Reconstruction following Destruction: Entrepreneurship in the Aftermath of a Natural Disaster

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ABSTRACT

Entrepreneurship is thought to be the engine of growth in many developing countries. There is, however, a paucity of evidence on the role that entrepreneurship plays in rebuilding economic livelihoods both in the short and longer-term in the aftermath of a large-scale shock. This is an important gap in the literature given the increasing frequency and severity of shocks across the globe. This paper contributes to filling that gap by investigating the evolution of entrepreneurial success following the 2004 Indian Ocean tsunami, a large-scale and unexpected shock. Using longitudinal survey data, the Study of the Tsunami Aftermath and Recovery (STAR), I find large declines in business ownership, profits, and capital for those most exposed to the tsunami that persisted through 10 years following the tsunami. These estimates can be given a causal interpretation under the plausible assumption that exposure to the tsunami can be treated as exogenous after taking into account individual-specific unobserved heterogeneity with fixed effects, including pre-tsunami geographical features that drove exposure. Individuals living in rural areas and individuals with the least resources pre-tsunami fared the worst in terms of developing new businesses. However, the massive *Build Back Better* reconstruction program promoted entrepreneurship. Receipt of housing aid as part of that program is linked to an increase in the development of non-agricultural businesses that spurred gains in real profits.

JEL classification: D1; Q54; H84; L26

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I Introduction

The role of entrepreneurial activities has played a central role in the development economic literature since Schumpeter (1934), which argued that entrepreneurial success is pivotal to development and long-run economic growth. An understanding of entrepreneurship and firm development is particularly important in the aftermath of a large shock—financial, disease-related, or environmental—that creates massive upheaval to economic systems and requires policymakers to facilitate recovery. Shocks such as the 2008 Global Financial Crisis, COVID-19 Pandemic, or Hurricane Katrina destroy businesses and their capital (Belitski et al., 2022; Kennickell et al., 2015), and building an understanding of how individuals respond to exposure to such shocks is a question of critical importance.

An alarming consequence of climate change is that the burden of natural disasters and environmental shocks is increasing in both their frequency and intensity (Stocker et al., 2013). Beyond their massive humanitarian consequences, large natural disasters such as the February 2023 earthquake in Turkey and Syria, the 2005 Hurricane Katrina, and the 2010 Haitian earthquake destroy infrastructure and businesses alike (Cavallo et al., 2010; Schrank et al., 2013)—limiting opportunities for the survivors to begin their economic recovery and potentially leaving them adversely affected through the long-term. The destruction of the business environment is markedly important in developing economies: business ownership in the form of small own-account enterprises (OAEs) is ubiquitous as safety nets are precarious (Gindling and Newhouse, 2014; Jayachandran, 2020). Consequently, the prevalence of own-account work and the extent to which it provides resources for families in the aftermath of a major shock is an important policy and research question. To best support economic reconstruction efforts, policymakers need to be aware of how the survivors responded to the new economic landscape and how post-disaster relief efforts impacted business development.

Despite the importance of entrepreneurship, it has escaped thorough investigation in other studies on how individuals respond to large shocks. This is due to two challenges: endogeneity of disaster exposure and data limitations. There is limited causal evidence of the effects of Hurricane Katrina or the 2008 Financial Crisis as there was scope for mitigation behavior and anticipation of the shock. Consequently, it is difficult to identify the effect of exposure to the shock as the exposed and non-exposed groups differ systematically (Baker, 1991). Beyond endogeneity concerns, estimation of meaningful exposure effects requires a sample that is representative of the at-risk population, prior to the event. Then, the study must follow the at-risk population over the short- and long-term after the shock. However, longitudinal data in the aftermath of a disaster tend to suffer from attrition issues as the shock can generate a large migration response, making tracking and re-interviewing the displaced individuals challenging (Gray et al., 2014).

In this work, I overcome both traditional difficulties of disaster research and study how individual decisions regarding entrepreneurial activities are shaped by exposure to an unanticipated and large-scale environmental shock: the 2004 Indian Ocean tsunami. This disaster killed over 160,000 people in Aceh, Indonesia, and resulted in substantial societal and economic damages (Frankenberg et al., 2011; Masyrafah and McKeon, 2008). However, in the years following the disaster, there was an enormous inflow of international relief aid as the *Build Back Better* reconstruction program sought to rebuild and improve upon the damaged communities. Given this context, the first half of this study seeks to investigate how entrepreneurial outcomes evolved in the aftermath of the tsunami. The second half investigates how the later inflow of reconstruction aid impacted their entrepreneurial decisions.

This work draws on uniquely rich population-representative longitudinal survey data collected as part of the Study of the Tsunami Aftermath and Recovery (STAR). 16,000 adult respondents were interviewed 10 months before the 2004 Indian Ocean tsunami, and respondents were tracked and re-interviewed throughout the 10 years following the tsunami. The remarkable tracking efforts of the STAR field team led to 98% of the tsunami survivors being interviewed at least once. This includes tracking and re-interviewing individuals who migrated outside of the study area, including to other provinces in Indonesia. Thus, I credibly estimate disaster exposure effects through the long term.

The argument for the exogeneity of disaster exposure in this study is similarly well-founded. The lack of an early warning system combined with the unexpected nature of the tsunami implies that its timing could not have been anticipated (Frankenberg et al., 2016). Further, it has been documented that within small groups of land, exposure to the heaviest degrees of tsunami damage was a result of the interaction between geographic features of the land and ocean floor (water depth, elevation, coastal topography) and wave characteristics including its direction and speed (Frankenberg et al., 2011). By employing an individual fixed effects strategy, I capture pre-existing differences across these geographical features, and thus the remaining variation in exposure is driven by the interaction of topography and tsunami wave features. This yields a credible argument that tsunami exposure is unrelated to the remaining individual heterogeneity. The individual fixed effects strategy also accounts for other time-invariant individual-specific factors such as pre-existing propensity to own a business, levels of resilience, entrepreneurial skill, and access to resources and support networks that would be complicated to control for without this design and render the estimates difficult to interpret. Thus, a key contribution of this research is that I address the endogeneity and data challenges faced in the disaster exposure literature by using STAR data in the aftermath of the 2004 tsunami with an individual fixed effects approach (Henry et al., 2020; Lawton et al., 2022; Noy and duPont IV, 2018; Schumacher and Strobl, 2011). I use this approach to investigate individual entrepreneurial outcomes, which to the best of our knowledge, have not been documented in prior natural disaster studies despite its prevalence in low-income nations and its importance to supporting family consumption.

Ex ante, it is unclear how exposure to natural disasters will impact own-account work. The loss of livelihood and capital pushed existing entrepreneurs to seek wage labor, but the simultaneous destruction of infrastructure and wage jobs may have pushed wage laborers to entrepreneurship out of necessity. In the long term, this dynamic complicates further as reconstruction aid flows into damaged areas, and individuals migrate elsewhere to seek new opportunities. Accordingly, this study contributes to the literature attempting to understand what drives entrepreneurial decisions in developing countries by disentangling these competing forces via empirical analysis to understand how individuals responded to the changing economic landscape (Gindling and Newhouse, 2014; Jayachandran, 2020).

This paper finds that exposure to the heaviest levels of tsunami damage reduced business ownership by 6pp through 12pp during the decade after the tsunami, relative to the control group. Compared to a pre-tsunami business ownership rate of 66%, this exposure effect is a substantial and lasting decline that impacted agriculture and non-agriculture businesses alike. Businesses operated by those from the most damaged areas experienced a reduction in their capital holdings of 60-80% and produced 25-60% less in real profits through the short- and long-term. The damage was concentrated in rural areas, where agricultural businesses suffered large and persistent declines.

Seawater and salt inundation into agricultural fields from tsunami waves destroyed productive land in the most affected areas, and thus much of this damage is likely to persist beyond the study period. Those with the greatest levels of consumption in the pre-tsunami baseline were resistant to the damages on business ownership and profits in the short- and long-term. Finally, I investigate the joint dynamics of entrepreneurial decisions and post-tsunami displacement. I find that those from heavily damaged areas that were displaced by the tsunami faced worse entrepreneurial outcomes, particularly in the immediate aftermath of the disaster.

The massive *Build Back Better* reconstruction program in the years following the tsunami—the largest reconstruction program ever undertaken in a developing country—may have played a role in the medium- and long-term entrepreneurial trajectories of exposed individuals. Thus, in the second half of the paper, I study how international relief aid impacted the post-tsunami entrepreneurial decisions of the beneficiaries.

This investigation is complicated by the possibility that aid receipt in the aftermath of a shock is endogenous. For a stronger exogeneity argument, I focus on housing aid as households were eligible for housing aid based on their location of residence before the tsunami, largely due to pressure from donors to re-build quickly as well as to avoid people moving to damaged areas in hopes to receive aid (Laurito et al., 2022; McCaughey et al., 2018, 2017). This limits the role of endogenous out-migration responses in driving housing aid receipt, as both the people that stayed and left were eligible for housing aid. Moreover, I estimate over the post-tsunami period and use an individual fixed effects estimation strategy to account for time-invariant features in the tsunami aftermath that impacted aid receipt, including the damage an individual faced due to the tsunami, initial damages to the road or infrastructure that impacted the timing in which housing aid could be delivered, and pre-existing levels of resources or exposure to social networks. The remaining variation is plausibly driven by the complexities associated with the aid disbursement process, which is unlikely to be related to the remaining individual heterogeneity. Consequently, a key contribution of this study is overcoming the endogeneity of aid receipt issues that can make interpretation of studies on post-disaster aid effects difficult (Kamel and Loukaitou-Sideris, 2004; Laurito et al., 2022; McCaughey et al., 2018; Sadiqi et al., 2012).

Via an event study estimation strategy that allows for heterogeneous treatment effects across individuals and over time (Callaway and Sant'Anna, 2021), I find that the receipt of housing aid led to increased non-agricultural business ownership through the long-term that brought gains in real profits, without increasing business capital holdings. Housing aid reduced the immediate decline in non-agricultural business ownership to those facing heavy tsunami damages by 40% to 70% across the sample period—a large impact that persisted years after receiving the aid. Thus, while there were major declines in entrepreneurial outcomes for those most exposed to the tsunami, reconstruction efforts mitigated some of the adverse effects in the medium- and long-term. This contributes to the literature that seeks to understand how policymakers can increase entrepreneurial activities in developing economies by investigating this question in the aftermath of a disaster (de Mel et al., 2014; Jayachandran, 2020; Quinn and Woodruff, 2019)—a particularly important context given the widespread economic and societal destruction generated by exposure to the shock.

The rest of this paper is organized as follows. Section II further discusses the prior literature on natural disaster effects and entrepreneurship in development economics. Section III discusses the natural experiment and STAR data. Section IV introduces the empirical strategy employed to estimate the effects of tsunami exposure. Section V presents the results on the effect of disaster exposure on subsequent entrepreneurial activities. Section VI describes the reconstruction aid response as well as the empirical strategy and results of estimating how housing aid impacts entrepreneurial propensities. Section VII concludes.

II Disaster and Entrepreneurship

Prior economic research on natural disasters has generally focused on short-term effects. These studies find that disaster exposure leads to a substantial increase in mortality and injury, loss of property, and decline in human capital levels (Anttila-Hughes and Hsiang, 2013; Cas et al., 2014; Guimaraes et al., 1993; Noy and duPont IV, 2018; Schumacher and Strobl, 2011). However, the long-term effects of exposure to environmental shocks are much less understood. Given the inflow of reconstruction aid and migration responses of the surviving population through the medium- and long-term, the effect of disaster exposure over the long run may differ substantially from immediate effects.

To understand the dynamics over a longer time horizon, macroeconomic empirical studies have investigated the economic output of exposed communities in the subsequent years after a disaster

(Cavallo et al., 2013; Hsiang and Jina, 2014). These studies find moderately negative or null effects of disaster exposure in the long-term. In particular, Heger and Neumayer (2019) argued that in the aftermath of the 2004 tsunami, regions of Aceh, Indonesia that experienced greater levels of flooding had greater long-term GDP growth, proxied by nightlights. Interpretation of their results is not obvious. It has been documented in this setting that the link between nightlights and economic activity in the aftermath of the 2004 tsunami is, at best, weak (Gillespie et al., 2014). Moreover, macroeconomic evidence cannot separate the effect of individuals migrating into and out of damaged areas, and so cannot capture how the exposed individuals responded in the long-term. To investigate how disasters impacted the people—rather than the areas—exposed to the disaster, population-representative pre-disaster data at the individual level is necessary. The importance of this distinction is apparent in Lawton et al. (2022), which documents the impact of exposure to the 2004 tsunami on consumption. In contrast to Heger and Neumayer (2019), they found that those most exposed to the damages reported lower levels of real consumption and that they failed to recover consumption levels through the long-term.

In particular, there is a lack of research on the role of entrepreneurship in rebuilding economic livelihoods after an environmental shock in the long-term. The results of Heger and Neumayer (2019) that long-term economic growth was greater for exposed regions suggest positive effects on firm growth. In other disasters, Gitter et al. (2007) find that Hurricane Mitch destroyed business capital in rural Honduras, and the adverse capital effect lasted for at least 3 years. However, Jakobsen (2012) found that Hurricane Mitch did not have an impact on productive assets for a population from rural Nicaragua. Consequently, there lacks a clear answer in the literature as to how entrepreneurship among exposed individuals evolved over the long term.

Beyond natural disaster studies, entrepreneurship and firm growth itself remain important and poorly understood topics in development economics. The interest in this question originates from seminal work by Schumpeter (1934)—arguing that sustaining a strong environment for entrepreneurial success incentivizes innovation and technological progress to facilitate development and long-term growth. Empirically, prior work has found that success in the self-employment sector translates into gains for the local economy; Glaeser et al. (2015) document a plausibly causal link where increased levels of entrepreneurship spur subsequent increases in city employment growth.

While a large body of literature investigates entrepreneurship in the U.S. or Europe (See Andrews et al. (2022) for a review), the substantial differences in self-employment between low- and high-income countries necessitate separate study. An oft-discussed dynamic of self-employment is whether individuals become entrepreneurs out of "necessity" or as a "calling". Some take up entrepreneurship as they cannot find wage labor, while others turn to it as they expect higher returns in self-employment than wage labor (Poschke, 2013). In developing nations, the weaker supply of wage labor opportunities forces a greater share of the workforce into entrepreneurship out of necessity than is the case in developed economies. Consequently, one-third of workers in low- and middle-income countries are self-employed whereas fewer than a tenth are self-employed in high-income countries (Gindling and Newhouse, 2014). Approximately 90% of these businesses have no employees aside from the owner (Jayachandran, 2020).

Studies in psychology and economics seek to understand the individuals that turn to self-employment in developed countries. In psychology, prior work has found that personality features such as being risk-taking and achievement-motivated are linked with increased probabilities of entrepreneurship (Brockhaus, 1980; Heinrichs and Walter, 2013; McClelland, 1965). Ingwersen et al. (2022) find that survivors exposed to the greatest levels of damage after the 2004 Indian Ocean tsunami are more willing to take on financial risks in hypothetical games. These patterns are also reflected in the behavioral choices of the respondents who are more willing to take on risks, suggesting that exposed individuals may have been more inclined towards entrepreneurship following the disaster.

The second part of this study investigates whether the flow of reconstruction aid into damaged areas supported the establishment of businesses. This question is closely tied to the traditional development entrepreneurial studies that investigate what factors can propel small businesses to grow, typically using experimental evidence.

A consistent pattern in low- and middle-income countries is the "Missing Middle" puzzle—there are many small firms and some large firms in poor countries, but very few mid-sized firms (Krueger, 2012). The small firms fail to grow. In an effort to explain this bimodal distribution of firm size, Hsieh and Olken (2014) show that small firms have lower levels of productivity relative to larger firms, suggesting that the lack of mid-sized firms is not due to differential capital or labor constraints between small and large firms.[1] In line with this finding, Karlan et al. (2012) show via an RCT that providing these smaller firms capital and managerial support fails to influence firm growth in the longer term. Conversely, Banerjee et al. (2015) find that offering a random set of neighborhoods access to lending services led to increased take-up of microcredit and increased small business investment. Field et al. (2013) find that the effects of these microfinance programs vary with the contract details for repayment: offering a grace period before loan re-payment raised investment and profits but also increased default rates.

III Setting and Data

III.A 2004 Indian Ocean Tsunami and *Build Back Better*

This work studies the surviving population in the Indonesian provinces of Aceh and North Sumatra following the devastating 2004 Indian Ocean tsunami. On December 26, 2004, a massive earthquake in the Indian Ocean propelled tsunami waves up to 30m high into the Aceh shoreline. The earthquake measured 9.1 on the Richter scale: the largest measured magnitude in the 21st century. Tsunami waves struck the shore within 15 minutes of the earthquake, leaving no time for the exposed population to escape.

The mortality consequences of the disaster were staggering. In the Aceh province, over 160,000 people died (5% of the province's population). Children, older adults, and females all faced higher probabilities of death from the tsunami. However, socio-economic features were weakly correlated with mortality (Frankenberg et al., 2011). The fact that gender, age, and pre-tsunami household composition were strong correlates of survival probabilities while SES features were not is consistent with a model presented by Yeh (2010) that physical strength and stamina are relevant in reducing tsunami mortality given the flow characteristics of the water.[2]

[1] Prior to this work, many argued that a lack of access to financial capital or other constraints explains why small firms stay small. However, if the high marginal cost of capital for small firms was the reason why they fail to grow, theory would predict a high marginal product of capital for the limited quantities they have. Instead, empirical analysis finds the opposite—that smaller firms are less productive. Instead, the authors argue that the lack of mid-sized firms is due to the high regulation costs in low-income countries placed on mid- and large-sized firms, which are insurmountable to small firms.

[2] This result also reduces concerns of mortality selection driving the results of this work. Mortality was predicted most strongly by pre-tsunami household composition as well as strength and stamina features of the individual, which will be absorbed in the following individual fixed effects estimates.

Beyond mortality effects, the earthquake and tsunami caused large economic damages to the impacted communities. Total damages from the tsunami exceeded 80% of the Aceh province's 2003 GDP (Masyrafah and McKeon, 2008). Data from the Community and Facility component of the STAR Survey found that the communities most exposed to the tsunami faced a heightened need to clear debris and rebuild homes, village halls, mosques, schools, and roads (Frankenberg et al., 2012). 60% of the most exposed communities stated that tsunami damages caused concerns over setting property rights among their residents.

As for the surviving individuals from the disaster, an estimated 700,000 people were displaced by the tsunami. Displaced groups moved to temporary housing in the form of tents or refugee camps, semi-permanent transitional shelters, or stayed with friends and family in less impacted areas. Prior research has shown that the tsunami had a lasting impact on a wide set of humanitarian and economic outcomes for those living in coastal areas near the Indian Ocean (Frankenberg et al., 2016; Lawton et al., 2022).

The possibility of exposure to a tsunami that could uproot their current livelihood with no notice was not the result of conscious choices made by the inhabitants of the affected areas. While earthquakes are more common in Indonesia, there had not been a major tsunami in mainland Aceh in over 600 years (Monecke et al., 2008). Within *kecamatan* (sub-district, 2 levels below province), exposure to the tsunami was driven by the interaction between wave features (speed, height, direction) and geographic features of the land and ocean floor, including water depth, elevation, vegetation, and coastal topography (Frankenberg et al., 2011; Kohl et al., 2005; Ramakrishnan et al., 2005; Umitsu et al., 2007). The geographical features protected some coastline communities but led others to face devastation. Moreover, there was no effective early warning system in place to notify the vulnerable populations of the impending tsunami (Cas et al., 2014). Consequently, unlike many other natural disasters where exposure is an outcome of socioeconomic factors and behavioral responses to avoid exposure (Baker, 1991), I argue that variation in tsunami exposure in this context is driven by idiosyncratic features of the landscape and disaster itself and can be plausibly treated as exogenous after accounting for basic differences across areas.

Following the destruction, international emergency aid and assistance funds poured into the damaged areas to begin reconstruction efforts. Approximately USD 7.7 billion in relief assistance flowed into the region from the Indonesian government, NGOs, and bi- and multi-lateral donors.

By 2007, efforts to *Build Back Better* by re-building and improving upon the damaged communities constituted the largest reconstruction project ever undertaken in a developing country.

The reconstruction process in Aceh roughly followed three phases. First, emergency assistance programs began immediately after the tsunami, intended to provide relief for basic needs such as food, shelter, and clean water (Jayasuriya et al., 2010). Then, roughly 5 months after the tsunami, the second phase was initiated which prioritized the reconstruction and development of housing, health, transportation, sanitation, and other programs designed to restore the livelihood of the survivors (Daly et al., 2016). The final phase of reconstruction began approximately 2.5 years after the tsunami and strove to rebuild the economy and government of the impacted areas (Laurito et al., 2022).

Given the complexity involved in aid disbursement, the Government of Indonesia created the Agency for Reconstruction and Rehabilitation (BRR - *Badan Rehabilitasi dan Rekonstruksi*) to assist in coordinating and monitoring the aid procurement process (Daly et al., 2012). Concurrently, the creation of the World Bank-managed multi-donor trust fund (MDF) pooled funds across donors to further simplify the aid disbursement process and reduce coordination burdens (Masyrafah and McKeon, 2008). Ultimately, over 400 independent donor organizations contributed to the reconstruction process, including the Government of Indonesia, NGOs, and bilateral and multilateral donors. Approximately USD 7.7 billion was committed to the reconstruction of Aceh—1.5 billion in excess of the projected minimum needed to rebuild to pre-tsunami levels. For typical large disasters, an average of 10% of damage costs are compensated for by relief aid (Freeman et al., 2002). In Aceh after the 2004 tsunami, the compensation received was an outstanding 150% of damage costs (Heger and Neumayer, 2019). Aid disbursement in this final phase of reconstruction was swift with over 65% of aid promises being disbursed by December 2007. This massive aid response to the devastation led to hopes for the possibility that Aceh could *Build Back Better* and improve upon its existing communities prior to the tsunami.

In short, this work studies how individuals respond to two related shocks. First, individuals suffered from an unanticipated and wide-reaching wave of destruction from the 2004 Indian Ocean tsunami. Then, in the years following the tsunami, many of the same individuals, and potentially new migrants, benefited from a massive influx of emergency aid and assistance for reconstruction in the impacted communities.

III.B Study of the Tsunami Aftermath and Recovery

To understand how individuals responded to this shock through the long-term, this study uses data from the Study of the Tsunami Aftermath and Recovery (STAR). STAR is an ongoing population-representative longitudinal survey from Aceh and North Sumatra, Indonesia that collected survey data from before and after the 2004 tsunami.

The post-tsunami waves of STAR were designed to provide novel evidence on the short- and long-term effects of the 2004 Indian Ocean tsunami on the surviving population. To do so, STAR was adapted from the 2004 SUSENAS—a population-representative survey conducted by Statistics Indonesia as part of a national socio-economic survey. The 2004 SUSENAS interviewed approximately 27,000 respondents in 14 *kabupaten* (districts, 1 geographic level below provinces) along the coast of Aceh and North Sumatra in February and March 2004, 9-10 months before the tsunami. After the tsunami, the STAR field team followed up with approximately 16,000 surviving adult SUSENAS respondents.[3] This paper uses 7 survey waves of data: the baseline pre-tsunami wave, 5 annual follow-ups in the first 5 years following the tsunami, and a 10-year follow-up.

The STAR survey contains a host of questions on the humanitarian and economic well-being of the surviving population. The Household and Individual component of the survey asks about agricultural and non-agricultural businesses, assets, income, consumption, and education for individual respondents and their households. STAR also includes a Community and Facility section, where within each study site, an expert community informant provides extensive information on infrastructure, credit markets, transportation, and other relevant features of their community. The community informant is often the leader of the *desa* (village, the lowest administrative level in Indonesia, 2 levels below *kabupaten*). Then, the field team interviews the schools, healthcare facilities, and markets that the respondents of the household survey said that they visit.

Aside from the natural research setting of the study, two features of the dataset make it ideal for studying the effects of natural disasters. First, the study follows up with the same respondents from a population-representative pre-tsunami survey. As STAR tracks those that migrated outside of the

[3] Approximately 2,000 of the 27,000 initial respondents interviewed died prior to the 1-year follow-up—almost entirely due to the tsunami. All surviving respondents among the 27,000 from 2004 SUSENAS were eligible for tracking and re-interview. The analysis sample in this study is limited to those that were adults at the pre-tsunami baseline, yielding approximately 16,000 surviving adults.

initial study site, attrition rates are extraordinarily low. The careful efforts of the field team ensured that over 98% of the baseline respondents that survived the tsunami were interviewed in at least one follow-up survey. This includes tracking individuals who moved to other parts of Sumatra, Java, and other islands in Indonesia as well as movers to Malaysia and Singapore. Focused tracking efforts in this context are especially important given the large migration response triggered by the tsunami. As the post-tsunami migration response is related to the determinants of our outcome of interest, causal claims in a setting where migrants are not followed are highly likely to yield biased estimates (Thomas et al., 2001).

The second unique strength of STAR is the existence of local price data in the Community and Facility component. Local price data allows the analysis to consider differential price movements across areas that faced different exposure levels to the disaster. Prior work has shown that prices vary systematically between exposed and unexposed areas following a disaster (Del Ninno et al., 2003; Hallegatte and Przyluski, 2010; Kirchberger, 2017). Consequently, this analysis exploits this unique data by using local price indices to deflate values to price-adjusted terms when appropriate.[4]

The purpose of this study is to investigate the evolution of entrepreneurial and business activities. Consequently, I draw heavily from the STAR Household survey components on household agricultural businesses, non-agricultural businesses, and business assets.

The agricultural and non-agricultural business sections ask households whether they operated a business, and the profits of their business if operated.[5] As rice is the main crop planted by farmers in Aceh, the agricultural business section separately asks about rice and non-rice agricultural businesses. I define a household as owning a business in a given wave if they report planting any agricultural crops or owning a non-agricultural business in the calendar year associated with this wave.[6] Agricultural businesses are largely made up of rice production, but non-rice

[4]Construction of the local price index series is described in the Appendix.

[5]To reduce missing values in profit, if a household did not recall its profits, interviewers follow a two-stage bracketing procedure to estimate their revenues and costs in that wave. I use this bracketing procedure to limit the number of missing profit values.

[6]For example, a household owned a business in the pre-tsunami wave if they report operating a business between Jan.-Dec. 2004. The years associated with each wave are 2004 for the pre-tsunami baseline wave, 2005 for the 1-year follow-up, 2006 for the 2-year follow-up, 2007 for the 3-year follow-up, 2008 for the 4-year follow-up, 2009 for the 5-year follow-up, and 2014 for the 10-year follow-up. Defining the timing in this way addresses concerns of seasonality because agriculture businesses tend to be conducted year-round—Aceh is on the equator. For example, there are three cropping seasons in a year for rice, the dominant crop in the study area. Further, the inclusion of month of interview fixed effects addresses concerns of recall error. Profit values are winsorized at the 0.5% level.

products include rubber plantations, chocolate, palm oil, chili peppers, clove, nutmeg, and peanuts. Non-agricultural businesses include local merchants, fishermen, seamstresses, chefs, and bakers.

The pre-tsunami baseline survey only included the rice agricultural business component, but the 1-year follow-up survey asked respondents about their full business ownership status and profits before the tsunami to supplement these initial responses. Consequently, I impute the remaining pre-tsunami business outcomes using responses from the 1-year follow-up wave.[7] The resulting business outcomes are measured at the household level for each wave. However, to avoid concerns of systematic household composition changes, we assign each business outcome to all adult individuals in that household during that survey wave.[8]

The business asset section asks each household a detailed set of questions regarding their ownership status and value of seven assets intended to support their business activities.[9] The pre-tsunami baseline survey did not include the business asset section, but the 1- and 2-year follow-up surveys retrospectively asked about asset holdings before the tsunami which I use to create baseline measurements. To correct for measurement error in self-reported asset value measurements, I flexibly estimate the relationship between asset value and household consumption and use this estimate to identify extreme outliers.[10] As before, the business asset data is measured at the household level, so I assign the results to all adult members of the household in that wave.

Note that this individual-specific data allows for investigation of the small-scale enterprises that make up much of the business activity in development economics. In empirical studies at higher levels of aggregation, aggregate production is heavily driven by the production of large firms and thus it is unclear how smaller firms evolved in the aftermath. However, approximately 90% of businesses in poorer nations have no employees aside from the owner (Jayachandran, 2020), and thus own account work plays a key role in individual and family economic well-being. Own account

[7] This may introduce concerns of measurement error in retrospective estimates. To the extent that this measurement error is selective on time-invariant individual-specific features such as pre-existing cognition levels or birth year, it will be absorbed in the individual fixed effects strategy I employ throughout.

[8] When households split or merge, the concept of tracking a household over time is unclear. In the post-disaster context that I study, many households merged to benefit from economies of scale in basic living expenses, or out of necessity given the destruction of prior housing. Similarly, households split as some members migrated out of damaged areas. These dynamics are surely related to the degree of exposure these households faced, so ignoring these household compositional changes is likely to contaminate the results. Assigning individuals to the outcome of the household that they are a member of in this wave addresses these concerns.

[9] The 7 business assets are wet land, dry land, livestock, buildings, machinery / equipment, transportation, other business assets.

[10] See Lombardo et al. (2022) for full details on how asset value outcomes were cleaned.

enterprises are often unobserved in data at higher levels of aggregation, meaning that any policy conclusions on firm behavior would fail to consider how the majority of firms are impacted.

To measure the effects of tsunami exposure, I stratify individuals into two exposure groups: those from communities that were heavily damaged by the tsunami, and those from communities that were not heavily damaged. This exposure measure is estimated largely via satellite imagery data intended to capture the effects of tsunami devastation on the land.[11] In the sample, individuals were from 407 communities in Aceh and North Sumatra at baseline. This classification system considers 96 of their communities to have faced heavy damage and 311 to have not faced heavy tsunami damage. Importantly, an individual's exposure level is fixed over time: if an individual was in an exposed area during the tsunami but then migrated to an area that was not damaged, I still consider them exposed to the tsunami. This eliminates endogenous out-migration responses away from the heavily damaged areas and properly estimates how entrepreneurial outcomes evolved for the people, rather than the areas, most impacted by the disaster.

In this study, I define the analysis sample to be the set of individuals that were respondents of the pre-tsunami baseline and aged 15+ during the pre-tsunami STAR baseline. Moreover, I restrict the sample to those with pre-tsunami and at least one wave of post-tsunami data. This yields 15,853 individuals across 7 survey waves.[12] Summary statistics of these individuals in the pre-tsunami baseline wave are displayed in Table 1. Note that Columns 2 and 3 split these values by exposure level. In aggregate, we observe mean pre-tsunami differences between the exposed and non-exposed groups of our study. However, as we will show below, when conditioning for *kecamatan*, elevation, and distance to the coastline, these differences are statistically insignificant and small in magnitude.

IV Empirical Strategy

This paper seeks to explore how an exogenous environmental shock impacts individual-level entrepreneurial work. Let T_i be an indicator for tsunami exposure, where $T_i = 1$ indicates that

[11] See the Appendix for full details on the calculation of exposure measure.

[12] 12,658 individuals appeared in every wave; 1,735 appeared in all but one wave. I do not impose an upper age limit to the sample as the share of elderly individuals is very small. Only 4.9% of the analysis sample is aged 65+ at the pre-tsunami baseline.

Table 1: Baseline Summary Statistics

	Full Sample	Tsunami Damage Exposure	
		Not Heavy	Heavy
Descriptive Features			
Male	0.49	0.48	0.51
Age	35.50	35.79	34.43
Years of Education	8.21	7.87	9.46
Married	0.35	0.34	0.39
Urban	0.29	0.20	0.59
Number of Males in Household 15+	1.83	1.79	1.95
Number of Females in Household 15+	1.91	1.87	2.04
Number of Children 0-14 in Household	1.52	1.55	1.39
Real Monthly Per-Capita Expenditure ('000s Rp)	384	367	450
Individual Real Wealth ('000s Rp)	27,321	23,401	41,645
Business Outcomes			
Household Operates Business?	0.66	0.68	0.61
Rice Business	0.28	0.32	0.16
Non-Rice Agriculture Business	0.28	0.30	0.24
Non-Agriculture Business	0.26	0.23	0.37
Real Household Business Profits ('000s Rp)	4,656	3,995	7,072
Rice Business	329	408	39
Non-Rice Agriculture Business	1,039	1,145	652
Non-Agriculture Business	3,395	2,489	6,704
Real Household Business Assets ('000s Rp)	6,893	6,196	9,440
Tsunami Exposure			
Mortality	0.07	0.01	0.27
Tsunami-Related Injury? (Among survivors)	0.02	0.01	0.07
Tsunami Damaged Home?	0.39	0.33	0.61
Number of People	15,853	12,866	2,987
Share Exposed		0.79	0.21

Notes. This table displays summary statistics for key variables in the baseline survey wave, adapted from the 2004 SUSENAS. Column 1 reports the mean across the full analysis sample, while Columns 2 and 3 partition the population based on their exposure to tsunami damages. The analysis sample is limited to original adult respondents of the 2004 SUSENAS that appeared in at least one post-tsunami wave. Reported profits are summed over the 2004 calendar year. Tsunami mortality is computed using the full set of 2004 SUSENAS respondents. Sample weights from 2004 SUSENAS are used.

individual i was in a community exposed to heavy tsunami damages at the pre-tsunami baseline.

As discussed above, tsunami exposure within *kecamatan* is a function of idiosyncratic land and ocean floor features such as water depth, distance to the coastline, and elevation. Exploiting the panel nature of the survey, I capture baseline differences across these pre-tsunami geographical features using an individual fixed effects. Individual fixed effects also capture pre-tsunami household composition, strength, and stamina which most strongly predict tsunami mortality

(Frankenberg et al., 2011), reducing concerns about mortality selection driving our results.

Beyond the controls necessary for exogeneity, individual fixed effects account for other individual features such as pre-existing entrepreneurial skill or access to resources and credit that would render the estimates difficult to interpret. Sweeping out all such factors allows for estimation of how tsunami exposure shaped individual-specific trajectories in entrepreneurship. Consequently, I estimate the following regression specification:

$$y_{it} = \beta_t T_i + \lambda_i + \xi_t + \gamma W_{it} + u_{it} \tag{1}$$

Where y_{it} is the entrepreneurial outcome of interest, λ_i, ξ_t are individual and wave fixed effects, respectively, W_{it} is a vector of controls, and u_{it} are remaining unobserved determinants of y_{it}. To address seasonality and recall error in business outcomes, W_{it} is a vector of interview month indicators. Note that T_i is time-invariant, or that an individual's exposure level to the tsunami is determined at the time of the tsunami and is, therefore, constant through the study period.[13] Post-tsunami migration is not modeled in this framework for two reasons. First, it is likely endogenous, whereas exposure to the tsunami is not. Second, it is not clear that there are feasible empirical strategies with these data to credibly identify the effect of migration. I discuss the joint determination of migration choices and business outcomes in subsection V.D below. To trace out the time-varying responses to exposure to the tsunami, T_i, β_t is allowed to vary over the entire post-tsunami follow-up period, thereby fully exploiting the long-term longitudinal study design. Standard errors are clustered at the individual-level.

I focus on 5 outcomes that track individual entrepreneurship: ownership of any business, ownership of an agricultural business, ownership of a non-agricultural business, the square root of total business assets, and the square root of total profits of all businesses.[14] Nominal values in

[13] As there is only one pre-treatment wave and I use individual fixed effects, the regression results are mathematically equivalent to setting $T_{it} = 0$ for $t = 0$, or setting exposure to 0 for all individuals in the pre-tsunami period. For notational clarity that exposure is absorbing following the tsunami, I simply use T_i throughout. The coefficient on T_i itself is absorbed by the individual fixed effect, but our coefficient of interest is β_t, the coefficient on the interaction between T_i and an indicator for wave t. Note further that in some research using STAR, exposure is split into three categories: none, moderate, and heavy. In my setting, entrepreneurial trajectories for the groups experiencing no tsunami damage and moderate tsunami damage were similar, so I pooled those facing no tsunami damage and moderate tsunami damage for the control group to increase precision.

[14] As discussed above, I set the business outcome variables to be within the 12-month window associated with the calendar year of this wave. Note that square root transformation is used to address zeroes in business assets and profits for those not owning a business—I choose this instead of inverse hyperbolic since (IHS) or log(1+x) as those

business assets and total profits are adjusted using local price indices at the *kecamatan*-exposure level, as discussed in Section III.B and the Appendix.

For each outcome, the coefficient of interest is β_t, which traces out the effects of exposure to tsunami destruction on the post-tsunami evolution of entrepreneurial outcomes. For β_t to estimate a causal effect, I assume that $\mathbb{E}[T_i u_{it} | \lambda_i] = 0$, or that conditional on time-invariant individual features, exposure is unrelated to unobserved determinants of entrepreneurial outcomes. Notably, this conditions on geographical features such as the elevation and distance to the coastline for individuals in their pre-tsunami community. Argued above, the remaining variation in exposure is driven by idiosyncratic features of the land and seafloor that are plausibly unrelated to the unobserved heterogeneity. I offer further empirical evidence supporting this assumption in Section V.A below.

In this work, β_t estimates how those from areas heavily exposed to tsunami damages fared, relative to those from areas that faced less damage. However, it is important to consider that the counterfactual in our analysis did experience minor levels of damage. The Sumatra–Andaman earthquake that triggered the 2004 Indian Ocean tsunami was the largest in magnitude over the 21st century—its effects were ubiquitous across our sample. The exposure measure used throughout is designed to track the tsunami damages, including water inundation, which yielded the most devastating effects. This is perhaps most clear when comparing tsunami mortality rates across communities using the exposure measure: tsunami mortality is less than 1% for control communities but 27% for communities facing heavy tsunami damage. That said, the control group experienced some levels of community infrastructure damage, especially on roads, that impacted entrepreneurial decisions. Thus, I frequently report estimates of β_t, which measures the effects relative to the control group in that wave, as well as the pre-tsunami level of the outcome for a reference that is not contaminated by the sweeping effects of the Sumatra–Andaman earthquake and 2004 Indian Ocean tsunami.

Prior to estimation, the trade-offs between wage labor and self-employment imply that the sign and magnitude of β_t are ambiguous. Immediately after the tsunami, the loss of livelihood and capital destroyed micro-enterprises and likely forced existing entrepreneurs to seek wage work as

transformations can yield very different results depending on arbitrary choices in the unit of measurement (Aihounton and Henningsen, 2021; Chen and Roth, 2022). That said, I find similar results using IHS, $\log(1 + y)$, or quartic root transformations.

they could no longer sustain their livelihood. On the other hand, the destruction of infrastructure and loss of wage jobs forced many into entrepreneurship out of necessity. The changing dynamics associated with wage employment and self-employment suggest the potential for re-sorting; those initially employed by others may be pushed into self-employment while those initially operating a business may be pushed into wage work. Over the longer term where the heavily damaged areas benefit from increased levels of aid and a promising reconstruction effort, new opportunities arise in both wage labor and entrepreneurship, yielding ambiguous effects in aggregate. Migration responses of the individual as well as how the populations of their local communities were shifted by the tsunami further complicate these dynamics. Consequently, empirical analysis is well-suited to investigate these theoretical uncertainties and understand how recovering individuals responded to the tsunami destruction through the long-term.

Recent work in the econometric literature finds that, in general, two-way fixed effects estimators such as Equation 1 may yield biased estimates of the ATT in settings with plausibly heterogeneous treatment effects (See de Chaisemartin and D'Haultfoeuille (2022) for a review of recent studies). Fortunately, estimates are unbiased for the ATT under only a parallel trends assumption when treatment is staggered, binary, and has no variation in its timing (de Chaisemartin and D'Haultfœuille, 2020). All three conditions hold in this setting, so estimating Equation 1 via standard regression methods yields unbiased estimates of β_t if exogeneity holds, even allowing for the treatment effect to vary across individuals and over time.[15]

After estimating the main specification, I then turn to heterogeneity analysis. The existing development literature on entrepreneurship and programs designed to support firm growth frequently finds heterogeneous effects by gender or educational attainment.[16] Consequently, I investigate effects by educational attainment, consumption, and urbanicity to understand how the changing incentives and costs of entrepreneurship in the disaster aftermath differentially impacted population subgroups.[17]

[15] Empirically estimating the ATT weight matrix using methods developed in de Chaisemartin and D'Haultfœuille (2020) validates this result, finding that all weights used to estimate the ATT are positive.

[16] Fafchamps et al. (2011) find that the effect of receiving in-kind services (i.e. offered machinery or equipment for their business) or cash grants differs between males and females. An RCT by Field et al. (2016) showed that a lack of female peers may contribute to a documented gender gap in entrepreneurial success. McKenzie and Woodruff (2015) showed that the profit and productivity of smaller firms are related to the education levels of the entrepreneurs.

[17] All characteristics investigated in these heterogeneity analyses are measured at the pre-tsunami baseline to avoid potential confounding related to behavioral choices after the tsunami. This is particularly important for the location of

V Main Results

V.A Exogeneity Tests

The central assumption underpinning this study is that the exposed and non-exposed groups are statistically exchangeable after controlling for individual time-invariant features. With only 1 period of panel data prior to the disaster, this assumption cannot be empirically verified within our analysis sample. Similarly, the devastating nature of the tsunami implies that there does not exist a meaningful placebo outcome. Instead, I test this assumption in two ways. First, I perform a balance test on the analysis sample. Then, I perform a pre-treatment trends analysis on the affected areas using the 2002-2004 waves of SUSENAS—a population-representative survey covering our study sites prior to the tsunami.

Balance Test

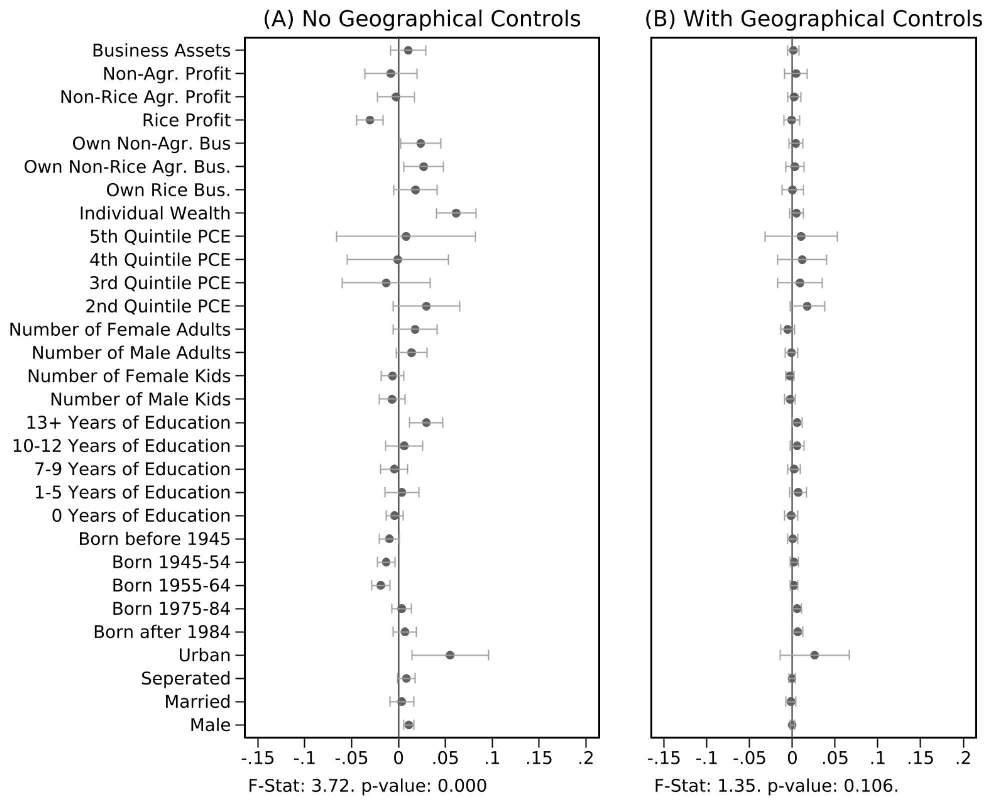
First, I examine the relationship between tsunami exposure T_i and socio-demographic features and business outcomes X_{i0}, measured in the pre-tsunami STAR baseline. Specifically, the following regression is estimated to test whether there are differences in the pre-tsunami characteristics of those who were exposed to the tsunami and those who were not.

$$T_i = \beta_0 + \beta X_{i0} + u_i \tag{2}$$

If exposure to the tsunami is not related to these characteristics the estimates, β will be small and insignificant. Those estimates are shown in Panel A of Figure 1. A simple hypothesis test that all coefficients in the regression are jointly equal to 0 yields F-statistic 3.89 (p-value < 0.001). Given the differences in simple means across exposure groups reported in the summary statistics table, this is not surprising. However, the context of this study implied that exposure conditional on basic geographic features is plausibly exogenous. Thus, Panel B of Figure 1 reports the results of estimating:

residence (because of migration) and consumption. See discussion in Section V.C for why I do not report heterogeneity estimates by age or gender.

Figure 1: Balance Test



Notes. Results of balance test for exogeneity. The figure shows the coefficient estimates of regressing an indicator for exposure to heavy tsunami damages T_i on a flexible set of pre-tsunami baseline descriptive features and business outcomes. All covariates are standardized for this test. Point estimates and 95% confidence intervals are displayed. Panel A includes only the listed features, while Panel B adds *kecamatan* fixed effects, elevation (quadratic), and distance to coastline (quadratic) controls. The F-statistics of the hypothesis tests that all coefficients on the flexible set of pre-tsunami baseline descriptive features and business outcomes are jointly zero are listed below each figure with the associated p-value of this test. Square root transformations are taken of all continuously distributed covariates prior to normalization. Standard errors are clustered at the enumeration area level.

$$T_i = \beta_0 + \beta' X_{i0} + \gamma W_{i0} + \epsilon_i \tag{3}$$

Where W_{i0} includes *kecamatan* fixed effects, the elevation of this respondent's community (quadratic), and the distance to the coastline of this respondent's community (quadratic). Each component of β' is shown in Panel B, where the coefficients are small in magnitude and almost always statistically insignificant at the 95% significance level. A test that the coefficient on each component of β' is jointly 0 yields F-statistic 1.42 (p-value = 0.106), failing to reject at the 95% significance level. Consequently, the balance test in Figure 1 suggests that the pre-tsunami differences between exposure groups in aggregate are accounted for by including these geographical controls. The individual fixed effects estimation strategy will absorb these differences which is one motivation for their inclusion in the models.

Pre-Exposure Trends Test

As a second test, I investigate the possibility of pre-exposure trends across exposed and non-exposed areas using the 2002-2004 SUSENAS waves of the 14 *kabupaten* that STAR represents, prior to the 2004 Indian Ocean tsunami. I estimate:

$$X_{it} = \beta_0 + \beta_t T_i + \xi_t + \gamma W_{it} + u_{it} \tag{4}$$

Where W_{it} includes *kecamatan* fixed effects, the elevation of this respondent's community (quadratic), and the distance to the coastline of this respondent's community (quadratic). I set X_{it} to be a wide set of socio-demographic and economic features available in SUSENAS: male, urbanicity, no completed education, completed primary education, completed junior secondary education, completed senior secondary education, completed tertiary education, age 0-14, age 15-29, age 30-49, age 50 plus, and household consumption.

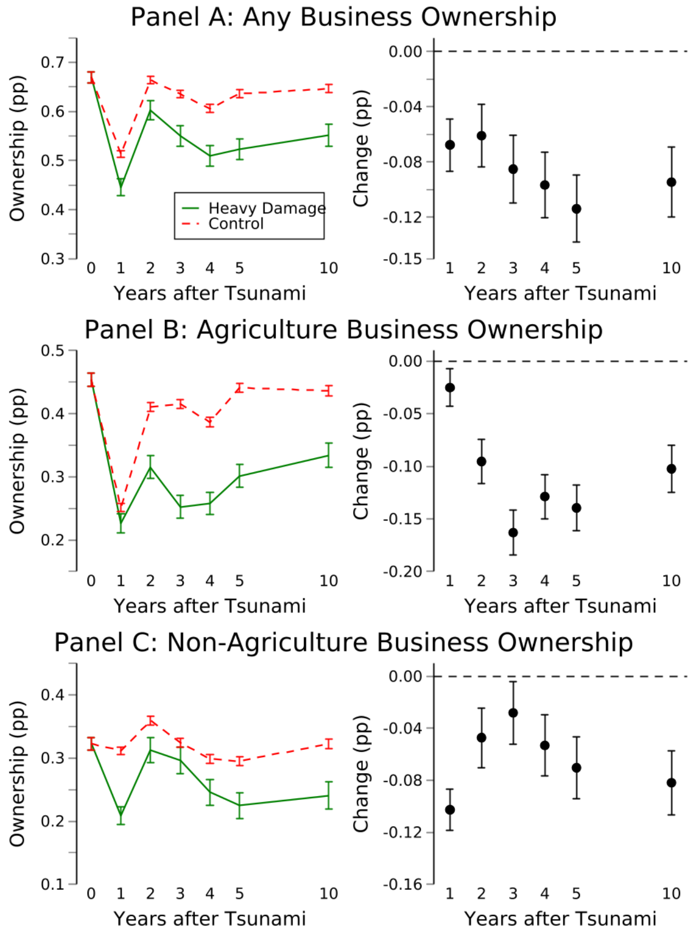
If there does not exist a pre-treatment trend, β_t will be small and insignificant for each of the features. Results are reported in Appendix Table A1. I find that the β_t coefficients are small in magnitude and statistically insignificant the 95% significance level for all years and outcomes. Consequently, both tests presented here find no evidence of systematic selection across observable features after controlling for *kecamatan* and basic land features, offering empirical evidence on the credibility of the identification strategy of this analysis.

V.B Exposure Effects on Entrepreneurship

Under the exposure exogeneity assumption, I estimate β_t from Equation 1 on any business ownership, agriculture business ownership, and non-agriculture business ownership. The trajectories by exposure levels and the resulting exposure effect are shown in Figure 2.

Panel A presents the results where y_{it} is an indicator for business ownership. Immediately following the tsunami, both exposed and control groups suffered a large decline in business ownership. The decline was 6.8pp greater in the exposed group—a 13% further decline relative

Figure 2: Business Ownership Trajectories and Exposure Effects



Notes. This figure reports the trajectories by damage exposure level and estimated exposure effects, $\widehat{\beta}_t$, in each post-tsunami STAR wave across for the business ownership outcomes. Point estimates and 95% confidence intervals are displayed. All regressions include individual and interview month fixed effects, with standard errors clustered at the individual level. See Appendix Table A2 for further estimation results.

to the control group 1-year after the tsunami. The control group largely recovered to pre-tsunami levels of business ownership by the second year, while the exposed group experienced substantial adverse effects on business ownership between 6pp and 12pp through the 10 years following the tsunami. As discussed below, the short-term decline in business ownership in the control group is driven by declines in agricultural business ownership.

Panel B and C of Figure 2 separate the effects by agriculture and non-agriculture business ownership. Immediately following the tsunami, the negative exposure effects were concentrated on non-agricultural businesses.[18] In the 1-year follow-up, individuals from heavily damaged areas

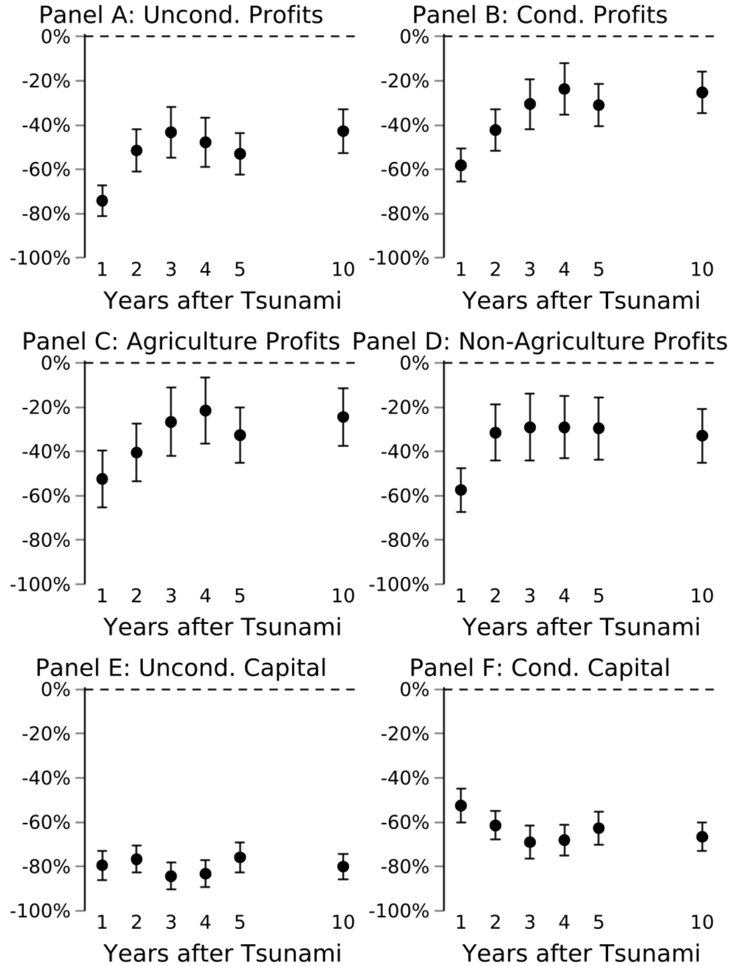
[18]Agricultural business ownership in a given wave is defined as planting or harvesting crops in the associated

operated non-agricultural businesses at 10.3pp (33%) lower rates than those from non-damaged areas, but the effect was only 2.5pp (10%) on agricultural business ownership. From the left sub-figure, the small short-term exposure effects on agricultural business ownership are explained by the large reduction in agricultural business ownership for those from control areas following the tsunami. That is, agricultural business declined dramatically across the sample immediately following the tsunami—not just in exposed groups. This initial decline across the full sample is driven by the fact that the control group still faced short-term damage from the tsunami and earthquake—damages to the main roads and markets was pervasive across study sites, limiting the ability of farmers to sell crops and thus lead many to not plant in the 1-year follow-up. There was reduced demand for agricultural products given the large displacement of survivors in the aftermath, many of whom migrated to areas outside the study sites. Moreover, the sweeping consequences of the tsunami generated large degrees of uncertainty over the future in the immediate aftermath, leading many to choose not to undertake the risks associated with maintaining an agricultural business in the first year after the tsunami.

Importantly, the control group recovered from the initial damage to agricultural businesses by the second year, while the exposed group did not. The negative exposure effects on agricultural business ownership increased in magnitude and persisted, ranging between 8 and 18pp for the later sample years following the tsunami. The control group was quickly able to recover their agricultural business production from the lower levels of damage they faced, while the destruction was severe enough in the heavily damaged groups that effects persisted through the 10-year sample period. The quick recovery of the control group is likely driven by the initial reconstruction efforts that focused on re-building roads—allowing for agricultural businesses to harvest and sell their products more easily. The long-term damage to agricultural businesses is in part due to seawater inundation of agricultural fields: rendering previously productive land permanently damaged by the high levels of salination. Panel C shows that for non-agricultural businesses, the adverse exposure effects of non-agricultural business ownership shrank in the 2nd through 4th year post-tsunami, but grew in the 5th year (7pp, 23%) and 10th year (8.2pp, 25%). Note that the medium- and long-term estimates of exposure will be impacted by the large reconstruction efforts to restore the local economy, as well as any migration responses or household re-combination efforts in the aftermath. Consequently,

calendar year—so it is not simply the case that individuals still reported themselves as farmers.

Figure 3: Exposure Effects on Profits and Assets



Notes. This figure reports the estimated exposure effects, $\widehat{\beta}_t$, in each post-tsunami STAR wave across the profit and asset outcomes. Point estimates and 95% confidence intervals are displayed. All regressions include individual and interview month fixed effects. Square root transformations are performed on all outcomes, after deflating to real values using the local price index series. All panels report percent effects relative to non-exposed individuals ($T_i = 0$) in that wave, with standard errors computed via the delta method and clustered at the individual-level. Panels B, C, and D condition on ownership of that business type, and Panel F conditions on having non-zero business assets. See Appendix Table A2 for further estimation results.

despite the short- and medium-term recovery of non-agricultural business ownership, the adverse effects on business ownership across both sectors persisted through the long-term—suggesting that the aid and migration efforts were insufficient in fully restoring entrepreneurial activity to those most exposed.

Figure 3 estimates β_t from Equation 1 on profit and business capital holdings. Panels A and B report the results on real business profit—Panel A does not condition on business ownership so individuals that do not operate a business have 0 business profits in that wave, while Panel

B conditions on business ownership. Unconditional real business profits declined immediately following the tsunami by 83%, and the adverse effects persisted between 50% and 65% through the sample period. Conditioning on business ownership, declines in real business profit for the most exposed groups are substantial. Panel C and D split the conditional profit by agricultural and non-agricultural businesses, and I find that profit declines were substantial in both sectors of business. Appendix Table A2 shows that non-exposed groups experienced real profit decline in the 1st year following the tsunami but recovered by the second year. Consequently, the decline in real profits through the long-term for individuals from heavily damaged areas results from the inability of their businesses to recover from the initial devastation, even 10 years after the tsunami.

The large negative exposure effects on business profits are driven by several factors. First, Figure 2 shows that exposed groups saw massive declines in business ownership in general, forcing many to no longer benefit from business profits. Second, prices in more devastated areas were approximately 10-20pp larger than less damaged areas through the long-term. Thus, the nominal profits received by household businesses in devastated areas provided less real purchasing power than those from non-exposed areas. Third, Panel B of Figure 3 documents that the businesses that remained in less damaged areas recovered less in profits.

Building on the last point, Panels E and F of Figure 3 shows how business assets evolved following the tsunami. Panel E uses the full sample, while Panel F conditions on non-zero business assets. Following the results found in Lombardo et al. (2022), business assets in exposed areas were devastated by the tsunami. In percent terms, unconditional real business asset value declined by 90% for those from areas heavily exposed to the tsunami, and this effect ranged between 83% and 93% throughout the sample period. Conditional business assets also declined substantially—between 60% and 80% relative to the control group. Lombardo et al. (2022) document that this decline in asset holdings is not driven by sales of business assets in response to the tsunami, but rather due to the greater damages faced by exposed groups. Appendix Table A2 shows that even non-exposed groups suffered substantial declines in real business capital following the tsunami but were largely able to recover their holdings by the 10th year. This was not the case for those from heavily damaged areas as they were unable to re-build their business assets through the sample period.

Drawing welfare conclusions from these results are complicated given the competing nature of wage labor and entrepreneurial activity. Reduced business ownership will hurt individual welfare if they would experience greater gains in the entrepreneurial sector than the wage labor sector—which is unclear without further analysis. That said, the fact that 66% of the sample prior to the tsunami operated a business suggests that the entrepreneurial sector is an important source of income to individuals in supporting their economic livelihood. The annual profits generated by the household businesses equate to approximately 12 months of real per-capita expenditures prior to the tsunami, meaning that the substantial declines in business profit in the aftermath greatly restrict their ability to support prior levels of consumption.

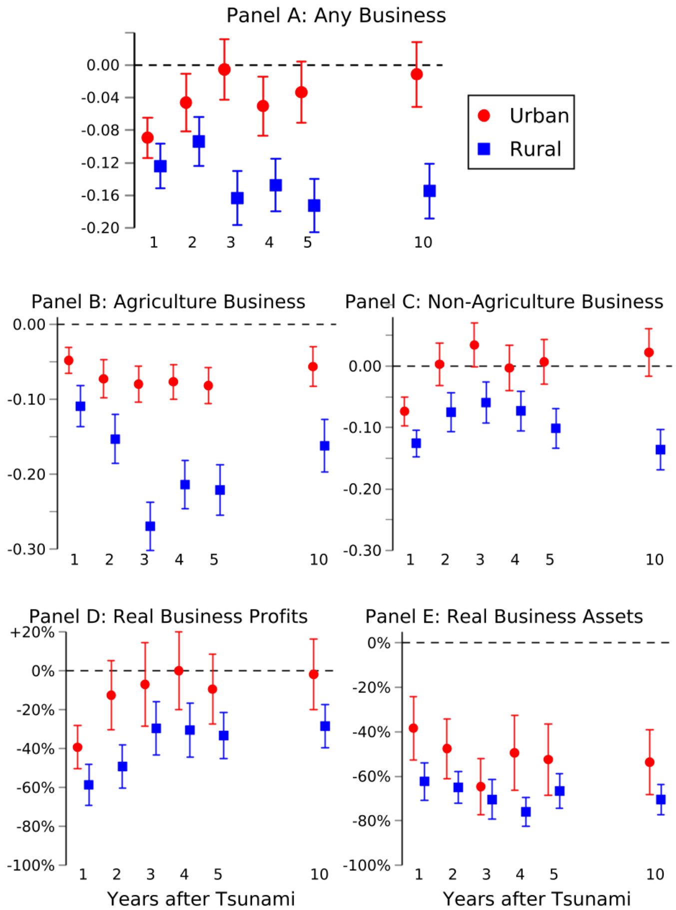
V.C Heterogeneity

I next investigate how the effects of heavy tsunami exposure vary across population subgroups. The individual fixed effects strategy captures individual-specific and time-invariant features, including pre-tsunami location and existing entrepreneurial talent. However, by computing stratified estimates within different population subgroups, I examine whether there are differential effects of these pre-existing characteristics on those who were exposed. For example, there are likely to be differences for those initially from urban versus rural areas given that large plots of rural land used for agricultural business were destroyed by the tsunami.

Figure 4 displays the results of estimating the effect of exposure separately between those who, at the time of the tsunami, were living in rural or urban areas. While those from heavily damaged urban areas were largely able to recover the loss in business ownership from the initial tsunami devastation within 3 years, business ownership in rural areas remained substantially depressed through the long term. Panel B and C show that the bulk of the business ownership damage to rural areas stemmed from declines in agriculture businesses. Those from heavily damaged rural areas suffered 15pp to 30pp declines in agricultural business ownership, relative to those from rural areas not exposed to heavy damages, in the 3rd through 10th year following the tsunami. Urban areas also experienced declined agriculture business ownership, but the effect was smaller than 10pp across the sample period. In non-agriculture business ownership, those from heavily damaged urban areas recovered from the damage in 2 years while rural areas continued to experience declines.

Recall that in the main results, the largest adverse effects on agriculture business ownership occurred in years 3 and 4, concurrently with the smallest effects on non-agriculture business

Figure 4: Exposure Effects across Urbanicity



Notes. Result of estimating the coefficients $\widehat{\beta_t}$ in Equation 1 separately for those from rural or urban areas in the pre-tsunami baseline. Point estimates and 95% confidence intervals are displayed. All regressions include individual and interview month fixed effects, with standard errors clustered at the individual-level. Panel D and E use real profit and business asset values, conditional on owning any business (in Panel D) or having non-zero business assets (Panel E). Square root transformations are performed on business profits and assets, after deflating to real values using the local price index series. Panels D and E report percent effects relative to non-exposed individuals ($T_i = 0$) in that urbanicity group in that wave, with standard errors computed via the delta method.

ownership. Panel B and C of Figure 4 highlight that the agricultural decline in that period was driven largely by rural areas, while the gains in non-agricultural in that period occurred in both rural and urban. Those from urban areas were able to maintain the non-agricultural business gains through the long-term, while non-agriculture business declined again in rural areas following this medium-term effect.

Panels D and E display the exposure effects on real profits and business assets by urbanicity. Urban areas were able to recover their profit losses by the 3rd year, while rural areas suffered massive declines through the long-term. The rebounding urban business profits are driven by

Table 2: Heterogeneity Analysis by Baseline PCE Quintiles

	(1)	(2)	(3)	(4)	(5)	(6)
	y_{it} = Owns Business			y_{it} = Real Business Assets		
	Bottom 2 PCE Quintile	Middle 2 PCE Quintile	Top PCE Quintile	Bottom 2 PCE Quintile	Middle 2 PCE Quintile	Top PCE Quintile
Baseline (Rp '000s)	0.714	0.672	0.591	2,936	4,145	9,029
	(0.010)	(0.009)	(0.012)	(141)	(156)	(508)
1 Year $\times$ T_i	-0.188	-0.052	0.030	-73.6%	-40.5%	-27.5%
	(0.018)	(0.015)	(0.017)	(5.1)	(6.8)	(9.3)
2 Year $\times$ T_i	-0.116	-0.089	0.048	-72.3%	-51.6%	-54.5%
	(0.020)	(0.018)	(0.022)	(5.0)	(5.9)	(6.5)
3 Year $\times$ T_i	-0.179	-0.119	0.074	-81.1%	-49.2%	-68.9%
	(0.022)	(0.020)	(0.024)	(4.6)	(8.0)	(7.5)
4 Year $\times$ T_i	-0.188	-0.093	-0.001	-81.3%	-57.3%	-60.3%
	(0.022)	(0.018)	(0.023)	(4.5)	(6.2)	(8.4)
5 Year $\times$ T_i	-0.193	-0.115	-0.029	-76.4%	-54.3%	-52.8%
	(0.022)	(0.019)	(0.023)	(5.0)	(5.8)	(9.9)
10 Year $\times$ T_i	-0.164	-0.116	0.018	-72.2%	-67.4%	-52.2%
	(0.022)	(0.021)	(0.025)	(4.8)	(4.6)	(8.7)
Observations	37,728	44,471	22,401	26,379	30,703	13,126

Notes. Results of estimating Equation 1 separately based on three groups of baseline real PCE quintile: the lowest two quintiles, the 3rd and 4th quintile, or the 5th quintile. All regressions include individual and interview month fixed effects, with standard errors clustered at the individual level. Columns 4 through 6 use real business asset values, conditional on having non-zero business assets. Square root transformation performed on business assets, after deflating to real values using the local price index series. Columns 4-6 report percent effects relative to non-exposed individuals in that baseline PCE quintile for that wave, with standard errors computed via the delta method.

non-agriculture businesses—urban agricultural businesses continued to suffer profit declines through the long-term.[19] Panel E shows that the difference in business outcomes is unlikely to be fully explained by assets: urban areas suffered somewhat smaller declines in real assets than rural areas, but the difference was statistically insignificant and exposure effects were massive through the long-term for both rural and urban areas. Instead, the effects are the result of a greater recovery of non-agriculture businesses in urban areas, which are generally less capital intensive than agricultural businesses.

Beyond the differences across urbanicity, Table 2 displays how exposure effects differed across the pre-tsunami distribution of resources, measured via pre-tsunami per capita expenditures (PCE). PCE serves as an indicator of welfare that is thought to reflect long-term resources

[19] Large discrepancies between urban and rural areas following a large-scale shock are common in other studies. Frankenberg et al. (2003) find that, in response to the Indonesian financial crisis in the late 1990s, self-employment earnings declined to a larger extent in urban areas than rural area.

availability. In Table 2, I estimate Equation 1 separately by an individual's position in the pre-tsunami PCE distribution. I find that those from the highest PCE quintile experienced small and typically statistically insignificant effects on any business ownership. This is driven by non-agriculture businesses: high PCE individuals still suffered large negative exposure effects on agricultural business ownership similar to other PCE quintiles but saw positive, though statistically insignificant, gains in non-agricultural business ownership. The same theme holds for real business profits: the highest PCE quintile had null exposure effects through the sample period, supported by non-agriculture business profits. Columns 4 through 6 show business asset evolution by pre-tsunami PCE, and again, those from the highest PCE quintile suffered substantially lower exposure effects on real business assets than lower PCE quintiles.

In Appendix Table A3, I examine if the effect of heavy tsunami exposure on the evolution of entrepreneurship differed across educational attainment. The business outcome data is constant within a household and wave, so there is very little identifying power to observe heterogeneity across gender or birth year despite interest in these results. Given assortative mating, there is more scope to identify heterogeneity by educational attainment. Consequently, in Table A3, I estimate Equation 1 separately by pre-tsunami levels of educational attainment. I find suggestive evidence that those with higher educational attainment experienced weaker exposure effects on business ownership and business assets, but the differences are generally not statistically significant.[20] In the case of real business profits, the differences are statistically significant and economically substantial: individuals with lower education at the pre-tsunami baseline experienced an approximate 20pp greater decline in real business profits across the sample period compared to higher educated individuals. This difference is driven by non-agricultural business profits.

V.D Migration and Joint Effect on Displacement and Entrepreneurship

The tsunami prompted a large-scale migration response, where an estimated 700,000 people in Aceh were displaced by the tsunami and migrated away from their pre-tsunami homes. Some of these people moved within the community, but in communities that sustained large damages, the

[20] High educational attainment is defined as having at least 9 years of educational attainment at baseline (completion of junior secondary school).

majority of those who were displaced moved to a different community. About half of those people moved to temporary housing such as a camp and the other half moved to private housing elsewhere within Aceh (Gray et al., 2014).

Given its prevalence in the areas most damaged, it is important to consider how migration may be linked with entrepreneurial decisions. On the one hand, migrants who had lost their livelihoods in the tsunami, including the capital underlying their businesses, the market they served, and the network they had developed may have greater difficulty rebuilding their businesses. On the other hand, in the immediate aftermath of the tsunami, with the large decline in demand for labor in the wage-paying sector, the expected return to entrepreneurship was likely high for many migrants.

In many post-disaster studies, analysis of populations that migrated out of the pre-disaster study site is both complex and expensive; few studies have successfully tracked large fractions of post-disaster migrants. STAR is an exception: with high levels of survey recontact, it is ideally suited for analyses of migrants and entrepreneurship over the long term.

In this analysis, I use the 1-year post-tsunami survey to define *tsunami-driven displaced individuals* as those that were displaced to a different area following the tsunami relative to where they were before the disaster. This includes those that were displaced to tents and camps immediately following the tsunami or those that migrated to private homes including renting a new home or staying with friends or relatives. This definition of migration is time-invariant—meaning that I continue to define individuals as out-migrants even if they later return to their pre-tsunami location of residence or move on to a new location.

Following this definition of migration, 35% of the analysis sample was displaced immediately after the tsunami. For those from areas facing heavy tsunami damage, 74% migrated elsewhere in the aftermath. Among those migrating from heavily damaged areas, 31% migrated to semi-permanent housing settlements and did not return by the 1-year follow-up; these semi-permanent settlements include camps, tents outside the campsite, barracks, places of worship, or hospitals. 24% of migrants from heavily damaged areas went to these semi-permanent settlements but returned to their original location by the 1-year follow-up. The remaining 45% migrated to private homes they rented or stayed with relatives or friends. Among those migrating to private homes, 59% did not return by the 1-year follow-up. Socioeconomic and demographic characteristics of tsunami-driven displaced individuals from heavily damaged areas were similar to their non-migrant peers in heavily damaged areas—although they were more likely to be male (6.1 pp, SE = 2.2pp).[21] This finding is also reported by Gray et al. (2014), which found that migration after a disaster is less selective overall than migration in other contexts.

Investigation of effects on migrant populations is difficult as many of the outcomes involved in the migration decision cannot be treated as exogenous. The decision to migrate or not itself depends on many individual and community features: the damage faced by the individual, their family, and their local social safety net; their current economic status; their expectations for the future and the information they have about opportunities they would have in all potential migration destinations; risk preferences; and a host of other factors. Among those migrating, the exact timing is also likely to be endogenous: individuals may wait to migrate until the present discount value of expected returns to leaving their community out-weigh the present discounted value of expected costs of leaving. Moreover, individuals may have some ability to select the exact area that they move to—perhaps moving to communities with more promising futures.

Rather than attempt to develop a dynamic model of the joint decision to move and engage in entrepreneurial activities, I focus on the key strength of the study setting: I exploit the exogenous nature of exposure to the tsunami and trace out the trajectories of the joint outcomes of entrepreneurship and migration:

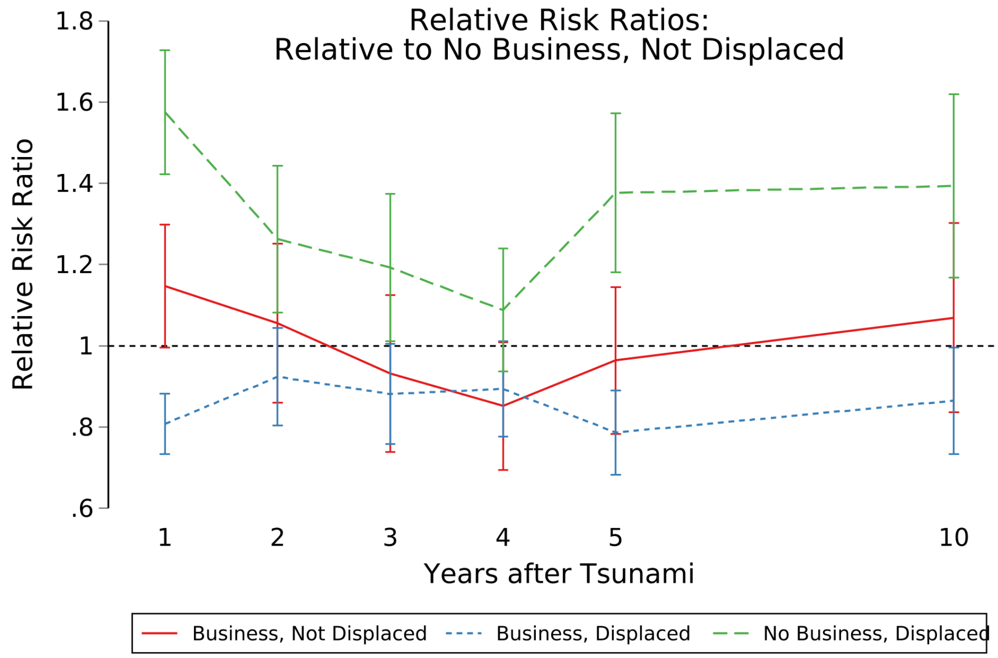
$$y_{it} = \beta_t T_i + \xi_t + \gamma X_{i0} + u_{it} \tag{5}$$

Where y_{it} is a multi-variable discrete indicator with 4 mutually exclusive outcomes: not a migrant nor business owner; non-migrant and business owner; migrant and non-business owner; and migrant and business owner. T_i is an indicator for heavy tsunami exposure as before, ξ_t are survey wave fixed effects, X_{i0} are controls for the individual's pre-tsunami baseline *kabupaten*, and their pre-tsunami community's elevation and distance to the coastline. As discussed before, the controls in X_{i0} are necessary for the exogeneity of exposure assumption to hold.[22] I estimate Equation 5 via multinomial logit, clustering standard errors at the person-level.

[21] They were also more likely to be younger (-0.6 years, SE = 0.8 years) and better educated (0.2 years, SE = 0.5 years), but differences are small and statistically insignificant.

[22] Note that I use individual fixed effects instead of X_{i0} in the main specification results. Since I estimate Equation 5 via multinomial logit, the non-linear estimates require more variation within groups for estimates to properly converge, so I employ *kabupaten* fixed effects and topographical controls instead here.

Figure 5: Exposure Effects on Joint Migration and Entrepreneurship Outcomes



Notes. Result of estimating $\widehat{\beta_t}$ from Equation 5: the effect of heavy tsunami exposure on the joint outcome of entrepreneurship and tsunami-driven displaced individual. Multinomial logit used to estimate $\widehat{\beta_t}$. Relative risk ratios and 95% confidence intervals displayed, relative to the no business ownership and non-migrant outcome. Pre-tsunami baseline *kecamatan* fixed effects included, as well as quadratic controls for elevation and distance to the coastline. Standard errors are clustered at the person level. Migration defined as those that were displaced, following the tsunami, from their pre-tsunami location of residence.

I report estimates of β_t from Equation 5 in Figure 5, presented in terms of relative risk ratios relative to the reference group of non-migrant non-business owners. The estimates of trace out the evolution of the probabilities of each of the other three outcome pairs relative to the stayers without a business: stay with a business, move, and have a business and move and do not have a business. A relative risk ratio of one indicates the probability is the same as the reference group; if the relative risk ratio is greater than one (less than one), the probability of that outcome is higher (lower) than the reference group.

As indicated by the estimates for stayers with a business (red line), which is not significantly different from 1, among those who were not displaced, there is no difference in the probability they started or maintained business enterprises. However, movers are far more likely to not have a business (green line) which is also reflected in their being less likely to have a business (blue line).

These results indicate that reduced entrepreneurial activity is linked to displacement because of the tsunami. The displaced population experienced reduced odds of entrepreneurship—particularly in the immediate aftermath of the tsunami.

VI Effect of Tsunami Reconstruction Assistance

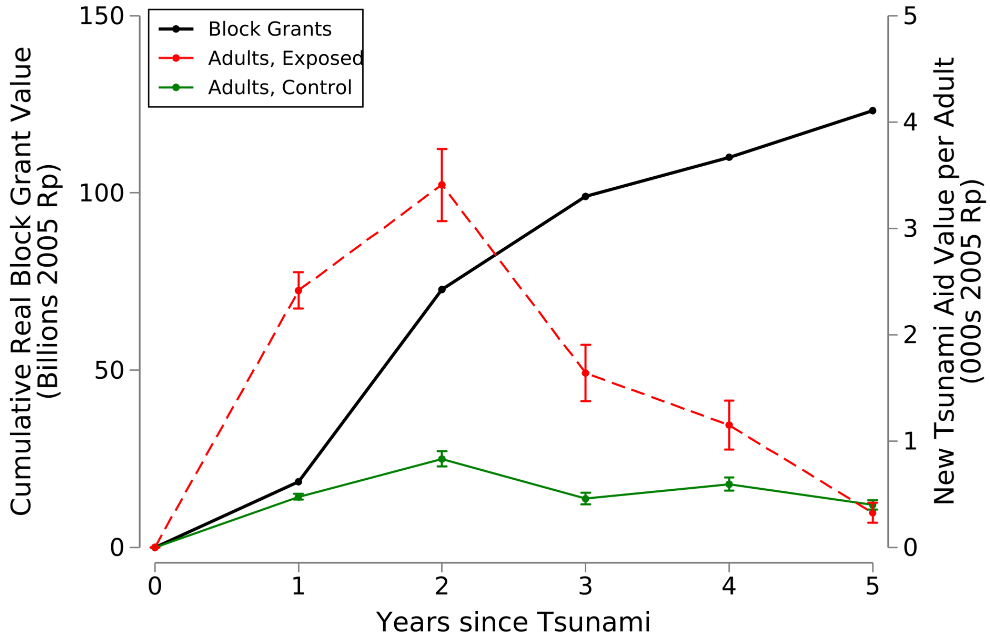
Following the destruction of the natural and built environment from the tsunami, there was an international response to the destruction as donors poured resources into the area. The *Build Back Better* campaign sought to use this massive resource shock to restore and improve upon the pre-existing infrastructure in Aceh. This goal was particularly apparent in the last phase of the reconstruction process—approximately 2.5 years after the tsunami, where aid was intended to rebuild the economy and government of the impacted areas (Laurito et al., 2022). The aid response constituted the largest reconstruction project ever undertaken in a developing country within 3 years of the tsunami, with approximately USD 7.7 billion committed to Aceh in the aftermath. Consequently, this section explores whether the influx of reconstruction aid impacted the entrepreneurial recovery of individuals exposed to the tsunami.

VI.A Descriptive Patterns in Aid Receipt

Research by Frankenberg et al. (2009) used data from STAR to describe patterns in aid receipt following the tsunami. They found that 65% of households reported receiving some tsunami assistance in 2005, 74% in 2006, and 79% in 2007. The most frequent provider of this assistance in later years was the Government of Indonesia. NGOs were more frequent providers of assistance in the first year after the tsunami, but their disbursements more quickly tapered out over time. The earlier study also found that households with fewer resources before the tsunami or those in which the head was less educated were more likely to receive aid in the aftermath.

Building on this work, Figure 6 describes the timing and amounts of reconstruction aid received by individuals and communities in our study sites. The colored lines depict average new tsunami aid per adult, received in that wave, as reported in the STAR individual survey. Two points are salient. First, there was emergency aid received immediately after the tsunami in the first year, but the bulk of the aid was received in the 2nd year onwards. Second, most of the aid went to the areas facing the most damages—individuals that faced heavy tsunami damages received substantially

Figure 6: Aid Inflow into Damaged Communities and Exposed Populations



Notes. This figure highlights the timing of tsunami reconstruction aid into the STAR study sites and received by the population of interest. The solid black line shows the mean cumulative value of Block Grants received across the STAR Enumeration Areas in each survey wave, as reported by the community leader, corresponding to the right y-axis. The other two lines report the mean value of new tsunami aid received by STAR households within twelve months of the survey wave, split across exposure levels to tsunami damage. Note that the 10-year follow-up is omitted from this figure as most reconstruction aid was received within the first 5 years of the disaster. All reported values are deflated to real values using the local price index series.

more public assistance than less damaged areas.

Not all recovery assistance was intended for individuals. Communities received block grants to support reconstruction of their roads, infrastructure, or other public facilities. Such assistance would fail to appear in individual accounts of aid receipt, so in Figure 6, I overlay cumulative estimates of the total real block grant value received by communities in our study site. Block grants were given to communities in effort to support the re-building of their infrastructure and important community systems. The same pattern is more exaggerated here—reconstruction aid rose dramatically in the 2nd and 3rd year following the tsunami.

VI.B Empirical Strategy for Aid Effects

In an ideal setting, I would measure the effect of aid receipt on entrepreneurial outcomes by estimating:

$$y_{it} = \alpha_0 + \alpha_t \text{Aid}_{it} + \lambda_i + \xi_t + u_{it} \tag{6}$$

Where, as before, y_{it} are the entrepreneurial outcomes of interest, λ_i, γ_t are individual and wave fixed effects, respectively, and u_{it} is remaining unobserved determinants of y_{it}. Aid_{it} is an absorbing indicator for if individual i received aid in period t or earlier. Then, α_t attempts to trace how entrepreneurial outcomes evolved following the receipt of aid in the post-tsunami period.

Estimation of Equation 6 is complicated by two concerns. First, Aid_{it} may be endogenously determined. Aid was directed at communities facing the most damage, and the extent of post-tsunami damage may be related to unobserved determinants of entrepreneurial success. The timing may also be endogenous: donors may have targeted aid toward the communities that appeared most promising to maximize their return, or perhaps the least promising communities to support the most vulnerable. Villages with more capable leadership or with closer political ties to certain agencies may have also received aid more quickly. The large-scale migration response yields another reason why Aid_{it} may be endogenous—resourceful individuals may have opted to migrate towards communities with greater anticipated aid inflows to benefit from the aid themselves.

The second concern is with the econometric strategy used to estimate α_t. Unlike our main exposure equations, individuals can receive aid at different points in the post-tsunami period so the treatment variable Aid_{it} is staggered in its timing. Given the staggered nature of treatment assignment, recent econometric research has shown that if the treatment effect α_t can vary across individuals or time, estimation of Equation 6 using the standard two-way fixed effects OLS regression will yield biased estimates of the true average treatment effect on the treated, even under strict parallel trends (de Chaisemartin and D'Haultfoeuille, 2022; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021).

The empirical strategy used in this paper addresses both concerns. To address endogeneity concerns, I first limit Aid_{it} to consider only the receipt of housing aid—the largest share of the reconstruction funds. Families who had their houses destroyed by the tsunami received a standard 36 m^2 home, and families that faced damages to their homes received monetary assistance for rehabilitation (Laurito et al., 2022). Importantly to the exogeneity assumption, the rebuilt houses were in the same location as their corresponding pre-tsunami house whenever possible, limiting endogenous out-migration concerns. This was due to donor pressure to rebuild homes quickly, as well as the concern that otherwise, people would move into damaged areas in hopes to receive aid (McCaughey et al., 2018, 2017). Consequently, as houses in a person's pre-tsunami community were being restored, many of those initially displaced returned to their pre-tsunami location to benefit from housing aid. The Reconstruction of Aceh Land Administration System (RALAS) was launched in August 2005 to address disputes in land titles resulting from the changed physical landscape and the destruction of pre-existing legal documentation of land ownership (Nazara and Resosudarmo, 2007).

Focusing on housing aid implies that individual migration towards areas receiving housing aid should not be an issue, as housing aid was assigned based on the location of one's pre-tsunami house. The establishment of RALAS to help identify pre-tsunami land titles addresses concerns of non-compliance with this policy. To address the concern that housing aid was concentrated in the areas most damaged, I restrict the sample to the set of individuals from *kecamatan* at the pre-tsunami baseline where at least one community faced heavy tsunami damages (N = 33,109). This yields a more comparable control group to those receiving aid, strengthening the credibility of the parallel trends assumption. In the Appendix, I further restrict the sample to those that received housing aid at some point in the study, and thus parallel trends is satisfied if the non-treatment trends for those that received aid in later waves are the same as the non-treatment trends for those receiving aid in earlier waves. Lastly, I limit the sample to the post-tsunami period and include individual fixed effects to capture any time-invariant individual heterogeneity in the tsunami aftermath that also impacted aid receipt over this period. Notably, this includes the individual's experience of the tsunami, whether the individual was eligible for aid because their house was destroyed or damaged, initial damages to the road or infrastructure that impacted the timing in which housing aid could be delivered, and pre-existing levels of resources or their exposure to networks that may have increased their likelihood of receiving aid.

The exogeneity assumption of this analysis is that, after controlling for these time-invariant individual features, the trends of those receiving housing aid in each period would match the trends of those that have not yet received housing aid or will never receive aid. I argue that this remaining variation is driven largely by the complexities associated with the aid disbursement process.[23] Over 400 independent donor organizations were assisting with the recovery and delivering the assistance to areas in which the natural and built environments changed rapidly is not trivial (Masyrafah and McKeon, 2008). While the BRR was created to assist with this coordination problem, its responsibilities did not include controlling how the funds were spent or the disbursement into damaged communities. There was limited coordination across donor organizations in this process. Moreover, the complications associated with sorting our property rights created further variation in the timing of housing aid receipt across communities. To the extent that these bureaucratic processes are unrelated to the remaining post-tsunami individual heterogeneity, the estimates can be given a causal interpretation.[24]

To address the econometric concerns, I employ an estimation strategy introduced by Callaway and Sant'Anna (2021) and developed by Rios-Avila et al. (2021) that allows for heterogeneous treatment effects in a staggered differences-in-differences design. I estimate the below event study regression:

$$y_{it} = \sum_k \delta_k \mathbb{1}[t - \text{Aid}_i = k] + \lambda_i + \xi_t + u_{it} \tag{7}$$

Where Aid_i is the first period that individual i reported received housing aid, with $\mathbb{1}[t - \text{Aid}_i = k] = 0$ across all periods t for individuals i that never received housing aid.[25] In Equation 7, δ_k is the parameter of interest, capturing the effect of housing aid k periods from the first wave of housing aid receipt.[26] The receipt of housing assistance serves as a large positive wealth shock to the beneficiaries, and I investigate how that impacted their entrepreneurial trajectories.

[23] I restrict to public housing assistance delivered by governmental organizations, NGOs, or religious groups. I exclude assistance received from family members, friends, or neighbors.

[24] Given the particular features of housing aid that lead to a stronger exogeneity assumption—including the limited role of migration response and that it is likely to be driven by international coordination problems—I chose this as a proxy for overall aid receipt. I do not use housing aid as an instrument for overall aid receipt as exclusion is unlikely to hold.

[25] Following Laurito et al. (2022), I define received housing aid as whether they received a house or construction materials. In each post-tsunami STAR wave, a respondent for the household reported whether any household member received a house or construction materials (and the timing if received). The responses were used to create at individual-level measure of housing aid receipt based on whether any members of the original pre-tsunami household received housing assistance.

[26] Then, in line with 2x2 DiD approaches, δ_k can be thought of as the coefficient on the interaction between an indicator for housing aid receipt and an indicator for if housing aid was received k periods prior to the current wave t.

VI.C Results and Heterogeneity

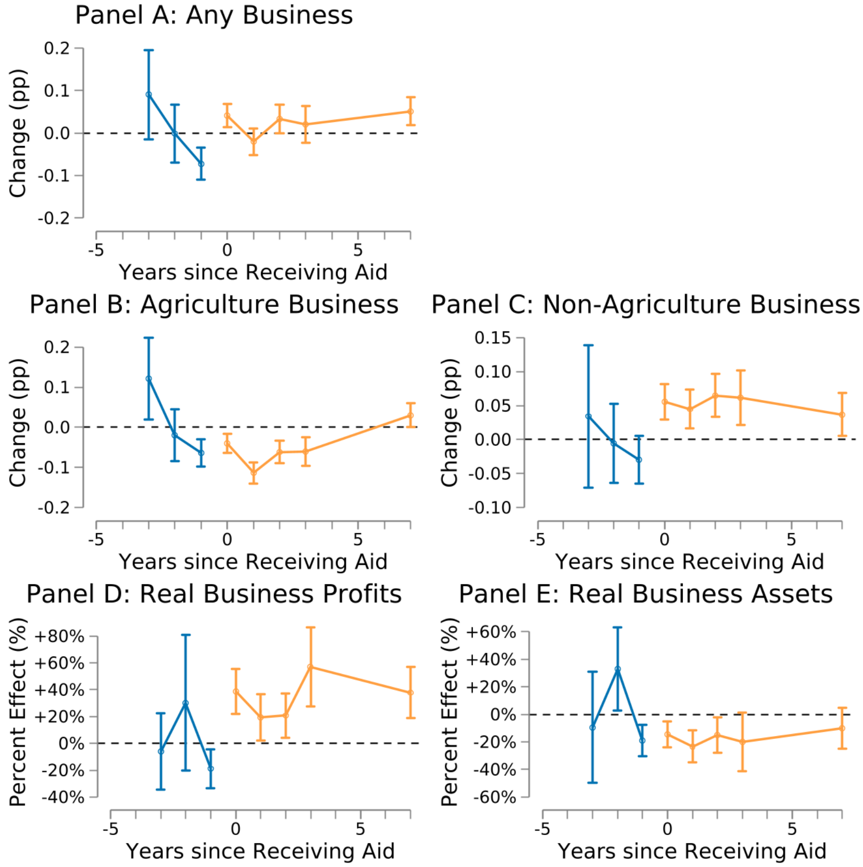
The estimates of δ_k from Equation 7 for the 5 entrepreneurial outcomes are shown in Figure 7. This is the effect of receiving housing aid on entrepreneurship across different periods before and after receiving aid, relative to those that did not yet receive housing aid in that period or those that never received housing aid. Note that 36% of the analysis sample for this section received housing aid, and among those receiving aid, 55% received it in the second year following the tsunami while the rest received housing aid between the third and fifth post-disaster year.

Panel A of Figure 7 finds weakly positive but imprecise effects of housing aid on any business ownership in the short-term. The long-term effects, however, are large and statistically significant: receipt of housing aid increased business ownership in the long-term by 5.1pp (8.7%).

Panels B and C show that the aggregate effects in Panel A hides heterogeneity across the sector of business ownership. Housing aid appeared to have negative effects on agricultural business ownership, but large and positive effects on non-agricultural businesses. In the short- and medium-term, housing aid increased non-agricultural business ownership by 4-7pp (12.7%-22.3%) but the effect fell to a 3.7pp (11.8%) increase in the long-term. Given the immediate decline in non-agricultural business ownership to those facing heavy tsunami damages of 10.3pp, housing aid reduced this initial damage by 36% to 68% across the sample period. Thus, receipt of housing aid had a substantive impact on the non-agricultural business ownership trajectories of exposed individuals.

Panel D displays the effect of housing aid receipt on profits. Consistent with the growth of non-agricultural businesses from housing aid, I find large increases in real business profits from housing aid: ranging from a 20% to 55% increase in the short- and medium-term relative to the control group that did not yet receive or never received housing aid, and a 38% increase in the long-term. Panel E shows that the increased business profits were not the result of a growth in business capital: I observe negative effects of receiving housing aid on real business value. Thus, housing aid spurred growth in non-agricultural and non-capital-intensive businesses that translated into increased real business profits through the long-term. The lack of an effect on agricultural businesses suggests that this is unlikely due to respondents using their newfound housing wealth as collateral for a business loan. Rather, it is likely that restored housing offered individuals

Figure 7: Effect of Receiving Housing Aid on Entrepreneurial Outcomes



Notes. Estimates of $\widehat{\delta_k}$, the effect of housing aid on entrepreneurial outcomes, as shown in Equation 7 across pre- and post-treatment periods. $t = 0$ is the first period in which the individual received housing aid. Point estimates and 95% confidence intervals are displayed. ATTs are estimated via an estimator proposed by Callaway and Sant'Anna (2020) that yields unbiased estimates accounting for individual and time fixed effects in the presence of heterogeneous treatment effects. Asymptotic standard errors are estimated using Influence Functions which account for clustering at the individual-level. Square root transformation is applied to business profits and assets, after deflating to real values using the local price index series. Panels D and E report percent effects relative to those never receiving housing aid or those that have not yet received housing aid in that period, with standard errors computed via a bootstrap procedure clustering at the individual-level. Panel D conditions on ownership of any business, and Panel E conditions on having non-zero business assets. The sample is limited to the set of individuals from *kecamatan* at the pre-tsunami baseline where at least one community faced heavy tsunami damages, and the time period is limited to post-tsunami waves.

greater time and focused energy on entrepreneurship rather than worrying about their basic living conditions. This is consistent with Laurito et al. (2022) that receipt of housing aid led to better psychological health.

Note that for many of the outcomes, there is a significant negative pre-treatment trend—suggesting that those that received aid had relatively worse entrepreneurial outcomes in the years prior to receiving housing aid. This is driven by tsunami exposure—the pre-treatment periods are closer to the immediate aftermath of the tsunami in which entrepreneurial activity was reduced. This adverse pre-treatment trend would under-state the positive effects of housing aid, and thus my estimates of δ_k are likely under-estimating the true benefits of housing aid on entrepreneurial outcomes. The fact that even with the existence of negative pre-treatment, I find strong positive effects of housing aid suggests that the positive wealth shock from housing aid played a crucial role in how individuals responded to their changing economic environment.

In Appendix Figure A1, I replicate our estimation of δ_k from Equation 7 but limit the sample to those that received housing aid at some point in the study. Thus, the estimation exploits only variation in the timing of aid receipt to estimate δ_k, implying that I cannot use that sample to identify long-term effects. Figure A1 finds similar but less precise results to that of Figure 7 aid led to reduced agricultural business ownership but increased non-agricultural business ownership (with effects of similar magnitude). Again, I also find increased real business profits in the years after receipt ranging from 25% to 50%, but negative effects on business capital stock holdings. In general, I find smaller negative pre-treatment trends, consistent with the notion that the timing of aid conditional on receipt faced a less severe endogeneity problem than the receipt outcome itself.

Last, I perform a similar heterogeneity analysis as before to investigate how the effects of housing aid varied across population subgroups. While the staggered DiD design removes time-invariant individual-specific features, I stratify individuals based on pre-tsunami characteristics and re-estimate Equation 7 using these population subgroups. For the same reasons as before, the nature of the business data in this study does not allow for identification of within-household variation, implying that the ability to estimate heterogeneity by age, gender, or educational attainment is unlikely. Beyond this, the Callaway and Sant'Anna (2021) estimation strategy yields unbiased but more imprecise estimates than typical TWFE estimators, further limiting the power to identify heterogeneity.

Appendix Figure A2 examines whether there are heterogeneous effects of housing aid receipt by pre-tsunami resources, using pre-tsunami household expenditures per capita as a proxy. I focus on the two outcomes that housing aid most directly impacted: non-agricultural business ownership and real business profits. I split the sample into those from the upper 3 quintiles of PCE at the pre-tsunami baseline and those from the lower two and estimate δ_k separately for these sub-groups. Interestingly, I find very similar results between the high and low pre-tsunami PCE groups for both non-agricultural business ownership and real business profits. The differences are never statistically significant—although the results suffer from reduced power as the estimates are made over smaller population groups. Note that housing aid was standardized among survivors with damaged or destroyed homes so richer individuals did not simply receive more aid—the magnitude of the wealth shock was stable across PCE groups. Consequently, I find evidence that receipt of housing aid did not yield substantially different effects by pre-tsunami resources—both the rich and poor were successful in using their housing aid to support non-agricultural business growth and benefit from increased levels of real profit.

I next explore heterogeneity in aid receipt effects on real business profits by urbanicity and educational attainment groups in Appendix Table A4. I find sizeable differences by urbanicity—individuals from urban areas experienced greater increases in real business profit from housing aid than those from rural areas in the short- and medium-term, but effects converged in the long-term. This is driven by increased growth in non-agriculture businesses for those receiving housing aid from urban areas. I find smaller differences by education: high-education people experienced large real business profit gains from housing aid than those with lower levels of education in the short- and medium-term, but the effects are imprecisely measured and thus not significantly different. Again, the effects across the groups converged in the long-term. I find little heterogeneity in the effect of housing aid on entrepreneurial outcomes across gender or birth cohorts. The negative pre-treatment trends tend to be very stable across observable heterogeneity groups.

Overall, this section provides novel evidence on how a positive wealth shock can impact individual entrepreneurial decisions. I find that those receiving housing aid were able to increase their non-agricultural business ownership and spur gains in real profits without increasing their business capital holdings. Given the illiquid nature of aid in the form of re-built housing or materials to repair their damages, it is unsurprising that business owners were unable to draw directly from this wealth shock and increase business assets. Instead, they turned to less capital-intensive businesses to raise new business profits.

VII Conclusion

The evolution of entrepreneurial activities is important in the aftermath of large shocks. Exposure to shocks bring large economic upheaval—destroying businesses and their necessary capital. Given the rising burden of natural disasters and environmental shocks, there is increased need to document the full economic response of impacted individuals. Entrepreneurial activities are a crucial aspect of individual economic livelihood that has escaped prior natural disaster research. The ability of communities devastated by environmental damages to recover through the long-term is reliant upon the entrepreneurial success of exposed individuals to create new opportunities. However, prior studies of individual responses to natural disasters have failed to document entrepreneurial decisions, despite the importance of this question to understanding recovery mechanisms as well as to policymakers attempting to restore economic activity.

This study addresses this important gap by investigating entrepreneurial decisions for those most exposed to the 2004 Indian Ocean tsunami. The magnitude of this shock, combined with its unanticipated nature and quasi-experimental variation in exposure within small areas, are very well suited for the identification of the causal effect of disaster exposure on short- and long-term entrepreneurial outcomes. Understanding these effects are increasingly important to policymakers given the increasing pressure of environmental shocks due to climate change, but there are seldom other contexts as opportune for estimating disaster exposure effects.

Drawing from rich longitudinal survey data, the Study of the Tsunami Aftermath and Recovery, I find that those most exposed to the tsunami suffer large declines in business ownership, profits, and capital holdings that persist through the decade after the tsunami. The destruction of agricultural businesses implies that those in rural areas faced more severe consequences, much of which may be permanent given salt inundation intro agricultural fields. Within communities exposed to the heaviest damages, those with the least amount of resources before the tsunami were the least capable of restoring entrepreneurial activity through the long-term. Likewise, individuals displaced from their original homes by the tsunami fared worse in entrepreneurial activities, particularly in the immediate aftermath of the tsunami. Understanding the magnitude of these effects and the groups that are most at-risk is paramount to crafting an appropriate policy response.

The large *Build Back Better* reconstruction program after the tsunami proved to support some level of entrepreneurial reconstruction in the medium- and long-term. Receipt of housing aid from this program led to increased non-agricultural business ownership and subsequent gains in real profits. The positive effects on non-agricultural business ownership amount to approximately 40% to 70% of the initial damages on this sector after the tsunami, offering evidence that policy responses directed at improving the living conditions of those that faced the most damages can also spur gains in supporting the establishment and growth of smaller firms.

While drawing welfare conclusions are complicated by the competing dynamic of wage labor and entrepreneurial activities, business ownership was highly prevalent at 66% of our sample prior to the disaster. The real profits that own-account work generated prior to the tsunami offered approximately 12 months of per-capita expenditures to the households, and so the large declines I estimate impose large constraints on the ability of individuals to maintain their prior levels of consumption. While the housing aid offered in *Build Back Better* yielded some growth in non-agricultural businesses, I do not find any benefit for agricultural businesses. Moreover, even with the aid effort, those exposed to heavy tsunami damage still experienced decreased rates of non-agricultural business ownership, lower real profits, and diminished business capital through the long-term. Consequently, if policymakers seek to restore prior entrepreneurial outcomes, recovery aid should be tailored more specifically towards rebuilding the business capital stock of exposed individuals. This holds particularly true for those from rural areas that previously relied on agricultural work—the changed economic landscape left rural farmers with far worse entrepreneurial outcomes through the decade after the tsunami.

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Appendix

A1. Construction of Tsunami Exposure Measure

Given the extreme severity and wide-reaching consequences of the 2004 tsunami, it is complicated to constitute exposure in this setting. The common measure used in studies employing STAR data stratifies individuals into 3 categories that classify their exposure levels to the tsunami damages: those living in communities not directly affected, moderately damaged, or heavily damaged. In this paper, the trajectories for those not directly affected were similar to those facing moderate damages, so I pooled the two less exposed categories into the control group to increase power.

The exposure measure was calculated primarily using satellite imagery data from NASA's Moderate Resolution Imaging Spectroradiometer (MODIS). Comparing satellite images of the land in the week before the tsunami to images collected 3 days after the tsunami, we split the full area of the study site into 0.6 km^2 blocks in which we then calculated the proportion of land cover that was damaged to bare earth. We then supplemented these calculations with damage estimates from remotely sensed imagery that was prepared by the USGS, USAID, Dartmouth Flood Observatory, and the German Aerospace Center (Gillespie et al., 2009). Lastly, within each community, responses from interviews with local leaders and survey field supervisors provided their own assessments of destruction (Frankenberg et al., 2011).

A2. Construction of Local Price Index

In the context of a severe natural disaster, market distortions resulting from the damage and changed economic circumstances will lead to relative price changes (Hallegatte and Przyluski, 2010). This price effect, especially on the price of food, can have the most severe implications to the most impoverished families (Ferreira et al., 2013; Frankenberg et al., 2003, 1999). For example, prior studies have documented direct relationships between disaster levels of an area and the price of rice, including a study focused on the 2004 tsunami (Del Ninno et al., 2003; Kirchberger, 2017). Consequently, we argue that it is imperative to consider price fluctuations across communities for our analysis.

Given the non-random cross-sectional market price distortions caused by tsunami exposure, we exploit one of the most unique aspects of the STAR data: the availability of quality price data collected at markets across the geographic areas our sample covers. Within each community, informants listed the most used shops and markets by members of this community. Then, enumerators collected prices and units for 70 major food items from up to three of these local markets within each community. The prices were then standardized by weight. For non-food prices, three community expert informants in each community provided prices of 27 other major household expenses, including electricity, fuel, transportation, health center costs, rent, and clothing. These prices were again standardized to the same units. Finally, the median price for each of these 97 goods was taken at the district-damage level.

Given the median prices of these 97 items, we then calculate price indices using the expenditure shares from the household survey for respondents in that community. We normalize this index to have median value 100 in the 1-year post-tsunami wave.

Note that as the baseline survey was part of the SUSENAS surveys, they did not collect price data following the methodology discussed above. However, Statistics Indonesia does compute a consumer price index for the three cities nearest to most of our households—Banda Aceh (capital of Aceh), Lhokseumawe, and Sibolga. Across these cities, pre-tsunami prices were almost identical. Assigning each household of the STAR surveys to their nearest city, we then define baseline prices such that the ratio between our prices 1-year after the tsunami and at baseline are equal to the ratio of the Statistics Indonesia prices over the same period.

Table A1: SUSENAS Pre-Exposure Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Age						Highest Completed Education				
	Male	0-14	15-29	30-49	50+	Urban	Consumption	None	Primary	Junior High	Senior High	Tertiary
$2003 \times T_i$	-0.004	-0.024	0.041	-0.029	0.012	-0.001	-27.119	-0.036	-0.007	0.000	0.025	0.017
	(0.023)	(0.033)	(0.033)	(0.024)	(0.030)	(0.046)	(48.208)	(0.040)	(0.029)	(0.035)	(0.040)	(0.036)
$2004 \times T_i$	-0.005	-0.008	0.021	-0.042	0.028	-0.034	-8.601	0.001	0.014	-0.000	-0.024	0.009
	(0.020)	(0.032)	(0.028)	(0.020)	(0.025)	(0.036)	(42.728)	(0.037)	(0.029)	(0.030)	(0.041)	(0.033)
F-Statistic	0.030	0.487	0.855	2.094	0.817	0.455	0.213	0.860	0.437	0.000	1.248	0.159
P-value	0.971	0.615	0.426	0.124	0.442	0.635	0.808	0.424	0.646	1.000	0.288	0.853
Kecamatan Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographical Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41,935	41,935	41,935	41,935	41,935	41,935	12,572	41,935	41,935	41,935	41,935	41,935
R-squared	0.002	0.009	0.007	0.007	0.012	0.672	0.279	0.039	0.030	0.016	0.062	0.033

Notes. Results of estimating $X_{it} = \beta_0 + \beta_t T_i + \xi_t + \gamma W_{it} + u_{it}$. The sample are SUSENAS respondents from Aceh and North Sumatra in 2002 through 2004. Estimates of β_t in 2003 and 2004 reported (2002 baseline), as well as the F-statistic and p-value of the test that β_t in 2003 and 2004 are jointly equal to 0. Regression includes *kecamatan* fixed effects, elevation (quadratic) of the respondent's desa, and distance to the coastline (quadratic) of the respondent's desa. Standard errors clustered at the desa-level. Unit of observation is individual in all columns except for Column 6 (square root consumption), which is measured at the household-level.

Table A2: Exposure Effects on Entrepreneurial Outcomes

	(1) Owns Business	(2) Owns Agr. Business	(3) Owns Non-Agr. Business	(4) Square Root Total Profit	(5) Square Root Business Assets
Exposure Effects					
1 Year × T_i	-0.068	-0.025	-0.103	-18.224	-15.096
	(0.010)	(0.009)	(0.008)	(1.581)	(1.444)
2 Years × T_i	-0.061	-0.095	-0.047	-14.973	-20.202
	(0.011)	(0.011)	(0.012)	(2.001)	(1.487)
3 Years × T_i	-0.085	-0.163	-0.028	-9.818	-24.301
	(0.013)	(0.011)	(0.012)	(2.089)	(2.000)
4 Years × T_i	-0.097	-0.129	-0.053	-7.734	-24.566
	(0.012)	(0.011)	(0.012)	(2.090)	(1.904)
5 Years × T_i	-0.114	-0.140	-0.070	-11.330	-25.525
	(0.012)	(0.011)	(0.012)	(2.032)	(2.174)
10 Years × T_i	-0.095	-0.102	-0.082	-10.116	-30.464
	(0.013)	(0.012)	(0.013)	(2.115)	(2.159)
Trajectories for Non-Damaged					
Constant (baseline)	0.669	0.454	0.323	62.299	66.541
	(0.006)	(0.005)	(0.005)	(0.804)	(0.884)
1 Year	0.513	0.252	0.311	51.591	48.533
	(0.004)	(0.003)	(0.003)	(0.453)	(0.424)
2 Years	0.664	0.411	0.359	62.344	53.306
	(0.004)	(0.004)	(0.004)	(0.490)	(0.442)
3 Years	0.635	0.415	0.324	58.952	54.850
	(0.004)	(0.004)	(0.004)	(0.532)	(0.549)
4 Years	0.606	0.387	0.299	61.141	56.496
	(0.004)	(0.004)	(0.004)	(0.515)	(0.558)
5 Years	0.636	0.441	0.295	66.962	65.581
	(0.004)	(0.004)	(0.004)	(0.508)	(0.598)
10 Years	0.646	0.436	0.322	74.602	72.233
	(0.004)	(0.004)	(0.004)	(0.578)	(0.677)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Interview Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	104,663	104,663	104,663	62,554	70,271
R-squared	0.431	0.511	0.494	0.519	0.588
Individuals	15,853	15,853	15,853	13,159	13,783

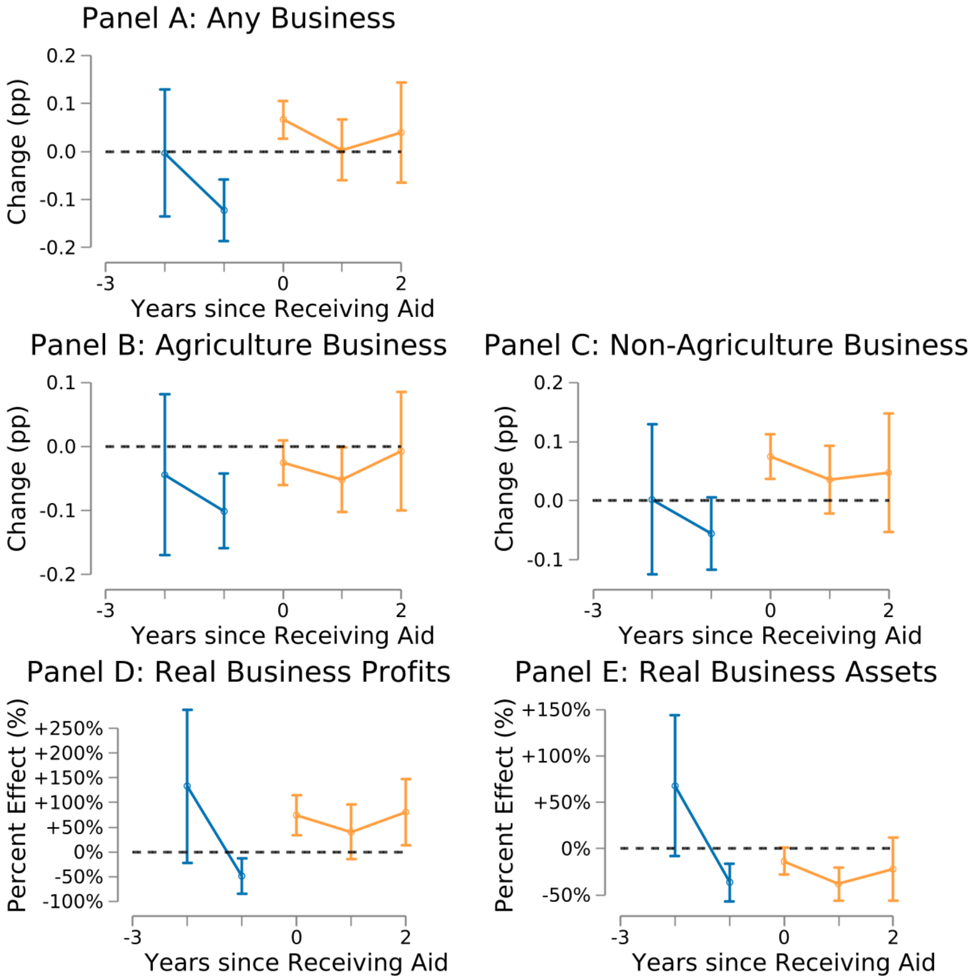
Notes. Results of estimating Equation 1. The exposure effects results reports β_t in each post-tsunami STAR wave. The trajectories for non-damaged are the control trajectories, computed from ξ_t. Standard errors clustered at the person-level. Columns 4 and 5 use real profit and business asset values, conditional on owning any business (in Column 4) or having non-zero business assets (Column 5).

Table A3: Heterogeneity Analysis across Educational Attainment on Business Ownership

	(1)	(2)
	Educational Attainment	
	High	Low
Constant (baseline)	0.613	0.731
	(0.008)	(0.009)
1 Year × T_i	-0.060	-0.105
	(0.012)	(0.016)
2 Year × T_i	-0.052	-0.085
	(0.015)	(0.018)
3 Year × T_i	-0.065	-0.132
	(0.016)	(0.021)
4 Year × T_i	-0.074	-0.134
	(0.016)	(0.019)
5 Year × T_i	-0.087	-0.157
	(0.016)	(0.020)
10 Year × T_i	-0.074	-0.137
	(0.017)	(0.021)
Observations	54,879	47,041

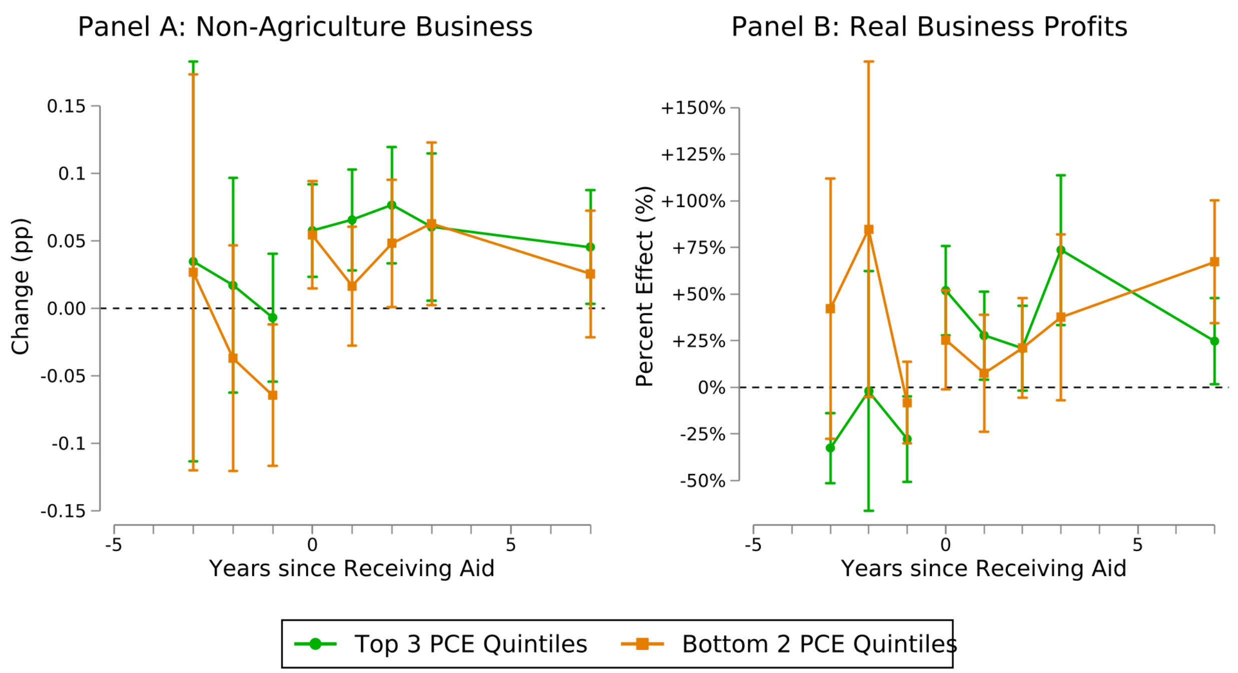
Notes. Results of estimating Equation 1 separately by education. High education individuals are those with at least 9 years of educational attainment at baseline (completion of junior secondary school). All regressions include individual and interview month fixed effects, with standard errors clustered at the individual level.

Figure A1: Housing Aid Effects on Entrepreneurial Outcomes, Conditional on Receipt



Notes. Estimates of $\widehat{\delta_k}$, the effect of housing aid on entrepreneurial outcomes as shown in Equation 7 across pre- and post-treatment periods. $t = 0$ is the first period in which the individual received housing aid. Point estimates and 95% confidence intervals are displayed. ATTs are estimated via an estimator proposed by Callaway and Sant'Anna (2020) that yields unbiased estimates accounting for individual and time fixed effects in the presence of heterogeneous treatment effects. Asymptotic standard errors are estimated using Influence Functions which account for clustering at the individual-level. Square root transformation is applied to business profits and assets, after deflating to real values using the local price index series. Panels D and E report percent effects relative to those that have not yet received housing aid in that period, with standard errors computed via a bootstrap procedure clustering at the individual-level. Panel D conditions on ownership of any business, and Panel E conditions on having non-zero business assets. The sample is limited to the set of individuals that received housing aid at some point in the study, and the time period is limited to post-tsunami waves.

Figure A2: Heterogeneity in Housing Aid Effects by Pre-Tsunami Per-Capita Expenditures



Notes. Estimates of $\widehat{\delta_k}$ separately for those from the top 60% and bottom 40% of pre-tsunami baseline household pre-capita expenditure. $t = 0$ is the first period in which the individual received housing aid. Point estimates and 95% confidence intervals are displayed. ATTs are estimated via an estimator proposed by Callaway and Sant'Anna (2020) that yields unbiased estimates accounting for individual and time fixed effects in the presence of heterogeneous treatment effects. Asymptotic standard errors are estimated using Influence Functions which account for clustering at the individual-level. Square root transformation is applied to business profits, after deflating to real values using the local price index series. Panel B reports percent effects relative to those never receiving housing aid or those that have not yet received housing aid in that period, with standard errors computed via a bootstrap procedure clustering at the individual-level. Panel B conditions on ownership of any business. The sample is limited to the set of individuals from *kecamatan* at the pre-tsunami baseline where at least one community faced heavy tsunami damages, and the time period is limited to post-tsunami waves.

Table A4: Heterogeneity Analysis of Housing Aid Effects

	(1)	(2)	(3)	(4)
	Square Root Real Business Profits			
	Urbanicity		Education	
	Urban	Rural	High	Low
Post-Treatment Aid Receipt Effects				
Period of Aid Receipt	33.765	3.825	16.617	6.897
	(6.065)	(2.040)	(3.721)	(2.428)
1 Year after Aid Receipt	11.572	3.726	4.706	6.774
	(5.161)	(2.695)	(3.808)	(3.180)
2 Years after Aid Receipt	15.425	3.256	10.284	4.433
	(6.492)	(2.317)	(3.901)	(2.748)
3 Years after Aid Receipt	30.696	10.320	21.653	8.296
	(8.854)	(3.612)	(5.869)	(4.158)
Aid Receipt Effect in Long-Term	4.083	13.126	6.927	13.684
	(5.994)	(2.399)	(3.869)	(2.796)
Pre-Treatment Trends				
1 Year before Aid	-17.380	-4.366	-9.127	-5.401
	(10.871)	(2.668)	(4.987)	(3.146)
2 Years before Aid	-16.525	12.072	3.884	10.085
	(10.842)	(7.398)	(11.871)	(8.039)
3 Years before Aid	-0.082	-1.304	-11.059	3.864
	(4.854)	(4.495)	(4.570)	(5.988)
Observations	5,415	12,857	9,110	8,672

Notes. Results of estimating Equation 7 separately by baseline subpopulation groups, with square root real business profits as the outcome. The first two columns separately estimate for those from rural or urban areas at baseline, and the latter two by educational attainment. High education individuals are those with at least 9 years of educational attainment at baseline (completion of junior secondary school). All regressions include individual fixed effects, with standard errors clustered at the individual level. Business profits are conditional on ownership of a business in that wave, with a square root transformation applied after deflating to real values using the local price index series. ATTs estimated via an estimator proposed by Callaway and Sant'Anna (2020) that yields unbiased estimates, even in the presence of heterogenuous treatment effects. Asymptotic standard errors in parenthesis, estimated using Influence Functions which account for clustering at the individual-level.