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Mental Health Over the Life Course: Evidence from Shocks and Interventions

Andika Ridha Ayu Perdana



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PhD in Economics

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Abstract

This dissertation investigates the long-term psychological effects of both environmental shocks and public health interventions, using Indonesia as a case study. Drawing on rich panel data from the Indonesian Family Life Survey, the three essays employ quasi-experimental methods to identify causal impacts on mental health across the life course. The first chapter examines the 2006 Yogyakarta earthquake, finding that exposure to higher earthquake intensity leads to persistent declines in mental health, especially among vulnerable populations. The second chapter evaluates the long-term effects of Indonesia's Village Midwife Program, revealing that early-life exposure to trained midwives is associated with improved adult mental health. The third chapter explores the consequences of early-life exposure to toxic smoke from the 1997 forest fires, documenting increased risks of psychological distress in adulthood among early-life exposed cohorts. These papers contribute new evidence on the long-run mental health consequences of both adverse and preventive events in a lower-middle-income country setting. Taken together, the findings emphasize the importance of integrating mental health considerations into disaster response, early-life health interventions, and broader development planning.

Keywords: mental health, environmental shocks, early-life exposure, public health intervention, health economics, human capital.

Salut Mental al Llarg del Cicle Vital: Evidència de Xocs i Intervencions

Resum

Aquesta tesi doctoral analitza els efectes psicològics a llarg termini d'esdeveniments ambientals adversos i d'intervencions en salut pública, utilitzant Indonèsia com a cas d'estudi. Parteix de la idea que la salut mental és un component essencial del capital humà i un determinant fonamental del benestar, la productivitat i el desenvolupament econòmic. Malgrat això, continua estant poc integrada en les polítiques públiques, especialment als països en desenvolupament. Amb dades longitudinals de l'Indonesian Family Life Survey, la tesi emprà mètodes quasi-experimentals per identificar efectes causals sobre la salut mental al llarg del cicle vital. Les dades es combinen amb fonts administratives i geoespacials per captar l'exposició a xocs i polítiques de manera precisa.

El primer capítol analitza l'impacte del terratrèmol de Yogyakarta de 2006. A partir d'una estratègia de diferències en diferències, es constata que les persones exposades a una intensitat sísmica més elevada pateixen un deteriorament significatiu i persistent de la salut mental, especialment entre col·lectius vulnerables com la gent gran o les llars amb pocs recursos. El segon capítol examina el Programa de de llevadores de poble, una política pública de salut impulsada a principis dels anys noranta per millorar la salut materna i infantil en zones rurals. L'estudi demostra que néixer en comunitats amb accés a una comare formada redueix el risc de malestar psicològic a l'edat adulta, a través de millores en la salut en les primeres etapes de vida. Finalment, el tercer capítol se centra en els incendis forestals de 1997, un dels desastres ambientals més greus de la regió, i evidencia que l'exposició al fum tòxic durant la infància incrementa significativament el risc de patir trastorns de salut mental en l'edat adulta.

En conjunt, la tesi aporta nova evidència sobre l'impacte a llarg termini dels xocs ambientals i de les polítiques públiques sobre la salut mental. Els resultats subratllen la importància d'incloure la dimensió psicològica en les estratègies de desenvolupament i en les polítiques de salut, especialment en contextos amb recursos limitats.

Paraules clau: salut mental, xocs ambientals, exposició en la infància, intervenció en salut pública, economia de la salut, capital humà.

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1. Introduction

Health is a fundamental component of human capital and an important determinant of productivity, educational attainment, and economic development. Traditionally, the field of health economics focused primarily on physical health outcomes and healthcare systems, with foundational contributions by Grossman (1972) and Arrow (2012) establishing the framework for understanding health as human capital. In recent decades, however, the scope of health economics has expanded to acknowledge other dimensions of well-being, including psychological and emotional health. Mental health, in particular, now accounts for a substantial and growing share of the global disease burden, becoming as critical as physical health. According to the World Health Organization (2022), nearly one in eight individuals worldwide lives with a mental disorder, and depression alone affects more than 280 million people. The economic implications are equally profound. A joint report by the World Health Organization and the World Bank estimates that the global cost of mental disorders was estimated to be approximately US\$2.5 trillion in 2010 and projected to rise by 240 percent by 2030 (Bloom et al., 2011; Mnookin, 2016). More specifically, depression and anxiety are estimated to result in annual global economic losses exceeding US\$1 trillion due to reduced productivity (Wilson, 2015; Chisholm et al., 2016). These numbers highlight the urgency of placing mental health at the center of global health and development programs.

A growing body of research shows that mental health is shaped not only by genetic and biological factors, but also by external circumstances, particularly sudden or large-scale events such as natural disasters or changes in public health provision (Clayer et al., 1985; Baird et al., 2013; Goldmann and Galea, 2014; Everly, 2021; Voit et al., 2022). Crucially, such events may occur at any stage of life. Mental health can be affected in adulthood through direct exposure to trauma (Kar et al., 2014; Baryshnikova and Pham, 2019; Civelek, 2023) or in early life through more indirect mechanisms that change developmental paths (Adhvaryu et al., 2014; Hinke et al., 2022). Although exposure can happen at any age, the consequences are often most severe for the most vulnerable groups, including children, the elderly, and individuals from socioeconomically disadvantaged backgrounds, who typically have

fewer resources to recover from such shocks (Jia et al., 2010; Maclean et al., 2016; Wang and Fattore, 2020). Understanding how different types of events influence mental health across the life course is, therefore, essential for designing timely and targeted policy responses that promote prevention, build resilience, and support long-term well-being.

This dissertation examines the long-term effects of both environmental and policy-driven events on mental health, using Indonesia as a case study. The mental health situation is particularly pressing in developing countries, where limited resources make it more difficult to recover from adverse events (Sweetland et al., 2014; Rathod et al., 2017). Indonesia, as a lower-middle-income country, provides a unique and policy-relevant context for examining the long-term mental health consequences of large-scale shocks. With a population exceeding 280 million¹, it is one of the most populous and diverse nations in the world, spanning thousands of islands and exhibiting substantial regional disparities in development and access to health services. Moreover, mental health infrastructure remains limited, and social stigma around mental illness is pervasive (Hidayat et al., 2023; Munira, 2023). The relevance of studying Indonesia is further emphasized by its frequent exposure to environmental shocks and the implementation of major public health reforms over the past three decades. These dynamics offer a valuable empirical setting for understanding how early-life and later-life exposures influence psychological well-being across the life course.

This dissertation comprises three independent but thematically interconnected essays. Each essay employs quasi-experimental methods to identify causal effects, drawing on the Indonesian Family Life Survey (IFLS), a rich longitudinal dataset that includes validated measures of mental health, along with a range of demographic, economic, and geographic information. These data are complemented by external administrative records and geospatial indicators, allowing for precise identification of exposure to various events and interventions. The three essays focus on distinct but policy-relevant sources of mental health variation, including a natural

¹ Statistics Indonesia (BPS). Mid-year population estimates (in thousands).
<https://www.bps.go.id/id/statistics-table/2/MTk3NSMy/jumlah-penduduk-pertengahan-tahun--ribu-jiwa-.html>

disaster in the form of an earthquake, an environmental catastrophe resulting from large-scale forest fire pollution, and a public health intervention through the implementation of the Village Midwife Program. While each paper investigates a different type of event, they are unified by a common objective, which is to understand how both adverse shocks and preventive policies influence mental health outcomes across the life course. Together, these studies contribute new evidence to the field of health economics by highlighting the long-term psychological consequences in a developing country context.

The first paper, "*Shattered ground, shaken minds: Mental health consequences of earthquakes*",² examines the lasting psychological impact of the 2006 Yogyakarta earthquake. The analysis combines geospatial data on earthquake intensity from the United States Geological Survey (USGS) with individual-level mental health outcomes from the Indonesian Family Life Survey. Employing a difference-in-differences approach that leverages variation in earthquake severity, the study finds that individuals residing in areas with higher intensity earthquakes experience a significant and persistent decline in mental health. The adverse effects are especially pronounced among vulnerable populations, such as older adults, residents of rural areas, and those from lower-income households. The findings are robust to a range of sensitivity tests. Additional analyses suggest that these mental health impacts are closely associated with family-level casualties and declines in physical health, which appear to serve as important pathways through which the shock affects long-term psychological outcomes.

The second paper, "*From infancy to adulthood: Mental health effects of early life interventions*", turns to a large-scale health policy intervention aimed at improving maternal and child outcomes in rural Indonesia. In the early 1990s, the Indonesian government launched the Village Midwife Program, which deployed trained midwives to underserved communities across the archipelago to reduce maternal and infant mortality (Shiffman, 2003; Shankar et al., 2008). This study investigates whether being born in a village with access to a midwife during the program's rollout leads to

² This paper has been published as: Perdana, A.R.A. and Vall Castelló, J. (2024). Shattered Ground, Shaken Minds: Mental Health Consequences of Earthquakes, *Economics & Human Biology*, 54, 101448. <https://doi.org/10.1016/j.ehb.2024.101448>

improved mental health outcomes later in life. The analysis exploits variation in the timing and location of midwife deployment across districts and links this to cohort-based data from the IFLS. Applying recent econometric methods for staggered adoption, as proposed by Callaway and Sant'Anna (2021) and Sun and Abraham (2021), the study finds that individuals born in areas with midwife presence exhibit significantly lower levels of psychological distress in adulthood. These effects are robust across a range of specifications, including placebo tests and alternative sample definitions. Further analysis indicates that improvements in maternal health behaviors and infant health characteristics are plausible channels through which early-life exposure to the program contributes to better psychological well-being later in life.

The third paper, "*Exposure to forest fire smoke and the long-term risk of psychological distress*", investigates the 1997 Indonesian forest fires, one of Southeast Asia's most severe environmental disasters. Driven by large-scale land clearing and prolonged drought, the fires produced massive amounts of smoke and haze that blanketed large parts of Indonesia and neighboring countries for several months (Neil and G., 1998; Kunii et al., 2002). This study examines whether exposure to forest fire smoke during critical periods of early development increases the risk of psychological distress in adulthood. Using satellite-derived Aerosol Index data to measure district-level smoke intensity, the analysis links early-life exposure to mental health outcomes observed in the 2014 wave of the IFLS data. The empirical strategy employs a cohort-based difference-in-differences framework, comparing individuals who were in utero or early childhood during the fire months to older cohorts across districts with varying pollution intensity. The results show that early-life exposure to high smoke intensity is associated with significantly higher risks of psychological distress in adulthood, particularly in the form of sleep disturbances and depressive symptoms. These findings are robust across alternative exposure definitions and estimation strategies, including an Instrumental Variable approach that uses wind direction as a source of exogenous variation.

In sum, this dissertation offers evidence on how both adverse events and public policy interventions influence mental health outcomes across different stages of life. Chapters 2, 3, and 4 explore distinct forms of exposure, each contributing uniquely to the existing literature in health

economics and mental health. These studies advance the understanding of the long-term consequences of environmental shocks and institutional interventions, while also adding new evidence from a developing country context where mental health research remains limited. Taken together, the findings highlight the importance of assessing public policy and environmental risk not only in terms of physical or economic outcomes, but also through their implications for psychological well-being and human capital development.

The final chapter, Chapter 5, concludes the dissertation by summarizing the main insights and considering their broader relevance for policy design. In particular, it emphasizes the urgency of integrating mental health into disaster preparedness, early-life health programming, and long-term development strategies aimed at promoting resilience and well-being. It also outlines directions for future research that can build on the dissertation's findings to further explore the mechanisms and contexts through which shocks and interventions shape mental health over the life course

2. Shattered Ground, Shaken Minds: Mental Health Consequences of Earthquakes

2.1. Introduction

The role of mental health in achieving global development goals is increasingly acknowledged in academic and policy discussions (De Silva, 2015; Jenkins, 2019). However, complex challenges associated with mental health persist in both developed and developing countries. According to the World Health Organization (2022), one in eight people lives with a mental disorder, with anxiety and depressive conditions being the most prevalent among both men and women. This sizeable proportion highlights a pressing universal health issue, emphasizing the immediate need for enhanced awareness. Moreover, the recent COVID-19 pandemic has introduced another layer of complexity, which is undoubtedly capable of weakening the mental health situation and creating major changes in people's well-being in many ways (Song, 2020; Mureşan et al., 2022; König et al., 2023). While mental health is influenced by a complex interaction of internal and external factors, it is well-recognized that experiencing unexpected and traumatic events may also contribute to this issue.

In this study, we investigate the impact on individuals' mental health resulting from unexpected incidents, particularly catastrophic natural disasters. Over the past two decades, the destructive impact of natural disasters has increased worldwide, resulting in the loss of approximately 1.23 million lives and an estimated economic toll of around US\$2.97 trillion (United Nations Office for Disaster Risk Reduction, 2020). These disasters have influenced a broad range of outcomes, including health (Raker et al., 2019; Khanal, 2022; Bertinelli et al., 2023), education (Caruso and Miller, 2015; Deuchert and Felfe, 2015; Paudel and Ryu, 2018; Rosales-Rueda, 2018; Herrera-Almanza and Cas, 2021; Shidiqi et al., 2023), labor market (Di Pietro and Mora, 2015; Kirchberger, 2017), social capital (Cipollone and Rosolia, 2007; Bai and Li, 2021), well-being (Goebel et al., 2015; Rehdanz et al., 2015; De and Thamarapani, 2022; Wang and Wang, 2023),

demographic (Nandi et al., 2018), migration (Gröger and Zylberberg, 2016), finance (Deryugina, 2017; Gallagher and Hartley, 2017), crime rates (García Hombrados, 2020), and even electoral outcomes (Masiero and Santarossa, 2020).

Research consistently demonstrates that the impact of natural disasters extends far beyond physical destruction and financial loss, pointing out complex consequences for quality of life, including well-being and mental health. For instance, Rehdanz et al. (2015), De & Thamarapani (2022), and Wang & Wang (2023) highlight that natural disasters tend to have adverse impacts on aspects of well-being, such as standard of living and happiness. In terms of mental health, interdisciplinary studies have shed light on the broader effects of disasters on this issue, exposing how survivors may experience a range of responses, both adverse and positive, from a clinical perspective. Research such as Norris et al. (2009), Galea et al. (2005), and Raker et al. (2019) illustrates that psychological responses can vary from immediate stress reactions to long-term disorders like post-traumatic stress disorder (PTSD) and depression, indicating that the psychological aftermath of disasters can affect mental health for years. The resilience of individuals and communities, as discussed by Bonanno et al. (2011) and Hobfoll et al. (2007), plays a critical role in recovery, indicating that mental health interventions should be designed to reinforce these inherent strengths. The impact on vulnerable groups such as children and the elderly is also substantial, as noted by Peek (2008) and Joseph & Jaswal (2021). This strand of literature indicates a complex relationship of factors that influence mental health outcomes post-disaster and points to the importance of comprehensive and varied approaches in disaster response and recovery strategies. Within this framework, we hypothesize three outcomes. First, greater exposure to the earthquake will be significantly correlated with heightened psychological distress. Second, these effects are expected to diminish over the long term due to potential resilience among individuals. Third, the elderly, being a vulnerable group, are more likely to experience adverse effects from the earthquake.

We analyze the changes in individuals' mental health following the 2006 earthquake in Yogyakarta Province, Indonesia. Our identification strategy relies on the unpredictable nature of the earthquake, including its timing, location, and severity, represented by the Modified Mercalli Intensity

(MMI) index from the US Geological Survey (USGS) data.³ By integrating this data with the Indonesian Family Life Survey (IFLS), we can connect individual characteristics with the MMI values. Employing a difference-in-differences approach, we aim to assess the earthquake's causal impact on mental health by comparing conditions of individuals in areas with varying MMI levels, both before and after the disaster. Our primary focus is on altered psychological states, including symptoms like feeling anxious, feeling depressed, having sleep disorders, and losing focus. To support our empirical strategy, we analyze responses to the earthquake from various subsamples and perform falsification tests. This study not only seeks to provide causal evidence of the impact of major disasters on mental health but also aims to explore pathways through which such events may heighten vulnerabilities. By examining changes in variables such as socioeconomic conditions and physical health status, we identify the primary drivers that contribute to the observed mental health effects, shedding light on how disasters can indirectly increase psychological distress.

Our findings show significant deteriorations in mental health in areas more intensely affected by the earthquake. Specifically, the disaster increases the likelihood of feeling anxious, depressed, and having sleep disorders by 8.39 %, 10.47 %, and 7.37 %, respectively, compared to pre-earthquake averages. These effects are particularly pronounced among vulnerable groups, including older adults, rural residents, and individuals from low-income families, though no significant differences were observed across genders. The robustness of our methodology is supported by consistent results across various treatment group definitions, including the use of binary MMI values, the exclusion of certain municipalities, and the execution of placebo tests, which collectively reinforce the validity of our assumptions. Furthermore, our analysis indicates that potential issues of endogenous migration and attrition do not bias our results. In exploring the mechanisms driving these mental health outcomes, we find that exposure to earthquakes leads to declines in both socioeconomic and physical health conditions. These adverse impacts on mental health appear closely linked to family casualties and deteriorations in physical health, which together contribute

³ See <https://www.usgs.gov/programs/earthquake-hazards/modified-mercalli-intensity-scale>

significantly to the overall effect. These findings also highlight the complex interaction between physical, social, and psychological factors in the aftermath of natural disasters.

This research aligns with the substantial body of literature investigating the relationship between natural disasters and mental health outcomes (Rhodes et al., 2010; Kar et al., 2014; Lowe et al., 2015; Maclean et al., 2016; Baryshnikova and Pham, 2019; Raker et al., 2019; Civelek, 2023; Mostafizur Rahman et al., 2023). In particular, this study adds to the specialized segment of the literature focusing on the mental health outcomes of earthquakes (Şalcioğlu et al., 2003; Livanou et al., 2005; Zhang et al., 2011; Qu et al., 2012; Sezgin and Punamäki, 2012; Kino et al., 2021; Bertinelli et al., 2023). Concerning the Yogyakarta earthquake, existing research with a causal emphasis has largely concentrated on its impact on well-being, education, social capital, and labor outcomes, as underlined in works by Bai & Li (2021), De & Thamarapani (2022), Kirchberger (2017), and Shidiqi et al. (2023). However, there is limited research focusing on its impact on mental health.

We aim to contribute to the literature in several ways. First, this paper analyzes the impact of the earthquake on self-reported mental health perceptions, with a focus on mild to moderate psychological distress symptoms. This approach complements existing research, which primarily focuses on specific outcomes like PTSD and psychiatric disorders, as highlighted in studies by Z. Zhang et al. (2011) and Bertinelli et al. (2023). Our work offers insights into the subtler, yet equally significant, psychological impacts of earthquakes, thus enriching the understanding of their broader mental health effects. Second, by employing panel data, we investigate the longitudinal effects of natural disasters on mental health. This methodology enables us to explore not only the immediate aftermath of the earthquake but also its prolonged impact, hence contributing to an understanding of the long-term mental health outcomes following such traumatic events.

Another substantial contribution of our study is that we provide insight into the use of continuous MMI values rather than high-exposure dummies to measure the earthquake intensity. The typical approach in earthquake studies often involves using distinct MMI thresholds to categorize

areas as affected or unaffected, as studied by Masiero & Santarossa (2020), Paudel & Ryu (2018), and Shidiqi et al. (2023). To our understanding, continuous MMI values provide a more sensitive measure, less likely to be confounded.⁴ This can also be particularly beneficial in understanding the gradations of effects on various outcomes, such as infrastructure damage, economic impacts, or human responses. Additionally, our research contributes to broadening the picture of the mechanisms driving the impact of natural disasters on mental health. This exploration is particularly crucial, given the limited evidence available on the long-term psychological impacts of such disasters and the underlying processes that contribute to them. Although the connection may be indirect, the insights derived from studying these mechanisms have potential implications for policy-making and economic planning.

The structure of this paper is as follows: Section 2.2 provides an overview of the Yogyakarta earthquake and explores the potential relationship between natural disasters, earthquakes, and mental health. Section 2.3 details the data used in this study and presents summary statistics. Section 2.4 outlines our empirical strategy. The main results of the analysis are presented in Section 2.5. Section 2.6 conducts a series of robustness tests. Section 2.7 discusses potential mechanisms driving our findings. The paper concludes in Section 2.8, and the Appendix contains supporting data and additional results.

2.2. Background

2.2.1. The 2006 Yogyakarta Earthquake

Indonesia is highly vulnerable to natural disasters, particularly earthquakes, due to its unique geographical and tectonic positioning (Priester, 2016). Over the years, the country has experienced numerous earthquakes of varying magnitudes and intensities, many of which have led to widespread destruction and significant loss of life. Table 2A.1 in the Appendix provides a detailed account of major earthquakes in Indonesia, documenting the year, location, magnitude, damages, and casualties. Historically, Indonesia has

⁴ We are grateful to an anonymous referee for suggesting that the use of continuous values as opposed to dummy variable is much more convincing in this study.

experienced frequent seismic activity, with hundreds of earthquakes recorded between 1990 and 2015. However, the seismic activity in Yogyakarta Province and the surrounding areas has typically been characterized by low to moderate magnitudes.⁵ This makes the extensive damage and destruction caused by the 2006 earthquake particularly unusual and unexpected for this region.⁶

Among the significant earthquakes in Indonesia, the Yogyakarta earthquake stands out as one of the most severe and unexpected in its impact (Amri et al., 2018). This earthquake struck Yogyakarta Province, located on the island of Java, Indonesia, on May 27, 2006, at 5:52 local time, registering a magnitude of 5.9⁷ on the Richter scale (Bappenas, 2006). In addition to the Richter scale,⁸ the USGS meticulously recorded the intensity of this earthquake, assigning specific MMI values to each affected municipality based on observed impacts and shaking severity. Figure 2.1 illustrates the geographic distribution of earthquake intensities across districts/municipalities in Java Island, with darker shades representing higher seismic intensities. The most intense shaking, depicted in the darkest red, was centered around the epicenter and gradually decreased in intensity further from the center.

While the magnitude was moderate compared to other significant events like the 2004 Aceh earthquake (9.1 on the Richter scale) or the 2018 Palu earthquake (7.5 on the Richter scale), the Yogyakarta earthquake resulted in substantial human and economic losses. The total death toll exceeded 5700, with more than 40,000 injuries reported. The earthquake severely affected all municipalities in Yogyakarta Province, including Bantul, Sleman, Yogyakarta City, Kulon Progo, Gunung Kidul, and one municipality in Central Java Province, Klaten. The severe damage caused by the earthquake was attributed to its shallow depth and the high population

⁵ Prior to 2006, Yogyakarta Province last experienced a major earthquake in 1937, which registered a magnitude of 6.9 on the Richter scale with a maximum MMI of VIII.

⁶ The unexpected and unpredictable nature of this earthquake makes it as an exogenous event, which is important to our identification strategy.

⁷ The USGS reported that the earthquake registered a magnitude of 6.3 on the moment magnitude scale, with a depth of 12.5 km (8 miles).

⁸ Richter scale measures the magnitude of the earthquake at its epicenter.

density in the affected areas (Walter et al., 2008). Additionally, the early morning timing of the earthquake, when most people were still asleep, contributed to the high number of fatalities. Communities in the most affected municipalities, particularly Bantul and Klaten, are predominantly lower-middle-class and share similar characteristics and living conditions. Moreover, Bantul and Klaten are among the ten most densely populated municipalities in Indonesia, each with a population density exceeding 1600 individuals per square kilometer (Bappenas, 2006).⁹ The rapid population growth in these areas has heightened vulnerability, as many buildings and infrastructure were not constructed to withstand the intensity of the 2006 earthquake. This likely exacerbated the impact, leading to extensive damage and loss. Figure A1 in the Appendix provides a breakdown of the destruction and casualties in each municipality according to the MMI values, illustrating the connection between increased seismic intensity and the extent of damage.

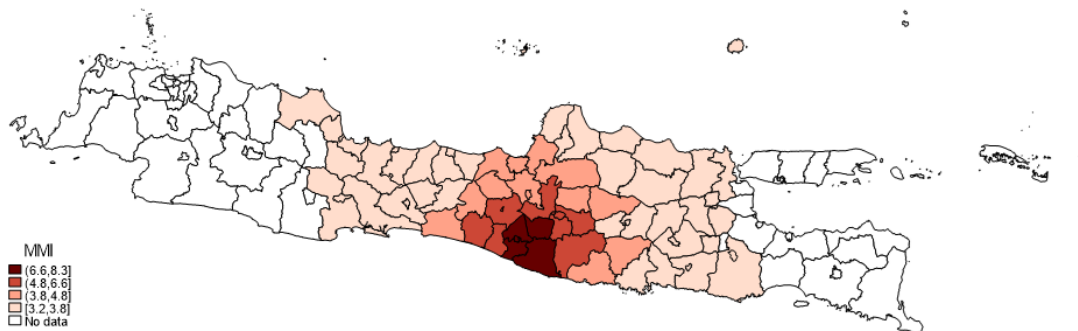
In addition to the physical destruction, the economic impact of the Yogyakarta earthquake was profound. It is estimated that the total loss associated with the disaster reached approximately US\$3.1 billion (World Bank, 2006). These losses were primarily carried by the housing, productive, and social sectors, with the housing sector absorbing more than half of the total cost. Productive sectors, including small and medium-sized enterprises, accounted for roughly 30 % of the overall loss, while social sectors such as education and health suffered about 13 % of the damage. Reconstruction efforts started approximately one month after the disaster, as reported by Resosudarmo et al. (2012). The government prioritized rebuilding homes and public infrastructure while also focusing on revitalizing the economy in the affected municipalities. A year after the earthquake, the housing sector showed meaningful recovery, with approximately 62.5 % of houses in Yogyakarta Province being reconstructed (Brata et al., 2018).

Despite this progress in reconstruction, the economic sector during the same period reflected a concerning lack of improvement, with reduced output activity compared to pre-earthquake levels (Java Reconstruction Fund,

⁹ Yogyakarta Province is among Indonesia's top five most densely populated provinces (<https://www.bps.go.id/indicator/12/141/1/population-density-by-province.html>). The Municipality of Klaten in Central Java also has a high density, even above the average of four Municipalities in Yogyakarta Province (Bappenas, 2006).

2007). The significant economic impact that occurred after the earthquake in Yogyakarta certainly has the potential to affect aspects of the lives of the affected communities. Beyond the visible destruction and high casualty rates, people faced ongoing economic challenges that weighed heavily on their mental well-being. The prolonged financial distress, coupled with the trauma of loss and confusion, could have profound implications for mental health. This underscores the necessity of incorporating mental health support into disaster response and recovery strategies, recognizing that the psychological aftermath is as critical to address as the physical and economic restoration.

Figure 2.1. Java Island and MMI Area at the District/Municipality Level



Notes: MMI values are recorded in the colored area. Darker shades reflect higher level of intensities (MMI) values.

2.2.2. Natural Disasters, Earthquakes, and Mental Health

The unpredictability of natural disasters such as earthquakes, coupled with human vulnerability to sudden disasters, can potentially place an emotional burden on those affected (Li and Li, 2022). In the aftermath of a disaster, people often experience an urgent need to return to their productivity to fulfill economic necessities. Concurrently, some of them also battle with unpleasant feelings and thoughts that may intensify over weeks, months, or even years, inhibiting daily activities. Particularly vulnerable are those who experience direct consequences of the disaster, such as physical injuries, fear of future loss, grief, restricted access to essential resources, and property damage, as well as individuals with pre-existing economic hardships and mental or physical health (Lowe et al., 2020). These experiences can heighten the risk of psychological distress, both in the immediate aftermath and in the long term. According to a conceptual framework introduced by Viertiö et al.

(2021), psychological distress is a frequently utilized indicator of mental health, which is typically defined as a state of emotional suffering characterized by stress, anxiety, and depressive symptoms. It manifests when an individual faces an overwhelming stressor, such as an adverse environmental event or a traumatic natural disaster experience.

A research by Norris et al. (2009) describes that there are six potential responses to distressing events: resistance, resilience, recovery, relapsing/remitting, delayed dysfunction, and chronic dysfunction, underscoring the significant variations in reactions to trauma among individuals. We believe this diversity in reaction to traumatic experiences may account for the varying results observed in studies on the impact of earthquakes on mental health. While some individuals demonstrate resilience, effectively adapting in the face of adversity (Xi et al., 2020; Timalsina et al., 2022), others may show signs of short and medium-term (Zhang et al., 2011; Messiah et al., 2014; Bertinelli et al., 2023) or even long-lasting distress (Şalcioğlu et al., 2003; Livanou et al., 2005; Kar et al., 2014; Gao et al., 2019; Kino et al., 2021). Individual capacities to overcome stress and trauma caused by natural disasters also vary significantly between individuals, influenced by a wide-ranging of factors including personal resilience, access to social support systems, previous experiences with trauma, socioeconomic status, and the availability of mental health resources (Galea et al., 2005; Norris et al., 2008; Bonanno et al., 2011; Lowe et al., 2020). In addition, genetic predisposition and cultural backgrounds play crucial roles in determining how effectively a person can adapt to the psychological impacts of such catastrophic events (Stein et al., 2002; Hinton and Lewis-Fernández, 2011).

Another dimension to consider is the differential impact of earthquakes on mental health that may vary according to demographic factors, including gender, age, educational attainment, and income level. (Fu et al., 2021; Mao and Agyapong, 2021). For example, research conducted by Bertinelli et al. (2023) and Soqia et al. (2024) suggests that women are more likely to develop psychological distress than men following earthquakes. Additionally, Jia et al. (2010) find that the elderly population is more susceptible to post-traumatic symptoms compared to younger adults. However, these findings are not universally consistent across all earthquake incidents. For instance, a study by Tsuboya et al. (2016) on the East Japan

earthquake shows that the impact on depressive symptoms is more pronounced in male survivors. There are also cases where male and female survivors do not show significant differences, as pointed out by Livanou et al. (2005), Fergusson et al. (2014), and Geng et al. (2018). Furthermore, a study by Rafiey et al. (2016) demonstrates that older earthquake survivors exhibited higher levels of positive mental health compared to younger ones, suggesting that increasing age may not always correlate with greater vulnerability to natural disasters. This differential impact highlights the need for targeted mental health interventions that consider these vulnerabilities to effectively support diverse populations in the aftermath of disasters.

2.3. Data and Statistics

2.3.1. Earthquake Intensity

Measuring the strength of an earthquake can be stated in terms of intensity and magnitude. Even though each earthquake has the characteristics of a single epicenter and magnitude, the intensity can vary greatly from one location to another. To account for these variations, we use data on local exposure and earthquake intensities from a ShakeMap file provided by the USGS.¹⁰ This database allows us to read the geographical spread of MMI, a metric that quantifies the severity of an earthquake's tremors at a specific spot by considering its influence on people and objects in the neighborhood. As stated by Masiero & Santarossa (2020), the MMI value is a more reflective measure as it pertains to the impact felt at the location of the earthquake. Moreover, MMI is exogenously recorded and hence less susceptible to biases that can arise from retrospective reporting (De and Thamarapani, 2022). To retrieve the earthquake's intensity value at the municipality level, we extract data from the ShakeMap file and yield a range of MMI values unique to the Yogyakarta earthquake, varying from a minimum of 3.2 to a maximum of 8.3.¹¹ Considering that the map file only covers certain areas on the Java

¹⁰ See <https://earthquake.usgs.gov/earthquakes/eventpage/usp000ej1c/shakemap/intensity>

¹¹ We acknowledge that within the larger areas, such as districts/municipalities, there may be variability in shaking intensity. However, the publicly available MMI data constrains our analysis to the municipality level. To address this, we explore the potential for interpolating the MMI data to estimate exposure at the sub-municipality/sub-district level.

Island, as depicted in Figure 2.1, municipalities located outside the colored areas are likely to have very low intensity values.

Various studies determine different MMI values as thresholds to distinguish between areas affected and unaffected by earthquakes (Paudel and Ryu, 2018; García Hombrados, 2020; Masiero and Santarossa, 2020; De and Thamarapani, 2022; Shidiqi et al., 2023). As described earlier, we utilize the continuous values of MMI, instead of holding to a specific cutoff value, to act as exposure, especially in our main analysis. The USGS notes that areas with lower MMI values generally experience earthquake shaking without experiencing physical damage, while higher MMI values are often associated with observable structural damage. This gradation underscores the justification behind the use of continuous values to understand the effect of varying intensities of the Yogyakarta earthquake. However, for robustness, we additionally estimate our findings using a threshold value. In this perspective, we define a municipality as affected if the MMI is equal to or greater than 7, and non-affected if the value is below 7, following the specification used in studies by Caruso & Miller (2015), Paudel & Ryu (2018), and García Hombrados (2020). According to the USGS, an intensity of 7 on the MMI scale corresponds to very strong perceived shaking and can cause moderate damage to buildings and structures.

This interpolation is done using an Inverse Distance Weighting (IDW) method, which estimates the shaking intensity at unmeasured points based on the known intensities at surrounding locations. This method assumes that points closer to each other are more similar than those further apart, making it suitable for estimating the spatial variation of earthquake intensity. To validate the interpolation results, we cross-reference them with the GPS coordinates of the sub-municipality/sub-district. This allows us to assess the consistency of our findings when using a more refined exposure variable. We include a map of Java Island and the MMI area at the sub-municipality/sub-district level in the Appendix (Figure 2A.2) to visually represent this finer level of analysis. The results of this analysis, as presented in Table 2A.2 in the Appendix, are consistent with our main findings at the municipality level in Table 2.1, which strengthens our confidence in the robustness of our results.

2.3.2. Survey Data

Our second data source is derived from the IFLS,¹² a comprehensive longitudinal survey presently consisting of five distinct waves,¹³ covering roughly 83 % of the Indonesian population across 13 provinces (Strauss et al., 2016a). The IFLS contains indicators covering demographics, socioeconomic status, and health at individual, household, and community levels, while ensuring minimal attrition rates. We specifically utilize data from the first, third, fourth, and fifth waves of the IFLS in our baseline model. The second wave is excluded due to the absence of critical data concerning our outcome variables. To formulate the estimation sample, we maintain individuals from the first wave who are over the age of 15,¹⁴ tracking them up to the final survey wave. The survey's detailed migration history records greatly facilitated our efforts, enabling us to identify the municipality-level location of respondents from their birthdate up to the last survey wave. This process allows us to accumulate data on individual residences during the year of exposure and correlate this with municipality-level MMI values. Subsequently, we managed to retain individuals who were residing on Java Island at the time of the earthquake.¹⁵ This procedure enables us to classify whether an individual was living in an area with a higher or lower intensity of the earthquake.

The IFLS dataset offers critical information for our analysis, specifically related to the mental health status of respondents, which is our primary outcome of interest. We construct several perceived mental health indicators derived from respondents' answers to the following queries: “In the past week, did you feel/experience [fear or anxiety?], [depressed?], [a hard time sleeping?], [difficulty concentrating on doing something?]”. There are four categories of answers available to these self-reported questions on

¹² The IFLS data can be obtained from <https://www.rand.org/well-being/social-and-behavioral-policy/data/FLS/IFLS.html>.

¹³ IFLS1 (1992/1994); IFLS2 (1997/1998); IFLS3 (2000); IFLS4 (2007/2008); IFLS5 (2014/2015).

¹⁴ We restrict our sample to all those individuals aged 15 years and over since the psychological health information is for individuals above 15.

¹⁵ Our individual sample size decreases from 20,347 (covering all of Indonesia in the first wave) to 6,246, as we focus on respondents with complete mental health information who were residing in Java Island at the time of the earthquake.

the original questionnaire. Individuals were coded as experiencing psychological distress (coded as 1) if they reported experiencing these symptoms “occasionally (3-4 days)” or “most of the time (5-7 days)”. Responses indicating “rarely (≤ 1 day)” or “some days (1-2 days)” were coded as 0 (no).

While the IFLS contains numerous questions about mental health, we have chosen to focus on four specific areas: feeling anxious, feeling depressed, having a sleep disorder, and losing focus. The use of mild to moderate psychological distress symptoms in our analysis, while distinct from employing standardized measures like the Center for Epidemiologic Studies Depression Scale (CES-D) for scoring depression due to limited data,¹⁶ is also considered a valid and informative approach. To validate our selected mental health indicators as proxies for the CESD-10 scoring, we calculated the correlation coefficients between the CESD scores from IFLS 4 and IFLS 5 and the indicators used in our main analysis (feeling anxious, feeling depressed, having sleep disorders, and losing focus). The results, presented in Table 2A.3 in the Appendix, show significant positive correlations, confirming that these indicators seem to be reliable proxies for the standardized CESD measure.

2.3.3. Summary Statistics

The summary statistics of our main variables are reported in Table 2A.4 and illustrated in Figure 2A.3 in the Appendix. Table 2A.4 presents sample means for the relevant mental health and individual characteristics in a pre- and post-earthquake context, differentiated between individuals

¹⁶ In the IFLS dataset, specific to wave 1, there are only six questions directly related to self-reported mental health, and in wave 3, there are eight questions. It is important to note that in waves 4 and 5, the dataset was expanded to include two additional questions. This expansion allows for the application of the 10-Item CESD scoring. However, due to the absence of these two critical questions in wave 3 and the absence of four critical questions in wave 1, it is not feasible to compute the CESD score for the periods prior to the earthquake. This limitation is important to highlight, as it restricts our ability to apply a standardized depression measure consistently across the entire dataset, particularly for the pre-earthquake period. Consequently, our analysis relies on the specific questions available in the initial waves to assess mental health, acknowledging that this approach differs from the comprehensive CESD scoring possible in the later waves. This methodological adaptation is essential for maintaining the consistency of our longitudinal analysis across all waves of the IFLS data.

residing in municipalities with an $\text{MMI} \geq 7$, designated as the affected areas (treatment group), and those in municipalities that were not affected (control group). While our primary analysis employs continuous MMI values to measure exposure, we use binary classification of MMI for the summary statistics, as it enables a more straightforward interpretation of the earthquake's impact. Detailed statistical evidence based on continuous MMI values is also presented in Figure 2A.3 of the Appendix, ensuring transparency and allowing for an in-depth assessment.

Before the event, the prevalence of all mental health outcomes is relatively comparable across groups, as indicated by the modest differences within the pre-earthquake context. Notably, in the post-earthquake period, significant increases in the mean values of feeling anxious and feeling depressed are observed for the affected group compared to the non-affected group. Conversely, the variables of having a sleep disorder and losing focus do not demonstrate any notable differences post-earthquake, implying an initial indication that these aspects may not be impacted by the earthquake. These findings suggest a substantive exacerbation of certain psychological conditions attributable to the earthquake. Demographically, the affected group is statistically older by about 2 years compared to the non-affected group. Individuals residing in affected municipalities have more years of education than those who were living in non-affected municipalities, both in pre- and post-earthquake.¹⁷ The marital status remained relatively unchanged after the earthquake, with a non-significant difference, as shown in the Diff-Diff column.

Figure 2A.3 in the Appendix presents a visual representation of statistical evidence illustrating the sample means of psychological outcomes in each municipality, segmented by MMI continuous values, both pre- and post-earthquake. This graph broadly suggests that psychological distress conditions, including feeling anxious, feeling depressed, and having sleep disorders, tend to develop post-disasters and become more severe in municipalities with higher MMI values. However, consistent with the

¹⁷ Following a study by Shidiqui et al. (2023), individuals living in affected municipalities, especially Yogyakarta Province, tend to have a more years of education due to high number of educational institutions, including schools and universities located in this province.

statistical findings presented in Table 2A.4, we do not find a similar trend for the outcome of losing focus.

2.4. Empirical strategy

This paper analyzes the impact of the Yogyakarta earthquake on mental health using panel data from periods before and after the seismic event. To attain this, we align MMI values with individual data based on their locations during the earthquake in 2006, thereby distinguishing between individuals who experienced varying intensities of the earthquake. We employ a difference-in-differences approach to estimate the impact, comparing changes in mental health outcomes between individuals exposed to higher and lower intensities of the earthquake before and after its occurrence. Our identification strategy leverages the variation in earthquake intensity while controlling for individual, municipality, and year fixed effects. The baseline specification is as follows:

$$Y_{ijt} = \alpha + \beta_1 Post_t + \beta_2 MMI_{j(2006)} + \beta_3 (Post_t * MMI_{j(2006)}) + \delta_i + v_t + \emptyset_{j(t)} + X_{it} + \epsilon_{ijt} \quad (2.1)$$

Y_{ijt} represents our main outcome of interest for individual i living in municipality j at period t . $Post_t$ is an indicator that takes the value of one for the observation after the earthquake and zero for the observation before the earthquake. $MMI_{j(2006)}$ captures the continuous values of MMI in municipality j based on the residence during the earthquake in 2006. β_3 is the main parameter of interest, which gives the coefficient of the impact of the earthquake on the psychological distress of individuals during the time of the earthquake in municipality j . The individual fixed effects δ_i effectively control for unobserved time-invariant individual characteristics, the term v_t considers the year/wave fixed effects, and $\emptyset_{j(t)}$ represents the municipality fixed effects based on the individual's residence in each wave. It is important to note that the $Post_t$ may be omitted due to perfect collinearity with the year

fixed effects.¹⁸ X_{it} is a vector of time-varying individual characteristics such as age and age squared, years of education, and marital status. As these control variables may also be affected by the earthquake¹⁹, we present the results of the estimations both with and without controls. The standard error is clustered at the municipality level to allow for serial correlations within each municipality.

This difference-in-difference specification enables us to assess whether areas experiencing higher intensities are more adversely affected compared to those experiencing lower intensities. To leverage the variation in shock intensity, we estimate Equation 2.1 as our primary specification. However, due to the lack of a clear functional relationship between intensity and outcomes, we also present results using a binary treatment variable for easier interpretation as a robustness check in Section 6.

2.5. Effects of the Yogyakarta Earthquake on Mental Health

2.5.1. Main Results

Table 2.1 presents the estimated effects of the Yogyakarta earthquake on various mental health outcomes, using the interaction of continuous MMI values with a post-earthquake indicator as the key explanatory variable. The results, adjusting for individual, year, and municipality fixed effects, show that the earthquake is associated with statistically significant increases in self-reported symptoms of feeling anxious, feeling depressed, and having sleep disorders, by 8.39 %, 10.47 %, and 7.37 %, respectively, as calculated by the percentage change from the baseline pre-earthquake mean.²⁰ The

¹⁸ The impact of the post-earthquake period is fully captured by the year fixed effects, leading to the omission of the post indicator from the regression output. However, the inclusion of year fixed effects is crucial for controlling time-specific factors that could influence the outcomes.

¹⁹ See Dodlova et al. (2023), Kirchberger (2017), and Pope et al.(2022) for discussions on the impact of natural disasters on education and marriage, respectively.

²⁰ Given the multiple comparisons, we also conduct a traditional Bonferroni correction by multiplying each p-value by the total number of tests, following studies by Conti et al. (2016). This approach leaves the original significance threshold unchanged while adjusting the p-values to account for multiple testing. Given that we analyze four main mental health outcomes in the main analysis, each estimated with two models (with and without controls), we adjust for a total of eight tests by multiplying each p-value by 8. In Table 2A.5 in the

models without additional controls, shown in odd-numbered columns, and those with controls for time-varying individual characteristics, shown in even-numbered columns, consistently indicate that the earthquake's impact on mental health is non-trivial. However, the analysis does not find statistically significant evidence that the earthquake led to an increase in self-reported loss of focus. These findings corroborate existing literature that suggests natural disasters exacerbate mental health challenges, highlighting the importance of considering mental health as a critical component of the post-disaster public health response. The absence of a detectable effect on loss of focus may suggest that this symptom is less sensitive to the effects of the earthquake or that the measures used to capture it are less reliable. Nonetheless, the overall pattern of results emphasizes the broad impact of the Yogyakarta earthquake on mental health.

Table 2.1. Impact of the Yogyakarta Earthquake on Mental Health

	Feeling Anxious		Feeling Depressed		Sleeping Disorder		Losing Focus	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
<i>MMI_{cont}x post</i>	0.0043** (0.0018)	0.0053*** (0.0016)	0.0057*** (0.0015)	0.0069*** (0.0014)	0.0063* (0.0033)	0.0078*** (0.0030)	0.0012 (0.0015)	0.0019 (0.0015)
<i>MMI_{cont}</i>	-0.0015 (0.0014)	-0.0011 (0.0013)	-0.0018 (0.0015)	-0.0019 (0.0015)	-0.0011 (0.0019)	-0.0020 (0.0019)	-0.0001 (0.0009)	-0.0001 (0.0010)
mean pre-earthquake	0.0631	0.0631	0.0659	0.0659	0.1059	0.1059	0.0224	0.0224
percentage change	6.81%	8.39%	8.65%	10.47%	5.95%	7.37%	5.36%	8.48%
Observation	24,984	24,984	24,984	24,984	24,984	24,984	24,984	24,984
R-squared	0.0192	0.0245	0.0039	0.0095	0.0230	0.0261	0.0296	0.0291
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: (1) Sample: Java Island without controls, (2) Sample: Java Island with controls. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Appendix, we present both the original and Bonferroni-adjusted p-values for transparency. After applying the Bonferroni correction, some previously significant outcomes become non-significant due to the more conservative adjustment. However, several key results remain statistically significant.

In addition to the primary analysis, we conducted further analysis using eight extended mental health items available in IFLS 3, 4, and 5 to capture an expansive range of psychological distress. The results are shown in Table 2A.6 in the Appendix, which provides insights into the earthquake's impact on additional symptoms, including feelings of loneliness, being easily bothered, difficulty in getting going, and lack of energy. Specifically, the earthquake is associated with a 10 % increase in feelings of loneliness and a 6.77 % increase in difficulty getting going, calculated from the pre-earthquake mean. The findings show that these additional items also display significant increases in post-earthquake in the most affected areas, though with varying degrees of intensity.

2.5.2. Heterogeneous Effects

Certain groups may be more susceptible to psychological distress due to increased exposure and vulnerability to unfavorable circumstances (Peek, 2008; Jia et al., 2010; Joseph and Jaswal, 2021). Table 2.2 outlines the mental health outcomes following the Yogyakarta earthquake across different demographic groups, applying triple interaction terms to explore the heterogeneity of these effects. In Panel A, we observe that the earthquake significantly increased the likelihood of experiencing psychological distress among males, as indicated by the positive and statistically significant coefficients across all outcomes: feeling anxious, feeling depressed, having a sleep disorder, and losing focus. However, when examining the interaction term between the earthquake's impact and female gender, the coefficients are negative but not statistically significant, suggesting that there is no significant difference between males and females in the psychological response to the earthquake. This is further supported by the p-values from the tests for coefficients' equality, which show no significant differences between the genders across the outcomes studied.

In Panel B, the analysis shows that individuals over the age of 45²¹ experience a greater psychological impact from the earthquake compared to younger individuals. Specifically, the coefficients for the interaction between MMI, the post-earthquake period, and being above 45 years of age are positive and statistically significant for feeling anxious, feeling depressed,

²¹ The age is defined as the age at the time the earthquake occurred.

and having a sleep disorder. This indicates that older individuals are more susceptible to the mental health impacts of the earthquake. The p-values associated with the tests for coefficients' equality confirm that these differences are statistically significant, particularly for feeling anxious, feeling depressed, and having a sleep disorder, highlighting the increased vulnerability of older adults in the aftermath of the earthquake.

Panel C focuses on the differences between urban and rural residents. The results show that individuals residing in rural areas are more likely to experience psychological distress due to the earthquake. The interaction term between MMI, the post-earthquake period, and residing in a rural area is positive and statistically significant for feeling anxious, feeling depressed, and losing focus. This suggests that rural residents face greater mental health challenges following the earthquake compared to their urban counterparts. The p-values for the coefficients' equality tests indicate that the differences in psychological distress between rural and urban residents are statistically significant for feeling anxious and feeling depressed.

Finally, in Panel D, the analysis examines the differences in psychological distress based on household income levels. The results show that individuals from low-income families are more likely to experience an increase in feeling anxious due to the earthquake, as indicated by the positive and statistically significant coefficient for the interaction between MMI, the post-earthquake period, and belonging to a low-income family. However, the interaction terms for feeling depressed, having a sleep disorder, and losing focus are not statistically significant, indicating no significant differences between income groups for these outcomes. The p-values associated with the tests for coefficients' equality further support this finding, showing no statistically significant differences in psychological distress between income groups.

The results suggest that while the earthquake has a broad impact on psychological distress, its effects are more pronounced among specific vulnerable groups, including older adults, rural residents, and individuals from low-income families. These findings highlight the importance of targeted mental health interventions in post-disaster contexts, particularly for these at-risk populations.

Table 2.2. Heterogeneous Impact of the Yogyakarta Earthquake on Mental Health

	<i>Feeling Anxious</i>	<i>Feeling Depressed</i>	<i>Sleeping Disorder</i>	<i>Losing Focus</i>
Panel A: Gender				
<i>MMI_c x post</i>	0.0058*** (0.0018)	0.0078*** (0.0024)	0.0053* (0.0027)	0.0022*** (0.0015)
<i>MMI_c x post x female</i>	-0.0010 (0.0028)	-0.0016 (0.0030)	0.0045 (0.0043)	0.0006 (0.0032)
Test for coefficients' equality, p-value	0.7137	0.5996	0.2988	0.8509
mean pre-earthquake	0.0735	0.0776	0.1121	0.0279
Panel B: Age group				
<i>MMI_c x post</i>	0.0024 (0.0016)	0.0033** (0.0015)	0.0039 (0.0026)	0.0031* (0.0016)
<i>MMI_c x post x above45</i>	0.0062** (0.0024)	0.0081*** (0.0021)	0.0083*** (0.0028)	-0.0019 (0.0027)
Test for coefficients' equality, p-value	0.0103	0.0002	0.0035	0.4741
mean pre-earthquake	0.0714	0.0825	0.1335	0.0247
Panel C: Residential area				
<i>MMI_c x post</i>	0.0024 (0.0025)	0.0038** (0.0018)	0.0049 (0.0032)	0.0028 (0.0019)
<i>MMI_c x post x rural</i>	0.0070** (0.0027)	0.0074*** (0.0024)	0.0072 (0.0047)	0.0046** (0.0022)
Test for coefficients' equality, p-value	0.0126	0.0021	0.1251	0.4741
mean pre-earthquake	0.0626	0.0655	0.1043	0.0241
Panel D: Household income				
<i>MMI_c x post</i>	0.0026* (0.0015)	0.0044** (0.0018)	0.0064** (0.0029)	0.0014 (0.0025)
<i>MMI_c x post x low-income family</i>	0.0050* (0.0027)	0.0046 (0.0034)	0.0032 (0.0035)	0.0013 (0.0030)
Test for coefficients' equality, p-value	0.0691	0.1808	0.3639	0.6669
mean pre-earthquake	0.0669	0.0674	0.1063	0.0231
Observation	24,984	24,984	24,984	24,984
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes

Notes: Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

2.5.3. Event Study Estimates

To investigate the time pattern of the earthquake's effect, we operate an event study analysis for all outcome variables. Our event study framework is constrained by the availability of only four waves of data, two before and two after the Yogyakarta earthquake. Despite this limitation, we utilize the event study to observe any immediate changes in mental health indicators

that can be attributed to the earthquake. The year 2000 serves as the reference period, allowing us to compare the relative changes in the subsequent periods. While the limited data points preclude a comprehensive analysis of long-term trends, they do enable us to assess the presence and direction of the earthquake's impact on mental health over the available period. Figure 2A.4 in the Appendix provides a visualization of the results of this analysis.²² The figure shows the pre- and post-earthquake coefficients of the interaction for all outcomes and 95 per cent confidence intervals. We do not find any significant coefficient for the interaction term observed in the pre-earthquake periods across all outcome variables, except the outcome of losing focus. At the same time, we note an increase in psychological distress post-earthquake, as signaled by the positive and statistically significant coefficients for feeling anxious, feeling depressed, and having a sleep disorder.

While our event study did not reveal significant pre-trends, suggesting an absence of confounding trends before the earthquake, the increasing magnitude of the coefficients over time indicates that the earthquake's psychological impact may persist up to eight years after the event. This could reflect long-term effects often observed in disaster aftermaths, which are sometimes exacerbated by insufficient post-disaster psychological support or compounded by ongoing stressors. We recognize that humans often demonstrate the ability to survive and recover quickly from challenging situations; however, some studies suggest that this is not universally guaranteed. For instance, empirical findings show that the psychological effects of disasters can persist beyond the initial event, resulting in long-term impacts that can last for several years (Livanou et al., 2005; Rhodes et al., 2010; Kar et al., 2014; Cénat and Derivois, 2015; Schwartz et al., 2017). Moreover, there is a possibility that individuals experience delayed dysfunction, where they initially exhibit resilience following a traumatic event, with psychological distress exhibiting later (Bonanno, 2004; Galea et al., 2005; Norris et al., 2009).

The lasting psychological impact observed in our study may also be partly attributed to the context of Indonesia's mental health infrastructure. Indonesia faces a notable challenge with a low ratio of mental health

²² The results for the event study are reported in Table 2A.7 in the Appendix, showing only the coefficients of the interaction terms.

professionals and facilities compared to its population size (Mahendradhata et al., 2017; Trinidad and Protacio-De Castro, 2020). Additionally, the enactment of Law No. 18 in 2014, which specifically regulates mental health in Indonesia and aims to integrate basic mental health services into community health centers, came eight years after the earthquake. This systemic issue in mental health care provision may have contributed to the prolonged mental health consequences experienced by disaster survivors.

2.6. Robustness Checks

2.6.1. Dummy for Treatment

In our main analysis, we utilize continuous measurements to take advantage of the detailed variations in earthquake intensity, which provides an understanding of how different levels of exposure affect mental health outcomes. However, to ensure the robustness of our findings, we also perform analyses using a binary treatment variable by classifying respondents into high-intensity and low-intensity exposure categories. Table 2A.8 in the Appendix shows the results using this binary measure of earthquake intensity ($\text{MMI} \geq 7$), confirming the statistically significant impact of the Yogyakarta earthquake on feelings of anxiety, depression, and sleep disorders, which increased by 4.36, 4.62, and 4.70 percentage points, respectively.

Further, we acknowledge that increases in MMI levels likely have non-linear impacts on mental health, particularly when comparing more severe transitions (e.g., from MMI 7-8) against milder changes (e.g., from MMI 3-4). To address this, this paper examines the impact of the Yogyakarta earthquake on mental health outcomes across varying levels of earthquake intensity, categorized by binned MMI scales. The MMI scale is divided into five categories, with the 0 MMI range serving as the reference group. The results presented in Table 2.3 indicate a clear trend: as earthquake intensity increases, the psychological impact on individuals becomes more pronounced. Specifically, individuals in areas with moderate earthquake intensity (MMI 5.5–6.4) show significant increases in feelings of depression and sleep disorders compared to those in the reference category. As the intensity further increases (MMI 6.5–7.4 and $\text{MMI} \geq 7.5$), the psychological distress becomes more severe, with significant effects observed across all mental health measures, except for losing focus. This non-linear relationship

suggests that the mental health consequences of the earthquake are disproportionately higher in areas with more severe shaking, particularly in terms of feeling anxious, feeling depressed, and having a sleep disorder. The lack of significant effects on losing focus might indicate that this outcome is less sensitive to the earthquake's intensity or that it manifests differently in response to such events. Overall, these findings emphasize the importance of considering varying levels of exposure when assessing the mental health impacts of natural disasters.

Table 2.3. Impact of the Yogyakarta Earthquake on Mental Health: Series of Binned MMI Scales

	Feeling Anxious		Feeling Depressed		Sleeping Disorder		Losing Focus	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
<i>MMI</i>₌₀ <i>x post</i>	Reference category							
<i>MMI</i>_{>0-3.4} <i>x post</i>	0.0298 (0.0211)	0.0282 (0.0207)	0.0179 (0.0277)	0.0183 (0.0271)	0.0232 (0.0174)	0.0276 (0.0179)	-0.0094 (0.0169)	-0.0096 (0.0173)
<i>MMI</i>_{≥3.5-5.4} <i>x post</i>	0.0065 (0.0114)	0.0011 (0.0116)	0.0014 (0.0096)	0.0046 (0.0095)	0.0032 (0.0149)	0.0050 (0.0147)	-0.0047 (0.0106)	-0.0049 (0.0098)
<i>MMI</i>_{≥5.5-6.4} <i>x post</i>	0.0749 (0.0484)	0.0768 (0.0525)	0.0859* (0.0512)	0.0861* (0.0525)	0.0320 (0.0267)	0.0328 (0.0261)	-0.0045 (0.0111)	0.0013 (0.0109)
<i>MMI</i>_{≥6.5-7.4} <i>x post</i>	0.0461*** (0.0095)	0.0576*** (0.0102)	0.0482*** (0.0099)	0.0496*** (0.0124)	0.0432** (0.0118)	0.0454*** (0.0200)	0.0029 (0.0308)	0.0037 (0.0303)
<i>MMI</i>_{≥7.5} <i>x post</i>	0.0418*** (0.0132)	0.0486*** (0.0117)	0.0525*** (0.0067)	0.0603*** (0.0068)	0.0416** (0.0155)	0.0442*** (0.0158)	0.0017 (0.0103)	0.0072 (0.0101)
Observation	24,984	24,984	24,984	24,984	24,984	24,984	24,984	24,984
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5 % level, and * indicates significance at the 10 % level.

2.6.2. Exclusion of Municipalities

In our robustness analysis, we consider the potential impact of geographic proximity on the estimation of earthquake effects. Specifically, we replicate our main estimation by excluding observations of individuals who were residing in areas far from the most intensely shaken regions. This approach allows us to determine whether the results are robust to the exclusion of less affected municipalities that might differ significantly from the most affected ones. First, we exclude the regions outside the MMI coverage area, which extends approximately 300 km from the closest

affected municipality. As shown in Figure 2.1, the MMI coverage includes only the colored regions, indicating areas with recorded earthquake intensity values. Then, we narrow down the sample further by excluding municipalities located more than 150 km away from the nearest affected municipality, which is roughly half of the MMI coverage area.²³

There is also a possibility of potential commuter behavior across municipal borders that could bias the results. To address this concern, we exclude intermediate municipalities that are geographically adjacent to the core affected areas but are not within the most severely impacted zones, thereby creating a design similar to the donut approach. As illustrated in Figure 2.1, the intermediate municipalities surround the darkest red areas but not within them, with MMI values ranging from 4.8 to 6.6. This method has been effectively applied in similar research to control for spillover effects and commuter behavior (see Anderson, 2020; Nian, 2023). The overall results, presented in Table 2A.9 in the Appendix, remain statistically significant and of similar magnitude. This consistency suggests that our results are not driven by the inclusion of certain geographic areas but reflect a generalizable impact of the earthquake on mental health outcomes.

2.6.3. Placebo Analysis

To validate that our mental health outcomes are truly caused by the Yogyakarta earthquake, we conducted a series of placebo tests by simulating fictitious earthquake locations and years. This approach aligns with established methodological practices to validate causal inferences in observational studies. Specifically, we identified individuals residing in municipalities on Sumatra Island, Bali, and West Nusa Islands, and Kalimantan and Sulawesi Islands, as depicted in Figure 2A.5 in the Appendix. These islands were selected due to their lack of any recorded seismic activity during the period of interest, thereby serving as counterfactuals to Java Island, which we deliberately excluded from the placebo sample to maintain the integrity of the test. The fictitious earthquake is simulated by randomly assigning a binary treatment indicator to municipalities on each selected island, designating municipalities as

²³ We perform geometry computations to obtain the latitude and longitude coordinates of each municipality. This allows us to establish a distance radius of 150 km between less affected municipalities and their nearest most affected municipalities.

'affected' if the indicator equals 1, and 'non-affected' if it equals 0. The random assignment ensures that, on average, the affected and non-affected municipalities are comparable on observed and unobserved pre-treatment characteristics, facilitating a credible estimation of causal effects.

The map presented in Figure 2A.5 visualizes the results of this random assignment. Municipalities shaded in darker tones represent those that have been assigned a fake earthquake, thus indicating affected status. In contrast, municipalities in lighter shades have been assigned a control status, indicating they have not been affected by the simulated earthquake. To assess the impact of this simulation on our outcome variables, we employ the same specification detailed in the main analysis, including controls for individual and year fixed effects. The underlying assumption of this approach is that if the actual earthquake influences mental health outcomes, we will not expect to observe significant effects in these placebo tests where the earthquakes are fictitious. Consistent with our expectations, the results presented in Table 2A.10 in the Appendix demonstrate that the simulated earthquakes in these non-affected regions did not statistically influence our outcomes, strengthening the conclusion that the psychological distress observed is specifically attributable to the 2006 Yogyakarta earthquake.

In addition to our spatial placebo tests, we extend the robustness by designating alternative years for the occurrence of the earthquake, following the approach utilized by Gröger and Zylberberg (2016) (same locations – different years). This specification describes as if the Yogyakarta earthquake occurred in 1995 and 1997, rather than the actual event year of 2006. We then assign the continuous MMI values to individuals based on their residential locations during these fictive years. Employing the same regression specification as in our primary analysis, we conduct a placebo regression to assess the impact on psychological distress, including an extensive set of controls and fixed effects. The empirical results presented in Table 2A.11 in the Appendix exhibit no significant association between the fictitious earthquake years and the outcomes. The coefficients for the interaction of MMI and the post-fake earthquake years are statistically indistinguishable from zero, indicating an absence of effect. The results thus reinforce our main findings that the psychological distress observed in the population is temporally connected to the actual 2006 Yogyakarta earthquake.

2.7. Migration and Attrition

We investigate the potential migration response following the Yogyakarta earthquake, which is a crucial consideration given that migration could serve as a coping mechanism and might confound the estimates of the earthquake's impact on mental health outcomes. Another concern is that if individuals who migrate after the earthquake have different mental health profiles compared to those who do not migrate, it could introduce bias into the analysis. To address this concern, we use a difference-in-differences model specified in Equation 2.1 with migration as the dependent variable, incorporating pre-earthquake mental health conditions as control variables. This approach allows us to isolate the impact of the earthquake on migration behavior while accounting for pre-existing differences in mental health. The analysis is conducted for the entire Java Island sample and subsets of the population within the MMI coverage area, within 150 km of the most affected areas, and excluding regions adjacent to the most affected areas (donut design). The estimated coefficients across all specifications, as presented in Table 2A.12 in the Appendix, are small and not statistically significant, even after controlling for pre-earthquake mental health conditions, suggesting that the earthquake did not lead to a significant likelihood of migration. We also perform an additional analysis in which we exclude individuals who moved to different municipalities post-earthquake. By eliminating these movers, we ensure that any potential selection bias caused by migration is minimized. The results, presented in Table 2A.13 in the Appendix, provide further evidence that the mental health impacts identified in the study are not the result of selection bias caused by migration due to the earthquake.

In addition to our migration checks, we examine attrition between survey waves within our dataset to investigate potential biases. Our primary objective is to determine whether individuals from affected municipalities are more likely to drop out due to the earthquake. To maintain a consistent and focused analysis, we assess attrition for periods that include both pre- and post-earthquake data, using a difference-in-differences design as specified in Equation 2.1. Specifically, we observe attrition between wave 3-4 (6 years before and 1 year after the earthquake) and wave 3-5 (6 years before and 8 years after the earthquake), and between wave 1-4 (13 years before and 1 year after the earthquake) and wave 1-5 (13 years before and 8 years after the earthquake). Attrition is defined as one if the individual is

present in one wave but absent in the following wave, and zero otherwise. Similar to the migration analysis, we include pre-earthquake measures of feeling anxious, feeling depressed, and having sleep disorders as control variables in our attrition analysis to account for any baseline differences in mental health that could influence an individual's likelihood of dropping out of the sample. The estimated coefficients, presented in Table 2A.14 in the Appendix, are small and statistically insignificant, suggesting that attrition in our sample is not systematically associated with the earthquake. This also indicates that the observed mental health impacts are unlikely to be driven by selective attrition based on pre-existing conditions.

2.8. Potential Mechanisms

This section explores potential pathways through which the 2006 Yogyakarta earthquake may have influenced mental health outcomes. The earthquake's immediate and indirect effects on individuals' environments, resources, and physical health likely shaped the observed psychological impact. We focus on damage and losses, socio-economic changes, and physical health deterioration as the primary mechanisms that may explain heightened psychological distress. These factors capture the multidimensional disruptions caused by the earthquake, which collectively contribute to anxiety, depression, and stress in the affected population.

The earthquake caused significant destruction in affected areas, leading to extensive housing damage, financial hardship, and family casualties. Using data from the fourth and fifth waves of the IFLS, we capture post-earthquake conditions through indicators such as destroyed or damaged houses, asset loss, and household casualties.²⁴ To test whether higher earthquake intensity correlates with increased damage and losses, we employ a difference-in-differences model specified in Equation 2.1, where these

²⁴ In our analysis, we encounter a limitation regarding the availability of pre-earthquake data on natural disasters. Specifically, the first (1993) and third (2000) waves of the IFLS do not contain information about individual or household experiences of natural disasters. This lack of data posed a challenge for constructing a pre-earthquake baseline using those waves. To address this gap, we utilized retrospective disaster data from the fourth wave (2007), which provided detailed information on natural disasters that occurred within the five years preceding the survey. By integrating this retrospective data, we were able to create variables reflecting pre-earthquake conditions.

indicators serve as dependent variables. The result, presented in Table 2.4, shows the association between earthquake intensity and damage-related outcomes, confirming that higher earthquake intensity led to greater property destruction, asset loss, and increased likelihood of death or injury among family members. These findings provide evidence that damage and loss serve as critical pathways influencing the psychological distress caused by the earthquake. The significant rise in these adverse outcomes suggests that the direct experience of loss and disruption could contribute substantially to elevated levels of anxiety, depression, and stress within the affected population.

The psychological distress caused by earthquakes may also be partially attributable to the disruption of socio-economic conditions and deterioration of physical health. Empirical evidence in academic literature has linked emotional well-being to individuals' socio-economic circumstances and physical health status (Turner and Kelly, 2000; Lorant et al., 2003; Ohrnberger et al., 2017; Ruiz-Pérez et al., 2017; Conversano, 2019; Reiss et al., 2019). To explore this assumption, we further employ the IFLS data concerning socio-economic status and physical health indicators. Our objective is to examine whether the earthquake influences specific aspects of socio-economic conditions, as indicated by occupational status, per capita expenditure, social engagement, and eating frequency.²⁵ Additionally, we assess the earthquake's impact on physical health dimensions, such as general health and physical ability.²⁶ A deterioration in these factors due to the

²⁵ Occupational status indicates whether a person is employed or unemployed, regardless of type and place of employment. It is equal to one if individual is unemployed at the time of the survey. Per capita expenditure reflects the average monthly consumption per household member, in the form of natural logarithm. Lack of social engagement equals to one if a person does not participate in any formal community or social activities at the time of the survey. Eating frequency is coded as one for individual whose food consumption falls below the standard adequacy level, as determined by the Food Consumption Score (FCS) (see Dembedza et al., (2022)).

²⁶ Unhealthy refers to a general state of poor health, encompassing a range of symptoms and conditions, including chronic illnesses, temporary illnesses, and general disease. General health is coded as one for the presence of an unhealthy condition and zero otherwise. Low physical ability refers to a reduced capacity to perform physical tasks and engage in daily activities, such as carrying heavy loads, walking short and long distances, and standing up without assistance. This may result from injury, chronic conditions, or general physical weakness. Low physical ability is coded as one for the presence of reduced physical capacity and zero otherwise.

earthquake would corroborate our preliminary findings. For a formal examination, we apply the same difference-in-differences models as in Equation 2.1, with socio-economic and physical health variables set as the dependent variables.

Table 2.4. Impact of the Yogyakarta Earthquake on Damage and Losses

	Destroyed/ damaged house	Asset loss	Family casualties
<i>MMI_{cont}x post</i>	0.0493** (0.0223)	0.0509** (0.0208)	0.0097** (0.0036)
<i>MMI_{cont}</i>	-0.0116 (0.0102)	-0.0098 (0.0094)	-0.0001 (0.0016)
mean pre-earthquake	0.0641	0.0792	0.0240
% change	76.91%	64.27%	40.41%
Observation	18,738	18,738	18,738
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes

Notes: Each coefficient comes from a different regression. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2.5. Changes in Individuals' Socio-economic Conditions

	Unemployed	Per capita exp	Lack of social engagement	Non-adequate eating frequency
<i>MMI_{cont}x post</i>	0.0074*** (0.0026)	-0.0019 (0.0051)	0.0181*** (0.0048)	0.0099*** (0.0031)
<i>MMI_{cont}</i>	0.0046 (0.0067)	-0.0205** (0.0080)	0.0371 (0.0432)	0.0005 (0.0022)
mean pre-earthquake	0.3505	12.2326	0.3884	0.0799
% change	2.11%	0.00%	4.66%	12.37%
Observation	24,984	24,984	17,822	24,984
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes

Notes: Each coefficient comes from a different regression. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2.5 presents the changes in socio-economic conditions following the earthquake. We hypothesize that the earthquake will lead to an increase in unemployment status in affected areas due to the destruction of workplaces and the disruption of economic activities. In alignment with our

expectations, the result indicates a statistically significant increase of 2.11 % relative to the pre-earthquake mean. However, we observe an increase in unemployment that did not correspond with a significant decrease in per capita expenditure. This seemingly paradoxical finding suggests that households may have employed coping mechanisms to stabilize their consumption levels despite the loss of income due to unemployment. To investigate this further, we explored the role of private transfers from relatives or friends as potential mitigating factors.²⁷ These forms of financial support could have compensated for the reduction in household income caused by increased unemployment, thereby preventing a decline in consumption levels. Using a difference-in-differences approach, we conduct additional analyses to examine the impact of the earthquake on the probability of receiving such transfers. The results, as presented in Table 2A.15 in the Appendix, indicate that households in areas more severely affected by the earthquake were indeed more likely to receive financial support in the form of private transfers by 7.17 %, with respect to the pre-earthquake mean. This external financial resource likely plays a crucial role in maintaining household per capita expenditure, despite the adverse effects of increased unemployment.

We also predict an increase in the lack of social engagement post-earthquake, as community infrastructures are disrupted, and individuals focus more on immediate survival and recovery needs. Supporting this hypothesis, we obtain a significant increase of 4.66 % in the lack of social engagement relative to the pre-earthquake average. With regards to per capita expenditure, we also expect a reduction reflecting economic hardships, as evidenced in a study by Rosales-Rueda (2018). However, we obtain a very small and statistically insignificant decrease. Additionally, the non-adequacy of eating frequency rises significantly by 12.37 %, with respect to the pre-event mean, pointing to a deterioration in nutritional habits following the disaster.

²⁷ Private transfers from relatives or friends differ from government aid in this context. To further explore the role of government aid, we present an analysis of its interaction effects, emphasizing the importance of structured and timely aid programs as a key recommendation. The results of this analysis are provided in Table 2A.16 in the Appendix.

Table 2.6 shows that individuals in areas experiencing higher earthquake intensity exhibit marked declines in physical health. Notably, there is a statistically significant increase in the prevalence of unhealthy conditions and low physical abilities, by 13.33 % and 7.14 %, respectively, compared to the pre-earthquake mean. The observed changes in physical health parameters, coupled with the socio-economic changes, suggest a complex interaction between factors influencing post-disaster psychological distress.

Table 2.6. Changes in Individuals' Physical Health Conditions

	Unhealthy	Low physical ability
<i>MMI_{cont}x post</i>	0.0100** (0.0041)	0.0030* (0.0015)
<i>MMI_{cont}</i>	-0.0044 (0.0119)	-0.0042 (0.0086)
mean pre-earthquake	0.0752	0.0418
% change	13.33%	7.14%
Observation	24,984	24,984
Controls	Yes	Yes
Year FE	Yes	Yes
Individual FE	Yes	Yes
Municipality FE	Yes	Yes

Notes: Each coefficient comes from a different regression. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

To further explore potential pathways through which the earthquake impacted mental health, we incorporate identified mechanisms, such as damage, losses, physical health, and socio-economic factors, as control variables in our main model.²⁸ This approach allows us to assess their collective influence on mental health outcomes. If the primary earthquake coefficient diminishes after these factors are included, it suggests that they contribute to explaining the psychological impact of the earthquake. The results, as presented in Table 2A.17 in the Appendix, show that while the psychological distress outcomes remain robust, the main coefficient decreases, indicating that these factors collectively play a role in mediating the earthquake's mental health effects.

²⁸ We draw inspiration from the process undertaken by Decker & Schmitz (2016) and Eibich (2015), who utilize a simple analysis to investigate potential mechanisms.

Among these potential pathways, family casualties and general health deterioration appear particularly influential, emphasizing the significant role of both immediate personal loss and physical health decline in exacerbating mental health issues. Importantly, the continued significance of some psychological distress outcomes, even with these controls, suggests that additional potentially unobserved elements influence mental health post-earthquake. These findings underscore the complex nature of psychological impacts following natural disasters, where multiple, interrelated factors are likely at play. While the identified pathways capture several key influences, further exploration into additional mechanisms may help to deepen our understanding of the full picture of mental health effects induced by the earthquake.

2.9. Conclusion

This study investigates the substantial mental health impact of sudden catastrophic events, focusing on the 2006 Yogyakarta earthquake in Indonesia. Leveraging the unpredictability of this devastating event, our analysis draws on extensive individual-level panel data from the IFLS integrated with detailed seismic intensity data from the US Geological Survey. By comparing individuals exposed to different MMI levels, we empirically demonstrate that those in higher MMI regions were significantly more likely to report severe psychological distress. Specifically, the findings reveal an increased probability of experiencing anxiety, depression, and sleep disorders among individuals in high-impact areas. The analysis of robustness checks, including alternative treatment group definitions and falsification analyses, strengthens the causal interpretation of these findings.

To further understand the mechanisms underlying these mental health outcomes, we test several potential channels that may be driving the effects. Our results show that family casualties and declines in physical health contribute significantly to the documented negative mental health effects. While these variables explain much of the earthquake's impact, we acknowledge that other potential channels, which remain unexamined due to data limitations, could also be playing a role.

Our mechanism investigation suggests that effective policy interventions for post-disaster recovery should prioritize multiple

dimensions. Addressing immediate physical damage, economic losses, and family casualties is critical. For example, grief counseling, mental health support programs, and community-based services can help survivors cope with the loss of loved ones, which has a profound effect on psychological well-being. Economic support also proves essential; our analysis of interaction effects with government aid underscores that such support plays a crucial role in mitigating earthquake-induced psychological distress. More specifically, we document that receiving disaster aid significantly lowers the likelihood of experiencing anxiety and depression post-earthquake. This result highlights the need for structured and timely aid programs that not only address immediate physical needs but also facilitate mental health recovery. Strengthening healthcare services is equally important to minimize the decline in physical health, a major determinant of psychological well-being. Finally, labor market policies aimed at integrating affected individuals in the labor market can provide crucial economic stability, reducing financial pressures that compound mental health challenges.

Despite the valuable insights provided by this study, we want to acknowledge several limitations. Our reliance on self-reported mental health indicators, while useful for assessing psychological states, inherently lacks the objectivity of clinical diagnostic measures. Self-reported data is subjective and can be influenced by factors such as cultural norms, individual experiences, or response biases. Such biases could arise from the influence of social desirability or the stigma associated with mental illness, potentially leading to over- or under-reporting. Although objective clinical measures would be ideal (see Bertinelli et al. 2023; Y. Wang & Fattore, 2020), such data is not available in the IFLS. Nevertheless, self-reported indicators remain a widely accepted approach in large-scale population studies, offering meaningful insights into the psychological effects of natural disasters.

In conclusion, this research highlights the far-reaching mental health impacts of natural disasters and the critical importance of comprehensive post-disaster interventions. Effective support strategies must address not only the direct physical and economic consequences of disasters but also the broader health and socio-economic pathways that shape mental health outcomes. The observed positive impact of government aid in reducing psychological distress underscores the necessity of structured and responsive aid programs in raising resilience and recovery. By enhancing our

understanding of the pathways through which disasters affect mental health, this study offers policymakers actionable insights for designing strategies that can more effectively support affected populations, contributing to a resilient and healthier society in the aftermath of such catastrophic events.

Appendix: Additional Tables and Figures

Figure 2A.1. Total Number of Damages and Casualties Based on Districts/Municipalities and MMI Values

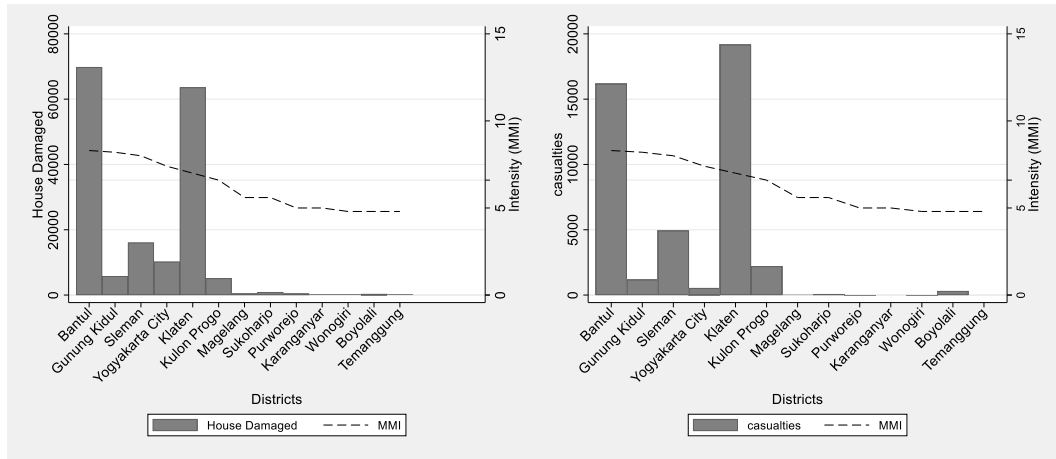
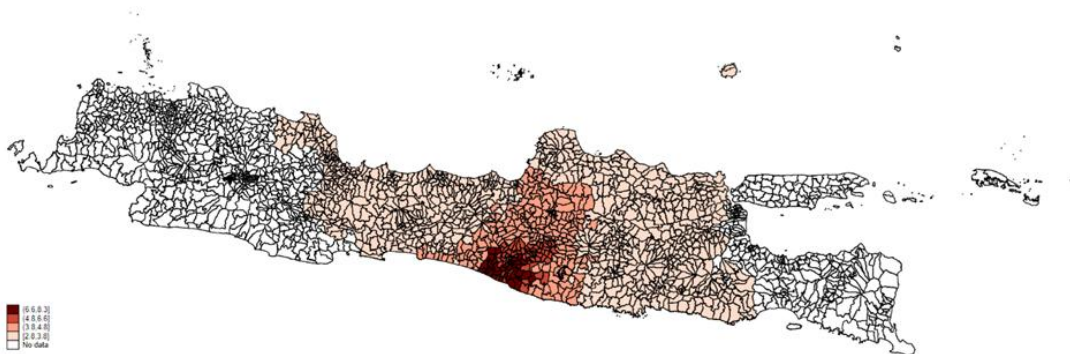
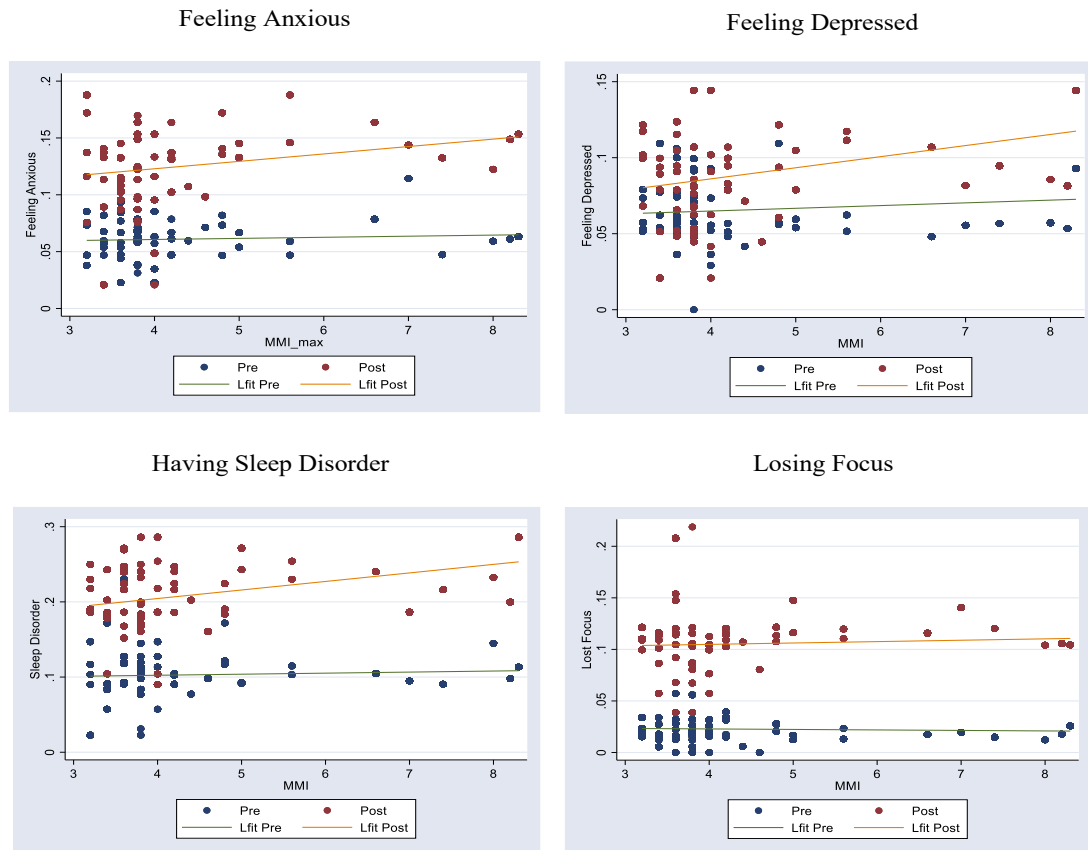


Figure 2A.2. Java Island and MMI Area at the Sub-District/Sub-Municipality Level



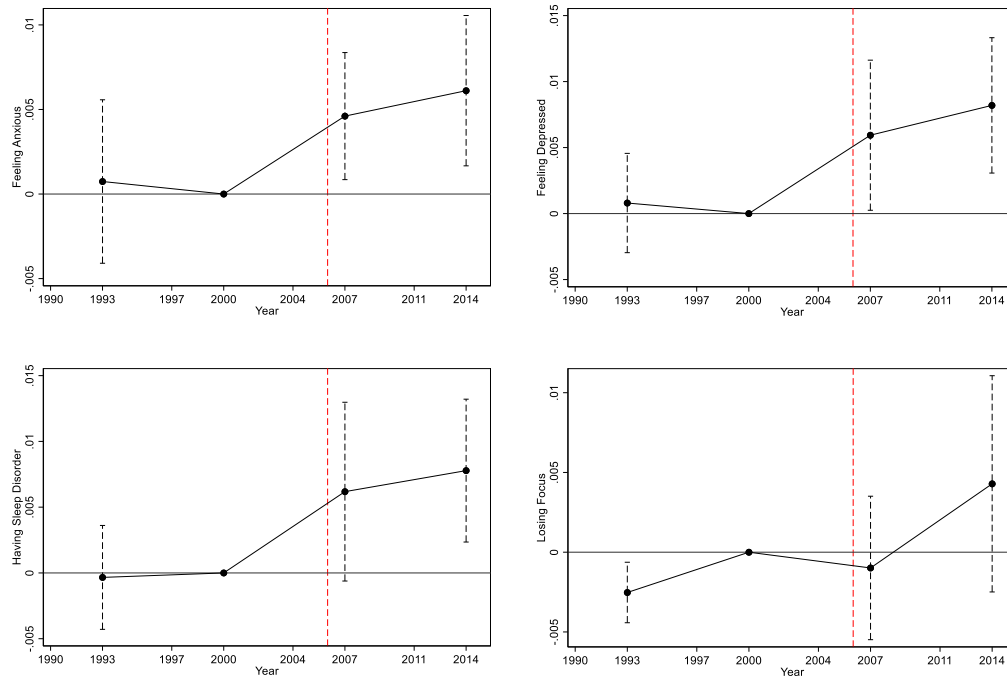
Notes: MMI values are recorded in the colored area. Darker shades reflect higher level of intensities (MMI) values.

Figure 2A.3. Mental Health Outcomes Based on Municipalities and the Range of MMI Values.



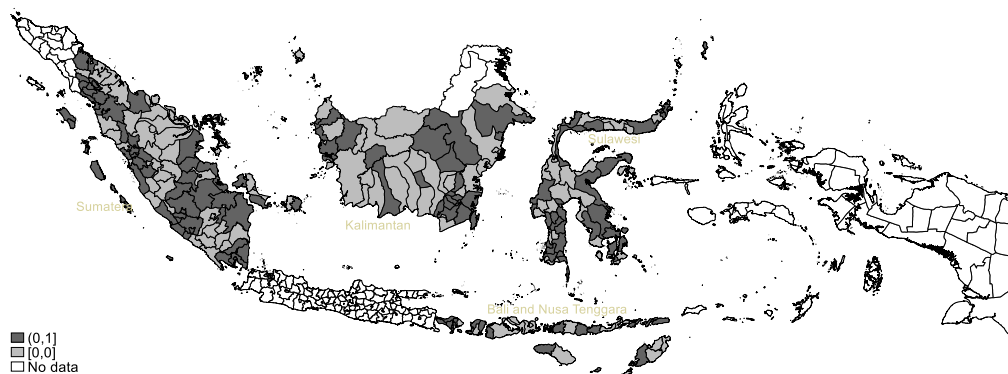
Notes: Blue dots indicate the pre-earthquake means, while red dots show the post-earthquake means.

Figure 2A.4. Effect of the Yogyakarta Earthquake on Mental Health over the Available Time Span.



Notes: This Figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interactions between year and continuous values of MMI on mental health outcomes. Year 2000 is the omitted category. The red vertical line indicates the time of the earthquake (2006). All regressions control for the year and individual fixed effect, and standard errors are clustered by municipality level.

Figure 2A.5. Randomly Assigned Fake Locations of Earthquakes.



Notes: Java Island is excluded from this estimate

Table 2A.1. Major Earthquakes in Indonesia 1990-2018

Year	Location	Magnitude (Richter)	Maximum MMI Scale	Damages (USD)	Casualties (Deaths/Injuries)
1992	Flores (Nusa Tenggara)	7.8	VIII	\$100 million	2,500 deaths, 2,103+ injuries
2000	Bengkulu (Sumatera)	6.7	VIII	\$41 million	103 deaths, 2,714+ injuries
2004	Aceh (Sumatera)	9.1	IX	\$4.5 billion	230,000 deaths, 125,000+ injuries
2005	Nias (Sumatera)	8.6	VIII	\$1 billion	915 deaths, 1,146+ injuries
2006	Yogyakarta (Java)	5.9	VIII	\$3.1 billion	5,778 deaths, 38,000+ injuries
2009	Padang (Sumatera)	6.6	VIII	\$700 million	1,195 deaths, 1,798 injuries
2009	Tasikmalaya (Java)	7.0	VI	\$160 million	128 deaths, 1,442+ injuries
2018	Palu (Sulawesi)	7.5	IX	\$911 million	4,140 deaths, 10,000+ injuries
2018	Banten (Java)	5.9	IV-V	\$250 million	453 deaths, 14,059+ injuries

Source: EM-DAT, The International Disaster Database

Table 2A.2. Impact of the Yogyakarta Earthquake on Mental Health: Shaking Exposure at Sub-District/Sub-Municipality Level

	Feeling Anxious		Feeling Depressed		Sleeping Disorder		Losing Focus	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
<i>MMI_{cont}x post</i>	0.0065*** (0.0016)	0.0073*** (0.0015)	0.0077*** (0.0014)	0.0087*** (0.0017)	0.0069** (0.0034)	0.0081** (0.0030)	0.0008 (0.0016)	0.0014 (0.0017)
<i>MMI_{cont}</i>	-0.0001 (0.0011)	0.0006 (0.0012)	-0.0010 (0.0014)	-0.0006 (0.0017)	-0.0001 (0.0013)	-0.0007 (0.0015)	0.0002 (0.0007)	0.0001 (0.0007)
mean pre-earthquake percentage change	0.0642 10.12%	0.0642 11.37%	0.0699 11.02%	0.0699 12.45%	0.1020 6.76%	0.1020 7.94%	0.0221 3.62%	0.0221 6.33%
Observation	24,984	24,984	24,984	24,984	24,984	24,984	24,984	24,984
R-squared	0.0218	0.0265	0.0094	0.0160	0.0260	0.0306	0.0335	0.0373
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: (1) Sample: Java Island without controls, (2) Sample: Java Island with controls. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the sub-district/sub-municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.3. Correlation Between Measures

	1	2	3	4	5
1. CESD -10	1.00				
2. Feeling anxious	0.21***	1.00			
3. Feeling depressed	0.21***	0.28***	1.00		
4. Having sleep disorder	0.24***	0.31***	0.26***	1.00	
5. Losing focus	0.22***	0.21***	0.27***	0.21***	1.00

Notes: This table presents correlations between CESD-10 scores and selected mental health indicators. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.4. Summary Statistics

	Pre-Earthquake			Post-Earthquake			Diff-Diff
	Not Affected (Control)	Affected (Treatment)	Diff	Not Affected (Control)	Affected (Treatment)	Diff	
Feeling Anxious	0.063 (0.242)	0.067 (0.249)	0.004 (0.007)	0.125 (0.331)	0.169 (0.375)	0.044*** (0.010)	0.040*** (0.011)
Feeling Depressed	0.065 (0.247)	0.074 (0.262)	0.009 (0.015)	0.083 (0.276)	0.134 (0.341)	0.051*** (0.018)	0.042*** (0.009)
Having a sleep disorder	0.105 (0.307)	0.113 (0.317)	0.008 (0.009)	0.210 (0.407)	0.261 (0.440)	0.051 (0.033)	0.043 (0.028)
Losing Focus	0.022 (0.147)	0.026 (-0.16)	0.004 (0.005)	0.106 (0.308)	0.109 (0.311)	0.003 (0.009)	-0.0011 (0.009)
Male	0.453 (0.498)	0.447 (0.497)	-0.006 (0.015)	0.453 (0.498)	0.447 (0.497)	-0.006 (0.015)	
Age (in years)	36.226 (12.595)	38.398 (14.238)	2.172 (0.389)	50.350 (12.575)	52.629 (14.121)	2.279 (0.389)	0.107 (0.076)
Married	0.768 (0.422)	0.727 (0.446)	-0.041*** (0.013)	0.805 (0.396)	0.798 (0.402)	-0.007 (0.012)	0.034 (0.037)
Education (years)	5.954 (4.111)	7.336 (4.568)	1.381*** (0.127)	6.053 (4.374)	7.401 (4.818)	1.348*** (0.135)	-0.034 (0.066)
Observations	11,306	1,186	12,492	11,306	1,186	12,492	

Table 2A.5. Impact of the Yogyakarta Earthquake on Mental Health: P-Value Adjustment for Multiple Hypothesis Testing Using the Bonferroni Approach

	Feeling Anxious		Feeling Depressed		Sleeping Disorder		Losing Focus	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
$MMI_{cont}x post$	0.0043** (0.0018)	0.0053*** (0.0016)	0.0057*** (0.0015)	0.0069*** (0.0014)	0.0063* (0.0033)	0.0078*** (0.0030)	0.0012 (0.0015)	0.0019 (0.0015)
Corresponding p-value	0.021	0.002	0.000	0.000	0.055	0.009	0.431	0.202
Adjusted p-value	0.168	<i>0.016</i>	<i>0.000</i>	<i>0.000</i>	0.440	<i>0.072</i>	1	1

Notes: The coefficients are derived from the main estimation in Table 1. (1) Sample: Java Island without controls, (2) Sample: Java Island with controls. Standard errors, in parentheses, are clustered at the municipality level. Adjusted p-values are calculated using the conservative Bonferroni method. Significant results after adjustment are marked in bold, and the adjusted p-values for significant results are italicized. Adjusted p-values that exceed 1 are set to 1. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.6. Impact of Yogyakarta Earthquake on Mental Health Using Extended Mental Health Items

	Feeling Anxious		Feeling Depressed		Sleeping Disorder		Losing Focus	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
<i>MMI_c x post</i>	0.0046***	0.0054***	0.0060***	0.0070***	0.0058*	0.0066**	0.0010	0.0019
	(0.0014)	(0.0016)	(0.0018)	(0.0017)	(0.0029)	(0.0027)	(0.0019)	(0.0020)
mean pre-earthquake	0.056	0.056	0.062	0.062	0.102	0.102	0.022	0.022
percentage change	8.21%	9.64%	9.67%	11.29%	5.69%	6.47%	4.54%	8.63%
Corresponding p-value	0.001	0.000	0.001	0.000	0.053	0.018	0.692	0.344
Adjusted p-value	<i>0.016</i>	<i>0.000</i>	<i>0.016</i>	<i>0.000</i>	0.848	0.288	1	1
	Feeling Lonely		Easily Bothered		Couldn't get going		Lack of Energy	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
<i>MMI_c x post</i>	0.0057***	0.0062***	0.0018	0.0019	0.0042*	0.0046*	0.0026	0.0027
	(0.0015)	(0.0016)	(0.0019)	(0.0021)	(0.0020)	(0.0022)	(0.0020)	(0.0020)
mean pre-earthquake	0.062	0.062	0.059	0.059	0.068	0.068	0.071	0.071
percentage change	9.19%	10%	3.05%	3.22%	6.18%	6.77%	3.66%	3.80%
Corresponding p-value	0.000	0.000	0.667	0.382	0.073	0.057	0.880	0.583
Adjusted p-value	<i>0.000</i>	<i>0.000</i>	1	1	1	0.912	1	1
Observation	18,738	18,738	18,738	18,738	18,738	18,738	18,738	18,738
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: (1) Sample: Java Island without controls, (2) Sample: Java Island with controls. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. Adjusted p-values are calculated using the conservative Bonferroni method. Significant results after adjustment are marked in bold, and the adjusted p-values for significant results are italicized. Adjusted p-values that exceed 1 are set to 1. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.7. Impact of Yogyakarta Earthquake on Mental Health: Leads and Lags for Difference-in-Difference Estimation

	Feeling Anxious	Feeling Depressed	Sleeping Disorder	Losing Focus
Lag (2) 1993	0.0007	0.0008	-0.0003	-0.0025**
	(0.0025)	(0.0019)	(0.0020)	(0.0010)
Lead (1) 2007	0.0046**	0.0059**	0.0062*	-0.0010
	(0.0019)	(0.0029)	(0.0036)	(0.0023)
Lead (2) 2014	0.0061***	0.0081***	0.0078***	0.0043
	(0.0023)	(0.0026)	(0.0028)	(0.0035)
Observation	24,984	24,984	24,984	24,984
R-squared	0.0201	0.0096	0.0262	0.0297
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes

Notes: Lag (1) 2000 is the omitted category. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

**Table 2A.8. Impact of the Yogyakarta Earthquake on Mental Health:
Binary Measure of MMI**

	Feeling Anxious		Feeling Depressed		Sleep Disorder		Lost Focus	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
<i>MMI_{≥7} × post</i>	0.0402*** (0.0113)	0.0436*** (0.0103)	0.0421*** (0.0087)	0.0462*** (0.0090)	0.0434 (0.0285)	0.0468* (0.0256)	-0.0011 (0.0086)	0.0027 (0.0085)
<i>MMI_{≥7}</i>	0.0035 (0.0127)	0.0108 (0.0124)	-0.0042 (0.0162)	0.0015 (0.0171)	-0.0053 (0.0184)	-0.0034 (0.0203)	0.0123 (0.0086)	0.0137 (0.0081)
mean pre-earthquake	0.0631	0.0631	0.0659	0.0659	0.1059	0.1059	0.0224	0.0224
Observation	24,984	24,984	24,984	24,984	24,984	24,984	24,984	24,984
R-squared	0.0147	0.0171	0.0047	0.0097	0.0234	0.0267	0.0295	0.0291
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

**Table 2A.9. Impact of the Yogyakarta Earthquake on Mental Health by
Geographical Proximity**

PANEL A:	Feeling Anxious			Feeling Depressed		
	MMI Coverage	150 km	Donut Shape	MMI Coverage	150 km	Donut Shape
<i>MMI_{cont} × post</i>	0.0131*** (0.0023)	0.0120*** (0.0034)	0.0051*** (0.0019)	0.0147*** (0.0033)	0.0146*** (0.0029)	0.0058*** (0.0016)
<i>MMI_{cont}</i>	0.0025 (0.0033)	0.0026 (0.0042)	-0.0012 (0.0015)	-0.0028 (0.0038)	-0.0004 (0.0050)	-0.0012 (0.0016)
Mean pre-earthquake	0.0586	0.0595	0.0630	0.0649	0.0609	0.0679
PANEL B:	Sleeping Disorder			Losing Focus		
	MMI Coverage	150 km	Donut Shape	MMI Coverage	150 km	Donut Shape
<i>MMI_{cont} × post</i>	0.0136** (0.0052)	0.0169*** (0.0058)	0.0063* (0.0032)	0.0022 (0.0029)	0.0018 (0.0036)	0.0017 (0.0009)
<i>MMI_{cont}</i>	-0.0043 (0.0039)	-0.0048 (0.0053)	-0.0010 (0.0022)	0.0013 (0.0019)	0.0006 (0.0020)	0.0005 (0.0010)
Mean pre-earthquake	0.1021	0.1006	0.1072	0.0214	0.0201	0.0233
Observation	14,195	9,163	23,316	14,195	9,163	23,316
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is determined based on the distance to the nearest affected municipalities. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.10. Effects of the False Location of the Yogyakarta Earthquake on Mental Health

	Feeling Anxious	Feeling Depressed	Sleeping Disorder	Losing Focus
Sample: Sumatra Island				
MMI_{>7} x post	-0.0071 (0.0104)	-0.0185 (0.0135)	0.0090 (0.0141)	-0.0101 (0.0131)
Observation	7068	7068	7068	7068
Sample: Bali and West Nusa				
MMI_{≥7} x post	-0.0116 (0.0187)	0.0010 (0.0152)	-0.0138 (0.0132)	0.0089 (0.0109)
Observation	4,984	4,984	4,984	4,984
Sample: Kalimantan and Sulawesi				
MMI_{≥7} x post	-0.0012 (0.0160)	0.0035 (0.0113)	0.0144 (0.0180)	0.0199 (0.0161)
Observation	3,860	3,860	3,860	3,860
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes

Notes: Each coefficient comes from a different regression. Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.11. Effects of the False Years of the Yogyakarta Earthquake on Mental Health

	Feeling Anxious	Feeling Depressed	Sleeping Disorder	Losing Focus
False Year: 1995				
MMI_{cont} x post	0.002 (0.001)	0.001 (0.002)	0.004 (0.002)	0.002 (0.001)
Observation	12,536	12,536	12,536	12,536
False Year: 1997				
MMI_{cont} x post	0.001 (0.001)	0.000 (0.002)	0.001 (0.003)	0.002 (0.001)
Observation	12,512	12,512	12,512	12,512
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes

Notes: Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.12. Impact of the Yogyakarta Earthquake on Migration

Sample:	Migration			
	Java Island	MMI Coverage	150KM	Donut Shape
<i>MMI_{cont}x post</i>	0.0048 (0.0042)	-0.0020 (0.0072)	0.0068 (0.0064)	-0.0062 (0.0043)
<i>MMI_{cont}</i>	-0.0225*** (0.0071)	-0.0121* (0.0068)	-0.0075 (0.0086)	-0.0234*** (0.0079)
Mean pre-earthquake	0.1956	0.1297	0.1036	0.1962
R-squared	0.2863	0.1419	0.1124	0.1419
Observation	24,984	14,195	9,163	23,316
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Pre-mental health controls	Yes	Yes	Yes	Yes

Notes: Individual controls include age, age squared, education, and marital status. Pre-mental health conditions include feeling anxious, feeling depressed, and having sleep disorders. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.13. Impact of the Yogyakarta Earthquake on Mental Health
(Excluding Individuals Who Migrate to Other Municipalities)

	Feeling Anxious		Feeling Depressed		Sleeping Disorder		Losing Focus	
	baseline	excl.mig	baseline	excl.mig	baseline	excl.mig	baseline	excl.mig
<i>MMI_{cont}x post</i>	0.0053*** (0.0016)	0.0054** (0.0019)	0.0069*** (0.0014)	0.0076*** (0.0014)	0.0078*** (0.0030)	0.0082** (0.0032)	0.0027 (0.0085)	0.0014 (0.0019)
<i>MMI_{cont}</i>	-0.0011 (0.0013)	-0.0013 (0.0015)	-0.0019 (0.0015)	-0.0025 (0.0018)	-0.0020 (0.0019)	-0.0021 (0.0023)	0.0137 (0.0081)	0.0005 (0.0011)
Mean pre-earthquake	0.0631	0.0628	0.0659	0.0671	0.1059	0.1041	0.0224	0.0221
Observation	24,984	20,964	24,984	20,964	24,984	20,964	24,984	20,964
R-squared	0.0245	0.0170	0.0095	0.0103	0.0261	0.0356	0.0291	0.0383
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.14. Attrition Between Survey Waves

	1 st and 4 th	1 st and 5 th	3 rd and 4 th	3 rd and 5 th
	(2)	(3)	(4)	(4)
<i>MMI_{cont}x post</i>	-0.0026 (0.0067)	-0.0063 (0.0113)	0.0044 (0.0027)	0.0036 (0.0029)
R-squared	0.1393	0.2372	0.3227	0.3142
Observation	9,124	9,124	7,658	8,796
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Pre-mental health controls	Yes	Yes	Yes	Yes

Notes: The dependent variable is equal to 1 if an individual observed in one wave but not in the following wave. Individual controls include age, age squared, education, and marital status. Pre-mental health conditions include feeling anxious, feeling depressed, and having sleep disorders. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.15. Impact of the Yogyakarta Earthquake on the Probability of Receiving Transfers from Families

	Receiving transfer
<i>MMI_{cont}x post</i>	0.0042** (0.0021)
<i>MMI_{cont}</i>	0.0201*** (0.0052)
mean pre-earthquake	0.0586
% change	7.17%
Observation	24,984
Controls	Yes
Year FE	Yes
Municipality FE	Yes
Individual FE	Yes

Notes: Each coefficient comes from a separate regression. Individual controls include age, age squared, education, marital status, home ownership, unemployment status, and household size. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.16. Impact of the Yogyakarta Earthquake on Mental Health by Government Aid/Assistance

	Feeling Anxious	Feeling Depressed	Sleeping Disorder	Losing Focus
<i>MMI_c x post</i>	0.0031 (0.0029)	0.0021 (0.0028)	0.0033 (0.0032)	0.0009 (0.0024)
<i>MMI_c x post x receiving disaster aid</i>	-0.0056** (0.0027)	-0.0062*** (0.0023)	-0.0067 (0.0051)	-0.0012 (0.0032)
Test for coefficients' equality, p-value mean pre-earthquake	0.0292 0.0368	0.0030 0.0449	0.1025 0.0341	0.5642 0.0377
Observation	11,805	11,805	11,805	11,805
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes

Notes: Individual controls include age, age squared, education, and marital status. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 2A.17. Effect on Mental Health, Including All Additional Controls

	Feeling Anxious		Feeling Depressed		Sleeping Disorder	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>MMI_{cont} x post</i>	0.0053*** (0.0016)	0.0021** (0.0015)	0.0069*** (0.0018)	0.0046*** (0.0019)	0.0078** (0.0029)	0.0038 (0.0035)
<i>MMI_{cont}</i>	-0.0011 (0.0013)	-0.0012 (0.0015)	-0.0019 (0.0015)	-0.0007 (0.0016)	-0.0020 (0.0019)	0.0003 (0.0020)
Destroyed house		0.0221 (0.0347)		0.0130 (0.0283)		0.0270 (0.0278)
Asset loss		0.0131 (0.0292)		0.0049 (0.0142)		0.0207 (0.0241)
Family casualties		0.0653*** (0.0221)		0.0239* (0.0140)		0.0514*** (0.0164)
Unemployed		0.0044 (0.0075)		0.0087 (0.0083)		0.0239** (0.0092)
Lack of social engagement		-0.0058 (0.0074)		0.0046 (0.0067)		0.0018 (0.0098)
Non-adequate eating frequency		0.0037 (0.0086)		0.0021 (0.0074)		0.0162* (0.0093)
Unhealthy		0.0480*** (0.0132)		0.0307*** (0.0106)		0.0468*** (0.0106)
Low physical ability		0.0254** (0.0114)		0.0330*** (0.0100)		0.0377*** (0.0137)
Observation	24,984	17,822	24,984	17,822	24,984	17,822
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The reduction in the number of observations is due to missing values for some observed potential mediators. All control variables are included simultaneously. Standard errors, in parentheses, are clustered at the municipality level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

3. From Infancy to Adulthood: Mental Health and Well-being Effects of Early Life Intervention

3.1. Introduction

The early years of human life represent a critical period of rapid brain development, during which foundational elements of cognitive, emotional, and physical capabilities are established (Tierney and Nelson, 2009; Haartsen et al., 2016; Kim et al., 2016). Recognizing the importance of early-life consequences, many public health programs aim to improve maternal and infant health through targeted interventions. These interventions may include supplemental nutrition, access to healthcare services, prenatal support, and extended maternity leave, all of which are designed to improve maternal well-being during and after pregnancy (Wüst, 2012; Dai et al., 2014; Ahammer et al., 2020; Fabel, 2021; Renard, 2022). For infants and young children, programs promoting breastfeeding, micronutrient supplementation, and appropriate weaning practices are equally critical, as they help support healthy physical growth and early development (Li et al., 2023). Such maternal and child health interventions are essential not only for reducing maternal and infant mortality but also for enhancing long-term health outcomes and mitigating the adverse consequences of inadequate prenatal and postnatal care (Lazuka, 2018; Noghanibehambari and Fletcher, 2023). These efforts are particularly important in low-resource settings, where health system limitations can exacerbate risks during the perinatal period and beyond.

This paper studies the impact of early life exposure through the deployment of qualified midwives in Indonesia, a program designed to enhance maternal and infant health care. The village midwife program, launched in 1989, involved the placement of approximately 54,000 trained midwives across the country, particularly in regions with limited access to skilled birth attendants. The program was designed to address persistent problems in prenatal and postnatal care and to reduce the high maternal and neonatal mortality rates caused by geographic isolation, inadequate

infrastructure, and a lack of qualified health personnel (Shankar et al., 2008). In this paper, I investigate the long-term effects of the midwife program's implementation on mental health and well-being later in life. While interventions that enhance maternal and infant health are well-documented to generate a range of short- and long-term benefits (See Barham, Tania ; Calimeris, 2008; Bharadwaj et al., 2013; Bütikofer et al., 2019; Campbell et al., 2014; Conti et al., 2016; East et al., 2023; Fabel, 2021; Hjort et al., 2017; Hoynes et al., 2016; Lazuka, 2018; Miller, 2006; Noghanibehambari & Fletcher, 2023; Olds et al., 2019; Sandner et al., 2018; Wüst, 2012), relatively little is known about their lasting effects on mental health and well-being aspects. Therefore, this study fills this gap by examining the causal impact of the program, contributing to a more wide-ranging understanding of the long-term impacts on psychological outcomes.

Evidence from prior research indicates that communities receiving village midwives typically have lower socioeconomic status, less-developed infrastructure, and worse health indicators than other regions (Frankenberg and Thomas, 2001; Frankenberg, Suriastini, et al., 2005). Consequently, communities that receive midwives systematically differ from those without access to the program, raising concerns about selection bias when making causal inferences based on direct comparisons across areas. To address this challenge, this paper exploits the program's expansive and staggered rollout across Indonesian communities. Specifically, I leverage spatial and temporal variation in the timing of midwife program implementation and individuals' birth years to allow for identify the causal impact of the program on mental health and well-being in adulthood. This study utilizes longitudinal data from the Indonesian Family Life Survey and employs a difference-in-differences strategy combined with event-study specifications to estimate the program's long-term effects.

The results consistently show that early-life exposure to the village midwife program leads to significant improvements in mental health and well-being outcomes in adulthood. Leveraging longitudinal data that tracks individuals from birth into adulthood, I find that the program reduces the probability of being at risk for depression by 4.9 percentage points, representing a 17.44% reduction relative to the pre-intervention mean. In terms of well-being, the program increases the likelihood of self-reported happiness and perceived standard of living by 7.3 and 7.0 percentage points,

corresponding to relative increases of 12.05 % and 12.82 %, respectively. These estimates are obtained using recent econometric approaches designed for staggered treatment adoption, including the difference-in-differences and event-study estimators developed by Callaway & Sant’Anna (2021) and Sun & Abraham (2021), which allow for more credible estimation of heterogeneous and dynamic effects.²⁹ The findings remain robust across multiple specifications, including placebo tests and alternative sample compositions. To shed light on potential mechanisms, the analysis further suggests that improvements in maternal health behaviors and infant characteristics are likely channels through which early-life exposure to the program translates into better mental health and well-being later in life.

This paper contributes to the growing body of research investigating early-life interventions and their impact on health outcomes. Research in this area is expanding, particularly within the context of developed countries. For instance, Goodman-Bacon (2021) explores the long-term effects of childhood Medicaid eligibility in the U.S., finding that early Medicaid exposure significantly improves adult health outcomes. Similarly, Hoynes et al. (2016) examine the long-term impacts of childhood access to the U.S. Food Stamp Program, showing significant improvements in self-reported health, as well as reductions in obesity, high blood pressure, and diabetes. In the case of Denmark, Hjort et al. (2017) demonstrate that the home visiting program in 1937, targeting all infants, led to lower mortality rates, fewer hospital admissions, and a reduced risk of cardiovascular disease in middle age. Positive health impacts from early-life interventions have also been observed in Sweden through various initiatives. Bhalotra et al. (2017) examine the infant care program introduced between 1931 and 1933, finding that exposure to the program during infancy reduced the risk of infant death and reduced the risk of dying before age 75. Lazuka (2018) explores the qualified midwife program of the 1880s, highlighting that trained midwives

²⁹ The Two-Way Fixed Effects model may introduce bias in the presence of staggered treatment adoption. Therefore, results are further examined using Callaway & Sant’Anna (2021) and Sun & Abraham (2021) estimators, which correct for potential issues related to treatment timing heterogeneity. For completeness, I also present results from alternative models to facilitate comparison in the main analysis.

attending home deliveries led to lower neonatal mortality and long-term reductions in cardiovascular disease and diabetes-related mortality. Additionally, Lazuka (2019) analyzes primary care reforms in the early 20th century and shows that exposure to these reforms at birth led to significant reductions in all-cause and cardiovascular mortality. In Norway, Bütikofer et al. (2019) find that improvements in well-child visits increased educational attainment, earnings, and intergenerational mobility while also leading to better nutrition and reduced infant mortality rates.

Previous research related to the village midwife program in Indonesia has also been linked to a range of positive outcomes. For example, Frankenberg & Thomas (2001) highlight that the program has contributed to significant increases in maternal body mass index (BMI), suggesting that improved maternal health services have had a direct impact on women's nutritional status during and after pregnancy. Additionally, research by Frankenberg et al. (2005) demonstrates that the program has led to notable improvements in children's nutritional status, with reductions in stunting and underweight rates. Further research has also linked the program to enhanced family planning practices. Weaver et al. (2016) find that the availability of midwives in rural communities has significantly increased the use of contraceptives and improved birth spacing practices. Beyond these direct health outcomes, the broader impacts of the program have also been explored. Cas (2012) investigates how early exposure to the program influences cognitive and educational outcomes, showing that children born in communities with midwives exhibit better school performance and higher cognitive abilities. Ahsan et al. (2021) extend these findings by examining the program's effects on adult health and economic outcomes. They highlight that individuals who were exposed to the program in early life experience better health in adulthood and higher economic productivity.

While extensive research has examined the effects of early-life exposure on physical health, the literature on mental health impacts remains relatively limited, particularly in low- and middle-income countries. This paper also contributes to the literature by focusing on mental health and well-being outcomes later in life, which have been previously unexplored in the context of Indonesia's village midwife program. This analysis provides valuable insights that may differ from those derived from developed countries. Emphasizing mental health and well-being outcomes is crucial, as

they form a fundamental component of overall health and economic productivity (Ohrnberger et al., 2017; de Oliveira et al., 2023). Research indicates that early-life conditions can have either beneficial or adverse consequences on mental health and well-being. For instance, Adhvaryu et al. (2014) explore how early life income fluctuations in Ghana affect adult mental health, finding that increased cocoa prices significantly diminish the likelihood of severe mental distress in adulthood. Similarly, Conti et al. (2021) report that home visiting programs considerably enhance mother-daughter interactions by encouraging positive behavior and reducing negative ones. Fabel (2021) also demonstrates that the extension of maternity leave in Germany decreased the likelihood of mental and behavioral disorders among children born following the reform. On the other hand, studies have shown that adverse early-life conditions, such as undernutrition, low socioeconomic status, financial instability, or natural disasters, can have long-term detrimental impacts on mental health and well-being (Singhal, 2019; Chen et al., 2020; Tosi and Grundy, 2021; David and Kumar, 2023).

The results from this study have important policy implications, particularly for developing countries. The presence of qualified midwives early in life not only improves maternal and newborn health but also positively influences mental health and well-being into adulthood. This suggests that investments in maternal health services yield long-term benefits beyond the immediate perinatal period. Moreover, the positive mental health outcomes associated with early access to midwifery services highlight the necessity of integrated health policies that address both physical and mental health from the earliest stages of life. These findings encourage the expansion of midwifery services and the development of similar initiatives, especially in underdeveloped and rural areas that lack adequate healthcare services. By expanding access to such services, these policies are expected to significantly contribute to the socioeconomic development of communities, promoting a healthier and more productive population.

The structure of this paper is as follows. Section 3.2 provides an overview of the village midwife rollout in Indonesia. Section 3.3 describes the survey data, sample selection, and summary statistics. Section 3.4 outlines the empirical strategy. The main results are presented in Section 3.5, followed by a series of robustness tests in Section 3.6. Section 3.7 discusses

the potential mechanisms underlying the main analysis, and the conclusion is described in Section 3.8.

3.2. Rollout of Village Midwife in Indonesia

In the late 1980s, Indonesia was struggling with significant challenges due to a high maternal and neonatal mortality ratio (Shankar et al., 2008). More than 50 percent of women delivered without skilled birth attendants, and about 20 percent gave birth outside medical facilities (Shiffman, 2003). Several factors may contribute to this problem. Beyond the lack of access to health services, maternal and newborn care practices in Indonesia are strongly influenced by diverse local belief systems. In many communities, traditional birth attendants are preferred, as women believe that sticking to traditional practices and following relatives' advice would ensure a healthy pregnancy and childbirth (Agus et al., 2012). Moreover, the challenge of accessing even the nearest health facilities leads many to depend on traditional birth attendants, who can offer their services directly at homes. In response to this complicated issue, the Indonesian Government, with financial support from the World Bank, initiated an ambitious program to underline the importance of healthcare services for pregnant women, especially in villages with no skilled birth attendance.

The village midwife program was established to provide services on prenatal, birthing, and postnatal care (World Bank, 2010). In addition to these core services, village midwives offer counseling on newborn care, ensuring proper nutrition (including breastfeeding and supplementary food intake), and promoting healthy practices for mothers and their families. They also undertake additional duties in the community, including encouraging healthy lifestyles, especially sanitation and nutrition, as well as providing basic immunization and contraception services (Frankenberg and Thomas, 2001; Weaver et al., 2016). During regular working hours, midwives conduct general practices at the nearest community health center, while they may operate private practices outside of these hours. A considerable benefit of village midwives is their capacity to provide home birth services, thus eliminating the need for pregnant women to travel to health facilities, unless medical complications develop. This initiative aimed to improve the accessibility of professional midwifery services, thus reducing the health risks associated with childbirth for both mothers and infants.

Known locally as *Bidan di Desa*, the village midwife program was first introduced in 1989, with placements continuing until the late 2000s (World Bank, 2010). The deployment of village midwives across Indonesian communities occurred in stages rather than all at once. Initially, about 5,000 midwives were available in communities at the time of the program's initiation, with the number growing annually to 54,000 by 1997 and 80,000 by 2009 (Shankar et al., 2008; Ahsan and Maharaj, 2022). The map in Figure 3.1 graphically represents the distribution of village midwives in select provinces and districts of Indonesia that were included in the 1993 and 1997/1998 IFLS surveys.³⁰ This color gradation shows temporal patterns in the placement of midwives, such as which regions were early adopters of the program, and which ones introduced midwives later. The white-colored districts are those where no village midwife program was recorded during the specified time frame. As the timeline progresses, I observe a predominant presence of green shades, which signifies a substantial increase in the placement of village midwives, particularly from 1993 to 1997. This green color saturation visually expresses the rapid expansion of the program during these years, suggesting a significant scaling up of efforts to provide midwife services across the surveyed districts.

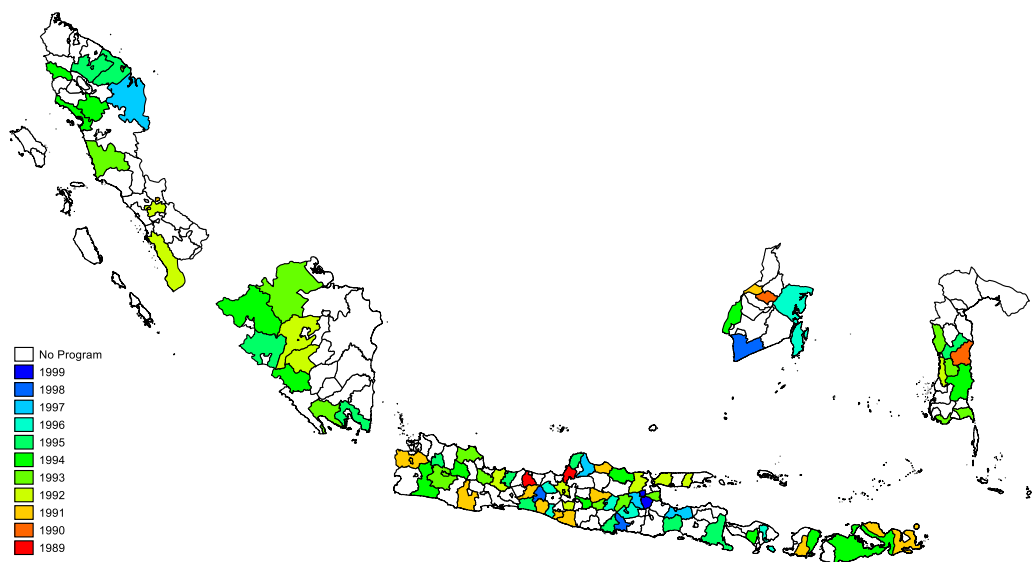
Figure 3.2 also exhibits a dual description of the midwives' rollout across communities from 1989 to 1999.³¹ The graph illustrates the annual percentage of communities receiving a village midwife for the first time each year and the cumulative percentage of communities with a village midwife. In detail, the right y-axis indicates the cumulative growth, reflecting the increasing reach of the program over the decade, while the left y-axis captures the year-by-year expansion, highlighting the program's annual placement into new communities. The period between 1992/1993 and 1996/1997 indicates a rapid acceleration, reflecting a significant increase in

³⁰ Due to the unavailability of community-level mapping data, Figure 2.1 utilizes district-level maps to illustrate the geographic distribution of village midwife placements during the period of interest. This figure is limited to the regions covered by the 1993 and 1997/1998 IFLS survey.

³¹ While the program continued into the 2000s, this analysis focuses on the 1989–1999 implementation period because the outcome data from the 2014 IFLS survey only include individuals aged 15 years and above, effectively limiting the analysis to individuals born before 1999. This restriction ensures alignment between program exposure and the availability of outcome data.

the annual placement of midwives within other communities. In line with the annual rollout, the cumulative coverage increases significantly over this period, indicating intensive efforts to expand the placement of midwives in villages across Indonesia.

Figure 3.1. Geographical Placement of Village Midwives Across IFLS Provinces and Districts

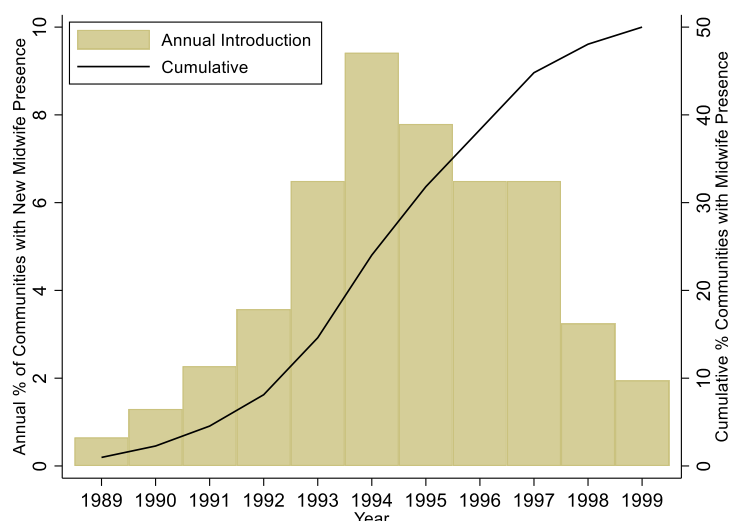


Notes: The map displays provinces and districts in Indonesia covered by the 1993 and 1997/1998 IFLS surveys. The different colors indicate the year when the first village midwife was placed in each district. White districts represent areas where no village midwife was introduced between 1989 and 1999.

This expansion of midwife placement also translated into increased utilization of midwifery services for childbirth. As presented in Figure 3A.1 in the Appendix, in the year of the program's introduction, the uptake of midwifery services reached approximately 30 percent, suggesting an immediate response to the availability of village midwives in newly covered communities. Over time, as the cumulative coverage of midwives expanded, the reliance on midwifery services continued to grow, with the uptake nearly doubling to 60 percent after five years. This trend indicates not only the effective integration of the program into maternal healthcare practices but also a broader shift towards institutionalized childbirth services, as more Indonesian women turned to professional midwifery care. The increasing reliance on village midwives suggests that the program played a crucial role in reducing traditional birth practices and encouraging safer, medically assisted deliveries. The alignment between the midwife rollout and the rising

uptake of midwifery services underlines the program's effectiveness in transforming maternal healthcare access and utilization across Indonesia.

Figure 3.2. Expansion of Village Midwife Program in Indonesian Communities: 1989-1999



Notes: The bars denote the percentage of communities that were introduced to their first village midwife in each respective year, indicating the time of the program's roll-out. The black line represents the cumulative percentage of communities with village midwives by year, illustrating the program's overall growth in coverage.

Source: Indonesian Family Life Survey-Community Information.

3.3. Data and Descriptive Statistics

3.3.1. Data and Sample Selection

To evaluate the impact of early-life exposure to village midwives on future mental health and well-being outcomes, this study utilizes data from the IFLS, an ongoing longitudinal survey that captures detailed information at individual, household, and community levels across five waves³². The first wave, initiated in 1993, sampled 7,224 households and covered approximately 22,000 individuals across 13 provinces and 312 communities, representing 83% of Indonesia's population. One of the important strengths of the IFLS is its high re-contact rate, which ensures consistent follow-up

³² IFLS data are freely available from this link: <https://www.rand.org/well-being/social-and-behavioral-policy/data/FLS/IFLS.html>. IFLS conducted in 1993, 1997, 2000, 2007, and 2014.

with participants, even for those who have migrated across districts or provinces (Strauss et al., 2016b). The longitudinal nature of the IFLS is particularly beneficial for this study, as it allows for the tracking of individuals who were exposed to the village midwife program during early life and enables the observation of their health and well-being outcomes in adulthood, as recorded in the fifth wave of the survey.

This paper primarily utilizes data from the 2014 wave of the IFLS, focusing on individual characteristics, mental health, and well-being outcomes. Additionally, data from the first three waves of IFLS are incorporated to provide a historical context, particularly concerning community, birth, and maternal characteristics. The community-level data offers detailed insights into the rollout of the village midwife program, capturing information such as the availability and number of midwives, the year they began service in each community, and the duration of their service.³³ Of the original 312 communities included in the dataset, approximately 154 had integrated a village midwife into their healthcare system between 1989 and 1999. The community data also includes critical variables related to infrastructure, such as the number of health facilities and proximity to markets, healthcare centers, and district capitals. In terms of birth and maternal characteristics, the study employs detailed questionnaires from IFLS, which record women's pregnancy histories, including information on birth dates, maternal age at childbirth, prenatal care, nutritional supplementation, midwife attendance at delivery, newborn weights, and breastfeeding duration. Since IFLS also tracks individuals' birthplaces, current residences, and migration histories, the longitudinal feature of this data allows the study to follow individuals from birth and correlate their early-life conditions with their characteristics in adulthood.

The main outcome variable in this paper is derived from self-reported measures that capture mild to moderate aspects of mental health. The analysis

³³ In each wave of the IFLS, specifically in community level questionnaire, both village leaders and heads of the village women's group were asked regarding the availability of village midwives. From IFLS 2 onwards, the survey included more detailed information like the timing of the first village midwife's arrival, their number, duration of service, and year they left the communities. This data also crosses verified with information from village health post volunteers about prenatal care and delivery services to ensure accurate reporting on the presence of village midwives in the communities.

uses the 10-item form of the Center for Epidemiologic Studies Depression Scale (CESD-10), a widely recognized instrument designed to identify depressive symptoms in the general population (Andresen et al., 2013). The CESD-10 is a shortened form of the original scale, consisting of ten items divided into two categories: eight items focusing on negative experiences, such as “I felt lonely”, “I felt depressed”, “I felt fearful”, which reflect the core symptoms of depression; and two items that capture positive experiences, specifically the frequency of “feeling happy” and “feeling hopeful”, serving to balance the scale by acknowledging positive affect. Respondents rate how often they experienced these emotions in the past week using a 4-point Likert scale. Each question is evaluated using a 4-point Likert scale ranging from (1) rarely or none of the time (<1 day); (2) some or a little of the time (1–2 days); (3) occasionally or a moderate amount of the time (3–4 days); and (4) most or all the time (5–7 days). For analysis, the eight negative items are scored from 0 (rarely or none of the time) to 3 (most or all the time (5–7 days)), whereas the two positive items are inversely scored from 3 (rarely or none of the time) to 0 [most or all the time (5–7 days)]. The total score ranges from 0 to 30, with higher scores indicating more severe depressive symptoms. For the analysis, a binary outcome is used, where individuals with a CESD-10 score of 10 or higher are classified as at risk of depression³⁴ (Singhal, 2019). Scores exceeding this threshold suggest the presence of depressive symptoms that may require clinical attention. The categorization into “at risk” versus “not at risk” simplifies the interpretation of results, providing a clear understanding of the mental health situation within the community and yielding a more straightforward conclusion to policymakers and practitioners.

In addition to mental health indicators, this study incorporates measures of subjective well-being from the IFLS data, which assess individuals' levels of happiness and their perceived standard of living. These indicators are crucial for a comprehensive understanding of quality of life and are closely linked to mental health outcomes (Zheng, 2016; Keller, 2020). By integrating the aspect of well-being, this analysis captures a broader range of emotional

³⁴ In the CESD-10 scale, a score of 10 or higher is indicative of depression, whereas the CESD-20 utilizes a higher cutoff point, with scores of 16 or above signaling the presence of depressive symptoms (Haley and Freeman, 2018).

and psychological states, contributing to a more universal assessment of mental health. It is also important to mention that all outcomes in this study are based on self-reports. While recognizing the limited access to clinical diagnoses, the self-reported measures employed in this study still offer valuable insights and may act as a conservative approximation of the prevalence of mental health issues within the population.

A main consideration for this study is the constraints imposed by the 2014 mental health survey, which is only administered to individuals aged 15 and above. This age restriction requires careful cohort selection to ensure that all participants included in the analysis are eligible for mental health evaluation, thereby maintaining alignment with the survey's design. As a result, the primary sample is limited to individuals born between 1989 and 1999, who would have been between 15 and 25 years old at the time of the 2014 survey. This age range is also particularly significant for analyzing mental health outcomes, as it covers a critical developmental period, spanning late adolescence and young adulthood, during which mental health is particularly dynamic (Arnett, 2000; Halfon et al., 2017). Although the primary focus is on individuals born between 1989 and 1999, potential limitations arising from the exclusion of older birth cohorts, those born before 1989, are acknowledged.³⁵ To address concerns related to pre-existing trends in mental health outcomes, a placebo test is conducted (further detailed in section 6). It assesses the impact of the midwife program as if it had been introduced 10 years earlier, using individuals born between 1979 and 1988 as the sample. The placebo test aims to verify that the observed effects can be attributed to the midwife program, rather than to underlying mental health trends that may have existed before the program's initiation.

3.3.2. Descriptive Statistics

To ensure a more precise comparison in the summary statistics, the analysis focuses on communities that eventually received the village midwife program. This restriction avoids potential heterogeneity arising from

³⁵ Individuals born after 1999 are not included in the analysis as the 2014 survey only includes respondents aged 15 and older. Given this age criterion, those born after 1999 would have been younger than 15 at the time of the survey, and thus, their mental health outcomes were not recorded in the survey.

including never-treated communities and confirms that differences between the treated and control groups are primarily driven by the timing of program implementation rather than broader structural differences. Table 3.1 presents summary statistics for individuals born between 1989 and 1999 in implementing communities, categorized into treated and control groups based on whether they were born before or after the midwife program's introduction in their community. The treated group consists of individuals born in or after the year the program was introduced, while the control group includes those born before the program's initiation.

Table 3.1. Summary Statistics

	Cohort Born 1989-1999 in Implementing Communities					
	Control	Std.Dev	Treated	Std.Dev	Diff	Std.Err
<i>Mental health and well-being outcomes</i>						
At Risk of Depression	0.281	(0.431)	0.239	(0.405)	-0.042**	(0.019)
Happiness	0.606	(0.489)	0.676	(0.468)	0.070***	(0.027)
Standard of living	0.546	(0.498)	0.620	(0.486)	0.072***	(0.024)
<i>Maternal health practice outcomes</i>						
Prenatal check-up	0.505	0.411	0.647	0.481	0.142***	(0.021)
Iron supplementation intake	0.237	0.484	0.322	0.442	0.083***	(0.025)
Exclusive breastfeeding	0.311	0.494	0.402	0.462	0.090***	(0.022)
<i>Individual characteristics</i>						
Female	0.488	(0.500)	0.489	(0.500)	0.001	(0.014)
Birthyear	1,991	(1.991)	1,996	(2.162)	4.581***	(0.112)
Age in 2014	22.542	(2.141)	18.087	(2.182)	-4.455***	(0.112)
Firstborn	0.080	(0.272)	0.050	(0.218)	-0.030***	(0.009)
<i>Maternal characteristics at childbirth</i>						
Mother's age	27.511	6.305	27.740	6.446	0.229	(0.267)
Mother's years of education	3.257	(3.613)	3.139	(3.712)	-0.118	(0.125)
<i>Community characteristics at childbirth</i>						
Community has paved main road	0.798	(0.402)	0.796	(0.403)	-0.002	(0.023)
Number of community health centers	4.116	(3.961)	3.999	(3.661)	-0.118	(0.272)
Distance to the nearest health facility	1.640	(1.951)	1.580	(1.789)	-0.060	(0.091)
Number of Individuals	2,508		2,019			

Notes: The sample consists of 2,019 individuals in the treated group and 2,508 in the control group. Total sample from implementing communities is 4,527. Treated group is individuals born in the year or after the midwife program was introduced to their communities. The control group includes those born before the arrival of the midwife program in their communities. P-values for differences between treated and control groups across all measured variables are less than 0.001, indicating statistical significance. No significant differences were observed on individual gender and maternal characteristics at childbirth.

In terms of mental health and well-being outcomes, the treated group exhibits a lower probability of being at risk of depression and reports higher levels of happiness and a better standard of living. These patterns suggest that early-life exposure to midwife services may have contributed to long-term

improvements in mental health and overall well-being. Maternal health practices outcomes also differ between the two groups. Mothers in the treated group are more likely to have accessed prenatal care, taken iron supplements during pregnancy, and practiced exclusive breastfeeding. These differences are consistent with the objectives of the midwife program, which aimed to improve maternal and infant health through greater access to trained healthcare providers.

From a demographic perspective, gender distributions are balanced, reducing concerns of gender-related selection bias. However, as expected given the treatment definition, individuals in the treated group are younger at the time of the 2014 survey compared to those in the control group. Additionally, a slightly smaller proportion of individuals in the treated group are firstborn children, which could have implications for early-life resource allocation within families. Maternal characteristics at childbirth, such as maternal age and education, do not exhibit significant differences between the treated and control groups. This similarity reduces concerns of selection bias related to maternal factors at childbirth. The table also includes community characteristics at childbirth to assess potential structural differences between treated and control groups.³⁶ The results indicate no significant differences in infrastructure-related variables, such as road access, the number of community health centers, or the distance to the nearest health facility. This lack of variation is expected, as all sampled communities eventually received the midwife program and likely shared similar characteristics before implementation.³⁷

³⁶ Community characteristics at childbirth are constructed using retrospective data from the IFLS, specifically the reported establishment years of infrastructure and health facilities. This approach ensures that the community-level controls accurately reflect conditions at the time of birth, therefore could mitigate potential biases from using survey-year observations that might not correspond to early-life exposure conditions. However, not all infrastructure and health facilities have recorded establishment years in the dataset. Therefore, this analysis utilizes only those facilities for which such information is available.

³⁷ Summary statistics comparing pre-1989 characteristics of implementing and non-implementing communities are also presented in the Appendix. This comparison provides insights into baseline differences that may have influenced program rollout decisions.

3.4. Empirical Strategy

To assess the impact of early-age exposure to the village midwife program on mental health and well-being outcomes in adulthood, I leverage the staggered rollout of the program across communities between the observed period, 1989 and 1999. This implementation creates variation in the timing of exposure across birth cohorts, enabling the application of a difference-in-differences framework. The treatment is defined based on the year the program was introduced in each community. Individuals born in or after the year the midwife program was introduced are categorized as treated, while those born before the program's introduction serve as controls. The outcome variables measure long-term mental health and well-being, allowing for the identification of causal effects from program exposure during early age. The empirical model is specified as follows:

$$Y_{ict} = \alpha + \beta D_{ict} + \gamma X_{ict} + \theta_c^{pre-1989} + \delta_t + v_c + \epsilon_{ict} \quad (3.1)$$

Where Y_{ict} denotes the mental health and well-being outcomes in adulthood for individual i , born in community c , at time t . D_{ict} is an indicator variable for midwife exposure, set to 1 for individuals born in or after the year the midwife program was introduced in their community, and 0 otherwise.³⁸ The main coefficients of interest, β , measure the effect of village midwife availability in the community at the time of birth on the mental health and well-being outcomes. It represents an intention-to-treat (ITT) effect, reflecting the impact of the midwife program's presence in the community rather than the direct effects of service uptake. This approach is particularly valuable because it accounts for the broader influence of the program beyond direct engagement with midwife services. These broader influences include information distribution, improved health awareness, and indirect benefits such as enhanced maternal and child health practices. X_{ict} describes a set of characteristics, including birth order of the individual, as well as maternal education and age at childbirth. $\theta_c^{pre-1989}$ expresses time-

³⁸ While this exposure variable is clear, it is important to clarify that this definition captures early-life exposure to the program, as midwives' roles were primarily to improve maternal and infant health during prenatal and postnatal periods. This definition ensures that individuals categorized as "treated" were exposed to the program's potential benefits during the critical developmental stages of infancy and early childhood.

varying community characteristics before the first initiation of the village midwife program to account for pre-existing differences in community characteristics that may influence the timing of program rollout.³⁹ δ_t and v_c represent the birth-year and community of birth fixed effects, respectively, with standard errors clustered at the community of birth level. To simplify the causal interpretation of the village midwife program's impact, the main analysis focuses on a sample that includes only communities where the program was implemented between 1989 and 1999, intentionally excluding communities that were never treated during the period of interest. This restriction helps to exploit variation in the timing of the program's introduction across communities.

Given the staggered implementation of the village midwife program, the standard difference-in-differences approach with OLS may be prone to bias, particularly when comparing individuals from communities that received the program at different times. Recent advances in econometric methodology have identified the potential biases in Two-Way Fixed Effects estimators when analyzing staggered treatment adoption across units (Goodman-Bacon, 2018; de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021). These biases arise due to the Two-Way Fixed Effects assumption of homogeneous treatment effects, which contradicts the presence of heterogeneity in effects over time and across units. A particular bias also occurs when units treated earlier are utilized as controls for those treated later, leading to misrepresentation of the counterfactual outcome and consequently, biased treatment effect estimates. To address this concern, this paper utilizes the estimator developed in Callaway & Sant'Anna (2021), which is specifically designed for settings with staggered treatment adoption. This approach estimates group-time average treatment effects (CATT), offering a more flexible framework that accounts for variations in treatment effects across cohorts and over time. This method divides individuals into treatment and control groups based on when

³⁹ Due to data limitations for the period preceding 1989, I utilize retrospective data from the IFLS community survey to account for a set of pre-1989 community characteristics. The variables include the presence of paved main roads, number of community health centers, and distance to the nearest health facility. These data reflect community conditions prior to the implementation of the village midwife program, ensuring that they are exogenous to the treatment.

the midwife program was introduced in their community and compares the outcomes of individuals exposed to the program with those not yet exposed, confirming valid comparisons and avoiding the bias in traditional Two-Way Fixed Effects models.

Complementing this difference-in-differences scheme, I integrate an event-study method into the analysis to investigate the temporal dynamics of the program's effects and assess the validity of the parallel trend assumption. The event study assesses whether the timing of the program's introduction has differential effects across communities. Primarily, I utilize the event-study estimators by Sun & Abraham (2021), incorporating their Interaction Weighted (IW) estimator, which is designed to correct the biases commonly associated with standard event-study estimations in the presence of staggered treatment adoption.⁴⁰ This estimator improves traditional event-study models by constructing group-specific comparisons and properly weighting units based on their treatment timing, thereby mitigating contamination from earlier or later-treated units. The IW estimator helps ensure that only comparable units, those untreated or treated in the same relative period, are compared, reducing bias caused by differential treatment timing across communities. The specification for the event-study model is defined as follows:

$$Y_{ict} = \alpha + \sum_{\ell=-6, \ell \neq -1}^6 \partial_{e,\ell} \cdot (\mathbf{1}\{E_i = e\} \cdot D_{ict}^\ell) + \delta_t + v_c + \gamma X_{ict} + \theta_c + \epsilon_{ict} \quad (3.2)$$

The equation estimates how the mental health outcomes Y_{ict} evolve before and after the introduction of the village midwife program. The treatment effects are estimated dynamically over time, with $\partial_{e,\ell}$ representing the effect on group e at time ℓ years before or after the event. ℓ denotes the periods before and after treatment. The event time window is specified as $[-6, +6]$ to capture the dynamic effects of early-life exposure to the village midwife program within a critical developmental period. This range ensures

⁴⁰ Two-Way Fixed Effects regressions that include leads and lags of the treatment (event-study approach) may also generate estimates with uninterpretable weights (Sun and Abraham, 2021). Specifically, when treatment timing varies across units, the coefficient for a specific lead or lag may be biased by the effects from other time periods, complicating the interpretation of dynamic treatment effects.

that the analysis focuses on the immediate prenatal and postnatal years surrounding the program's introduction, which are most relevant for maternal and infant health interventions. Longer-term effects beyond this range (e.g., -7, +7 and beyond) are not explicitly illustrated, as the results show similar trends with larger standard errors due to fewer observations.⁴¹ The event time dummy $\ell = -1$ is the omitted category; therefore, the estimated coefficient measures the impact of the midwife's program relative to the year before the event. By interacting with the time indicators D_{ict}^ℓ ⁴² with group-specific indicators $1\{E_i = e\}$ ⁴³, the model ensures that the treatment effects are measured for individuals exposed to the program. The specification also includes a set of control variables X_{ict} , birth-year fixed effect δ_t , and the community of birth fixed effects ν_c , with standard errors clustered at the community of birth level.

I expand the analysis by testing several analyses to confirm the robustness of the findings. First, I conduct a placebo test by assigning fictitious program implementation dates, set 10 years before the actual initiation of the village midwife program. This test ensures that the observed effects are not driven by pre-existing trends or other unrelated factors. Second, I implement sample restrictions to address potential confounding effects. Specifically, I restrict the sample to individuals whose families remained in the same community throughout the program's implementation period to check that the estimated effects are not influenced by selective population movements. Additionally, I conduct a time sensitivity analysis by dividing the sample into three distinct implementation phases: early, rapid, and late implementation. This test evaluates whether the program's effects vary across different implementation periods. Lastly, I expand the analysis to include communities that did not receive the village midwife program during 1989–1999. This broader sample analysis consists of two approaches. The first incorporates all community samples from the IFLS, providing a

⁴¹ To address this, I aggregate all observations -7 to -10 and +7 to +10 into binned periods -6 and +6. This approach increases the number of observations contributing to the estimates in the tails.

⁴² D_{ict}^ℓ is a variable that equals 1 if individual i born ℓ periods (years) before or after the introduction of the treatment in their community, 0 otherwise

⁴³ $1\{E_i = e\}$ is an indicator function that equals 1 if individual i belongs to a group e .

comparison between implementing and non-implementing communities during the study period. The second approach involves constructing a matched sample based on pre-1989 community characteristics, following methods outlined in Hjort et al. (2017) and Lazuka (2019).

To understand the mechanisms through which the village midwife program influences mental health and well-being later in life, this analysis examines the program's impact on maternal health behaviors, infant characteristics, and fertility responses during the implementation period. These intermediary outcomes provide insight into the pathways linking early-life exposure to long-term well-being. First, I investigate maternal health behaviors, including the likelihood of receiving prenatal care, consuming iron supplements, and practicing exclusive breastfeeding. The presence of a village midwife can enhance these behaviors by expanding access to antenatal care, delivering essential health education, and reinforcing maternal nutrition. Second, I examine infant characteristics, focusing on birthweight outcomes and the probability of receiving full vaccination during the first year of life. Adequate prenatal care and skilled birth attendance can lead to improved birth outcomes, while greater maternal awareness and increased healthcare access through midwives may promote higher immunization rates.

Third, I assess fertility responses by analyzing the probability of having an additional child during the midwife program's implementation period. Since fertility patterns can influence the composition of mothers utilizing maternal healthcare services, any systematic changes in fertility behavior could potentially shape estimates of the program's overall impact. To estimate treatment effects, I employ difference-in-differences and event-study methodologies, as outlined in Equations 3.1 and 3.2. Additionally, I conduct a heterogeneity analysis for all intermediary outcomes based on maternal characteristics, including younger mothers, less-educated mothers, and mothers from larger families. This approach provides insights into whether the program's benefits were more pronounced among disadvantaged groups who typically face greater barriers to accessing healthcare.

3.5. Results

This section examines the impact of early-life exposure to village midwives on mental health and well-being in adulthood. I utilize various

estimation methods for difference-in-differences and event study, as presented in Table 3.2 and Figure 3.3, to provide robust evidence of the program's effects and to explore differences in estimation techniques needed by staggered treatment adoption. The table and figure present results from three approaches: Callaway & Sant'Anna (2021), which account for heterogeneous treatment effects, Sun & Abraham (2021), which apply interaction-weighted estimators to address bias, and the Two-Way Fixed Effects approach. The findings consistently indicate that the presence of village midwives in communities had a significant positive impact on mental health and well-being later in life.

Specific to Table 3.2, Panel A indicates that the presence of village midwives significantly reduces the probability of being at risk of depression by approximately 4.4 percentage points when including covariates and controls (Column 2). This decrease represents a 15.66 % reduction in symptoms relative to the pre-event mean. These findings highlight the protective mental health benefits associated with early-life exposure to the program. Panel B reveals a positive and significant impact of the program on self-reported happiness. Specifically, exposure to the program increases the likelihood of happiness by approximately 7.5 percentage points (Column 2), equivalent to a 12.37 % improvement over the pre-event mean. This result suggests that the program's influence extends beyond physical health, enhancing subjective well-being in adulthood. Panel C shows that the program improves perceived living standards, with a significant increase of approximately 7.9 percentage points (Column 2), translating to a 14.47 % increase relative to the pre-event mean. This finding underscores the program's broader socioeconomic benefits. Across all panels, the estimated impacts are consistent across different estimation methods.

The event study results, displayed in Figure 3.3, provide a dynamic analysis of the impact of early-life exposure to village midwife programs on mental health and well-being outcomes. The coefficients and their 95% confidence intervals are plotted for individuals born six years before and after the program's introduction, with year -1 serving as the omitted reference period. The pre-treatment coefficients across all outcomes are statistically insignificant, confirming that there were no systematic differences in trends before the program's implementation. Importantly, the results consistently illustrate that the village midwife program generated substantial and

sustained improvements in mental health and well-being outcomes. Post-treatment, the coefficients for the risk of depression exhibit a consistent negative trend. Following the introduction of the midwife program (year 0), the likelihood of being at risk of depression decreases significantly, starting with an immediate reduction of approximately 4-6 percentage points. This decline intensifies over time, reaching its largest effect around the fifth and sixth years post-implementation. The consistency of these patterns across all estimation methods highlights the robustness of the results, with no significant variations in the magnitude or direction of the effects.

Table 3.2. Impact of Village Midwife Program on Mental Health And Well-Being

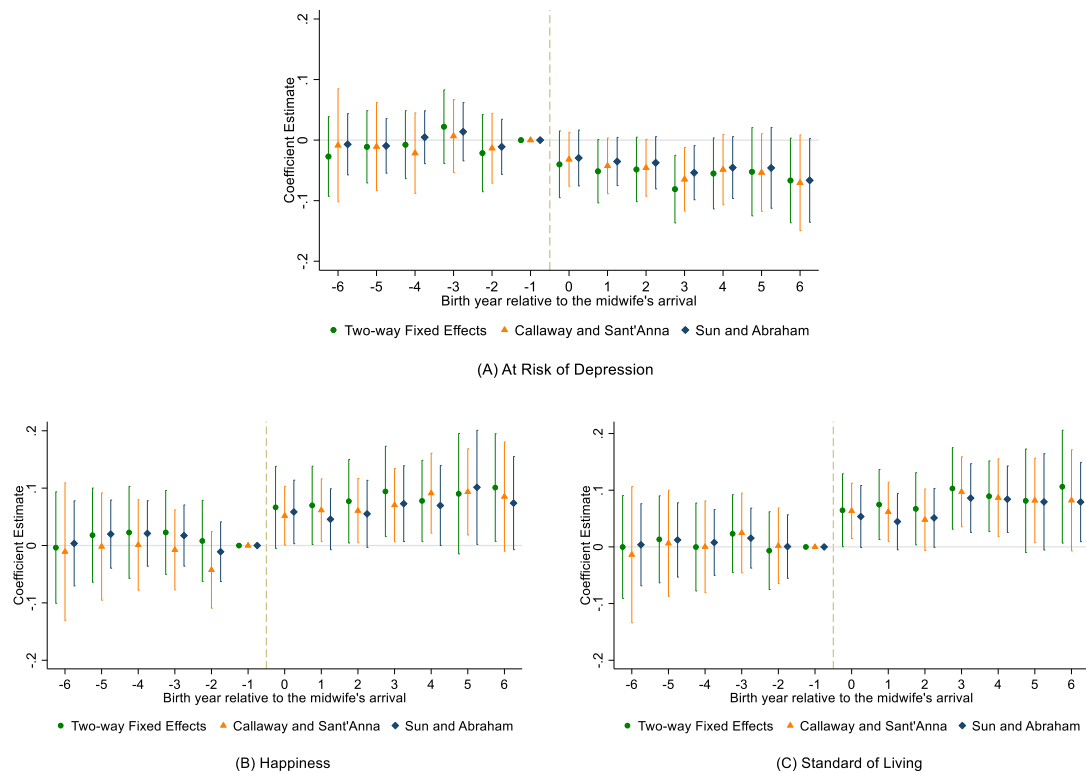
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: At risk of depression						
<i>Midwife Exposure</i>	-0.048** (0.023)	-0.049** (0.024)	-0.047*** (0.017)	-0.048*** (0.017)	-0.042** (0.019)	-0.044** (0.019)
Mean pre-event	0.281	0.281	0.281	0.281	0.281	0.281
Percentage change	17.08%	17.44%	16.73%	17.08%	14.94%	15.66%
Panel B: Happiness						
<i>Midwife Exposure</i>	0.072** (0.029)	0.073** (0.029)	0.065*** (0.021)	0.066*** (0.021)	0.069** (0.027)	0.076*** (0.026)
Mean pre-event	0.606	0.606	0.606	0.606	0.606	0.606
Percentage change	11.88%	12.05%	10.73%	10.89%	11.39%	12.54%
Panel C: Standard of living						
<i>Midwife Exposure</i>	0.068** (0.028)	0.070** (0.028)	0.068*** (0.019)	0.069*** (0.019)	0.072*** (0.024)	0.080*** (0.023)
Mean pre-event	0.546	0.546	0.546	0.546	0.546	0.546
Percentage change	12.45%	12.82%	12.45%	12.64%	13.19%	14.65%
Observation	4,527	4,527	4,527	4,527	4,527	4,527
Set of controls	No	Yes	No	Yes	No	Yes
Estimation method	Callaway & Sant'Anna	Callaway & Sant'Anna	Sun & Abraham	Sun & Abraham	Two-way Fixed Effect	Two-way Fixed Effect

Notes: Point estimates shown in each column are derived from separate regressions. The sample comprises individuals born in communities that implemented the program between 1989 to 1999. *Midwife exposure* set to one for individuals born in or after the year the midwife program was introduced in their community, 0 otherwise. Column (1) and (2) present estimates using the method developed by Callaway & Sant'Anna (2021). Column (3) and (4) show the results of the method described in Sun & Abraham (2021). Column (5) and (6) present results from the Two-Way Fixed Effects model. Odd-numbered columns exclude control variables, while even-numbered columns include a set of covariates, such as maternal and community characteristics. All regressions include community of birth and birth-year fixed effects. Robust standard errors, in parentheses, are clustered at the community level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

For happiness, the program's effects show as a statistically significant and sustained upward trend post-treatment. Immediately following the program's introduction, there is an increase of around 6-8 percentage points

in the likelihood of reporting greater happiness. This positive effect stabilizes in the first two years after the program but then intensifies slightly, reaching its peak around the third and fourth years. The trends across methods are also consistent for this outcome. Similarly, the coefficients for perceived standard of living show a positive line after the program's implementation. The likelihood of reporting improvements in living standards increases by approximately 7-9 percentage points in the years following the midwives' arrival. As with happiness, this effect is stable in the initial post-treatment years and shows a gradual strengthening in later years.

Figure 3.3. Event-Study Estimates of the Impact of Village Midwife Program on Mental Health and Well-Being



Notes: This Figure presents coefficient estimates with 95% confidence intervals for the dynamic effects of the village midwife program on mental health and well-being of individuals aged 15-25, who were born six years before and after a new village midwife was introduced in the community. The sample includes individuals born in communities that implemented the program between 1989 and 1999 (implementing sample). Year $t = -1$ serves as the reference category. The estimation includes community and birth-year fixed effects, with standard errors clustered at the community level.

3.5.1. Who Is More Impacted? A Heterogeneous Analysis

This section examines whether the effects of the village midwife program vary across different maternal and household characteristics, specifically maternal age at childbirth, maternal education, family size, and birth order. I hypothesize that younger mothers benefit more from midwife exposure due to their limited experience with pregnancy care, whereas the impact on older mothers remains uncertain, as they may have greater health awareness and prior childbirth experience. Additionally, I expect that less-educated mothers and those in larger households benefit more from the program, as they typically face greater barriers to accessing maternal healthcare. To assess these differences, I interact the midwife exposure indicator specified in Equation 3.1 with each subgroup.⁴⁴ The first analysis explores whether the effects of midwife exposure differ by maternal age at childbirth, with a focus on young mothers (≤ 19 years old). This group is of particular interest due to the elevated obstetric risks and socioeconomic vulnerabilities associated with adolescent pregnancies (Cavazos-Rehg et al., 2015; Londero et al., 2019). In this specification, the reference group includes all mothers above age 19, allowing for a direct comparison between young and non-young mothers.

The results, presented in Table 3.3 Panel A, indicate that for risk of depression and standard of living outcomes, the interaction between midwife exposure and young mothers is not statistically significant, suggesting that the program's effects do not differ significantly by maternal age for these outcomes. The equality test supports this conclusion. In contrast, for the happiness outcome, the interaction term for midwife exposure and young mothers is positive and statistically significant, indicating that individuals born to younger mothers benefited more in this aspect compared to those born to non-young mothers. The equality test also confirms that the program has a differential effect on happiness based on maternal age. This finding further suggests that younger mothers, who are often less experienced and have lower initial access to healthcare knowledge, may rely more on

⁴⁴ For the heterogeneous analysis, I use a Two-Way Fixed Effects model, as it allows for a direct estimation of treatment effect heterogeneity via interaction terms. This approach ensures that I can identify how midwife exposure differentially affects mental health and well-being across subgroups while still controlling for location and time-fixed effects.

midwives for guidance and support, leading to improved maternal well-being and greater long-term happiness for their children.

Second, I examine the impact based on maternal education levels, where the binary variable is coded as “1” for mothers who did not complete high school. This is also relevant since less-educated mothers traditionally have limited access to health literacy and prenatal care resources (Dai et al., 2014; Cantarutti et al., 2017; Noghanibehambari et al., 2022; Granés et al., 2023). If the midwife’s presence has a particularly advantageous effect on individuals who were born to these maternal backgrounds, it would be indicative of the critical role that midwifery services play in linking healthcare gaps. The results show that the midwife program’s effects are stronger for individuals born to less-educated mothers, particularly for happiness and standard of living. While the interaction term for depression is not significant, the coefficients for happiness and standard of living are both positive and highly significant, with the equality test confirming a significant difference. These results suggest that midwife exposure plays a compensatory role for individuals born to mothers with lower education, likely by improving maternal knowledge, health behaviors, and early-life conditions, thereby reducing socioeconomic disadvantages.

Finally, the analysis examines whether the effect of midwife exposure varies based on family size and birth order.⁴⁵ The results indicate that the program has a greater impact on children from larger families, particularly in happiness and standard of living, as confirmed by the significant interaction coefficients and equality test. One explanation is that larger households face more resource constraints, and midwife interventions provide additional support that helps mitigate these disadvantages, strengthening the role of maternal and child health services in reducing early-life inequalities. Similarly, firstborn children experience significantly greater benefits in these outcomes, with first-time mothers likely relying more on midwives for guidance, leading to better maternal health practices that particularly benefit their firstborns. Additionally, firstborns tend to receive greater parental attention and investment, which may amplify the long-term effects of

⁴⁵ Large family is coded as 1 if the individual was born into a large family and 0 otherwise. Firstborn is coded as 1 for firstborn children and 0 otherwise

midwife exposure. These findings suggest that midwife programs not only provide direct health benefits but also influence parental behavior, particularly in resource-limited environments and for firstborn children.

Table 3.3. Impact of Village Midwife Program on Mental Health and Well-Being by Maternal and Household Characteristics

	At risk of depression	Happiness	Standard of living
Panel A: Born to young mothers			
Midwife exposure	-0.047* (0.022)	0.056* (0.030)	0.071*** (0.027)
<i>Midwife exposure × young mother</i>	0.006 (0.030)	0.075** (0.033)	0.023 (0.033)
Test for coefficients' equality, p-value	0.847	0.032	0.661
Panel B: Born to low-educated mothers			
Midwife exposure	-0.047** (0.022)	0.073** (0.029)	0.071** (0.028)
<i>Midwife exposure × low educated mother</i>	-0.024 (0.026)	0.096*** (0.032)	0.068** (0.032)
Test for coefficients' equality, p-value	0.351	0.003	0.031
Panel C: Born in a large family			
Midwife exposure	0.049** (0.022)	0.078*** (0.028)	0.075*** (0.028)
<i>Midwife exposure × large family size</i>	0.024 (0.028)	0.090*** (0.029)	0.067** (0.032)
Test for coefficients' equality, p-value	0.382	0.003	0.038
Panel D: Firstborn			
Midwife exposure	0.045** (0.022)	0.079*** (0.028)	0.075*** (0.028)
<i>Midwife exposure × firstborn</i>	0.025 (0.027)	0.094*** (0.029)	0.069** (0.031)
Test for coefficients' equality, p-value	0.357	0.001	0.031
Observation	4,527	4,527	4,527

Notes: Each panel outlines the effects of midwife presence within a community along with an interaction term with the heterogeneous variables. The sample includes individuals born between 1989 and 1999 in communities which eventually received a village midwife program during the timespan (implementing sample). The specification in all columns includes community fixed effects, birth-year fixed effects, and a set of control variables. Robust standard errors are reported in parentheses, adjusted for clustering at the community level. Significance: *** 1% level, ** 5% level, and * 10% level.

Overall, the results indicate that while maternal age does not significantly influence the program's impact, individuals from more disadvantaged backgrounds, such as those with less-educated mothers, those from larger families, and firstborns, gain the most from the intervention. These findings highlight the broad reach of the midwife program while also

emphasizing its role in reducing socioeconomic disparities. Notably, the significant impacts are more consistently observed for well-being outcomes, particularly the standard of living and happiness in adulthood, rather than for psychological distress. This pattern suggests that the program may be especially effective at improving long-term perceptions of life quality among vulnerable populations.

3.6. Robustness Checks

3.6.1. Placebo Tests

This section introduces a set of placebo tests designed to detect any differential pre-existing trends. This involves constructing a counterfactual scenario in which I simulate the implementation of the village midwife program during the years preceding the actual start date of the program. Specifically, I assign program implementation to each year, a full decade before the true initiation of the program, and select an alternative cohort of individuals born between 1978-1987. This cohort selection is intentional, to exclude any potential influence on those who may have been in utero in 1988, the year immediately before the program was initiated. By doing so, I make sure that the placebo cohort is not affected by any anticipatory effects that the approaching program's implementation might have induced, thus maintaining the reliability of the analysis. I then re-estimate the impact on this new sample, applying the same methodological approach as in the main analysis.

The results from the placebo test, as presented in Table 3.4, indicate that the presence of the village midwife program does not have a statistically significant impact on the outcomes measured during the placebo period. The consistency of non-significant results across all outcomes and specifications suggests that any changes observed in the actual treatment period are unlikely to be driven by pre-treatment trends or other confounding factors present before the program's implementation. Similar to the main analysis, I also complement the placebo test with an event-study approach. This methodology graphically illustrates the dynamic effects of the fictitious village midwife program on mental health and well-being outcomes. As depicted in Figure 3.4, the event study spans six years, both before and following the fictitious program's start date, systematically examining any deviations from the trend in the years leading up to and after the supposed

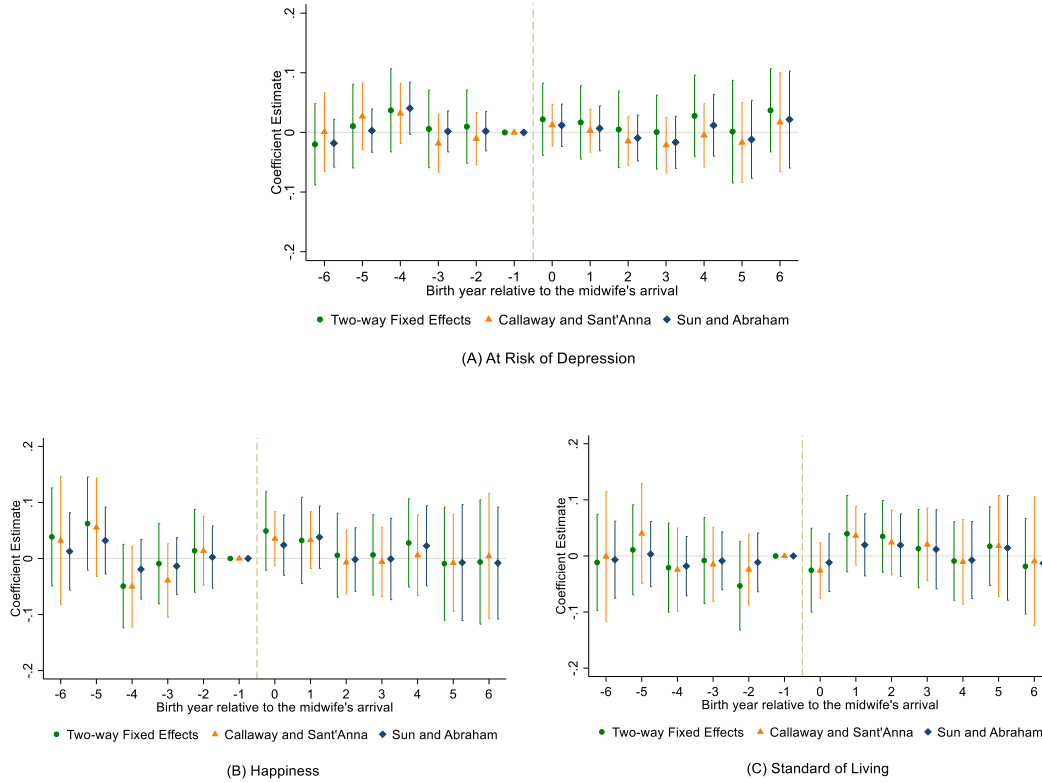
arrival of the midwife services. The sample includes individuals born in communities that would later implement the program, mirroring the treatment group under the actual program conditions and specifications. As shown in Figure 3.4, the coefficients during the placebo period do not indicate any systematic patterns that would suggest significant impacts on the outcomes of interest, thus offering further credibility to the interpretation that the positive effects observed in the actual treatment period are unlikely to be due to pre-existing trends or confounding variables.

Table 3.4. Placebo Test: Impact of Village Midwife Program on Mental Health and Well-Being

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: At risk of depression						
<i>Midwife Exposure</i>	-0.009 (0.021)	-0.010 (0.021)	-0.003 (0.016)	-0.005 (0.016)	-0.001 (0.016)	-0.005 (0.015)
Mean pre-event	0.277	0.277	0.277	0.277	0.277	0.277
Percentage change	3.24%	3.61%	1.08%	1.81%	0.36%	1.81%
Panel B: Happiness						
<i>Midwife Exposure</i>	0.009 (0.028)	0.012 (0.029)	0.005 (0.027)	0.015 (0.026)	0.007 (0.024)	0.011 (0.023)
Mean pre-event	0.608	0.608	0.608	0.608	0.608	0.608
Percentage change	1.43%	1.97%	0.82%	2.47%	1.15%	1.81%
Panel C: Standard of living						
<i>Midwife Exposure</i>	0.007 (0.028)	0.011 (0.028)	0.012 (0.024)	0.022 (0.024)	0.018 (0.017)	0.023 (0.017)
Mean pre-event	0.528	0.528	0.528	0.528	0.528	0.528
Percentage change	1.33%	2.08%	2.27%	4.17%	3.41%	4.36%
Observation	5,108	5,108	5,108	5,108	5,108	5,108
Set of controls	No	Yes	No	Yes	No	Yes
Estimation method	Callaway & Sant'Anna	Callaway & Sant'Anna	Sun & Abraham	Sun & Abraham	Two-way Fixed Effect	Two-way Fixed Effect

Notes: Point estimates shown in each column are derived from separate regressions. The sample comprises individuals born in communities that implemented the *placebo* program between 1978 to 1987. *Midwife exposure* set to one for individuals born in or after the year the midwife program was introduced in their community and 0 otherwise. Column (1) and (2) present estimates using the method developed by Callaway & Sant'Anna (2021). Column (3) and (4) show the results of the method described in Sun & Abraham (2021). Column (5) and (6) present results from the Two-Way Fixed Effects model. Odd-numbered columns exclude control variables, while even-numbered columns include a set of covariates, such as maternal and community characteristics. All regressions include community of birth and birth-year fixed effects. Robust standard errors, in parentheses, are clustered at the community level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Figure 3.4. Event-Study Estimates of the Impact of *Placebo Village* Midwife Program on Mental Health and Well-Being



Notes: This Figure presents coefficient estimates with 95% confidence intervals for the dynamic effects of the placebo village midwife program on mental health and well-being of individuals aged 15-25, who were born six years before and after the program was introduced in the community. The sample includes individuals born in communities that implemented the placebo program between 1979 and 1987 (implementing sample). Year $t=-1$ serves as the reference category. The estimation includes community and birth-year fixed effects, with standard errors clustered at the community level.

3.6.2. Sample Restrictions

To address potential confounding due to migration, I restrict the sample to individuals whose families did not migrate between 1989 and 1999, the key period during which the village midwife program was introduced. This restriction avoids exposure misclassification, as program assignment is based on the community of birth. Specifically, I exclude individuals whose families moved across communities, subdistricts, districts, or provinces during this period to ensure that the analysis focuses on non-

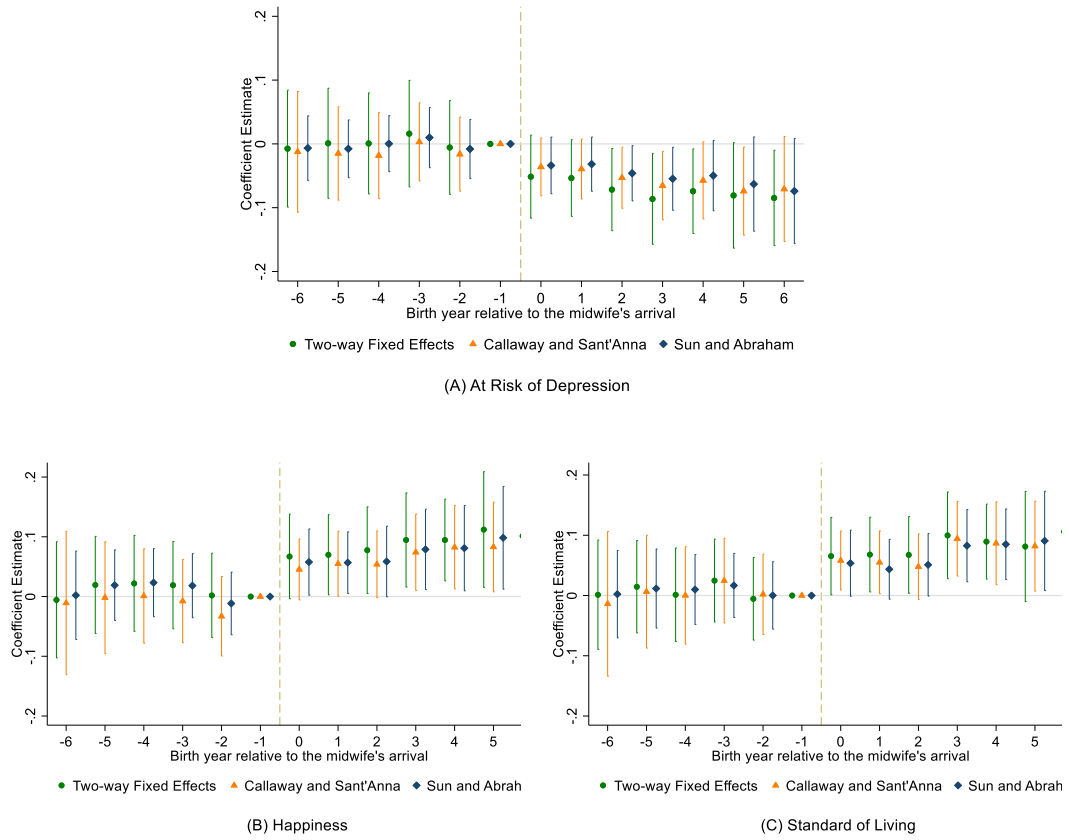
migrant households. This restriction reduces the sample from 4,527 to 3,541 individuals, implying that approximately 22 % of the baseline sample is excluded. This figure is consistent with prior evidence on internal migration patterns in Indonesia, where cumulative household mobility over a decade ranged between 20 % and 30 %, particularly among younger households (Badan Pusat Statistik, 2000; Pardede et al., 2020). By focusing on stable communities, this approach isolates the program's direct effects on mental health and well-being and mitigates bias from selective migration that could alter the composition of treated and control groups.

Table 3.5. Impact of Village Midwife Program on Mental Health and Well-Being: Non-Migrant Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: At risk of depression						
<i>Midwife Exposure</i>	-0.050** (0.025)	-0.051** (0.025)	-0.047*** (0.019)	-0.048*** (0.019)	-0.043** (0.019)	-0.043** (0.019)
Mean pre-event	0.283	0.283	0.283	0.283	0.283	0.283
Percentage change	17.66%	18.02%	16.61%	16.96%	15.19%	15.19%
Panel B: Happiness						
<i>Midwife Exposure</i>	0.069** (0.028)	0.075** (0.027)	0.064*** (0.020)	0.065*** (0.021)	0.067** (0.026)	0.077*** (0.026)
Mean pre-event	0.612	0.612	0.612	0.612	0.612	0.612
Percentage change	11.27%	12.25%	10.46%	10.13%	10.95%	12.58%
Panel C: Standard of living						
<i>Midwife Exposure</i>	0.069** (0.025)	0.076** (0.023)	0.069*** (0.020)	0.071*** (0.020)	0.075*** (0.024)	0.081*** (0.024)
Mean pre-event	0.549	0.549	0.549	0.549	0.549	0.549
Percentage change	12.57%	13.84%	12.57%	12.93%	13.66%	14.75%
Observation	3,541	3,541	3,541	3,541	3,541	3,541
Set of controls	No	Yes	No	Yes	No	Yes
Estimation method	Callaway & Sant'Anna	Callaway & Sant'Anna	Sun & Abraham	Sun & Abraham	Two-way Fixed Effect	Two-way Fixed Effect

Notes: Point estimates shown in each column are derived from separate regressions. The sample consists of individuals born in communities where the program was implemented between 1989 and 1999 and whose families did not experience residential changes. *Midwife exposure* set to one for individuals born in or after the year the midwife program was introduced in their community and 0 otherwise. Column (1) and (2) present estimates using the method developed by Callaway & Sant'Anna (2021). Column (3) and (4) show the results of the method described in Sun & Abraham (2021). Column (5) and (6) present results from the Two-Way Fixed Effects model. Odd-numbered columns exclude control variables, while even-numbered columns include a set of covariates, such as maternal and community characteristics. All regressions include community of birth and birth-year fixed effects. Robust standard errors, in parentheses, are clustered at the community level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Figure 3.5. Event-Study Estimates of the Impact of Village Midwife Program on Mental Health and Well-Being: Non-Migrant Sample



Notes: This Figure presents coefficient estimates with 95% confidence intervals for the dynamic effects of the village midwife program on mental health and well-being of individuals aged 15-25, who were born six years before and after a new village midwife was introduced in the community. The sample includes individuals born in communities that implemented the program between 1989 and 1999 (implementing sample) who have not experienced changes in their residence. Year $t = -1$ serves as the reference category. The estimation includes community and birth-year fixed effects, with standard errors clustered at the community level.

Using this restricted sample, I implement the difference-in-differences strategy described in Equation 3.1. The results, reported in Table 3.5, show that the estimated impacts of the village midwife program on mental health and well-being remain consistent with the main findings. Although the restriction reduces the sample size and limits generalizability to more mobile populations, it provides a more precise assessment of the program's effects in communities with uninterrupted exposure, further strengthening the credibility of the results.

To assess whether the impact of the village midwife program varies across different phases of implementation, I also conduct a robustness check by segmenting the sample based on the timing of the program's introduction. Specifically, I restrict the sample to individuals born between 1989 and 1999 in communities where the program was introduced in one of three phases: early implementation (1989–1992), intense expansion (1993–1997), and late implementation (1998–1999).⁴⁶ This segmentation allows for an evaluation of whether program effects are consistent across different rollout periods or vary based on the timing of exposure. This restriction also ensures that all individuals were born during the period when the program was actively rolling out while maintaining comparability across different implementation times. For each phase, I estimate the impact of the village midwife program on mental health and well-being outcomes using the same difference-in-differences framework as in the main analysis.⁴⁷

The results, presented in Table 3A.1 in the Appendix, indicate that the impact of the program is generally consistent across phases, though the magnitude and statistical significance vary. While the effects in early and late implementation communities are not significant without control variables, they become significant at the 10 % level after including maternal and community characteristics, implying that baseline differences influence program impact in these phases. In contrast, the program's effect on intense expansion communities is slightly stronger than in the main analysis and remains significant both with and without additional controls. The results suggest that higher program intensity, better infrastructure, and greater midwife adoption may enhance its effectiveness. These findings support the

⁴⁶ Late implementation refers to the latter part of the program's rollout within the defined analysis period (1989–1999). This period reflects the program implementation as captured in the study due to the restriction of outcomes measured in 2014, which only includes individuals aged 15 years and above. Consequently, individuals born after 1999 are not included in the mental health and well-being analyses, even though the program continued into the 2000s.

⁴⁷ It is important to note that the sample size decreases substantially in the early and late implementation phase, which may contribute to larger standard errors and lower statistical power. Therefore, the staggered difference-in-differences estimators by Callaway & Sant'Anna, (2021) and Sun & Abraham (2021) yield large standard errors and unstable estimates. To ensure reliable inference, I rely on the traditional Two-Way Fixed Effects model for this subsample, which remains consistent with the main analysis while providing more stable estimates in smaller samples.

overall conclusion that the village midwife program has long-term benefits for mental health and well-being, with some variation based on timing and contextual factors.

3.6.3. Broader Sample

This subsection expands the analysis to include communities that did not receive the village midwife program during the 1989–1999 period. First, I utilize all community samples from the IFLS, which allows for comparisons between implementing and non-implementing communities. The results are estimated using a standard difference-in-differences approach with Two-Way Fixed Effects, as the inclusion of non-implementing communities does not align with the staggered implementation assumptions required for the Callaway & Sant’Anna (2021) and Sun & Abraham (2021) estimators. Since the broader sample introduces untreated communities that may differ systematically from treated ones, there is an increased likelihood of regional differences influencing the results. To address this, province-by-birth-year time trends are incorporated, capturing region-specific dynamics such as economic growth, policy changes, or healthcare advancements that may evolve independently of the treatment. This specification ensures that the estimated program effects are not confounded by regional heterogeneity.

Columns (1) to (3) in Table 3.6 report consistent program effects across all specifications, even after including additional controls and province-by-birth-year time trends. For the outcome in Panel A, midwife exposure significantly reduces the probability of being at risk of depression by approximately 4 percentage points, corresponding to a 13.64 %–13.99 % reduction relative to the pre-treatment mean. In Panel B, exposure to midwife programs is associated with a significant increase in happiness by 6.2–7.3 percentage points, reflecting a 9.89 %–11.64 % improvement compared to the pre-treatment mean. Similarly, in Panel C, perceived standard of living improves by approximately 7–7.8 percentage points, or 12.21 %–13.81 %, relative to the pre-treatment mean.

Table 3.6. Impact of Village Midwife Program on Mental Health and Well-Being: Non-Migrant Sample

	All communities sample			Matched sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: At risk of depression						
<i>Midwife Exposure</i>	-0.039*** (0.013)	-0.041*** (0.013)	-0.040*** (0.013)	-0.041*** (0.014)	-0.042*** (0.014)	-0.042*** (0.015)
Mean pre-event	0.286	0.286	0.286	0.282	0.282	0.282
Percentage change	13.64%	14.34%	13.99%	14.54%	14.89%	14.89%
Panel B: Happiness						
<i>Midwife Exposure</i>	0.062*** (0.017)	0.073*** (0.017)	0.072*** (0.016)	0.067*** (0.021)	0.074*** (0.021)	0.073*** (0.022)
Mean pre-event	0.627	0.627	0.627	0.611	0.611	0.611
Percentage change	9.89%	11.64%	11.48%	10.97%	12.11%	11.95%
Panel C: Standard of living						
<i>Midwife Exposure</i>	0.069*** (0.017)	0.074*** (0.017)	0.078*** (0.017)	0.070*** (0.018)	0.078*** (0.019)	0.080*** (0.020)
Mean pre-event	0.565	0.565	0.565	0.551	0.551	0.551
Percentage change	12.21%	13.10%	13.81%	12.70%	14.16%	14.52%
Observation	7,497	7,497	7,497	4,521	4,521	4,521
Set of Controls	No	Yes	Yes	No	Yes	Yes
Prov × birth-year time trends	No	No	Yes	No	No	Yes

Notes: Point estimates shown in each column are derived from separate regressions. All communities sample includes individuals born in both implementing and non-implementing communities between 1989 and 1999. The matched sample comprises individuals from the same timeframe, born in matched implementing and non-implementing communities based on pre-1989 community characteristics. All regressions include community of birth and birth-year fixed effects. Prov × birth-year time trends are incorporated for each sample composition. Robust standard errors are reported in parentheses, adjusted for clustering at the community level. Significance: *** 1% level, ** 5% level, and * 10% level.

The second approach involves constructing a matched sample based on pre-1989 community characteristics to enhance comparability between treated and control communities in this broader sample context. Using propensity score matching, treated communities are matched to control communities with similar pre-treatment characteristics.⁴⁸ To preserve the uniqueness of matches and no duplication, each control community is used only once. The analysis then focuses on this matched sample, where pre-

⁴⁸ Due to data limitations for the period preceding 1989, the matching procedure utilizes only a subset of community infrastructure and access-related variables. To perform the matching procedure, I use a nearest neighbor matching, impose a caliper 0.05 without replacement, which is processed with psmatch2. The distribution of propensity scores for treated and untreated units illustrated in Appendix Figure 3A.3. The community characteristics before and after matching are presented in Table 3A.2 in the Appendix.

treatment covariate balance is achieved, as demonstrated in Table 3A.2 in the Appendix. Post-matching, the differences between treated and control groups become statistically insignificant, supporting the validity of the causal interpretation of the estimated effects. The results for the matched sample, presented in Table 3.6, columns (4) to (6), indicate that the impact of midwife program exposure on the probability of being at risk of depression, self-reported happiness, and perceived standard of living remains consistent in magnitude and statistical significance, strengthening the robustness of the main findings.

3.7. Potential Mechanisms

This section explores the potential pathways through which early-life health interventions influence mental health and well-being later in life. The village midwife program may shape long-term outcomes by improving maternal health behaviors, enhancing early-life conditions, and influencing household dynamics during critical developmental periods. To better understand these mechanisms, I examine the program's immediate effects on maternal health behaviors and infant health characteristics, as these factors represent the earliest and most direct channels. In addition, I assess whether the program may influence fertility decisions during the implementation period. These immediate impacts will, in turn, contribute to longer-term improvements in mental health and well-being.

3.7.1. Maternal Behaviors

Understanding the mechanisms through which early-life health interventions influence long-term mental health and well-being requires considering maternal health attributes during pregnancy and birth. This paper investigates these mechanisms by assessing the impact of the village midwife program on maternal health behaviors, specifically the likelihood of regular prenatal visits, iron supplementation, and exclusive breastfeeding.⁴⁹ The presence of a village midwife plays a crucial role in improving maternal health behaviors by expanding access to antenatal care, delivering essential

⁴⁹ Maternal mental health is a crucial factor that may mediate the impact of the program on a child's mental health later in life (Rogers et al., 2020; Hinke et al., 2022). However, the absence of direct measures of maternal mental health for the period under investigation limits a comprehensive examination of this channel.

health education, and promoting awareness of maternal nutrition and immunization protocols (Ngy et al., 2007; Roux et al., 2020). These behaviors reflect a sustained commitment to health throughout the perinatal period and are essential for maternal and infant well-being. To analyze these mechanisms, I use pregnancy summary data from the IFLS, which includes records on pregnancy outcomes and maternal health behaviors from the first pregnancy onward. This dataset enables the identification of both children and their mothers, allowing for a linkage between maternal health behaviors during pregnancy and individuals observed in 2014. The analysis employs difference-in-differences and event-study methodologies, as outlined in Equations 3.1 and 3.2, replacing the main outcomes with maternal health behaviors.

Regular prenatal visits are important for monitoring and managing potential health risks during pregnancy, helping to reduce complications for both mothers and children (Aguiar et al., 2023). In this study, prenatal visit is coded as 1 if the mother attended regular check-ups at non-traditional health facilities, with at least one visit per trimester. Additionally, I assess the likelihood of iron supplementation intake, another essential component of prenatal care. Sufficient iron intake during pregnancy is vital for preventing iron deficiency anemia, a condition associated with preterm birth, low birthweight, and impaired cognitive and emotional development in children (Naaz and Muneshwar, 2023). I define iron supplementation equals to 1 if the individual's mother takes iron supplements during her pregnancy. Finally, I assess the program's impact on the probability of exclusive breastfeeding, which is well-documented for its immune-boosting benefits in infancy and its role in fostering maternal-infant bonding, ultimately contributing to improved mental health outcomes as the child grows (Krol and Grossmann, 2018). Exclusive breastfeeding is coded 1 if the child is exclusively breastfed for at least 120 days, with no other food or liquids introduced in the first four months of life.

The results, presented in Table 3.7, show strong and consistent effects of the impact of village midwife presence on maternal health behaviors. Particularly, the results indicate that midwife exposure significantly increases the likelihood of mothers attending regular prenatal check-ups at non-traditional health facilities. The coefficients range from 12.3 to 14.3 percentage points, all highly significant. Given the pre-event mean of 35.2

%, this represents an increase of approximately 40 % in prenatal visits. These findings highlight the critical role of midwives in promoting maternal healthcare action, ensuring regular health monitoring, early detection of complications, and improved overall maternal care. Supporting this, the event-study analysis in Figure 3A.3 Panel A in the Appendix confirms a positive and sustained increase in prenatal visits, with some variation in effect size over time.

Similarly, midwife exposure leads to a significant increase in iron supplementation during pregnancy, with coefficients around 8.5 percentage points, corresponding to a 35.86 % increase from the pre-event mean. This effect is particularly notable given that in the 1990s, iron deficiency was widespread among pregnant women in Indonesia, with over half experiencing anemia (Gross et al., 1997; Kodyat et al., 1998; Schultink et al., 1993). The results suggest that the village midwife program effectively addressed this public health issue. Further supporting this, Frankenberg et al. (2005) document that by 1997, 93 % of village midwives provided services that included iron supplementation. The event-study analysis of this outcome, presented in Figure 3A.3 Panel B in the Appendix, shows a gradual increase in iron supplementation rates, with the most pronounced effects occurring for infants born three years after midwives arrived in their communities.

Table 3.7. Impact of Village Midwife Program on Maternal Health Behaviors

	Prenatal Visit			Iron Supplementation			Exclusive Breastfeeding		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Midwife Exposure</i>	0.143*** (0.028)	0.142*** (0.022)	0.123*** (0.026)	0.085*** (0.021)	0.090*** (0.020)	0.083*** (0.025)	0.091*** (0.024)	0.098*** (0.020)	0.095*** (0.027)
mean pre-event	0.352	0.352	0.352	0.237	0.237	0.237	0.311	0.311	0.311
percentage change	40.62%	40.34%	34.94%	35.86%	37.97%	35.44%	29.26%	31.51%	30.55%
Observation	4,527	4,527	4,527	4,527	4,527	4,527	4,527	4,527	4,527
Estimation method	Callaway & Sant'Anna	Sun & Abraham	Two-way Fixed Effect	Callaway & Sant'Anna	Sun & Abraham	Two-way Fixed Effect	Callaway & Sant'Anna	Sun & Abraham	Two-way Fixed Effect

Notes: Point estimates shown in each column are derived from separate regressions. The sample comprises individuals born in communities that implemented the program between 1989 to 1999. *Midwife exposure* set to one for individuals born in or after the year the midwife program was introduced in their community, 0 otherwise. Column (1), (4), and (7) present estimates using the method developed by Callaway & Sant'Anna (2021). Column (2), (5), and (8) show the results of the method described in Sun & Abraham (2021). Column (3), (6), and (9) present results from the Two-Way Fixed Effects model. All regressions include community fixed effects, birth-year fixed effects and set of control variables. Robust standard errors, in parentheses, are clustered at the community level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

The presence of village midwives is also linked to a 9.1 to 9.8 percentage point increase in exclusive breastfeeding, translating to a 29 % to 31 % rise from the pre-event mean.⁵⁰ According to the event-study analysis presented in Figure 3A.3 Panel C, there is a visible positive shift, with the effect slightly increased, up to 15.8 percentage points, for individuals born 2 years after the midwife's arrival. Even though there is a reduction in the third year, the trend resumes its increasing path until the fifth year after the midwives arrive in the communities. This improvement is particularly considerable given the historical context of breastfeeding in Indonesia. In the 1990s, exclusive breastfeeding rates were troublingly low, and early weaning practices were prevalent, significantly shortening the duration for which infants were exclusively breastfed (Suharyono and Paul, 1997). The improvement of breastfeeding practice observed during the midwife program highlights that midwives play in shifting community norms and enhancing maternal education about the benefits of exclusive breastfeeding.

The effectiveness of early-life health interventions, such as the village midwife program, depends not only on their average impact but also on who benefits the most and why. While the program aims to improve maternal health behaviors, its effectiveness may vary due to knowledge gaps and socioeconomic constraints. Therefore, in addition to assessing the overall impact of midwife exposure on maternal health behaviors, I examine whether these effects differ across maternal and household characteristics. Specifically, I explore whether midwife exposure has differential impacts based on maternal age at childbirth, maternal education, and family size. This analysis provides insights into how midwife interventions shape health-seeking behaviors across different populations.

The results, presented in Table 3A.3 panel A, B, and C in the Appendix, indicate a consistent pattern across subgroups, showing that younger mothers, less-educated mothers, and mothers from larger families benefit more from midwife exposure in terms of prenatal visits and exclusive breastfeeding. These findings suggest that midwives play a crucial role in bridging gaps in maternal healthcare access and knowledge, particularly for

⁵⁰ It is important to note that in this analysis, exclusive breastfeeding is defined as a minimum of 120 days, and no other foods or fluids are provided for the first four months of life, following the guidelines promoted by the Indonesian government in the 1990s.

groups that may face greater informational and resource constraints. Younger mothers often have limited experience with pregnancy and childcare, making midwife support essential for promoting healthier prenatal and postnatal practices. Similarly, less-educated mothers may lack access to maternal health information, and midwives help compensate for this gap by encouraging health-seeking behaviors. Likewise, mothers from larger families may struggle with resource limitations within their household, and midwife support ensures that maternal healthcare remains a priority.

Despite these positive effects, a notable exception emerges with iron supplementation, where young mothers, less educated mothers, and mothers from larger families are significantly less likely to take iron supplements, even though they receive midwife exposure. This common pattern suggests barriers that midwives may not be able to fully address, perhaps due to misunderstandings about supplementation, financial constraints, or conflicting household priorities. Cultural beliefs also play a role. Some pregnant women worry that taking iron tablets might lead to bigger babies and more difficult deliveries (Winarni et al., 2024). These results highlight the importance of complementary interventions, such as targeted health education campaigns and improved accessibility to iron supplements, to ensure that maternal health gains translate into broader long-term benefits.

3.7.2. Infant Outcomes

Another potential mechanism explored in this paper is infant health characteristics. The presence of a village midwife enhances maternal and neonatal care by providing access to skilled birth attendants, prenatal care, and postnatal follow-ups, which can contribute to improved birth outcomes (Sehhatie et al., 2014; Dol et al., 2022; Fikre et al., 2023). To assess this mechanism, I examine whether the program influences birthweight and full immunization uptake during infancy. For birthweight analysis, I use pregnancy summary data from the IFLS, consistent with the data used to study maternal health behaviors. The questionnaire first asks whether the baby was weighed immediately after birth, followed by a question on the exact birthweight in kilograms. Among the 4,527 observations in the sample, only 3,622 infants were weighed at birth, meaning that birthweight data are available for a subset of the sample. The immunization records are obtained from the child questionnaire in the IFLS, which collects information on

whether the child has received the complete set of basic immunizations during the first year of life.⁵¹ I define full immunization as a binary variable, coded as 1 if the child received all recommended vaccines and 0 otherwise. Similar to the maternal behavior analysis, this investigation utilizes difference-in-differences and event-study methodologies, as outlined in Equations 3.1 and 3.2, replacing the main outcomes with infant outcomes.

Table 3.8 presents the estimated impact of village midwife exposure on birthweight and complete infant immunization, using the same estimation strategy as the main analysis. The results show that midwife exposure has a statistically significant effect on birthweight, with estimated increases ranging from 0.210 kg to 0.278 kg. Given the pre-event mean birthweight of 3.04 kg, these estimates correspond to a 6.91 % to 9.14 % increase. Similarly, midwife exposure significantly improves the likelihood of full immunization, with estimated effects between 0.057 and 0.064. Considering the pre-event immunization rate of 37.2 %, this translates to a 15.32 % to 17.20 % relative increase in vaccination coverage. The event-study estimates, presented in Figure 3A.4 in the Appendix, further illustrate the dynamic effects of midwife exposure on birthweight and immunization uptake over time. After midwife arrival, birthweight increases progressively, suggesting that improvements in maternal health behaviors contribute to better birth outcomes. These findings align with existing research linking prenatal healthcare access and skilled birth attendance to reductions in low birthweight and neonatal complications (De Bernis et al., 2003; Ngy et al., 2007; Singh et al., 2015). Correspondingly, the probability of full infant immunization uptake steadily increases after midwife introduction. This also suggests that midwives play a crucial role in educating mothers, promoting immunization schedules, and facilitating vaccine access, particularly in areas where maternal awareness of vaccination was previously low.

Given that maternal health behaviors significantly differ by socioeconomic background, it is plausible that infant health outcomes also vary across maternal and household characteristics. I hypothesize that the

⁵¹ Specifically, I describe complete infant immunization as receiving all recommended vaccines (BCG, DPT, Polio, and Measles) by 12 months of age, in line with the World Health Organization (WHO) and the Demographic and Health Surveys (DHS) definitions of full immunization coverage.

program's effects on birthweight and full immunization uptake will be stronger for younger mothers, less-educated mothers, and mothers from larger families, as these groups typically face greater barriers to accessing maternal and child healthcare. The results, presented in Table 3A.5 in the Appendix, indicate significant improvements in infant health outcomes, particularly among the disadvantaged groups. These findings highlight that midwife services are particularly effective in addressing early-life health disparities, especially for infants born to younger, less educated mothers and those from larger families.

Table 3.8. Impact of Village Midwife Program on Infant Outcomes

	Birthweight			Complete Infant Immunization		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Midwife Exposure</i>	0.278*** (0.063)	0.274*** (0.062)	0.210*** (0.060)	0.064*** (0.022)	0.063*** (0.022)	0.057** (0.023)
mean pre-event	3.040	3.040	3.040	0.440	0.440	0.440
percentage change	9.14%	9.01%	6.91%	14.55%	14.32%	12.95%
Observation	3,622	3,622	3,622	4,527	4,527	4,527
Estimation method	Callaway & Sant'Anna	Sun & Abraham	Two-way Fixed Effect	Callaway & Sant'Anna	Sun & Abraham	Two-way Fixed Effect

Notes: Point estimates shown in each column are derived from separate regressions. The sample comprises individuals born in communities that implemented the program between 1989 to 1999. *Midwife exposure* set to one for individuals born in or after the year the midwife program was introduced in their community, 0 otherwise. Column (1) and (4) present estimates using the method developed by Callaway & Sant'Anna (2021). Column (2) and (5) show the results of the method described in Sun & Abraham (2021). Column (3) and (6) present results from the Two-Way Fixed Effects model. All regressions include community fixed effects, birth-year fixed effects and set of control variables. Robust standard errors, in parentheses, are clustered at the community level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.7.3. Fertility Outcomes

This subsection investigates whether midwife exposure influences fertility behavior. The program may affect childbearing decisions by changing women's expectations and preferences regarding maternal and reproductive health (Strupat, 2017; Tsonis et al., 2019). For example, if midwife exposure increases the likelihood of childbirth, this reflects greater confidence in maternal health services and safer delivery practices, leading to an increase in planned pregnancies. Alternatively, if exposure reduces the probability of childbirth, this indicates that improved access to family planning services and reproductive health information enables women to

better control fertility. Both scenarios suggest that the program may not only improve health inputs but also shape fertility preferences and behaviors.

To explore these possibilities, I construct an unbalanced panel of women observed annually between 1989 and 1999, focusing on those who were of childbearing age (15–49) at any point during this period and resided in communities that eventually received the program.⁵² Specifically, this includes women born between 1940 and 1984, as these cohorts were between 15 and 49 years old during the implementation window. Each observation corresponds to a woman-year combination, where women enter the panel when they turn 15 and exit when they reach 50 or at the end of the sample period. Because some women aged into or out of the sample during the period, the resulting panel is unbalanced, with each woman contributing a varying number of observations depending on her age. This structure enables the examination of within-woman changes in birth probabilities before and after program exposure. For example, if a woman gave birth in 1994 (before the program arrived in her community in 1996) and again in 1998 and 1999 (after implementation), this variation contributes to identifying the program's effects.

In this analysis, the dependent variable is a binary indicator equal to one if the woman gave birth in a given year and zero otherwise. The treatment variable is an indicator for the presence of the village midwife program in the woman's community, which equals one if the program had been implemented by year t and zero otherwise. Table 3.9 presents the results of the impact on the probability of giving birth. While the specification includes individual fixed effects to control for time-invariant unobserved heterogeneity, this approach identifies the effect only among women who gave birth both before and after the program. To complement this analysis, I also estimate a model without individual fixed effects, which allows the use

⁵² The defined implementation period of the midwife program in this study offers a suitable setting for assessing fertility responses, as it allows for the identification of changes in birth probabilities during and around the period of program introduction.

of between-woman variation and captures the fertility response among women who gave birth in only one period.⁵³

Table 3.9. Impact of Village Midwife Program on Fertility Outcomes

	Probability of giving birth					
	With individual FE			Without individual FE		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Midwife Exposure</i>	0.009 (0.029)	0.009 (0.030)	0.011 (0.030)	0.010 (0.027)	0.011 (0.030)	0.013 (0.025)
mean pre-event	0.141	0.141	0.141	0.146	0.146	0.146
percentage change	6.38%	6.38%	7.80%	6.85%	7.53%	8.90%
Observation	40,565	40,565	40,565	60,339	60,339	60,339
Estimation method	Callaway & Sant'Anna	Sun & Abraham	Two-way Fixed Effect	Callaway & Sant'Anna	Sun & Abraham	Two-way Fixed Effect

Notes: Point estimates shown in each column are derived from separate regressions, with and without individual fixed effects. The sample comprises all women who were of childbearing age at any point between 1989 to 1999 and resided in communities that eventually implemented the program. *Midwife exposure* is set to one for a woman-year if the program had been implemented in her community by the calendar-year in which the woman is observed; zero otherwise. All regressions include calendar-year fixed effects, community fixed effects, and set of control variables. Robust standard errors, in parentheses, are clustered at the community level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The results show no evidence that the village midwife program significantly affects the probability of giving birth during the implementation period, with consistently small and statistically insignificant estimates across all specifications, including models both with and without individual fixed effects. This suggests that the program does not induce measurable changes in annual fertility behavior in the short term. Event study estimates, presented in Figure 3A.5 in the Appendix, further support this conclusion, showing no significant pre-treatment trends and no changes in birth probabilities following program exposure. In addition, the heterogeneity analysis in Table 3A.5 in the Appendix does not display differential effects across women's characteristics, indicating that midwife exposure does not change fertility behavior even among subgroups that might be more responsive to such interventions. Taken together, these findings suggest that the program does not substantially shift fertility timing or intensity during the rollout period.

⁵³ The sample includes 60,339 woman-year observations from an unbalanced panel. In the specification with individual fixed effects, the number of observations falls to 40,565, as women with no variation in childbirth status over time are excluded from the identifying sample.

Therefore, the observed long-term improvements in mental health and well-being are more plausibly attributed to enhancements in maternal and early-life health environments, rather than to changes in fertility composition.

While maternal health behaviors and early-life conditions provide important pathways through which the village midwife program influences child health and development, they are unlikely to fully explain the observed improvements in mental health and well-being. Other factors, such as increased access to early-life nutrition, improved caregiving practices, sustained child health investments, and enhanced educational opportunities, may also contribute to these effects. Although this study focuses primarily on immediate and early-life mechanisms, the potential influence of broader developmental input cannot be ruled out. Future research is needed to examine how these complementary pathways interact with early health interventions to shape mental health and well-being over the course of life.

3.8. Conclusion

Given the vulnerability of the early years of life, positive health interventions can have a major impact that may persist in adulthood. This study highlights the important role of such intervention in shaping long-term mental health and well-being outcomes, filling a gap in existing research, especially in developing country contexts. This is in line with the increasing recognition of mental well-being as a fundamental component of overall health, which is important for maintaining economic productivity and improving quality of life. This paper contributes to the literature by investigating the effects of the placement of qualified village midwives, a policy aimed at enhancing maternal and childbirth care, on mental health outcomes later in life. To do so, I utilize longitudinal data from IFLS and follow individuals born in a community with and without village midwives until their adulthood age and examine their impact on mental health indicators observed 15-25 years post-intervention.

Given the staggered rollout of the village midwife program, this paper accounts for potential treatment effect heterogeneity by employing modern difference-in-differences and event study methods. These approaches ensure that comparisons are made between appropriately treated and untreated cohorts, mitigating biases that could arise from using already-treated units as

controls. Given the nature of the intervention, exposure to the village midwife program is defined based on individuals' birth cohorts relative to the program's introduction in their community. Treated individuals are those born in the year or after the program's implementation, while control individuals are those born before its introduction. Event study methods further validate the parallel trends assumption by examining pre-treatment cohort trends in outcomes.

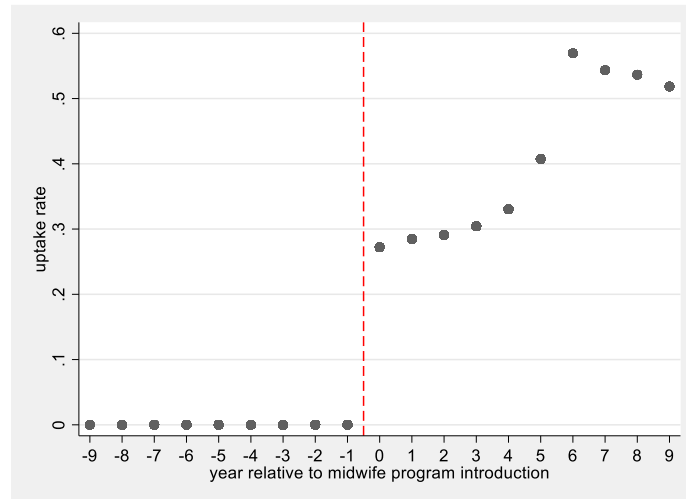
The results of this study indicate that the presence of village midwives is associated with significant improvements in mental health and well-being outcomes. I find a reduction in the probability of being at risk for depression and an increase in self-reported happiness and perceived standard of living. These findings remain robust across multiple checks, including placebo tests, migration responses, and different sample compositions, supporting the credibility of the estimated effects. This study also demonstrates that midwife exposure enhances maternal health behaviors, leading to higher rates of prenatal visits, iron supplementation, and exclusive breastfeeding, as well as improvements in birthweight and full immunization uptake. These findings suggest that the midwife program's effects on mental health and well-being are, at least in part, linked to improvements in maternal and child healthcare. While this study provides insights into immediate mechanisms, further research is needed to explore additional intermediate pathways, such as parenting practices, early childhood cognitive and emotional development, and long-term educational attainment. A deeper understanding of these factors would contribute to a more comprehensive picture of how early-life health interventions shape mental health and well-being outcomes over the life course.

These findings strongly highlight the expansion of health interventions that target the early stages of life, especially in developing countries. The presence of village midwives that facilitates direct health benefits, such as prenatal and postnatal care, nutritional status, and family planning practices, potentially promotes long-term mental health and well-being indirectly. Consequently, integrated health policies that prioritize comprehensive care from the earliest stages of life are immediately needed to ensure that investments in public health yield the broadest possible benefits across the lifespan. The evidence presented in this paper should serve as a convincing argument for policymakers to enhance and broaden the scope of

maternal and child health programs, particularly in regions where access to qualified health professionals remains limited. The continuation and expansion of health programs is crucial to set a path towards healthier and more productive adult populations.

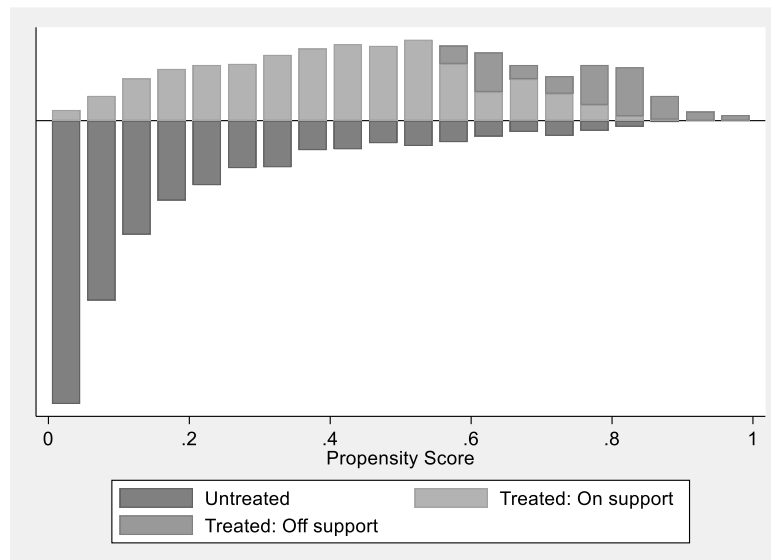
Appendix: Additional Tables and Figures

Figure 3A.1. Uptake Rate Relative to Village Midwife's Arrival



Notes: The figure displays uptake rate of midwife's service, proxied by the number of births attended by trained village midwives, relative to the placement year of the program.

Figure 3A.2. Distribution of Propensity Score



Notes: The figure illustrates the distribution of propensity scores calculated via `psmatch2`, employing nearest-neighbor matching (one neighbor, `n(1)`), without replacement, and applying a caliper width of 0.05. "Untreated" denotes communities that did not implement the village midwife program between 1989 to 1999. "Treated" represents communities that eventually implement the village midwife program between 1989 to 1999.

Table 3A.1. Impact of Exposure to Village Midwife Program on Mental Health and Well-Being: Sensitivity to Implementation Timing

	Early implementation (1989–1992)		Late implementation (1998–1999)		Intense expansion (1993–1997)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: At risk of depression						
<i>Midwife Exposure</i>	-0.043*	-0.045*	-0.044*	-0.045*	-0.049**	0.054**
	(0.026)	(0.026)	(0.026)	(0.027)	(0.026)	(0.026)
Mean pre-event	0.283	0.283	0.278	0.278	0.280	0.280
Percentage change	15.19%	15.90%	15.83%	16.19%	17.50%	19.29%
Panel B: Happiness						
<i>Midwife Exposure</i>	0.074**	0.075**	0.068*	0.072**	0.074**	0.078***
	(0.030)	(0.031)	(0.035)	(0.035)	(0.030)	(0.030)
Mean pre-event	0.600	0.600	0.598	0.598	0.608	0.608
Percentage change	12.33%	12.50%	11.37%	12.04%	12.17%	12.83%
Panel C: Standard of living						
<i>Midwife Exposure</i>	0.066**	0.071**	0.065**	0.068**	0.073***	0.079***
	(0.029)	(0.029)	(0.033)	(0.033)	(0.028)	(0.029)
Mean pre-event	0.560	0.560	0.548	0.548	0.545	0.545
Percentage change	11.79%	12.68%	11.86%	12.41%	13.39%	14.50%
Observation	767	767	212	212	3,548	3,548
Set of controls	No	Yes	No	Yes	No	Yes

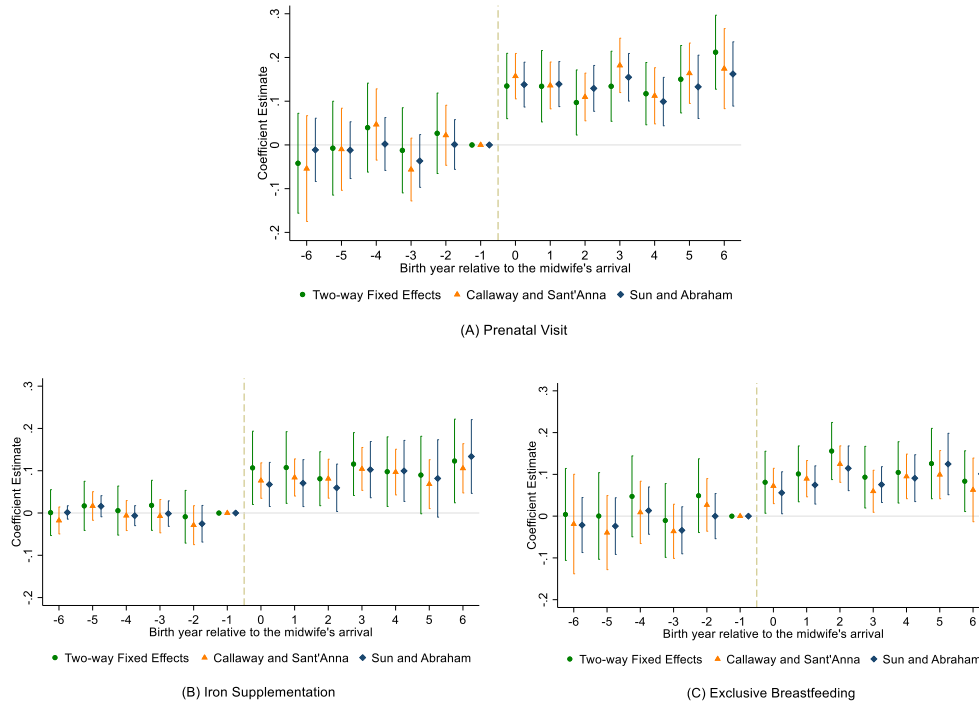
Notes: Point estimates shown in each column are derived from separate Two-Way Fixed Effects regressions. The sample consists of individuals born between 1989 to 1999 in communities that implemented the program during the early, late, and intense implementation phases. *Midwife exposure* set to one for individuals born in or after the year the midwife program was introduced in their community and 0 otherwise. Odd-numbered columns exclude control variables, while even-numbered columns include a set of covariates, such as maternal and community characteristics. All regressions include community of birth and birth-year fixed effects. Robust standard errors, in parentheses, are clustered at the community level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 3A.2 Pre-Treatment Community Facility Characteristics

	Before Matching			After Matching		
	Implementing Community	Non- implementing Community	p-value	Implementing Community	Non- implementing Community	p-value
Community has paved main road	0.286	0.385	0.000	0.290	0.292	0.969
Community has local Govt. electricity	0.230	0.421	0.000	0.239	0.228	0.601
Community using pipe water	0.198	0.245	0.019	0.201	0.207	0.811
Number of Puskesmas in community	1.545	2.182	0.009	1.512	1.518	0.792
Number of doctors in community	2.267	3.321	0.004	2.289	2.341	0.307
Number of nurses in community	4.743	4.933	0.052	4.778	4.801	0.260
Number of clinics in community	1.017	2.180	0.000	1.019	1.023	0.587
Number of TBA in community	5.015	4.660	0.000	4.464	4.468	0.972
Distance to nearest Puskesmas (Km)	1.589	1.225	0.002	1.584	1.588	0.623
Distance to nearest clinic (Km)	3.604	2.678	0.075	3.623	3.563	0.736
Distance to nearest hospital (Km)	14.162	11.209	0.000	14.010	13.656	0.700
Distance to district capital (Km)	28.376	13.358	0.000	25.035	24.389	0.597
Number of Communities	154	158		76	76	
Total Communities	312			152		

Notes: The dataset utilized in this table is derived from the IFLS community facility characteristics, with the initial survey (IFLS 1) conducted in 1993. To capture infrastructure and health facility information preceding 1989, I document the establishment year of each facility within the surveyed communities. Subsequently, this information was converted into binary indicators to signify whether each infrastructure and health facility was present by the end of 1988. This process ensures that all facilities included in the analysis were established prior to the initiation of the midwife program. In Indonesia, “Puskesmas” refers to Community Health Centers that provide medical services at the community level. The term “TBA” is Traditional Birth Attendant. The distances to these facilities, as well as others, are measured from the central point of the community, typically the village headman's office.

Figure 3A.3. Event-Study Estimates of the Impact of Village Midwife Program on Maternal Health Behaviors



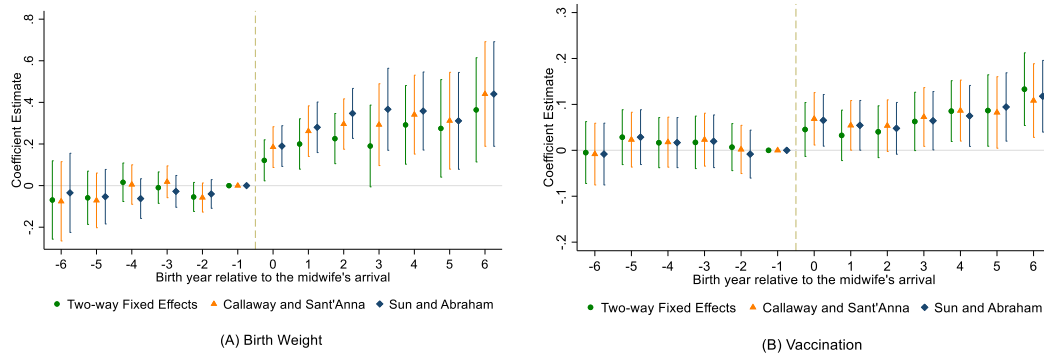
Notes: This figure presents coefficient estimates with 95% confidence intervals for the dynamic effects of the village midwife program on maternal health behaviors of the mothers of individuals aged 15-25, who were born six years before and after a new village midwife was introduced in the community. The sample includes individuals born in communities that implemented the program between 1989 and 1999 (implementing sample). Year $t = -1$ serves as the reference category. The estimation includes community and birth-year fixed effects, with standard errors clustered at the community level.

Table 3A.3. Impact of Village Midwife Program on Maternal Health Behaviors by Maternal and Household Characteristics

	Prenatal Visit	Iron Supplementation	Exclusive Breastfeeding
Panel A: Born to young mothers			
Midwife exposure	0.071*** (0.022)	0.148*** (0.028)	0.028 (0.026)
<i>Midwife exposure × young mother</i>	0.099*** (0.033)	-0.239*** (0.030)	0.157*** (0.029)
Test for coefficients' equality, p-value	0.003	0.000	0.000
Panel B: Born to low-educated mothers			
Midwife exposure	0.057** (0.024)	0.098** (0.027)	0.050* (0.029)
<i>Midwife exposure × low educated mothers</i>	0.144*** (0.027)	-0.072** (0.028)	0.112** (0.033)
Test for coefficients' equality, p-value	0.000	0.011	0.001
Panel C: Born in a large family			
Midwife exposure	0.091*** (0.025)	0.087*** (0.024)	0.046 (0.030)
<i>Midwife exposure × large family size</i>	0.084*** (0.032)	-0.054** (0.026)	0.130** (0.037)
Test for coefficients' equality, p-value	0.009	0.043	0.000
Observation	4,527	4,527	4,527

Notes: Each panel outlines the effects of midwife presence within a community along with an interaction term with the heterogeneous variables. The sample includes individuals born between 1989 and 1999 in communities which eventually received a village midwife program during the timespan (implementing sample). The specification in all columns includes community fixed effects, birth-year fixed effects, and a set of control variables. Robust standard errors are reported in parentheses, adjusted for clustering at the community level. Significance: *** 1% level, ** 5% level, and * 10% level.

Figure 3A.4. Event-Study Estimates of the Impact of Village Midwife Program on Infant Outcomes



Notes: This figure presents coefficient estimates with 95% confidence intervals for the dynamic effects of the village midwife program on birth and infant characteristics of individuals aged 15-25, who were born six years before and after a new village midwife was introduced in the community. The sample includes individuals born in communities that implemented the program between 1989 and 1999 (implementing sample). Year $t = -1$ serves as the reference category. The estimation includes community and birth-year fixed effects, with standard errors clustered at the community level.

Table 3A.4. Impact of Village Midwife Program on Infant Outcomes by Maternal and Household Characteristics

	Birthweight	Infant complete immunization
Panel A: Born to young mothers		
Midwife exposure	0.208*** (0.023)	0.055* (0.029)
<i>Midwife exposure × young mother</i>	0.090*** (0.029)	0.071** (0.030)
Test for coefficients' equality, p-value	0.002	0.038
Panel B: Born to mothers that never attend high school		
Midwife exposure	0.229*** (0.022)	0.073** (0.029)
<i>Midwife exposure × low educated mothers</i>	0.020 (0.025)	0.096*** (0.032)
Test for coefficients' equality, p-value	0.425	0.003
Panel C: Born in a large family		
Midwife exposure	0.234*** (0.022)	0.078*** (0.028)
<i>Midwife exposure × large family size</i>	0.014 (0.025)	0.090*** (0.029)
Test for coefficients' equality, p-value	0.586	0.003
Observation	3,622	4,527

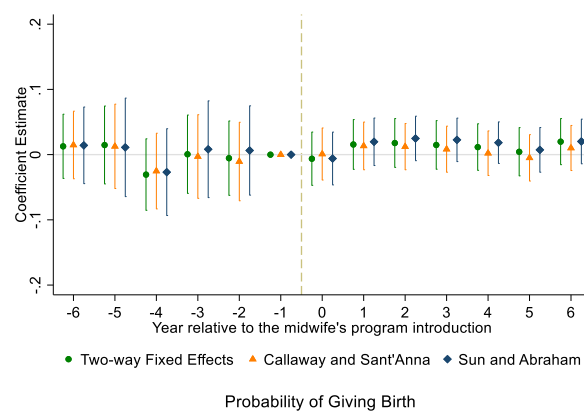
Notes: Each panel outlines the effects of midwife presence within a community along with an interaction term with the heterogeneous variables. The sample includes individuals born between 1989 and 1999 in communities which eventually received a village midwife program during the timespan (implementing sample). The specification in all columns includes community fixed effects, birth-year fixed effects, and a set of control variables. Robust standard errors are reported in parentheses, adjusted for clustering at the community level. Significance: *** 1% level, ** 5% level, and * 10% level.

Table 3A.5. Impact of Village Midwife Program on Fertility Outcome by Women's Characteristics

	Prob. of giving birth
Panel A: Young women	
Midwife exposure	0.012 (0.029)
<i>Midwife exposure × young women</i>	0.020 (0.030)
Test for coefficients' equality, p-value	0.557
Panel B: Low-educated women	
Midwife exposure	0.024 (0.023)
<i>Midwife exposure × low educated women</i>	0.025 (0.027)
Test for coefficients' equality, p-value	0.280
Panel C: First-time mothers	
Midwife exposure	0.017 (0.026)
<i>Midwife exposure × first-time mothers</i>	0.026 (0.027)
Test for coefficients' equality, p-value	0.356
Observation	60.339

Notes: Each panel presents the estimated effects of midwife program exposure on the probability of giving birth, including interaction terms with heterogeneous characteristics. The sample consists of woman-year observations from an unbalanced panel of women who were of childbearing age (15–49) at any point between 1989 and 1999, residing in communities that eventually received village midwife program (implementing sample). All specifications include calendar-year fixed effect, community fixed effects, and a set of control variables. Robust standard errors are reported in parentheses, adjusted for clustering at the community level. Significance: *** 1% level, ** 5% level, and * 10% level.

Figure 3A.5. Event-Study Estimates of the Impact of Village Midwife Program on Fertility Outcome



Notes: This figure presents coefficient estimates with 95% confidence intervals for the dynamic effects of the village midwife program on the annual probability of giving birth. The analysis is based on an unbalanced panel of women aged 15–49 who were of childbearing age at any point between 1989 and 1999, residing in communities that eventually implemented the program during that period (implementing sample). Year $t = -1$ serves as the reference category. The estimation includes calendar-year and community fixed effects, with standard errors clustered at the community level.

4. Exposure to Forest Fire Smoke and Long-Term Risk of Psychological Distress

4.1. Introduction

Environmental shocks during early life are increasingly recognized as fundamental determinants of long-term human development (Almond and Currie, 2011a; Rangel and Vogl, 2019). This perspective reflects a broader shift in economic and public health research toward understanding how early-life conditions shape outcomes across the life course. Over the past two decades, a substantial literature has established that early-life conditions can have a lasting effect on an individual's well-being outcomes. Specifically, adverse exposures in utero or during the first few years of life, whether in the form of disease, malnutrition, or environmental hazards, have been linked to lower educational attainment, reduced earnings, poorer physical health, and increased risk of psychological distress (Maccini and Yang, 2009; Neelsen and Stratmann, 2011; Miller, 2017; Rosales-Rueda, 2018; Singhal, 2019; Aguilar and Vicarelli, 2022; Le and Nguyen, 2022). Among these hazards, exposure to severe air pollution has gained particular attention, especially when it occurs during sensitive developmental periods in which the respiratory and neurological systems are still maturing (Currie and Neidell, 2005).

Air pollution, particularly from forest fires, has become one of the most pressing public health concerns in many developing countries (Crippa et al., 2016; Reddington et al., 2021). Unlike urban pollution, smoke from forest fires leads to high-intensity exposure to hazardous air pollutants over large geographic areas, generating episodes of extremely high particulate concentrations, particularly in areas with extensive peatland degradation (Page et al., 2002). These fires release a complex mixture of fine particulate matter (PM_{2.5}), carbon monoxide, ozone precursors, and other toxic compounds that significantly degrade air quality (Jaffe et al., 2020; Jin et al., 2025). While historically concentrated in the tropics, wildfire risk has expanded to temperate regions due to climate change and land-use pressures

(Flannigan et al., 2000; Carvalho et al., 2011; Haines et al., 2020). Catastrophic fire seasons in the United States, Canada, Australia, and parts of Europe have increasingly exposed large populations to dense smoke plumes, similar to the severe haze episodes long observed in Southeast Asia and the Amazon (Le et al., 2014; Fann et al., 2018; Jin et al., 2025). Projections by UNEP (2022) indicate that, under current policies, the global incidence of extreme wildfires could increase by as much as 50 percent by 2100. These alarming trends underline the urgent need to monitor the potential human consequences of wildfire pollution.

This study examines the long-term mental health effects of early-life exposure to smoke pollution from the 1997 Indonesian forest fires. Specifically, I assess whether exposure to the fires in utero or during early childhood is associated with elevated psychological distress nearly 17 years later. The 1997 fires were among the most severe and geographically extensive haze events in Southeast Asia's history, which resulted in the sudden and uneven exposure of millions of individuals to extreme levels of particulate pollution (Quah, 2002; Sastry, 2002). The fires were largely driven by land-clearing practices for agriculture, exacerbated by unusually dry conditions associated with the El Niño climatic event. Given the scale and severity of the disaster, these fires may have far-reaching effects on individuals, not only through physical health risks but also by contributing to psychological distress that can persist into later life. Psychological distress is an essential measure of adult well-being and functioning, encompassing a broad range of symptoms including emotional strain, fatigue, and difficulties with sleep and concentration (Veit and Ware, 1983; Ravichandran and Ramadass, 2024).

This study uses data from the Indonesia Family Life Survey (IFLS), a rich longitudinal dataset that provides information on demographics and socioeconomic conditions. I focus on the 2014 wave, as by that time all individuals who were in utero or early childhood during the 1997 fires had reached the minimum age (15 years) required to be asked mental health questions, enabling consistent and comprehensive measurement of psychological distress across the relevant cohorts. Exposure to pollution is measured using NASA's satellite-derived Aerosol Index (AI), averaged at the district level during the peak months of September to November 1997. To identify causal effects, I employ a cohort-based difference-in-differences

design that exploits variation in developmental timing across birth cohorts and geographic intensity of exposure. The treated group consists of individuals who were either in utero (born in 1998) or in early childhood (born 1993–1997) at the time of the fires. The control group includes individuals who were already five years or older during the fires, and thus past the most sensitive early-life developmental periods. Psychological distress outcomes are regressed on the interaction between cohort exposure status and continuous AI values, controlling for district and cohort fixed effects, socioeconomic characteristics, and parental background.

The results indicate that early-life exposure to fire-related air pollution is associated with worse psychological distress in adulthood. Individuals who were in utero or early childhood during the 1997 fires and who lived in districts with higher AI values exhibit, on average, a 0.77-point increase in CESD-10 scores, a 4–5 percentage point increase in the likelihood of clinically relevant depression, and more frequent reports of sleep disturbances. To strengthen the credibility of these findings, I implement a comprehensive set of robustness checks addressing concerns about measurement error, functional-form assumptions, and sample selection. First, an instrumental variables strategy exploiting wind-driven variation in haze dispersion yields similar point estimates, suggesting that attenuation from measurement error is unlikely. Second, expanding the sample beyond Sumatra and Kalimantan to include all Indonesian districts, many with low exposure, results in only a modest reduction of the coefficients, indicating that the effects are not driven solely by high-exposure regions. Third, I re-estimate the models using binned terciles of AI to allow for non-linear exposure effects. Finally, a placebo test assuming the fires that occurred in 1987 yield null results, ruling out spurious cohort trends or residual confounding. Overall robustness checks, alongside supporting evidence on physical health deterioration and household economic stress as potential mechanisms, provide consistent and credible evidence that early-life exposure to severe haze has lasting adverse consequences for adult mental health.

This study contributes to the literature in several ways. First, it broadens the evidence on the long-term health impacts of fire-related air pollution by focusing on early-life exposure. A growing number of studies document that early exposure to polluted air can have persistent

consequences for human capital accumulation later in life (Currie et al., 2014; Bharadwaj et al., 2016; Lo Bue, 2019; Balietti et al., 2022; von Hinke and Sørensen, 2023). Recent research has also documented the adverse physical health effects of fire-sourced air pollution, linking it to increased risks of respiratory illnesses, cardiovascular conditions, and lower life expectancy (Moeltner et al., 2013; Cascio, 2018; Reid and Maestas, 2019; He et al., 2020; Chen et al., 2021; Rocha and Sant’Anna, 2022). In addition to these acute outcomes, a growing body of work suggests that early-life exposure to fire-related air pollution may lead to persistent impairments in physical development, immune function, and cognitive ability, particularly when exposure occurs during critical developmental windows (Black et al., 2017; Brown et al., 2022; Bottenhorn et al., 2024). These long-term effects are especially pronounced among vulnerable populations, such as infants, young children, and the elderly, who are biologically less equipped to cope with high pollution loads (Le et al., 2014; Pullabhotla and Souza, 2022; Zheng, 2023; Pakhtigian et al., 2024).

Second, this study is the first to examine the long-term psychological effects of early-life exposure to the 1997 Indonesian forest fires. A growing body of research has examined a range of consequences associated with this event. Frankenberg et al. (2005) show declines in adult health among Indonesians following the crisis. Similarly, Kim et al. (2017) find that early-life exposure has lasting health effects, particularly for the elderly, although children appeared to recover more fully. Focusing on early-life vulnerability, Jayachandran (2009) provides evidence that wildfire-related air pollution significantly increased neonatal mortality, while Rosales-Rueda & Triyana (2020) show that early-life exposure negatively affects adolescents’ education and self-reported health. Other studies document economic burdens, including reduced labor productivity and income (Shrestha, 2019; Tan-Soo and Pattanayak, 2019). The impacts also extended beyond Indonesia, with Sheldon & Sankaran (2017) reporting elevated hospitalization rates in Singapore and Sastry (2002) documenting a sharp rise in infant and child mortality in Malaysia. Despite these findings, the long-term mental health effects of early exposure to this large-scale pollution event remain underexplored. This study addresses that gap by providing causal evidence on psychological distress in adulthood following early-life exposure to the fires.

This contribution builds on a growing literature linking air pollution and wildfire smoke exposure to mental health outcomes. While much of the existing research has focused on physical effects, recent studies show that exposure to wildfire-related pollution is also associated with psychological effects and increased emergency visits for anxiety, depression, and substance use disorders (Johnston et al., 2021; Mirabelli et al., 2022; Chen et al., 2023; Jung et al., 2025). Further, evidence suggests that early-life exposure to ambient air pollution may impair emotional regulation and increase the risk of mental health disorders later in life, likely through neuroinflammatory and stress-response pathways (Olvera Alvarez et al., 2016; Morris et al., 2021; Reuben et al., 2021). However, little is known about whether such mechanisms operate in the context of the 1997 Indonesian fires.

Third, this study makes a methodological contribution by applying a cohort-based difference-in-differences design that exploits variation in both developmental timing and geographic exposure to pollution. While this strategy has been used in studies of early-life shocks, its application to long-term mental health outcomes following wildfire-related air pollution exposure remains unexplored, particularly in low- and middle-income country settings. By combining high-resolution satellite-derived pollution data with longitudinal mental health outcomes from the Indonesia Family Life Survey, this study demonstrates how quasi-experimental designs can be leveraged to credibly estimate the psychological effects of early-life environmental shocks, even in data-constrained contexts.

The remainder of this paper is structured as follows. Section 4.2 provides an overview of the 1997 Indonesian forest fires. Section 4.3 describes the data and presents summary statistics. Section 4.4 outlines the identification strategy, and Section 4.5 reports the main empirical results. Section 4.6 discusses a series of robustness checks. Section 7 explores potential mechanisms underlying the findings, and Section 4.8 concludes. Additional supporting tables and figures are provided in the Appendix.

4.2. Background and Context: The 1997 Indonesian Forest Fire

Between August and November 1997, Southeast Asia was covered in a thick, persistent haze caused by widespread forest fires in Indonesia. The smoke did not remain within the country's borders. It reached Malaysia, Singapore, Brunei, and even parts of southern Thailand, carried by prevailing

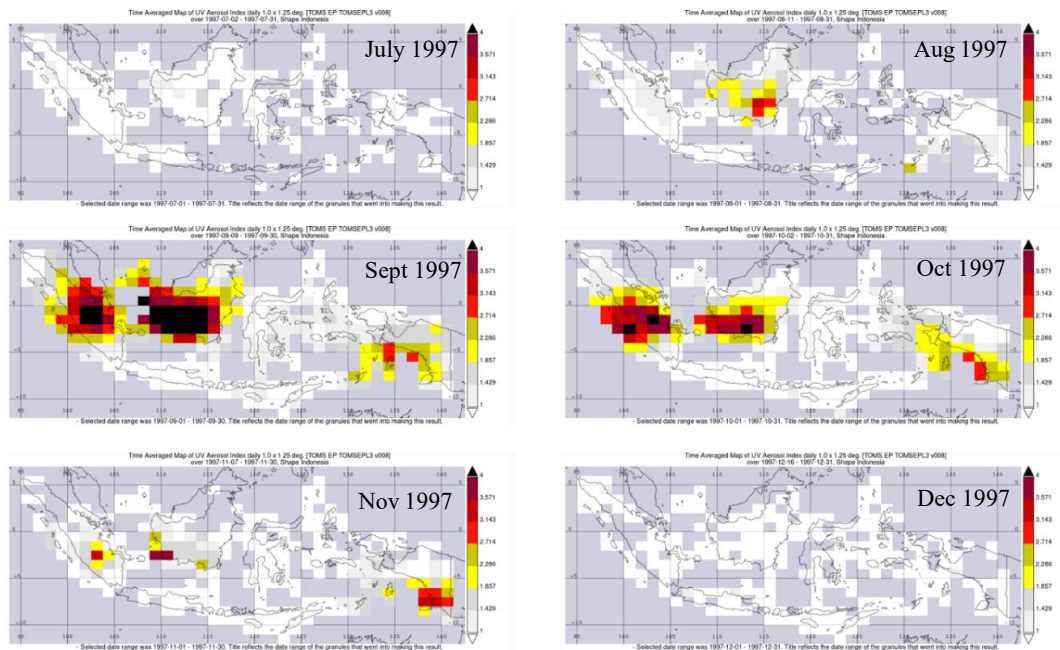
winds (Wooster et al., 1998). Much of the smoke originated from peat-rich areas, where fires produced intense and long-lasting haze (Marlier et al., 2021). The spatial and temporal extent of the pollution is illustrated in Figure 4.1, which displays monthly maps of satellite-derived aerosol concentrations across Indonesia during the July–December 1997 period. The maps show that aerosol levels began rising in August and peaked dramatically in September and October, with the most severe pollution concentrated over Sumatra and Kalimantan. The aerosol concentrations in the figure reflect the presence of smoke particles from large-scale biomass burning, and the spatial patterns highlight both the intensity of the fires and the wind-driven spread of haze across the archipelago.

These fires were not purely the result of natural phenomena. Rather, they emerged from the interaction of unsustainable land-clearing practices and extreme climatic conditions. Agricultural expansion, especially for oil palm and timber plantations, relied heavily on slash-and-burn methods (Neil and G., 1998; Wooster et al., 1998). Under typical weather conditions, such fires were expected to remain controlled. However, in 1997, a strong El Niño episode led to an unusually severe drought across the Indonesian archipelago (Khandekar et al., 2000). As a result, vegetation and peat soil became extremely dry and combustible. Rainfall levels were dramatically lower than average, allowing fires to spread far beyond their intended boundaries (Neil and G., 1998; Fanin and Van Der Werf, 2017). This prolonged dryness is confirmed by satellite-derived rainfall data, which show that the number of days with at least 20 mm of precipitation dropped to its lowest levels during the peak fire months of August to October in Sumatra (see Figure 4A.1 in the Appendix). The limited frequency of substantial rainfall events during this period provided minimal relief from accumulating fire intensity and haze conditions.

The consequences of the 1997 forest fires in Indonesia were extensive, affecting the economy and environmental stability. Economically, the fires burned nearly 10 million hectares of land and caused losses amounting to approximately US\$4.5 billion, including losses in forestry, agriculture, transportation, and health care costs (Heil and Goldammer, 2001). In addition to the financial damages, the fires contributed emissions equivalent to approximately 13–40 % of global annual fossil fuel emissions at the time (Page et al., 2002). Air quality deteriorated to hazardous levels, particularly

in areas close to the main fire zones. Schools, factories, and public offices were temporarily closed in the most severely affected districts due to dangerously poor air quality. Children, the elderly, and individuals with respiratory conditions were advised to remain indoors to avoid exposure to fine particulate matter and toxic haze (Heil and Goldammer, 2001; Sastry, 2002).

Figure 4.1. Monthly Distribution of UV-Absorbing Aerosols During the 1997 Indonesian Forest Fires



Notes: These figures show satellite-based maps of UV-absorbing aerosol index (AI) from the Earth Probe TOMS instrument. The spatial patterns illustrate the progression and peak intensity of smoke pollution, with the highest concentrations observed over Sumatra and Kalimantan. *Source:* Author's visualization using data from NASA Giovanni (<https://giovanni.gsfc.nasa.gov/giovanni/>), Earth Probe TOMS Aerosol Index, July–December 1997.

Beyond their immediate economic and environmental impacts, the 1997 fires have also been studied as a natural experiment to examine the effects of early-life exposure to environmental shocks. Prior research has linked this event to adverse outcomes in birth weight, physical development, and educational attainment (Jayachandran, 2009; Lo Bue, 2019; Tan-Soo and Pattanayak, 2019). These studies underscore the developmental vulnerability of children to ambient pollution, particularly during gestation and early childhood. However, despite the scale and severity of the fires, relatively

little is known about their long-term psychological consequences. Given the extensive literature linking early-life adversity to mental health risks later in life, this setting provides a unique opportunity to assess whether such early exposure to toxic haze has enduring effects on emotional well-being and psychological functioning.

4.3. Data

4.3.1. Satellite-Based Data

To measure pollution exposure during the 1997 Indonesian forest fires, I utilize satellite-based data as the primary source for constructing district-level exposure measures.⁵⁴ The main pollution variable is AI, derived from the Earth Probe Total Ozone Mapping Spectrometer (TOMS) and made publicly available by NASA's Goddard Earth Sciences Data and Information Services Center (GES DISC).⁵⁵ AI captures ultraviolet-absorbing particles such as smoke, dust, and ash in the atmosphere and has been widely applied in climate, environmental, and health studies where direct measurements of particulate matter are unavailable (Duncan et al., 2003; Frankenberg, McKee, et al., 2005; Smith et al., 2014). The TOMS instrument reports daily AI values at a spatial resolution of 1° latitude by 1.25° longitude, roughly corresponding to grid cells of 100–175 kilometers across at Indonesia's equatorial latitude.

To align the coarse spatial resolution of the TOMS product with administrative district boundaries, I assign exposure by computing a distance-weighted average of all grid values within a 100-kilometer radius of each district's geographic centroid. The interpolation applies an inverse distance-squared weighting scheme, assigning greater influence on closer grid points to account for spatial proximity in haze dispersion. For each district, I first calculate the average AI value for each day during the fire season. These daily AI values are then aggregated into monthly means, and

⁵⁴ Fine particulate matter (PM_{2.5} and PM₁₀) concentrations were not systematically recorded in Indonesia during this period. Neither reliable satellite-based PM estimates nor a national network of ground-based air quality monitors existed in the 1990s, especially outside major urban centers. As a result, satellite-derived proxies such as the AI are used to obtain spatially consistent measures of pollution exposure.

⁵⁵ The satellite data is obtained from <https://giovanni.gsfc.nasa.gov/giovanni/>

the final exposure variable is defined as the average of the monthly means for September, October, and November 1997, the peak period of the haze crisis.⁵⁶ This approach produces a spatially consistent and temporally rich measure of pollution intensity across districts.⁵⁷

To validate the spatial distribution of smoke emissions, I utilize fire hotspot counts from the European Space Agency's Along Track Scanning Radiometer (ATSR) system.⁵⁸ The ATSR detects thermal anomalies that indicate active fire events based on elevated surface temperatures. By aggregating these hotspots at the district level during the 1997 fire season, I construct a proxy for local fire activity. As shown in Table 4A.1 in the Appendix, although smoke can disperse over long distances and affect districts far from the original fire sites, the observed positive correlation between fire hotspot counts and AI exposure suggests that areas with more intense burning also experienced higher atmospheric pollution. This supports the spatial validity of the AI-based exposure measure.

In addition to pollution data, I include meteorological variables from the Modern-Era Retrospective Analysis for Research and Applications, Version 2 (MERRA-2). MERRA-2 provides gridded atmospheric data at a resolution of $0.5^\circ \times 0.5^\circ$, finer than the TOMS resolution. These data are spatially linked to districts using inverse-distance interpolation based on each district's centroid coordinates. The variables used are eastward (u) and northward (v) wind components, wind speed, and precipitation. The u and v wind components measure air movement along the east-west and north-south axes. Because wind rarely blows strictly along one direction, I combine the

⁵⁶ Although the spatial maps in Figure 4.1 show the most intense smoke coverage in September and October, I define the exposure window as September to November 1997 to capture the full duration of elevated pollution levels. This definition is supported by the time-series plots of area-averaged daily aerosol index values over Sumatra and Kalimantan, presented in Figure 4A.2 in the Appendix, which indicate that high levels of atmospheric pollution persisted well into November. Given the health relevance of sustained exposure and the gradual decline of haze intensity, incorporating November ensures a more comprehensive assessment of pollution burden during the fire season.

⁵⁷ The pollution exposure variable is constructed at the district level rather than at finer administrative units such as subdistricts or communities for both methodological and practical reasons. Given the relatively low spatial resolution of the TOMS satellite data (1° latitude $\times$ 1.25° longitude), multiple subdistricts often fall within a single grid cell or share similar exposure values.

⁵⁸ <https://sentinels.copernicus.eu/web/sentinel/home>

u and v components using the arctangent function, which converts the vector ratio into compass degrees. This transformation generates a directional measure of prevailing wind flow. Finally, wind speed and rainfall are used as control variables. Rainfall is measured as the number of days per month with at least 20 mm of precipitation, reflecting sustained wet conditions likely to suppress fire activity. Wind speed helps account for variation in the horizontal transport of pollutants. Both variables are averaged over the September–November 1997 period to match the timing of the AI-based exposure measure.

4.3.2. Survey Data and Cohort Definition

The individual-level data are drawn from the Indonesian Family Life Survey (IFLS)⁵⁹, a longitudinal socioeconomic and health survey that covers over 83 percent of the Indonesian population. Conducted in five waves (1993, 1997, 2000, 2007, and 2014), the IFLS collects detailed information on the household, individual, and community levels. It has been widely used in research on long-term health and development in Indonesia, including earlier studies examining the impacts of the 1997 forest fires (Frankenberg, McKee, et al., 2005; Weaver et al., 2016; Kim et al., 2017; Tan-Soo and Pattanayak, 2019; Rosales-Rueda and Triyana, 2020).

This study focuses on a range of psychological outcomes measured in the 2014 wave of the IFLS, 17 years after the fires, when respondents had reached adulthood. To capture the broad scope of psychological distress, I construct outcome variables that involve both self-reported symptoms and clinically recognized conditions. First, depressive symptoms are measured using the Center for Epidemiologic Studies Depression Scale (CESD-10), utilizing both a continuous index ranging from 0 to 24 and a binary indicator equal to one if the score is 10 or higher, corresponding to the commonly used threshold for potential clinical depression. Second, I construct a binary indicator of sleep disturbance, which captures aspects of sleep quality,

⁵⁹ The IFLS data can be obtained from <https://www.rand.org/well-being/social-and-behavioral-policy/data/FLS/IFLS.html>.

fatigue, and daily functioning.⁶⁰ In addition to these self-reported measures, I include binary indicators for emotional and memory-related problems based on respondents' reports of whether they had ever been informed by a doctor or paramedic of such conditions. These indicators serve as proxies for clinically identified psychological impairments. Together, these outcomes provide an extensive evaluation of the long-term psychological consequences of early-life exposure to the 1997 Indonesian forest fires.

To define the relevant exposure cohorts, I classify individuals based on their age during the peak fire period of September to November 1997. The first group consists of individuals who were in utero during this period, those born between late 1997 and mid-1998, who would have been at different stages of gestation during the months of intense haze.⁶¹ Some were likely exposed during the final trimester of pregnancy, while others were in the earliest stages of fetal development. This group represents exposure during a biologically sensitive window for fetal growth, neurological development, and immune system formation. The second group comprises individuals who were in early childhood at the time of the fires, meaning they were between infancy and age four during the exposure window. These individuals were born several years before the fires and experienced rapid brain development and physiological maturation, making them particularly vulnerable to environmental shocks.⁶² Exposure during this stage may have lasting consequences for emotional and cognitive development. These age cutoffs

⁶⁰ The IFLS sleep questions are not part of a validated clinical instrument such as the Pittsburgh Sleep Quality Index (PSQI) or the Insomnia Severity Index (ISI), and they do not come with established clinical cutoffs. Instead, they are based on general self-reported symptom frequency over the past four weeks, using a 4-point Likert scale (ranging from "never" to "almost always"). Therefore, I construct a binary sleep disturbance indicator equal to one if the respondent reported experiencing any of the three sleep-related problems, like difficulty falling asleep, waking up during the night and having trouble getting back to sleep, or waking up too early and being unable to return to sleep, as occurring "often" or "almost always". This classification follows common practice in the literature (e.g., Steptoe et al., 2006; Ferrie et al., 2011), which uses similar frequency-based thresholds to identify individuals at elevated risk of poor sleep quality.

⁶¹ Assuming a standard nine-month gestation period, individuals born between November 1997 and July 1998 would have been in utero during the peak fire months.

⁶² Individuals aged 0–4 at the time of the fires would have been born between 1993 and 1997, consisting of children in early developmental stages, including infancy and early childhood.

are consistent with prior studies in fetal origins and early childhood development literature, which emphasize heightened sensitivity to adverse exposures during gestation and the early postnatal years (Almond and Currie, 2011b; Currie and Vogl, 2013).

As a comparison group, I include individuals born between 1988 and 1992, who were at least five years old during the fire months in 1997. By this age, they are generally considered to have passed the most sensitive windows of neurodevelopment, immune system formation, and other early-life physiological processes, making them less biologically susceptible to long-term damage from acute environmental shocks. This group thus serves as a baseline for assessing the differential impacts of pollution exposure during the earlier, more vulnerable stages of life. Moreover, because this cohort is relatively close in age to the in utero and early childhood groups at the time psychological outcomes are measured in 2014, the comparison helps to minimize potential confounding arising from age-related differences in mental development or reporting behavior.

Using birth date and residential history information reported in the IFLS, I assign individuals to exposure cohorts as described above and determine their location during the 1997 fire period. I use detailed migration data available in the IFLS to reconstruct the district of residence for individuals as of September–November 1997. This allows for more accurate exposure assignments by capturing the individual’s actual geographic location during the haze episode. By linking this location to the district-level AI values, I measure the intensity of pollution for individuals plausibly experienced during the critical developmental period. This strategy reduces potential misclassification due to post-exposure migration. Individuals with missing or inconsistent data on birth date or residential location during the fire period are excluded from the analysis to preserve the accuracy of exposure assignment.⁶³ The resulting cohort and exposure definitions form the empirical foundation for estimating the long-term psychological consequences of early-life pollution exposure.

⁶³ The number of individuals excluded due to missing or inconsistent birth date or residence information during the 1997 fire months is small and does not materially affect the size or representativeness of the analysis sample.

4.3.3. Descriptive Statistics

Table 4.1 presents summary statistics for individuals residing in regions most affected by the 1997 forest fires. The sample is divided into low and high exposure groups based on whether the district-level mean AI during the peak fire season falls below or above the sample median of 1.41.⁶⁴ Districts with an average AI above this threshold are classified as high exposure, and those below this threshold are classified as low exposure.⁶⁵ Notably, this cutoff is broadly consistent with prior studies that define high exposure using an AI threshold of 1.5 over several days (Jayachandran, 2009; Rosales-Rueda and Triyana, 2020). This comparison does not differentiate between treatment and control cohorts but instead highlights patterns across exposure intensity.

Panel A reports outcomes related to adult psychological distress. Individuals in high-exposure areas report higher CESD-10 scores (8.28 vs. 6.80) and a greater prevalence of depressive symptoms, sleep disturbances, memory problems, and emotional instability than those in low-exposure districts. These differences are consistent with the hypothesis that early-life haze exposure adversely affects long-term mental health. Panel B shows that individuals in high-AI districts are slightly younger on average and have completed fewer years of schooling (8.68 vs. 9.11), although the gender distribution is balanced. Panel C displays environmental indicators, confirming substantial differences in pollution exposure: the mean AI in high-AI districts is more than twice that in low-AI areas (2.48 vs. 1.13). Downwind exposure is also notably higher in high-AI districts, and these areas received less rainfall, which may have exacerbated fire intensity and haze persistence.

⁶⁴ The sample is restricted to Sumatra and Kalimantan, where haze exposure was most severe, leading to a larger number of observations in the high-AI group (1,486 vs. 674)

⁶⁵ The median-based split provides a data-driven, non-arbitrary benchmark to summarize differences in exposure intensity across districts. This categorization is used for descriptive purposes. All main estimations retain the full variation in AI by modeling exposure as a continuous variable.

Table 4.1. Summary Statistics

	Low AI		High AI	
	Mean	Std.Dev	Mean	Std.Dev
<i>Psychological variables:</i>				
Self-reported CESD-10 (continuous)	6.80	4.87	8.28	4.79
Self-reported CESD-10 (1=yes)	0.17	0.37	0.21	0.41
Sleep disturbance (1=yes)	0.26	0.45	0.29	0.46
Memory-related problem (1=yes)	0.08	0.24	0.09	0.26
Emotionally unstable (1=yes)	0.06	0.40	0.08	0.43
<i>Individual characteristics:</i>				
Age	18.92	1.71	18.71	0.17
Years of schooling	9.11	3.61	8.68	3.81
Female (1=yes)	0.47	0.49	0.48	0.49
<i>Environmental indicators:</i>				
AI Sept–Nov 1997	1.13	0.18	2.48	0.46
AI Sept–Nov 1997 Kalimantan	1.22	0.22	2.97	0.17
AI Sept–Nov 1997 Sumatra	1.14	0.17	2.35	0.43
Downwind districts Sept–Nov 1997 (1=yes)	0.35	0.21	0.74	0.24
Rainfall (days with ≥ 20 mm Sept–Nov 1997)	10.20	2.51	6.40	2.04
<i>Observation:</i>	674		1,486	

Notes: High and low exposure groups are defined based on the sample median of district-level average Aerosol Index (AI) during the peak fire season (September–November 1997). All outcome variables are measured at the individual level using IFLS 2014, except for environmental indicators, which are based on 1997 data. The sample is restricted to Sumatra and Kalimantan, where fire exposure was most severe. As a result, the number of observations in the high-AI group exceeds that of the low-AI group, with a total sample size of 2,160 individuals.

4.4. Empirical Strategy

This study investigates the long-term psychological effects of early-life exposure to severe air pollution resulting from the 1997 Indonesian forest fires. The fires led to extreme levels of air pollution across large parts of Indonesia, particularly in the islands of Sumatra and Kalimantan (Heil and Goldammer, 2001; Kunii et al., 2002; Frankenberg, McKee, et al., 2005). While the exact timing and location of fire outbreaks varied somewhat across districts, the smoke spread over a wide area. As a result, the haze was not restricted to districts with direct fire incidents; rather, smoke pollution intensity was also concentrated in neighboring or downwind districts due to atmospheric transport, wind patterns, and topographic conditions. This

spatial separation between fire origin and pollution exposure highlights the importance of using air quality data, such as AI, to measure exposure levels, rather than relying only on the presence of fire hotspots. Accordingly, the main exposure variable is defined as the average AI in each district during the peak fire months of September to November 1997. This period has been identified by satellite observations and supported by prior research as the most severe phase of the haze crisis. It also represents the most relevant exposure window for assessing the long-term consequences of the fires.

To identify the causal effects, I adopt a cohort-based difference-in-differences strategy that exploits variation in pollution intensity across districts and developmental timing across birth cohorts.⁶⁶ As described in Section 4.3.2, I classify individuals into two treatment groups based on developmental stage during the 1997 peak fire months: those in utero and those aged 0–4. The comparison group consists of individuals born between 1988 and 1992, who were age five or older during the fires and are assumed to have passed the most sensitive windows of neurodevelopment and physiological vulnerability (Almond and Currie, 2011a; Currie and Vogl, 2013; Singh et al., 2019). This cohort is also close in age to the treated groups at the time mental health outcomes are measured in 2014, minimizing concerns about age-related outcome heterogeneity. The estimation strategy involves interacting district-level pollution intensity, measured by the average AI during September–November 1997, with cohort indicators for in utero and early childhood exposure. This approach allows for the separate identification of long-term psychological effects of pollution exposure in utero and during early childhood, each relative to the same comparison group. To ensure sufficient variation in exposure, I restrict the analysis to individuals residing in Sumatra and Kalimantan, where AI levels during the

⁶⁶ To support the validity of the difference-in-differences strategy, Appendix Table 4A.2 presents pre-treatment balance tests for important covariates across birth cohorts and levels of AI exposure. Household characteristics are drawn from the 1997 IFLS wave. Although the peak fires occurred between September and November of that year, variables such as maternal education, household size, and urban/rural residence are plausibly predetermined and unlikely to change substantially in the short term. The balance tests reveal no systematic differences between exposure groups at baseline. However, covariates that exhibit statistically significant differences are included as controls in all regression specifications to reduce the risk of confounding.

fire period were substantially elevated and spatially heterogeneous.⁶⁷ The main estimation equation is specified as follows:

$$Y_{id} = \alpha + \beta_1(AI_d \times \textit{in utero}_{id}) + \beta_2(AI_d \times \textit{early child}_{id}) + \gamma X_{id} + \phi W_d + \delta_d + \theta_b + \epsilon_{id} \quad (4.1)$$

Where Y_{id} is the range of psychological outcomes of individual i in district d measured in the 2014 survey. $\beta_1 \textit{in utero}_{id}$ is an indicator equal to 1 if the individual was in utero during the peak fire period in district d , 0 otherwise. Similarly, $\beta_2 \textit{early child}_{id}$ is an indicator equal to 1 if the individual was aged 0–4 during the period, and 0 otherwise. AI_d is the average AI in district d during the peak fire months. The interaction between exposure indicators and AI_d capture the effect of pollution intensity on cohorts vulnerable during the fires. The coefficients of interest are β_1 and β_2 , which identifies the differential long-run impact of air pollution exposure during early development. X_{id} is a vector of individual and household-level covariates, including gender and birth order as time-invariant characteristics, as well as mother's age at the time of the survey, parental education, urban/rural residence, household size, and per capita household expenditure, all measured in the 1997 IFLS wave. W_d is a vector of district-level weather controls, including average rainfall and wind speed during September–November 1997. δ_d are district fixed effects, controlling for time-invariant district-level factors. θ_b are birth-year fixed effects, absorbing any nationwide temporal trends affecting cohorts equally. ϵ_{id} is the individual-level error term.

In addition to the difference-in-differences specification, I implement an instrumental variable (IV) approach to address potential measurement errors in satellite-derived pollution exposure and to strengthen causal identification. The IV strategy exploits plausibly exogenous variation in the dispersion of haze due to meteorological conditions, specifically, wind direction during the peak fire months of September to November 1997. While the severity and geographic spread of the fires were driven in large part by

⁶⁷ Other regions of Indonesia are excluded from the main analysis due to uniformly low aerosol values, indicating minimal exposure to fire-related pollution. However, I include all regions of Indonesia in a robustness check to examine whether the main results are sensitive to sample selection and to assess the generalizability of the findings across the broader population.

the 1997 El Niño event, which caused prolonged drought across Indonesia, the actual spread of smoke across districts was determined by high-frequency variation in wind direction. Wind patterns are shaped by large-scale atmospheric systems and are unlikely to correlate with district-level socioeconomic characteristics or policy responses. As such, they provide a valid source of exogenous variation in pollution exposure. The important assumption is that wind direction affects long-term psychological outcomes only through its effect on air pollution exposure, and not through other direct or indirect channels.

Conditional on fire occurrence, wind direction provides plausibly exogenous variation in whether a district was downwind from haze-producing regions and thus received higher pollution intensity.⁶⁸ Following (Deryugina et al., 2019), I construct a wind-based instrument by identifying districts that were downwind from major fire-prone regions during the fire months. This instrument captures spatial variation in smoke exposure that is unrelated to individual confounders. Downwind districts are coded as '1' if the angular difference between the prevailing wind direction and the fire-to-district vector is within ± 30 degrees, indicating that the 1997 wind patterns would plausibly have carried smoke from major fire-prone areas toward them. This binary downwind indicator is designed to capture plausibly exogenous variation in smoke exposure due to meteorological factors.

This downwind indicator is then used as an instrument for district-level average AI in the following first-stage regression:

$$AI_d = \pi_0 + \pi_1(Downwind_d \times in\ utero_i) + \pi_2(Downwind_d \times early\ child_i) + \phi W_d + \epsilon_d \quad (4.2)$$

Where AI_d is the average AI in district d during the fire months. $Downwind_d \times in\ utero_i$ is a binary indicator equal to 1 if individual i , who

⁶⁸ Major fires were concentrated in South Sumatra, Jambi, and Central/Southern Kalimantan. Satellite-based AI data show that thick haze during September 1997 spread primarily northward and northeastward from these areas, reaching regions such as Riau, West Sumatra, East and North Kalimantan, and even crossing national borders into Malaysia, Singapore, and Brunei. This dispersion pattern suggests that prevailing winds blew from the South–Southwest to the North–Northeast during the fire months, carrying smoke toward central and northern population centers. Accordingly, areas located to the North–Northeast of the fire zones are classified as downwind.

was in utero during the fire period, resided in a district classified as downwind of the fires, 0 otherwise. Likewise, $Downwind_d \times early\ child_i$ equals 1 for individuals aged 0–4 during the fires who lived in downwind districts. W_d is a vector of district-level weather controls, including average rainfall and wind speed during September–November 1997. In the second stage, the predicted pollution exposure from the first stage replaces the observed AI in the main estimation equation. The two predicted AI variables, interacted with developmental stage indicators, allow estimation of the causal impact of early-life pollution exposure on long-term psychological outcomes:

$$Y_{id} = \alpha + \beta_1(\widehat{AI}_d \times in\ utero_{id}) + \beta_2(\widehat{AI}_d \times early\ child_{id}) + \gamma X_{id} + \phi W_d + \delta_d + \theta_b + \epsilon_{id} \quad (4.3)$$

where $\widehat{AI}_d$ is the instrumented average AI, and the rest of the notation follows as previously defined in equation 1.⁶⁹ Standard errors are clustered at the district level in all IV regressions.

In addition to the IV strategy, I conduct several robustness checks to assess the sensitivity and validity of the main findings. First, I expand the analysis to include all Indonesian districts with available AI data, rather than restricting the sample to Sumatra and Kalimantan. This broader sample tests whether results are driven by regional selection and whether similar patterns emerge when incorporating districts with lower haze exposure levels. Second, I relax the linearity assumption by dividing districts into terciles based on their average AI levels and interacting these bin indicators with developmental stage dummies. This non-parametric approach allows for flexible exposure effects and identifies whether impacts are concentrated in the most severely exposed regions. Individuals with the lowest exposure tercile serve as the reference group. Third, I implement a falsification test by restricting the sample to districts with minimal haze exposure, defined as having an average AI below 0.5 during the fire months. Under a valid identification strategy, no significant treatment effects should be detected in this placebo sample. Finally, to address potential exposure misclassification

⁶⁹ To maintain comparability with the main difference-in-differences specification, the second-stage IV regressions include the same set of individual and household-level covariates, district fixed effects, and birth-year fixed effects.

due to internal migration, I restrict the sample to individuals who remained in their district of birth from the time of the fires through 2014 to ensure that estimated exposure reflects actual early-life location.

To explore potential mechanisms linking early-life haze exposure to adult psychological distress, I conduct additional analyses examining intermediate outcomes that may plausibly serve as pathways. Specifically, I assess whether individuals exposed in utero or early childhood exhibit differences in educational attainment, economic status, or health-related behaviors in adulthood. These outcomes reflect long-term developmental and socioeconomic consequences of early-life shocks, which may contribute to later psychological vulnerability. While not intended to identify causal mediation, these exploratory analyses provide suggestive evidence on how early environmental adversity may shape adult mental health through disrupted human capital accumulation or reduced life-course stability.

4.5. Results

Table 4.2 presents the main estimates of the impact of early-life exposure to the 1997 Indonesian forest fires on psychological well-being in adulthood. The analysis exploits variation in district-level haze intensity, measured by mean peak AI during September–November 1997, and cohort-based exposure windows, measured by in utero and early childhood, following a difference-in-differences identification strategy. All regressions include district and birth-year fixed effects, individual and household controls, and district-level weather controls, if indicated. The sample is restricted to individuals born between 1988 and 1998 and residing in Sumatra and Kalimantan Islands at the time of the fires.

Panel A reports the effects on the CESD-10, which serves as the main outcome. This self-reported index captures mild to moderate symptoms of psychological distress. Results are presented for both the continuous CESD-10 score and a binary indicator for CESD-10 scores ≥ 10 , which signals elevated depressive risk. Across all specifications, individuals exposed to higher levels of AI in utero exhibit significantly worse mental health outcomes later in life. A one-unit increase in AI is associated with a 0.77-point increase in CESD-10 scores, representing an 11.72 % increase relative to the pre-event mean of 6.565. The likelihood of reporting elevated

depressive symptoms ($\text{CESD} \geq 10$) increases by 4 percentage points, a 20 % relative rise from a baseline of 19.70 %. These effect sizes are well within the range reported in the early-life exposure literature and are particularly plausible in this case, given the high spatial heterogeneity in haze exposure and the long-term nature of the psychological outcomes assessed, which may appear later in adulthood. The estimated impacts remain robust when district-level weather controls are included, which helps rule out potential confounding due to concurrent meteorological shocks. The estimated effects of early childhood exposure are also positive and significant, though smaller in magnitude and in line with theoretical expectations that emphasize gestational periods as a critical window of vulnerability.

Panel B extends the analysis to complementary psychological outcomes. In addition to the CESD index, I incorporate self-reported sleep quality and two clinical indicators, which are emotional instability and memory-related problems, based on respondents' reports of whether a doctor or paramedic had ever informed them of such conditions. These outcomes complement the CESD-based results by capturing both subjective symptoms and medically informed psychological conditions. The results in Panel B support the core findings from Panel A. Exposure in utero is associated with a 4.8 percentage point increase in the probability of experiencing sleep disturbance, 18.88 % relative to the baseline, and a 2.5 percentage point increase in memory-related problems, 25 % relative to the baseline. These findings remain robust after including district-level weather controls. While the estimated coefficient for emotional instability is positive, it is not statistically significant.

In contrast, early child exposure (ages 0–4) is linked to a 5.1 percentage point increase in the likelihood of experiencing sleep disturbance, slightly exceeding the in utero effect. However, no significant associations are found with memory-related or emotional outcomes in this group. This pattern is consistent with developmental theory, which highlights the gestational period as a particularly sensitive window for long-term neuropsychological development. The comparatively weaker effects on CESD scores and other clinical symptoms for early childhood exposure may reflect the brain's greater adaptive capacity in postnatal years. Meanwhile, the slightly stronger association with sleep disturbance may suggest that certain behavioral or sleep-related problems are more sensitive to stress

encountered after birth, especially when children face prolonged exposure to haze during their early developmental years.

Table 4.2. Effect of Early-Life Exposure to the 1997 Forest Fires on Psychological Outcomes

Panel A	CESD-10 scores		CESD-10 ≥ 10	
	(1)	(2)	(3)	(4)
<i>AI × In utero</i>	0.769*** (0.266)	0.767*** (0.261)	0.041** (0.020)	0.040** (0.020)
<i>AI × Early childhood</i>	0.604** (0.260)	0.601** (0.260)	0.039* (0.021)	0.038* (0.022)
Mean of non-exposed	6.565	6.565	0.197	0.197
% change (<i>In utero</i>)	11.72%	11.68%	20.81%	20.30%
% change (<i>Early childhood</i>)	9.20%	9.15%	19.79%	19.28%
Observation	2,160	2,160	2,160	2,160
Weather controls	No	Yes	No	Yes

Panel B	Emotionally unstable		Memory-related problem		Poor sleep quality	
	(5)	(6)	(7)	(8)	(9)	(10)
<i>AI × In utero</i>	0.010 (0.022)	0.008 (0.020)	0.026* (0.015)	0.025* (0.015)	0.050** (0.022)	0.048** (0.021)
<i>AI × Early childhood</i>	0.008 (0.023)	0.007 (0.023)	0.017 (0.020)	0.015 (0.020)	0.052** (0.021)	0.051*** (0.020)
Mean of non-exposed	0.078	0.078	0.099	0.099	0.270	0.270
% change (<i>In utero</i>)	12.82%	10.25%	26.26%	25.25%	18.51%	17.77%
% change (<i>Early childhood</i>)	10.25%	8.97%	17.17%	15.15%	19.25%	18.88%
Observation	2,160	2,160	2,160	2,160	2,160	2,160
Weather controls	No	Yes	No	Yes	No	Yes

Notes: Each column reports point estimates from separate regressions. Panel A presents results for the CESD-10 depression scale, reported as both a continuous score and a binary indicator (CESD-10 ≥ 10). Panel B reports binary indicators of specific psychological symptoms. All regressions include district fixed effects, birth-year fixed effects, and a set of individual and household covariates: gender, birth order, mother's age at survey, parental education, urban residence, household size, and log per capita expenditure measured in 1997. Weather controls, where included, consist of monthly district-level rainfall and wind speed during September–November 1997. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4.3. Heterogeneous Effect of Early-Life Exposure to the 1997 Forest Fires on Psychological Outcomes

	CESD-10 Score	CESD-10 ≥ 10	Emotionally unstable	Memory- related problem	Poor sleep quality
	(1)	(2)	(3)	(4)	(5)
Panel A: Impact by gender					
<i>AI × In utero</i>	0.760*** (0.260)	0.041** (0.020)	0.004 (0.018)	0.015 (0.017)	0.040** (0.020)
<i>AI × In utero × female</i>	0.043 (0.280)	0.016 (0.025)	0.034* (0.020)	0.041** (0.020)	0.010 (0.023)
Equality test, p-value	0.672	0.499	0.089	0.045	0.422
<i>AI × Early childhood</i>	0.604** (0.257)	0.039* (0.021)	0.006 (0.019)	0.027 (0.017)	0.052** (0.021)
<i>AI × Early childhood × female</i>	0.020 (0.276)	0.007 (0.024)	0.020 (0.023)	0.031 (0.021)	-0.006 (0.024)
Equality test, p-value	0.349	0.578	0.773	0.749	0.618
Panel B: Impact by household status					
<i>AI × In utero</i>	0.320 (0.251)	0.025 (0.020)	0.010 (0.019)	0.011 (0.019)	-0.009 (0.021)
<i>AI × In utero × low SES</i>	0.558** (0.264)	0.044** (0.022)	0.010 (0.022)	0.033 (0.022)	0.051** (0.025)
Equality test, p-value	0.039	0.042	0.894	0.487	0.032
<i>AI × Early childhood</i>	0.352 (0.250)	0.034 (0.022)	0.011 (0.020)	-0.018 (0.018)	-0.008 (0.020)
<i>AI × Early childhood × low SES</i>	0.593** (0.259)	0.048** (0.024)	0.008 (0.022)	0.025 (0.023)	0.055** (0.026)
Equality test, p-value	0.028	0.046	0.527	0.670	0.043
Panel C: Impact by residential area					
<i>AI × In utero</i>	0.236 (0.250)	0.019 (0.021)	0.008 (0.019)	0.014 (0.019)	0.020 (0.020)
<i>AI × In utero × rural</i>	0.710*** (0.265)	0.050** (0.020)	0.011 (0.022)	0.028 (0.023)	0.048** (0.024)
Equality test, p-value	0.004	0.048	0.699	0.601	0.046
<i>AI × Early childhood</i>	0.220 (0.249)	0.030 (0.022)	0.010 (0.018)	-0.010 (0.018)	0.021 (0.021)
<i>AI × Early childhood × rural</i>	0.654** (0.270)	0.042* (0.024)	0.009 (0.020)	0.035 (0.024)	0.050** (0.024)
Equality test, p-value	0.039	0.077	0.632	0.670	0.039
Observation	2,160	2,160	2,160	2,160	2,160
Weather controls	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports point estimates from separate regressions. All models include district fixed effects, birth-year fixed effects, and a set of individual and household-level covariates: gender, birth order, mother's age at survey, parental education, urban residence, household size, and log per capita expenditure measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

To assess whether the psychological effects of early-life exposure to forest fire smoke vary across population subgroups, I examine heterogeneity by interacting the exposure variables with relevant baseline characteristics. The first dimension is gender, given prior evidence of sex-specific vulnerability to early-life shocks. The results, presented in Table 4.3, suggest that females may experience slightly larger effects on depressive symptoms, particularly for in utero exposure and memory-related problems, though the gender differences are not consistently significant across all outcomes. Second, I consider household socioeconomic status, proxied by the bottom quintile (Q1) of the per capita expenditure (PCE) distribution. The findings indicate that individuals from lower-PCE households face consistently stronger impacts, particularly for depressive symptoms and poor sleep quality, pointing to socioeconomic disadvantage as a potential source of long-term psychological risk. Finally, I examine heterogeneity by residential location, urban versus rural, motivated by possible differences in haze exposure, housing quality, and access to health services. While some estimates suggest slightly larger effects among rural populations for depressive and sleep disturbance outcomes, there is no clear evidence of heterogeneity across other psychological dimensions, such as memory problems or emotional instability.

4.6. Robustness Checks

4.6.1. Instrumental Variable Strategy

To address potential concerns about measurement error in the satellite-derived AI and to strengthen the causal interpretation of the main findings, I implement an instrumental variable strategy that exploits exogenous variation in smoke exposure induced by wind direction during the fire season. As a preliminary validation, I regress district-level average AI during the fire months on a binary indicator for whether the district was classified as downwind.⁷⁰ The goal here is simply to confirm that downwind districts experienced systematically higher pollution levels during the haze

⁷⁰ Validation regression equation: $AI_d = \alpha + \beta \text{Downwind}_d + \epsilon_d$ (4.4), where AI_d is the average aerosol index in district d during the 1997 fire season, and Downwind_d is a binary variable equal to 1 if the district is located downwind of major fire zones.

episode. This regression, which does not incorporate developmental stage interactions, serves to establish the relevance of the instrument by demonstrating its strong predictive power for the endogenous exposure variable. The result, presented in Table 4A.3 in the Appendix, shows that the downwind status raises the average AI by roughly 0.25 units, consistent with the expected meteorological pattern of smoke transport. This provides initial empirical support for the instrument's validity. Following this validation.

Because IV credibility also depends on demonstrating that the instrument is not spuriously correlated with the outcome through channels other than pollution, I report the reduced form coefficients of downwind status on each psychological outcome.⁷¹ The reduced form is informative for two reasons. First, it provides the total effect of the instrument on mental health outcomes without scaling by measured AI. Second, under the exclusion restriction, the reduced form coefficient must equal the product of the first stage and the second-stage coefficients, so the signs and magnitudes across these steps serve as an additional internal validity check. As shown in Table 4A.4 in the Appendix, downwind exposure significantly increases CESD-10 scores and the probability of distress, mirroring the sign and relative size implied by the 2SLS estimates below. After establishing instrument relevance and inspecting the reduced form, I estimate the full IV regression.

Table 4.4 reports the IV/2SLS estimates of early-life exposure to the 1997 forest fires on psychological distress. The first-stage estimates show that districts classified as downwind experienced significantly higher levels of aerosol pollution, with coefficients ranging from 0.339 to 0.355 and F-statistics above the conventional threshold of 10 (62.28–76.63), indicating a strong and relevant instrument. In the second stage, higher early-life exposure to haze leads to substantial and statistically significant increases in long-term psychological distress. Specifically, being in utero during the fire season and exposed to elevated AI is associated with a 0.824-point increase

⁷¹ The reduced-form specification is given by:

$$Y_{id} = \pi_0 + \pi_1(\text{Downwind}_d \times \text{in utero}_i) + \pi_2(\text{Downwind}_d \times \text{early child}_i) + \phi W_d + \epsilon_d \quad (4.5)$$

in CESD-10 scores, along with higher probabilities of depressive symptoms and poor sleep quality. Early childhood exposure also yields significant impacts, particularly on CESD scores and sleep disturbance, though with somewhat smaller magnitudes.

The direction and statistical significance of these estimates are consistent with the main difference-in-differences results, offering additional causal support. These findings are plausible given the continuous, unitless nature of AI and the CESD-10 scale (0–24), and the fact that the instrument induces meaningful but not excessive shifts in pollution exposure. Overall, the IV estimates reinforce the conclusion that early-life exposure to severe haze causally impairs adult psychological health.

Table 4.4. Robustness Check – Using IV/2SLS Estimates

IV/2SLS	Aerosol Index (AI)				
First stage					
$Downwind_d \times In\ utero$	0.349*** (0.014)	0.347*** (0.013)	0.347*** (0.012)	0.347*** (0.013)	0.350*** (0.014)
$Downwind_d \times Early\ childhood$	0.344*** (0.013)	0.339*** (0.013)	0.346*** (0.013)	0.340*** (0.014)	0.349*** (0.013)
	CESD-10 score	CESD-10 ≥ 10	Emotionally unstable	Memory- related problem	Poor sleep quality
	(1)	(2)	(3)	(4)	(5)
Second stage					
$\widehat{AI} \times In\ utero$	0.824*** (0.267)	0.052*** (0.021)	0.014 (0.020)	0.028* (0.017)	0.061*** (0.022)
$\widehat{AI} \times Early\ childhood$	0.677** (0.262)	0.045** (0.022)	0.011 (0.023)	0.020 (0.019)	0.065*** (0.023)
First stage F-stat: <i>In utero</i>	62.28	60.55	54.98	53.32	63.88
First stage F-stat: <i>Early childhood</i>	76.63	66.43	57.21	60.93	75.25
Observation	2,160	2,160	2,160	2,160	2,160
Weather controls	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports 2SLS estimates from separate regressions. The endogenous exposure variable is average district-level Aerosol Index during the 1997 fire season. The instrument is a binary indicator for being downwind from major fire regions interacted with developmental timing (in utero, early childhood). The first and second stage includes weather controls. The second-stage regressions include district and birth-year fixed effects and a full set of individual and household-level controls: gender, birth order, maternal age at birth, parental education, urban residence, household size, and log per capita household expenditure (1997). Standard errors are clustered at the district level. *** p < 0.01, ** p < 0.05, * p < 0.10.

4.6.2. Expanded Sample

The main analysis restricts the sample to individuals residing in Sumatra and Kalimantan, the islands most severely affected by the 1997 forest fires and the only regions with consistently elevated AI values during the peak fire months. While this restriction enhances internal validity by ensuring meaningful variation in exposure, it raises the question of whether the estimated effects are generalizable beyond these high-exposure regions. To address this, I re-estimate the main difference-in-differences specification on a national sample covering every Indonesian district for which AI data are available, thereby adding the highly populated island of Java and several smaller provinces. The expanded sample ($N = 7,642$) serves two purposes. First, it tests whether the baseline results are driven by selection into high-exposure regions. Second, it allows the analysis to exploit the full spatial heterogeneity of Indonesia, where baseline health, socioeconomic conditions, and pollution levels differ markedly across islands.

Table 4.5. Robustness Check – Using Expanded Sample

	CESD-10 score	CESD-10 ≥ 10	Emotionally unstable	Memory- related problem	Poor sleep quality
	(1)	(2)	(3)	(4)	(5)
$AI \times In\ utero$	0.705*** (0.267)	0.037** (0.018)	0.006 (0.019)	0.023 (0.015)	0.040** (0.020)
$AI \times Early\ childhood$	0.599** (0.262)	0.034* (0.020)	0.010 (0.020)	0.011 (0.019)	0.049** (0.021)
Mean of non-exposed	6.193	0.211	0.082	0.094	0.283
% change (<i>In utero</i>)	11.39%	17.53%	7.06%	24.46%	14.13%
% change (<i>Early childhood</i>)	9.67%	16.11%	12.19%	11.70%	17.31%
Observation	7,642	7,642	7,642	7,642	7,642
Weather controls	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports point estimates from separate regressions. Panel A presents results for the CESD-10 depression scale, reported as both a continuous score and a binary indicator ($CESD-10 \geq 10$). Panel B reports binary indicators of specific psychological symptoms. All regressions include district fixed effects, birth-year fixed effects, and a set of individual and household covariates: gender, birth order, mother's age at survey, parental education, urban residence, household size, and log per capita expenditure, measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

As shown in Table 4.5, the main findings remain broadly robust. Individuals exposed in utero or during early childhood continue to exhibit significantly higher CESD-10 scores and elevated risks of depressive

symptoms and poor sleep quality. Outcomes that were insignificant in the baseline (e.g., emotional instability) remain null, while the effect size for memory-related problems declines slightly and becomes statistically insignificant. This change is expected given the inclusion of low-exposure districts. Nevertheless, the overall pattern of associations remains consistent with theoretical expectations. These results reinforce the interpretation that early-life haze exposure has lasting psychological impacts, even when analyzed in a more diverse, nationally representative sample.

4.6.3. Binned Exposure Analysis

The baseline specification assumes a linear relationship between the intensity of pollution exposure and long-term psychological outcomes. However, the effect of haze exposure may be nonlinear, particularly if health and behavioral consequences arise only after a certain pollution threshold is reached. To capture the potential non-linearities, I implement a binned exposure analysis that divides districts into three exposure groups (terciles) based on their average AI during the fire months. Specifically, I classify districts into low, medium, and high exposure terciles and interact these categorical indicators with the in utero and early childhood exposure dummies. Individuals residing in districts within the lowest tercile of AI serve as the reference group. This approach offers three advantages. First, tercile binning allows internally valid comparisons across the distribution of exposure intensity without imposing functional form assumptions, such as linearity or arbitrary thresholds. This is particularly important in this setting, where the true dose-response relationship may be non-linear or exhibit threshold effects. Second, terciles preserve statistical power by ensuring approximately balanced group sizes. Third, the interpretation of terciles is sample-specific, helping to explore effect heterogeneity across the relative distribution of haze intensity within the population at risk.

Table 4.6. Robustness Check – Using Binned Aerosol Index Levels

	CESD-10 score	CESD-10 ≥ 10	Emotionally unstable	Memory- related problem	Poor sleep quality
	(1)	(2)	(3)	(4)	(5)
Panel A - Sample: Kalimantan and Sumatra					
<i>Tercile 1: $AI_{=0.95 - 1.79} \times In\ utero$</i>	Reference category				
<i>Tercile 2: $AI_{=>1.79 - 2.38} \times In\ utero$</i>	0.551** (0.266)	0.035* (0.021)	0.012 (0.021)	0.021 (0.016)	0.042** (0.022)
<i>Tercile 3: $AI_{=>2.38 - 3.22} \times In\ utero$</i>	0.870*** (0.268)	0.058** (0.022)	0.030 (0.021)	0.036** (0.017)	0.050** (0.022)
<i>Tercile 1: $AI_{=0.95 - 1.79} \times Early\ childhood$</i>	Reference category				
<i>Tercile 2: $AI_{=>1.79 - 2.38} \times Early\ childhood$</i>	0.438* (0.260)	0.034* (0.020)	0.007 (0.023)	0.013 (0.021)	0.051** (0.021)
<i>Tercile 3: $AI_{=>2.38 - 3.22} \times Early\ childhood$</i>	0.509* (0.261)	0.045** (0.022)	0.014 (0.024)	0.018 (0.022)	0.066*** (0.023)
Observation	2,160	2,160	2,160	2,160	2,160
Weather controls	Yes	Yes	Yes	Yes	Yes
Panel B - Sample: All Indonesian Districts with AI records					
<i>Tercile 1: $AI_{=0 - 1.18} \times In\ utero$</i>	Reference category				
<i>Tercile 2: $AI_{=>1.18 - 2.03} \times In\ utero$</i>	0.498* (0.269)	0.033* (0.019)	0.007 (0.020)	0.020 (0.017)	0.036* (0.021)
<i>Tercile 3: $AI_{=>2.03 - 3.22} \times In\ utero$</i>	0.724*** (0.267)	0.040** (0.020)	0.027 (0.021)	0.029 (0.018)	0.047** (0.023)
<i>Tercile 1: $AI_{=0 - 1.18} \times Early\ childhood$</i>	Reference category				
<i>Tercile 2: $AI_{=>1.18 - 2.08} \times Early\ childhood$</i>	0.400 (0.262)	0.021 (0.020)	0.002 (0.022)	0.016 (0.018)	0.041* (0.021)
<i>Tercile 3: $AI_{=>2.08 - 3.22} \times Early\ childhood$</i>	0.535** (0.261)	0.039* (0.021)	0.014 (0.024)	0.024 (0.019)	0.058*** (0.022)
Observation	7,642	7,642	7,642	7,642	7,642
Weather controls	Yes	Yes	Yes	Yes	Yes

Notes: Each panel reports coefficients from separate regressions using different samples. Panel A uses individuals residing in Sumatra and Kalimantan, while Panel B includes all Indonesian districts with available AI data. Exposure intensity is defined based on terciles of district-level peak Aerosol Index (AI) values observed during September–November 1997. All models include district fixed effects, birth-year fixed effects, and a rich set of covariates: gender, birth order, mother's age at survey, parental education, urban residence, household size, and log per capita expenditure, measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Results presented in Table 4.6 show a clear gradient, as expected. In the primary sample (Sumatra and Kalimantan) in Panel A, which includes the regions most affected by the 1997 fires, the lowest tercile spans AI values from 0.95 to 1.79. This range still includes districts with considerable pollution, as values above 1.5 are often considered hazardous (see Jayachandran, 2009; Rosales-Rueda & Triyana, 2020). In contrast, when I expand the sample to all Indonesian districts in Panel B, the first tercile spans

only 0.95 to 1.17, reflecting the inclusion of many regions with minimal haze exposure. I find broadly consistent results across both the core affected regions and the expanded national sample.

Individuals in the highest exposure tercile exhibit significantly higher CESD-10 scores and elevated probability of depressive symptoms and sleep disturbances compared to those in the lowest tercile. The coefficients for the middle tercile are smaller in magnitude and, in some cases, not statistically significant, suggesting a threshold effect in which only sufficiently high exposure levels lead to measurable long-term psychological harm. The adverse effects are concentrated among individuals exposed to the most intense levels of pollution, aligning with existing literature on the non-linear health effects of environmental shocks. This strengthens the case for using a high-exposure cutoff in defining treatment intensity and supports the interpretation that the observed mental health effects are not spurious or driven by functional form assumptions.

4.6.4. Placebo Timing Test

In this subsection, I implement a falsification test to examine whether the estimated effects of early-life haze exposure are driven by arbitrary cohort trends rather than actual exposure to the 1997 forest fires. Specifically, I reassign the treatment timing as if the fires had occurred in 1987, ten years before the true event, while keeping the original 1997 AI exposure data unchanged.⁷² This placebo timing test allows me to assess whether the estimated impacts would still emerge in a context where no large-scale fire or haze occurred. If the main results simply capture arbitrary cohort differences, the same interaction structure should falsely detect an effect in 1987. If the results are causal, the coefficients should be null.

To implement this test, I redefine the treatment cohorts as follows: individuals born in 1988 are considered to have been in utero during the hypothetical 1987 fires, those born between 1983 and 1987 are classified as early childhood, and those born in 1982 or earlier serve as the older cohort.

⁷² I select 1987 as the placebo year, ten years before the actual 1997 fires, to ensure the placebo cohorts were unexposed during early development. Individuals born in 1983–1988 were at least 9 years old by 1997 and thus beyond the sensitive exposure window. These cohorts were also at comparable ages in 2014 as those in the main analysis. Importantly, 1987 predates any major fire events in Indonesia.

At the time of the psychological outcome measurement in 2014, these groups were approximately 26 years old (in utero), 27–31 years old (early childhood), and 32+ years old (older group). Since these cohorts had already passed early childhood by the time of the actual 1997 fires, they could not have been affected by haze exposure in utero or during early development.

Table 4.7. Placebo Test – Using Pseudo-Treatment Year Falsification

	CESD-10 score	CESD-10 ≥ 10	Emotionally unstable	Memory- related problem	Poor sleep quality
	(1)	(2)	(3)	(4)	(5)
<i>AI × In utero in 1987</i>	0.029 (0.265)	0.009 (0.017)	-0.005 (0.018)	0.010 (0.017)	0.016 (0.022)
<i>AI × Early childhood in 1987</i>	0.030 (0.259)	0.014 (0.021)	0.002 (0.019)	0.007 (0.018)	0.019 (0.023)
Mean of non-exposed	5.722	0.180	0.076	0.090	0.265
Observation	2,833	2,833	2,833	2,833	2,833
Weather controls	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports point estimates from separate regressions. All regressions include district fixed effects, birth-year fixed effects, and a set of individual and household covariates: gender, birth order, mother’s age at survey, parental education, urban residence, household size, and log per capita expenditure measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 4.7 reports the estimates. As expected, the interaction coefficients are small in magnitude and statistically indistinguishable from zero across all five psychological outcomes. These null findings confirm the absence of systematic cohort differences under a false treatment timing, supporting the validity of the identification strategy. The results support the interpretation that the estimated psychological effects observed in the main analysis are attributable to actual early-life exposure to the 1997 forest fires.

4.6.5. Potential Bias from Migration

A potential concern in identifying early-life exposure to the 1997 forest fires is internal migration. Because exposure is assigned based on individuals’ district of residence during the fire season (September–November 1997), subsequent mobility could lead to exposure misclassification if individuals relocated after the fires. This could introduce misclassification bias, particularly if migration is non-random and correlated with both exposure and later mental health outcomes. To address this concern, I implement a robustness check by restricting the sample to non-movers, defined as individuals who never changed their district of residence

between 1997 and 2014. This restriction ensures a more accurate assignment of exposure, aligning district-level AI values with individuals' actual early-life environments during the fire event. While this restriction reduces the sample size, it offers a cleaner estimate by minimizing exposure misclassification.

The results from the non-mover subsample remain robust and largely consistent with the main findings. In fact, for several mental health outcomes, the estimated effects are slightly larger in magnitude, suggesting that measurement error due to internal migration may have attenuated the baseline estimates. As shown in Table 4.8 for the Sumatra and Kalimantan sample, and in Table 4A.5 in the Appendix for the expanded national sample, the coefficients remain statistically significant and directionally stable. These findings imply that the observed association between early-life exposure to forest fire smoke and long-term psychological distress is not driven by mobility-related selection and supports the internal validity of the exposure assignment.

Table 4.8. Restricted Sample – Individuals Remaining in the Same District (1997–2014)

	CESD-10 score	CESD-10 ≥ 10	Emotionally unstable	Memory- related problem	Poor sleep quality
	(1)	(2)	(3)	(4)	(5)
<i>AI × In utero in 1987</i>	0.803*** (0.312)	0.044* (0.023)	0.008 (0.026)	0.027* (0.016)	0.050** (0.025)
<i>AI × Early childhood in 1987</i>	0.684** (0.296)	0.040* (0.024)	0.006 (0.026)	0.019 (0.017)	0.055** (0.024)
Mean of non-exposed	6.990	0.195	0.075	0.097	0.267
% change (<i>In utero</i>)	11.48%	22.56%	10.66%	27.83%	18.73%
% change (<i>Early childhood</i>)	9.79%	20.51%	8.00%	19.59%	20.59%
Observation	1,571	1,571	1,571	1,571	1,571
Weather controls	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports point estimates from separate regressions. All regressions include district fixed effects, birth-year fixed effects, and a set of individual and household covariates: gender, birth order, mother's age at survey, parental education, urban residence, household size, and log per capita expenditure measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

4.7. Exploring Potential Mechanisms

In this section, I examine potential mechanisms underlying the relationship between early-life exposure to severe haze and long-term psychological distress. Identifying the possible channels is essential for both policy targeting and for understanding whether the long-run impacts are driven by biological damage, disrupted human capital accumulation, or socioeconomic adversity. Drawing on the literature on early-life shocks and human capital formation (see Almond & Currie, 2011; Currie et al., 2014), I group potential mechanisms into three areas: educational disruption, deterioration in physical health, and household-level impacts. While I do not conduct a formal mediation analysis that quantifies the exact share of the total effect attributable to each pathway, I analyze secondary outcomes such as cognitive test scores, years of schooling, adult physical health indicators, and retrospective household characteristics that plausibly capture lasting disruptions in these fields. The resulting patterns provide suggestive evidence on how early haze exposure can translate into persistent psychological harm.

4.7.1. Disruption in Educational Development

Disruptions to educational development caused by early-life environmental shocks may have lasting consequences for psychological well-being in adulthood. Exposure during sensitive periods of brain maturation can impair cognitive functioning, delay school readiness, and increase the risk of grade repetition or school dropout. To explore this channel, I consider two complementary proxies: a cognitive ability score measured during childhood (while respondents were of school age) and years of schooling completed, measured in adulthood.

The cognitive measure is based on a standard test included in IFLS, which evaluates basic reasoning ability using Raven's matrices and mathematical tests.⁷³ For older cohorts (i.e., those born between 1988 and 1992), cognitive test scores are taken from IFLS 3 (2000), when they were between 8 and 12 years old and participated in the child module. For the

⁷³ The cognitive test was managed to children aged 7 to 14 years and included 12 Raven's matrices questions and 5 basic mathematics questions, for a total of 17 items. The cognitive outcome is constructed as the proportion of correct answers out of the total.

younger cohorts (i.e., those born between 1993 and 1998, who were in utero or aged 0–4 during the 1997 fires), scores are drawn from IFLS 4 (2007), when they reached a comparable schooling age. In cases where respondents have valid test scores in both waves within the 7–12 age window, I keep the earliest available score to reduce potential measurement bias due to age progression. The second variable, years of education, reflects cumulative human capital investment. Since most respondents had completed formal education by 2014, this measure can be considered fixed at the time the psychological outcomes were assessed. However, to avoid measurement bias due to ongoing schooling among younger individuals, I restrict the sample to those who reported not currently attending school in 2014.

Table 4.9. Potential Mechanisms – Impact on Educational Outcomes

	Cognitive test	Completed years of schooling
	(1)	(2)
<i>AI × In utero</i>	-0.054 (0.126)	-0.229 (0.200)
<i>AI × Early childhood</i>	-0.022 (0.095)	-0.665*** (0.210)
Mean of non-exposed	0.706	8.819
% change (<i>In utero</i>)	7.64%	2.59%
% change (<i>Early childhood</i>)	3.11%	7.54%
Observation	2,011	1,780
Weather controls	Yes	Yes

Notes: Each column reports point estimates from separate regressions. All regressions include district fixed effects, birth-year fixed effects, and a set of individual and household covariates: gender, birth order, mother’s age at survey, parental education, urban residence, household size, and log per capita expenditure measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Estimates reported in Table 4.9 indicate that individuals exposed in utero or early childhood to high levels of haze score lower on the childhood cognitive test, although the differences are not statistically significant. The negative sign of the coefficients is nonetheless consistent with theoretical expectations of developmental impairment. In contrast, completed years of schooling decline markedly for the early-child cohort. Exposure to high haze is associated with a statistically significant reduction of 0.67 years (about 7.5 percent of the baseline mean). This reduction may reflect delayed school entry, missed school days due to illness, or lower cognitive readiness during early developmental stages, which can increase the likelihood of grade

repetition or early dropout. The in-utero point estimate is smaller and imprecise. These results suggest that while the Raven test may not capture the full extent of early cognitive damage, educational attainment does appear to be adversely affected, particularly for children who were already aged 0–4 during the fires. Reduced schooling, therefore, emerges as a plausible pathway linking early-life haze exposure to the persistent psychological distress documented in the main results.

4.7.2. Deterioration in Physical Health

Early life exposure to severe haze may have lasting physiological consequences, particularly by compromising respiratory and immune system development. Airborne pollutants from forest fires can disrupt lung maturation and increase vulnerability to infectious diseases. These early insults may manifest as both short-term morbidity in childhood and long-term health limitations in adulthood, which can in turn contribute to elevated psychological distress. To examine this pathway, I use proxy outcomes that reflect both early-life and later-life health conditions.

To capture early-life morbidity directly, I draw on retrospective questions from the Early Health (EH) section of IFLS 5, which asks adult respondents about health events that occurred from birth up to and including age 15. First, respondents report whether they ever missed school for a month or more because of a health condition during childhood. Extended school absence is a strong signal of serious or recurrent illness and is frequently used in literature as a marker of early health shocks that disrupt human-capital formation. Second, respondents indicate whether they ever experienced specific illnesses before age 15, including respiratory diseases, plausibly linked to haze exposure, and psychiatric problems, which may reflect early psychological vulnerability. These childhood variables were recorded for events that happened long before the 2014 mental health outcomes were measured, ensuring that exposure temporally precedes the outcome and reduces the risk of reverse causality.⁷⁴

⁷⁴ The Early Health module in IFLS 5 asks adult respondents whether specific illnesses occurred ‘from birth up to and age 15.’ Because part of the early-child cohort (those aged 0–4 during the 1997 fires) had already lived several years before the fire episode, including illness that began in the 0–5 age window could confound post-exposure effects with pre-

Table 4.10. Potential Mechanisms – Impact on Physical Health Outcomes

	Childhood respiratory problem	Childhood psychiatric problem	Missed school for ≥1 month due to sickness	Adulthood lung disease	Low physical functioning	Hypertension
	(1)	(2)	(3)	(4)	(5)	(6)
<i>AI × In utero</i>	0.036* (0.019)	0.030** (0.015)	0.004 (0.011)	0.031** (0.014)	0.027** (0.012)	0.023* (0.012)
<i>AI × Early childhood</i>	0.040** (0.020)	0.026* (0.015)	0.017* (0.011)	0.039*** (0.015)	0.018 (0.014)	0.022* (0.013)
Mean of non-exposed	0.190	0.154	0.078	0.198	0.185	0.110
% change (<i>In utero</i>)	18.94%	19.48%	5.12%	15.65%	14.59%	20.90%
% change (<i>Early childhood</i>)	21.05%	16.88%	21.79%	19.69%	9.72%	20.00%
Observation	1,982	1,767	2,160	2,160	2,160	2,160
Weather controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports point estimates from separate regressions. All regressions include district fixed effects, birth-year fixed effects, and a set of individual and household covariates: gender, birth order, mother's age at survey, parental education, urban residence, household size, and log per capita expenditure measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

To capture longer-term health consequences, I also examine adulthood outcomes, including diagnosed lung-related conditions, physical functioning limitations, and self-reported hypertension as an early indicator of cardiovascular risk. These outcomes reflect the cumulative burden of early physiological damage and offer insight into whether early-life exposure led to persistent physical health deficits. Together with the childhood health proxies, these variables allow for a more comprehensive assessment of the biological channel through which early haze exposure may contribute to long-run psychological distress.

The results are presented in Table 4.10. Columns (1) to (3), which report childhood morbidity outcomes, show that individuals exposed in utero exhibit a significantly higher probability of ever having suffered a childhood respiratory illness or an emotional, nervous, or psychiatric problem, with effect sizes representing 18–19 percent increases over the baseline means. Point estimates for prolonged illness-related school absence are positive but

exposure conditions. To mitigate this bias, I exploit the follow-up question that records the age at first occurrence of each illness (0–5, 6–10, or 11–15). For the respiratory and psychiatric illness outcomes, I code an indicator equal to one if the respondent reports diagnosis at ages 6–15, thereby ensuring that the illness began after the fire period for all cohorts. Missed-school-due-to-illness is less problematic, as the event necessarily occurs when children are of primary-school age, hence it already lies in the post-exposure window for both the in-utero and early-child cohorts.

statistically imprecise. Similar patterns emerge for the early-child cohort, with slightly larger coefficients for respiratory illness and somewhat smaller effects for psychiatric problems.

In line with the main findings, I find that early-life exposure to high levels of haze is associated with a statistically significant increase of approximately 3 percentage points in the probability of reporting childhood emotional, nervous, or psychiatric problems. While this self-reported measure does not isolate clinical diagnoses, it captures a broad range of psychological difficulties and has been used in prior research as an indicator of early-inception distress. The observed association suggests that prolonged exposure to toxic air pollution during sensitive developmental windows may disrupt emotional development or trigger early symptoms of psychological distress. Nonetheless, given the general and retrospective nature of the measure, the results should be interpreted as suggestive rather than conclusive evidence of early psychiatric morbidity.

Columns (4) to (6) present adult health results, showing that early-life exposure is also associated with worse physical health in adulthood. Both cohorts have a higher probability of being diagnosed with lung disease in adulthood, translating into a 16-20 percent increase over the baseline. The in-utero cohort additionally shows higher rates of physical-functioning limitations and hypertension, while the early-child cohort coefficients are similar in magnitude and significant at the 10 percent level. These adult conditions are consistent with cumulative respiratory and cardiovascular stress from early exposure to smoke pollution.

The pattern across childhood and adult measures is coherent. Respiratory problems emerge early, persist into adulthood, and coincide with elevated risks of chronic disease and functional limitation. Because the childhood variables refer to events that occurred well before the 2014 mental health survey, and the adult health outcomes are measured concurrently with the psychological outcomes, they provide evidence of a biological pathway. Although recall errors in the retrospective childhood questions may not be ruled out, any such error is likely to be non-differential across exposure groups, biasing the estimates toward zero. In conclusion, the results in Table 4.9 support the interpretation that early-life haze exposure damaged physical health, in that way contributing to the persistent psychological distress observed two decades later.

4.7.3. Parental and Household Impacts

In addition to educational and health-related pathways, early-life exposure to environmental shocks may affect long-run psychological well-being through household and parental-level disruptions. Smoke episodes can influence family economic stability, reduce parental availability, and increase stress within the household, all of which may shape a child's developmental environment in enduring ways. These mechanisms are particularly relevant in contexts where income volatility and food insecurity are common, and public safety nets are limited.

To isolate household-level channels, I link each individual in the 2014 analysis sample to their parental household information recorded in IFLS2, most of which was collected between June and December 1997, contemporaneous with the fires. For every respondent, I observe parental employment status, the log of per-capita expenditure (PCE), and an indicator of food insecurity in 1997. To capture short-run employment shocks, I examine labor force participation of household heads and their spouses in 1997. Temporary job loss, informal sector displacement, or reduced working hours during the fire season may reflect household-level stress that affects caregiving and investment in children. In parallel, I assess nutritional stress using reported food consumption.⁷⁵ These proxies capture both income constraints and coping responses to environmental shocks. I then re-estimate the baseline interaction model using these variables as outcomes. Because these measures were captured during the fire season, significant associations would indicate that haze exposure imposed immediate economic or nutritional stress on the family, providing a plausible pathway to the long-run psychological effects documented earlier.

The results are presented in Table 4.11. Although household-level economic outcomes like per-capita expenditure and parental unemployment

⁷⁵ Food insecurity is coded as one for individuals whose household food consumption falls below the minimum threshold of dietary adequacy, as defined by the Food Consumption Score (FCS). The FCS is a composite measure that captures dietary diversity, food frequency, and relative nutritional importance of different food groups, and is commonly used to assess household food security status (see (Dembedza et al., 2022)). A low FCS indicates insufficient intake of essential nutrients and calories, signaling vulnerability to food insecurity.

show no significant deterioration, the sharp increase in food insecurity (18–19%) indicates a nutritional shock concurrent with the fires. This shock plausibly constrained children’s dietary quality during a critical period of physical development. Such early-life nutritional stress is well-documented to compromise immune function, respiratory health, and general physiological resilience. In this study, it complements the observed rise in childhood respiratory and psychiatric problems, school absence due to illness, and adult lung-related conditions, suggesting that household food insecurity may be a contributing pathway to the adverse physical health and subsequent psychological outcomes.

Table 4.11. Potential Mechanisms – Impact on Household Outcomes

	Per capita expenditure	Parental unemployment	Food insecurity
	(1)	(2)	(3)
<i>AI × In utero</i>	0.162 (0.142)	0.011 (0.014)	0.038** (0.015)
<i>AI × Early childhood</i>	0.157 (0.140)	0.013 (0.017)	0.041*** (0.016)
Mean of non-exposed	13.244	0.321	0.217
% change (<i>In utero</i>)	1.22%	3.42%	17.51%
% change (<i>Early childhood</i>)	1.18%	4.05%	18.89%
Observation	2,160	2,160	2,160
Weather controls	Yes	Yes	Yes

Notes: Each column reports point estimates from separate regressions. All regressions include district fixed effects, birth-year fixed effects, and a set of individual and household covariates: gender, birth order, mother’s age at survey, parental education, urban residence, and household size measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

4.8. Conclusion

This study provides new evidence on the long-term psychological consequences of early-life exposure to the 1997 Indonesian forest fires, one of the most intense and sustained haze episodes in recorded history. Leveraging individual-level data from the Indonesian Family Life Survey and exploiting spatial and temporal variation in pollution exposure based on remotely sensed AI data, I document significant increases in depressive symptoms and related psychological problems among individuals who were in utero or early childhood at the time of the fires. These effects are observed

nearly two decades later, highlighting the enduring impact of early environmental shock on mental health.

The estimates are robust across a range of empirical strategies, including alternative exposure definitions, different age cutoffs, and falsification tests using both placebo regions and timing. Consistency across specifications lends credibility to the causal interpretation of the findings, especially in the context of limited pre-treatment measurement of psychological outcomes. The results also reveal that the developmental timing of exposure matters. Individuals exposed during the fetal period or early postnatal years consistently exhibit the largest increases in psychological distress, sleep disturbance, and other related symptoms in adulthood. These patterns align with the developmental origins of health and disease framework and contribute to a growing body of literature documenting long-term impacts of early-life adversity.

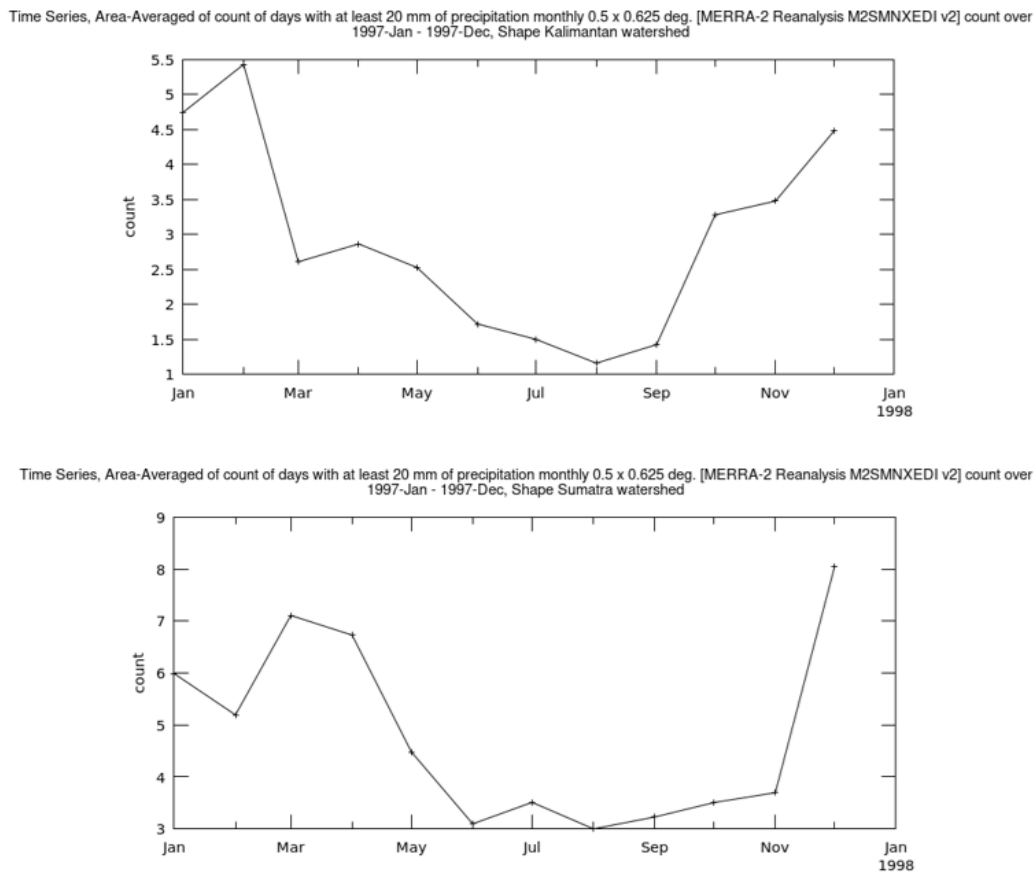
To understand potential pathways linking early-life haze exposure to long-term psychological distress, I examine a range of secondary outcomes. The findings point to a combination of educational disruption, deterioration in physical health, and household-level stressors, such as food insecurity, as plausible mechanisms. Individuals exposed during early childhood exhibit significantly lower educational attainment, which may partly result from prolonged school absence due to illness. In parallel, early exposure is associated with a higher incidence of childhood and adult health problems, including respiratory conditions and functional limitations, as well as signs of food insecurity during the fires. Although the analysis does not formally decompose these effects, the consistent patterns across outcomes suggest that biological, behavioral, and socioeconomic channels jointly contribute to the observed long-term psychological consequences.

In sum, these findings underline the broad and long-lasting consequences of large-scale environmental disasters. The evidence suggests that the human costs of forest fires extend well beyond immediate respiratory illnesses and economic disruptions, potentially damaging mental health and limiting human capital formation. This has important implications for public policy. As climate-related disasters are expected to become more frequent and intense, especially in low- and middle-income countries, there is a growing need to design disaster response frameworks that integrate mental

health support as a core component. Failing to address these issues could lead to substantial economic losses and compound the social burden. Global estimates by Bloom et al. (2011) valued the total cost of mental health conditions at US\$2.5 trillion in 2010, with projections rising to over US\$6.0 trillion by 2030, largely driven by productivity losses. This highlights the broader macroeconomic importance of prioritizing mental health as part of disaster preparedness and recovery strategies. Particular attention should be given to the population exposed during early life, who may bear the greatest long-term consequences. Strengthening investments in early mental health interventions and support systems can help alleviate future economic burdens and contribute to more resilient and equitable development outcomes.

Appendix: Additional Tables and Figures

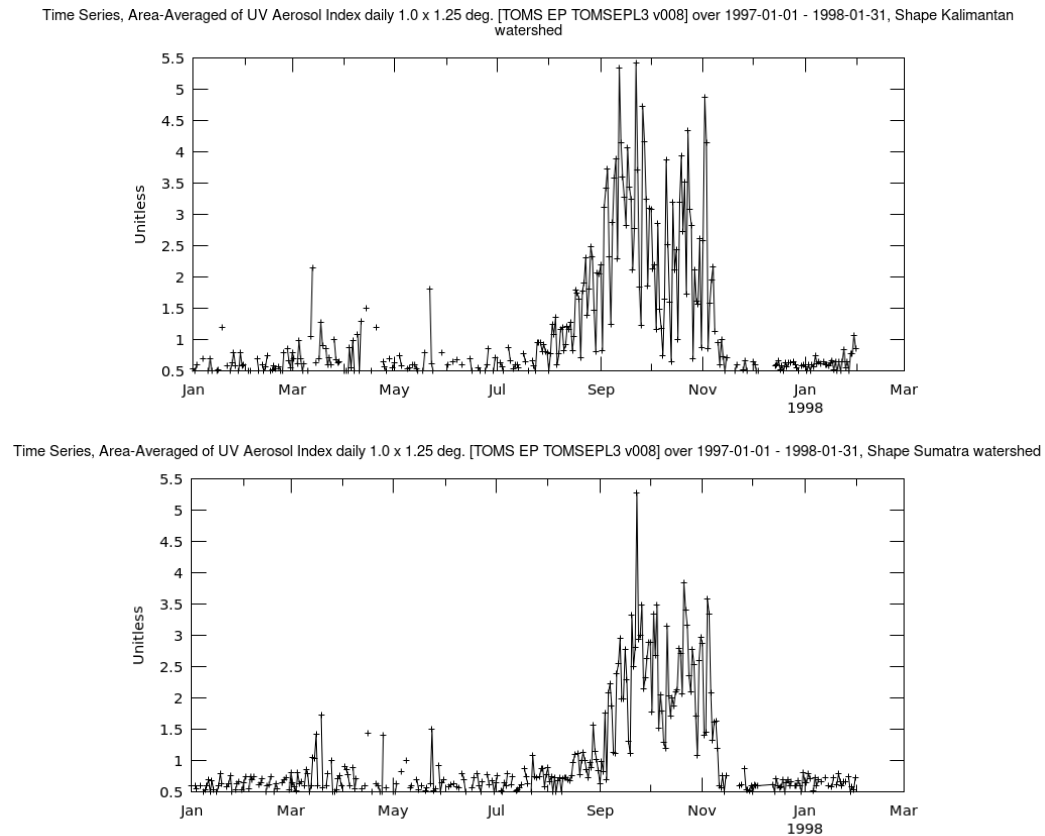
Figure 4A.1. Monthly Count of Heavy Rainfall Days in Sumatra and Kalimantan, 1997



Notes: The graphs display the monthly count of days with at least 20 mm of precipitation in 1997, averaged over the Sumatra and Kalimantan, respectively. Data are derived from the MERRA-2 Reanalysis (M2SMNXEDI v2). The pronounced decline in rainfall from June to October, particularly during the peak fire months of September and November, illustrates the severe drought conditions associated with the 1997 El Niño event.

Source: Author's visualization using data from NASA Giovanni (<https://giovanni.gsfc.nasa.gov/giovanni/>).

Figure 4A.2. Daily Aerosol Index over Sumatra and Kalimantan, January 1997 – January 1998



Notes: The figures display the area-averaged daily UV Aerosol Index (AI) derived from the TOMS EP satellite (TOMSEPL3 v008) for the Sumatra (top panel) and Kalimantan (bottom panel) Islands. A sharp increase in AI levels is observed beginning in early September 1997, peaking in September–November, and gradually declining in December. These patterns confirm that elevated levels of atmospheric pollution persisted across both islands throughout the September–November fire season, justifying the definition of the exposure window used in the analysis.

Source: Author's visualization using data from NASA Giovanni (<https://giovanni.gsfc.nasa.gov/giovanni/>).

Table 4A.1. Validation of Exposure Measure: Correlation Between District-Level Aerosol Index and Fire Hotspot Counts During Fire Season

	Aerosol Index (AI)
<i>hotspots count_d</i>	0.0026*** (0.0010)
Observation	52
R-squared	0.233

Notes: The regression sample is restricted to districts with non-zero fire hotspot counts during the September–November 1997 fire season, based on ATSR thermal anomaly data (N = 52 Districts). While some smoke-exposed districts may not have recorded active fires locally, the observed correlation provides supporting evidence that districts with more fire activity also experienced greater atmospheric pollution. Standard errors are clustered at the district level. *** p < 0.01, ** p < 0.05, * p < 0.10. *

Table 4A.2. Balance Check Between Covariates of Exposed and Non-Exposed Cohorts.

	Older cohorts			Younger cohorts		
	Birthyear: 1988 - 1992			Birthyear: 1993 - 1998		
	Low AI	High AI	P-value Diff	Low AI	High AI	P-value Diff
Mother's years of schooling	3.28	3.17	0.220	3.39	3.30	0.267
Father's years of schooling	5.03	5.78	0.012	5.65	5.80	0.079
Household head employed	0.72	0.70	0.301	0.86	0.76	0.152
Household size	6.01	6.44	0.137	7.14	7.23	0.056
Q1 of household PCE	0.09	0.12	0.204	0.10	0.12	0.227
Rural area	0.42	0.68	0.001	0.47	0.53	0.021

Notes: Each column reports mean values of the listed baseline covariates for districts with Low versus High fire-season Aerosol Index, based on sample median. The left panel refers to the older cohort (birth years 1988–1992, already ≥ 5 years old during the fires); the right panel refers to the younger cohort (birth years 1993–1998, in utero or aged 0–4 during the fires). Mother's and Father's years of schooling are completed grades at the time of the 1997 survey. Household head employed equals 1 if the household head reported working during the reference week. Q1 of household PCE equals 1 if the household's per-capita expenditure in 1997 falls in the bottom quintile of the national distribution. Rural area equals 1 for households located outside urban enumeration areas. Standard errors are clustered at the district level. *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 4A.3. Validation of Instrument: Effect of Downwind Status on District-Level Aerosol Index During Fire Season

	Aerosol Index (AI)
$Downwind_d$	0.251*** (0.011)
Observation	2,160

Notes: Standard errors are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4A.4. Robustness Check – Reduced-Form Estimates of Early-Life Exposure to Downwind Pollution on Psychological Outcomes

	CESD-10 score	CESD-10 ≥ 10	Emotionally unstable	Memory- related problem	Poor sleep quality
	(1)	(2)	(3)	(4)	(5)
Reduced form:					
$Downwind_d \times In\ utero$	0.287*** (0.082)	0.020** (0.009)	0.004 (0.005)	0.010* (0.006)	0.021*** (0.008)
$Downwind_d \times Early\ childhood$	0.233*** (0.090)	0.016** (0.008)	0.003 (0.005)	0.007 (0.007)	0.023*** (0.009)
Observation	2,160	2,160	2,160	2,160	2,160
Weather controls	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports reduced-form estimates from separate regressions linking early-life exposure to downwind pollution to adult psychological outcomes. The exposure proxies are interactions between district downwind status during the 1997 fire season and developmental timing (in utero or early childhood). All specifications include district and birth-year fixed effects, a full set of individual and household controls (gender, birth order, mother's age at birth, parental education, residence, household size, and log per capita expenditure measured in 1997), and weather controls. Standard errors are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4A.5. Restricted Sample: Individuals Remaining in the Same District (1997–2014, National Sample)

	CESD-10 score	CESD-10 ≥ 10	Emotionally unstable	Memory- related problem	Poor sleep quality
	(1)	(2)	(3)	(4)	(5)
<i>AI × In utero in 1987</i>	0.710*** (0.272)	0.039* (0.020)	0.005 (0.020)	0.025 (0.016)	0.049** (0.024)
<i>AI × Early childhood in 1987</i>	0.611** (0.270)	0.037* (0.022)	0.009 (0.021)	0.014 (0.019)	0.053** (0.024)
Mean of non-exposed	6.233	0.215	0.080	0.097	0.290
% change (<i>In utero</i>)	11.39%	18.13%	6.25%	25.77%	16.89%
% change (<i>Early childhood</i>)	9.80%	17.21%	11.25%	14.43%	18.27%
Observation	5,822	5,822	5,822	5,822	5,822
Weather controls	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports point estimates from separate regressions. All regressions include district fixed effects, birth-year fixed effects, and a set of individual and household covariates: gender, birth order, mother's age at survey, parental education, urban residence, household size, and log per capita expenditure measured in 1997. Weather controls are also included. Standard errors are clustered at the district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

5. Concluding Remarks

5.1. Summary and Policy Implications

This doctoral dissertation examines the long-term impacts of both adverse events and public policy interventions on mental health outcomes, with a particular focus on the Indonesian context. Across three empirical studies, it highlights how early-life and adulthood exposure to environmental shocks and health policies shape psychological well-being in later life. These findings contribute to a growing body of work in health economics that recognizes mental health as a central component of human capital and development.

Chapter 2 explores the long-term psychological consequences of natural disasters, with a focus on adult exposure to earthquakes. The findings show that individuals living in areas struck by high-intensity earthquakes experience sustained psychological distress after the disaster period. These effects are persistent and disproportionately affect older adults, rural residents, and those from low-income households. The results call attention to the urgent need for mental health support that goes beyond emergency response and extends into long-term recovery. Mental health services must be sustained until affected individuals regain psychological stability, which requires a shift from short-term trauma care to continuous psychosocial support. This is especially critical in developing countries like Indonesia, where access to mental health infrastructure is limited, and integrated services within the health system remain scarce (Rizal et al., 2024).

In such contexts, meaningful progress cannot rely on the health sector alone. A coordinated effort involving individuals, communities, and government institutions is essential to promote recovery and reduce stigma. Cultural attitudes toward mental illness in Indonesia often lead people to withdraw from public life and avoid seeking help, exacerbating their isolation (Basrowi et al., 2024). Therefore, improving mental health outcomes requires not only expanded service delivery but also public education and community outreach, so that individuals feel supported and encouraged to seek care. The findings from this chapter strongly support the development of inclusive,

community-based mental health strategies that remain active well beyond the initial disaster response.

Chapter 3 examines the long-term psychological effects of the Village Midwife Program, a large-scale health policy intervention aimed at improving maternal and child health in rural Indonesia. The results show that individuals born in communities with midwife presence during early life exhibit significantly lower levels of psychological distress in adulthood. The mental health benefits are particularly pronounced among women and individuals from disadvantaged socioeconomic backgrounds, suggesting that access to skilled maternal care in the earliest stages of life can have lasting protective effects on psychological well-being.

These findings highlight the broader value of maternal health interventions, not only in reducing infant and maternal mortality but also in promoting long-term mental health outcomes. In a developing country context like Indonesia, where rapid population growth and limited access to healthcare services for pregnant women and infants were critical challenges in the early 1990s, the introduction of village midwives brought a new sense of hope to rural communities (Shankar et al., 2008). At a time when labor complications and infant mortality were major public health concerns, the program's presence provided essential care where it had been previously inaccessible.

The lasting mental health benefits observed in this study strengthen the case for sustaining and expanding community-based maternal health services, particularly in geographically remote or underserved regions. In many parts of Indonesia, barriers such as difficult topography, transportation constraints, and financial limitations continue to delay access to formal health facilities (Laksono et al., 2023; Leosari et al., 2023). The mechanisms identified, such as increased prenatal care, iron supplementation, and direct breastfeeding, highlight behaviors that should be further promoted through health education and service delivery. Reinforcing such practices and ensuring that midwifery services reach all communities remain critical steps toward enhancing both physical and mental health outcomes across the life course.

Chapter 4 investigates the long-term psychological consequences of early-life exposure to the 1997 Indonesian forest fires. The analysis shows that individuals who were in utero or early childhood during the period of

intense smoke exposure exhibit significantly higher levels of psychological distress in adulthood, particularly symptoms of depression and sleep disturbances. These effects are strongest in districts with prolonged and severe pollution, where satellite-based Aerosol Index data confirm extended periods of hazardous air quality lasting weeks or even months. Importantly, the findings highlight that the timing of exposure matters. The developmental vulnerability of fetuses and young children, combined with the cumulative nature of prolonged exposure, suggests that even indirect environmental insults during these stages can have lasting impacts on psychological well-being.

These three studies carry significant relevance for both research and policy. First, they underscore the urgent need to prioritize mental health, alongside physical health, in disaster preparedness and environmental policy. In particular, pregnant women and young children must be recognized as high-risk groups and provided with proactive protection and continued support during and after environmental crises. This includes not only evacuation and health monitoring during the event, but also long-term psychological care and social services. In developing countries like Indonesia, where mental health infrastructure remains weak and stigma is widespread, it is essential that climate adaptation strategies explicitly integrate mental health dimensions and promote coordinated responses between government, communities, and healthcare providers.

Second, the increasing frequency and intensity of natural disasters globally heighten the urgency of this issue. According to the EM-DAT International Disaster Database, the number of reported disasters more than tripled over the past four decades, rising from approximately 4,200 events between 1980 and 1999 to over 7,300 between 2000 and 2019 (United Nations Office for Disaster Risk Reduction, 2020). In Southeast Asia, Indonesia remains among the most disaster-prone countries, frequently experiencing earthquakes, floods, landslides, and wildfires (Bronkhorst and Bhandari, 2021). This trend not only highlights the persistent threat of environmental shocks but also reinforces the importance of understanding their long-term mental health consequences. As the climate crisis worsens, the psychological toll will grow, demanding resilient, inclusive policies that address both immediate and lasting effects.

Together, these three studies provide new empirical evidence on the long-term mental health consequences of both adverse and preventive events. They highlight the importance of adopting a life-course perspective when evaluating the mental health impacts of policy and environmental shocks, particularly in developing country contexts where resources are constrained and vulnerabilities are high. These findings carry clear policy implications: they call for the integration of mental health into disaster risk reduction frameworks, the expansion of early-life health programs such as maternal care, and the routine inclusion of psychological outcomes in the evaluation of social and development interventions. Addressing these dimensions is essential to promoting population well-being and building resilience in the face of future shocks.

5.2. Future Research Directions

While this dissertation contributes to our understanding of how adverse events and public health interventions shape mental health outcomes across the course of life, there remains considerable scope for further research to deepen, broaden, and contextualize these insights, particularly concerning the mechanisms involved, the diversity of affected populations, and the long-term developmental consequences in low-resource settings.

First, future research could examine how the timing, duration, and intensity of environmental shocks interact with individual resilience factors, such as education, social capital, or prior health status, to shape psychological outcomes. Richer longitudinal data capturing post-disaster living conditions, access to recovery services, and social disruptions would allow for a bigger understanding of how individuals and communities adapt over time, and which groups are most vulnerable to prolonged distress. Such insights are crucial for designing targeted mental health interventions that respond to diverse needs in the aftermath of crises.

Second, the long-term developmental spillovers of early-life health interventions deserve further exploration. Future research could investigate how early gains in psychological well-being contribute to broader life-course outcomes, such as school performance, educational attainment, labor market integration, or the timing and quality of family formation. This would offer a more complete assessment of the benefits of public health investments and

help clarify whether early mental health improvements serve as pathways to enhance human capital accumulation and socioeconomic mobility.

Third, a critical but underexplored area involves the intergenerational transmission of trauma and resilience. Future research could investigate whether parents' exposure to environmental disasters or stressful policy environments, such as displacement, conflict, or economic shocks, affects their children's mental health and psychological development. These effects may operate through multiple channels, including heightened parental stress, shifts in parenting behavior, reduced household resources, or changes in expectations and aspirations. Understanding these pathways is essential to identifying long-term vulnerabilities and designing interventions that not only support affected individuals but also buffer the next generation from inherited psychological risks.

Fourth, given the growing intensity and frequency of climate-related disasters, future studies should move beyond historical cases to examine more recent and emerging exposures, including prolonged forest fires, urban flooding, extreme heat waves, or droughts. These events are likely to interact with existing inequalities and institutional weaknesses in new and complex ways. Research in this area could also assess the role of adaptive social protection systems, such as cash transfers, community health programs, and mobile mental health units, in mitigating psychological harm. Evaluating the effectiveness of such mechanisms would help inform climate-resilient policy frameworks that integrate mental health as a core pillar of disaster preparedness and response.

Finally, there is a pressing need to evaluate the effectiveness of community-based mental health interventions, particularly in low-resource settings. As mental health infrastructure remains limited in many developing countries, integrating psychological support into existing platforms, such as primary care clinics, maternal and child health programs, or school-based services, offers a promising path to expand access and improve continuity of care. Such integration could also help reduce stigma, normalize help-seeking behavior, and ensure that mental health becomes a routine component of general health service delivery, especially in geographically isolated or socioeconomically disadvantaged areas where needs are often greatest. Learning how these integrated models work in practice and what results they

deliver would be very useful for expanding mental health services that fit local needs.

Collectively, these future research directions are essential for advancing mental health as a core pillar of sustainable development. They highlight the urgency of inserting mental health considerations across a wide range of policy domains. Mental health is not a stand-alone issue but is closely connected to broader societal processes such as economic resilience, intergenerational mobility, and social cohesion. As such, addressing it effectively requires cross-sectoral collaboration that bridges health, education, environmental, and social policy scopes.

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