(week 21)	39.80	13.21	1.70	95.00	2645
06/26/2021(week 25)	42.85	13.94	3.00	95.00	2645
07/31/2021(week 30)	45.06	14.27	4.00	95.00	2645
08/28/2021(week 34)	47.42	14.46	4.10	95.00	2709
09/25/2021(week 38)	50.79	14.13	4.60	95.00	2842
10/30/2021(week 43)	53.54	14.03	4.70	95.00	3095
11/27/2021(week 47)	54.74	14.19	5.20	95.00	3095
12/31/2021(week 52)	56.79	13.72	10.30	95.00	3083

Note: CDC has capped estimates of vaccination coverage at 95 percent.

Table C2: Impact of Tuskegee Study on vaccination rate (non-linear specification)

	Baseline results		County-month fixed effect		County × week linear trend	
	At least one dose	Fully vaccinated	At least one dose	Fully vaccinated	At least one dose	Fully vaccinated
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Black × distance × week	0.161*	0.167**	0.158*	0.153**	0.194***	0.192***
	(0.090)	(0.079)	(0.086)	(0.071)	(0.068)	(0.054)
Share of Black × week	-1.201***	-1.542***	-0.940**	-1.452***	-0.525	-0.750**
	(0.428)	(0.424)	(0.391)	(0.369)	(0.408)	(0.306)
Distance × week	-0.006	-0.004	-0.002	-0.002	0.011**	0.012**
	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Observations	134,259	143,666	133,971	143,533	133,971	143,533
R-squared	0.966	0.971	0.999	0.999	0.999	0.999
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes	No	No
Share of Hispanic × week	Yes	Yes	Yes	Yes	No	No
Share of White^2 × week	Yes	Yes	Yes	Yes	No	No
Share of Hispanic^2 × week	Yes	Yes	Yes	Yes	No	No
Share of HS grads and above × week	Yes	Yes	Yes	Yes	No	No
County unemployment rate × week	Yes	Yes	Yes	Yes	No	No
County-month FE	No	No	Yes	Yes	Yes	Yes
County × week linear trend	No	No	No	No	Yes	Yes
County^2 × week	No	No	No	No	Yes	Yes

Note: The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * p < 0.01; ** p < 0.05; *** p < 0.01.

Table C3: Impact of Tuskegee Study on vaccination rate (residential segregation)

	At least one dose			Fully vaccinated		
	Bottom 25%	Top 25%	Cross-Model Difference (p)	Bottom 25%	Top 25%	Cross-Model Difference (p)
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Black × distance × week	0.290**	0.257***	0.530	0.252**	0.246***	0.427
	(0.122)	(0.071)		(0.106)	(0.062)	
Share of Black × week	-0.241	-2.037***	0.013	-0.045	-2.210***	0.017
	(0.581)	(0.453)		(0.534)	(0.486)	
Distance × week	0.000	-0.004	0.647	0.006	-0.006	0.274
	(0.013)	(0.004)		(0.013)	(0.004)	
Observations	21,879	23,310		22,935	24,388	
R-squared	0.958	0.973		0.955	0.978	
County FE	Yes	Yes		Yes	Yes	
Week FE	Yes	Yes		Yes	Yes	
Share of White × week	Yes	Yes		Yes	Yes	
Share of Hispanic × week	Yes	Yes		Yes	Yes	
Share of HS grads and above × week	Yes	Yes		Yes	Yes	
County unemployment rate × week	Yes	Yes		Yes	Yes	

Note: The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. The segregation index between Black and White county residents with higher values indicates more residential segregation. Columns 1 and 4 show those using the sub-sample with counties on the bottom 25% of the distribution of residential segregation. Columns 2 and 5 show those using the sub-sample with counties in the top 25% of the distribution of residential segregation. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * p < 0.01; ** p < 0.05; *** p < 0.01.

Table C4: Robustness checks: constructing differential Black share

	Baseline (1)	1990 (2)	1995 (3)	2000 (4)	2005 (5)
Panel A: At least one dose					
Share of Black × distance × week	0.160*	0.136*	0.140*	0.144*	0.154*
	(0.087)	(0.074)	(0.076)	(0.079)	(0.081)
Share of Black × week	-1.200***	-0.563**	-0.656**	-0.802***	-0.979***
	(0.383)	(0.244)	(0.249)	(0.261)	(0.312)
Distance × week	-0.006	-0.002	-0.002	-0.003	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	134,259	134,237	134,237	134,259	134,259
R-squared	0.966	0.966	0.966	0.966	0.966
Panel B: Fully vaccinated					
Share of Black × distance × week	0.164**	0.143**	0.147**	0.148**	0.160**
	(0.077)	(0.066)	(0.068)	(0.071)	(0.072)
Share of Black × week	-1.442***	-0.801**	-0.886***	-1.022***	-1.206***
	(0.398)	(0.318)	(0.321)	(0.322)	(0.353)
Distance × week	-0.005	-0.001	-0.002	-0.002	-0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	143,666	143,644	143,644	143,666	143,666
R-squared	0.970	0.970	0.970	0.970	0.970
County FE	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes	Yes
Share of Hispanic × week	Yes	Yes	Yes	Yes	Yes
Share of HS grads and above × week	Yes	Yes	Yes	Yes	Yes
County unemployment rate × week	Yes	Yes	Yes	Yes	Yes

Note: The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * $p < 0.01$; ** $p < 0.05$; *** $p < 0.01$.

Table C5: Robustness checks: impact of migration

	Baseline		Excluding top 5 percentile		Excluding top 10 percentile	
	At least one dose	Fully vaccinated	At least one dose	Fully vaccinated	At least one dose	Fully vaccinated
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Black × distance × week	0.160*	0.164**	0.159*	0.165**	0.158*	0.163**
	(0.087)	(0.077)	(0.087)	(0.076)	(0.087)	(0.077)
Share of Black × week	-1.200***	-1.442***	-1.207***	-1.453***	-1.201***	-1.442***
	(0.383)	(0.398)	(0.387)	(0.401)	(0.389)	(0.404)
Distance × week	-0.006	-0.005	-0.006	-0.005	-0.006	-0.005
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
Observations	134,259	143,666	128,755	137,733	122,996	131,566
R-squared	0.966	0.970	0.967	0.971	0.967	0.971
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes	Yes	Yes
Share of Hispanic × week	Yes	Yes	Yes	Yes	Yes	Yes
Share of HS grads and above × week	Yes	Yes	Yes	Yes	Yes	Yes
County unemployment rate × week	Yes	Yes	Yes	Yes	Yes	Yes

Note: The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * $p < 0.01$; ** $p < 0.05$; *** $p < 0.01$.

Table C6: Ruling out supply side confounding effects

	At least one dose			Fully vaccinated		
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Black × distance × week	0.144*	0.139*	0.139*	0.148**	0.143**	0.144**
	(0.081)	(0.079)	(0.079)	(0.071)	(0.069)	(0.069)
Share of Black × week	-1.013***	-0.905***	-0.896***	-1.259***	-1.131***	-1.081***
	(0.324)	(0.323)	(0.333)	(0.329)	(0.331)	(0.345)
Distance × week	-0.001	-0.002	-0.002	-0.000	-0.001	-0.002
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	134,251	134,251	134,251	143,658	143,658	143,658
R-squared	0.967	0.968	0.968	0.971	0.972	0.972
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Share of White × week	Yes	Yes	Yes	Yes	Yes	Yes
Share of Hispanic × week	Yes	Yes	Yes	Yes	Yes	Yes
Share of HS grads and above × week	Yes	Yes	Yes	Yes	Yes	Yes
County unemployment rate × week	Yes	Yes	Yes	Yes	Yes	Yes
Pharmacies × week	Yes	Yes	Yes	Yes	Yes	Yes
Primary physicians × week	No	Yes	Yes	No	Yes	Yes
Nurses × week	No	No	Yes	No	No	Yes

Note: The distance is scaled by dividing the raw distance by 100 miles for interpretation purposes. The number of pharmaceutical sites, primary physicians, and nurses are normalized by the county population. All regressions are weighted by total population age 18 and older at the county level and standard errors in parentheses are clustered at the state level. * $p < 0.01$; ** $p < 0.05$; *** $p < 0.01$.

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