Saving Lives with Pre-arranged Disaster Aid: Evidence from Mexico

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Abstract

Developing economies are not disproportionately exposed to natural disasters, but they experience significantly more deaths. Exploiting a discontinuity in the eligibility rules to Mexico's pre-arranged disaster fund (Fonden), I show that accelerated reconstruction of public infrastructure can fully reduce postdisaster excess mortality in the short-run and up to 75 percent two years after. Fonden's impact is concentrated in areas with medical infrastructure and among conditions responsive to basic and freely available medical care. These findings suggest that Fonden operates by restoring access to health services. I also show that Fonden is cost-effective, and its benefit-cost ratio is at least 4.9. (JEL: G22, H12, H84, Q54, I15, O18.)

In the last century, there have been 10,514 hydrometeorological events recorded and 8.3 million deaths that can be directly attributed to these events (CRED, 2020). The actual death toll is likely to be far larger as these records fail to account for indirect deaths, such as deaths attributable to the prolonged disruption of health and social services. While there are no global estimates of indirect deaths, evidence from Hurricane Maria in Puerto Rico indicates that total deaths could be more than 70 times larger (Kishore et al., 2018). One striking feature of the global distribution of deaths from disasters is that it is strongly skewed toward developing economies despite disasters not occurring disproportionately in these countries (Kahn, 2005; Strömberg, 2007).

A possible contributing factor to the larger death toll in developing economies is the bottlenecks in the disbursement of disaster relief, which results in delays in the restoration of critical services, including roads, safe water, and medical infrastructure. Two common sources of delays and leakage in the disbursement of disaster relief are reliance on postdisaster financing and lack of rules and administrative capacity to disburse these resources (Clarke and Dercon, 2016).

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In this paper, I study Fonden Mexico's pre-arranged disaster aid fund. This federal program insures public infrastructure and low-income housing against natural disasters. Fonden overcomes bottlenecks in the delivery of disaster relief by instituting and implementing a plan for disaster response. This plan has two key features. First, Fonden guarantees the availability of reconstruction funds before a disaster, using a protected budget allocation, excess loss reinsurance, and catastrophe bonds. Second, Fonden enforces a rules-based system for the disbursement of reconstruction funds. The rules define the process to verify a disaster's occurrence (primarily using indexes), conduct damage assessments, disburse funds, and audit reconstruction projects.

To study whether Fonden saves lives relative to the discretionary disaster relief efforts of local governments, I calculate for every municipality (the administrative unit below the state) the difference in the mortality rate before and after a disaster, at four-month intervals for two years. I then take advantage of the Fonden rules for disaster eligibility to derive causal estimates of the impact of Fonden on postdisaster excess deaths. Specifically, a municipality that experiences a hydrometeorological event (heavy rainfall, flooding, or a tropical cyclone) is eligible for Fonden resources under the heavy rainfall rule if rainfall exceeds a predetermined threshold. This rule is the key to the research design because it allows me to estimate the impact of Fonden by comparing those municipalities that were barely eligible to those barely ineligible. Because municipalities can also become eligible to Fonden by meeting the tropical cyclone or flooding criteria (which I do not observe), I derive estimates of Fonden impact using a fuzzy regression discontinuity design.

My results show that Fonden led to a large and persistent decline in postdisaster excess mortality. I find that eight months after a disaster, municipalities barely eligible to Fonden experience a reduction of 0.81 excess deaths per 1,000 person-years relative to those municipalities barely ineligible. This reduction implies that Fonden can entirely reduce postdisaster excess mortality over this period. Subsequent estimates of the impact of Fonden over longer time horizons reveal that this reduction is largely permanent and not the result of mortality displacement (harvesting). For example, the estimate of Fonden's impact two years after a disaster indicates that the program reduced postdisaster excess mortality by 75 percent.

I consider and test several not mutually exclusive mechanisms for the Fonden led decline in mortality. These mechanisms include Fonden's rapid reconstruction allowing for risk reduction in traffic accidents, self-harm and violence, epidemics, and disruptions in access to health services. I also test for an indirect income channel where Fonden curbs mortality by accelerating economic recovery.

Several pieces of evidence suggest that the primary mechanism for the decline in mortality is the restoration of access to health services. Briefly, I find that the Fonden led reduction in

postdisaster excess mortality is driven by non-communicable conditions responsive to basic medical care for which treatment and medications are freely available through Mexico's public health insurance program. This finding provides supporting evidence for the mechanism because access to these life-saving services and drugs in the aftermath of a disaster hinges only on Fonden's ability to quickly restore access and not on the capacity of households to pay for medical services. Also consistent with the mechanism, I show that the Fonden led reduction in postdisaster excess mortality is concentrated among municipalities where public medical infrastructure was widely available before the disaster.

This pattern of results is unlikely to be explained by an income channel. Specifically, I find that municipalities with more public medical infrastructure experience the same Fonden led economic recovery (measured by night lights) as municipalities with less public medical infrastructure. Moreover, I also find that municipalities that experienced differential Fonden led economic growth effects don't experience differential reductions in mortality.

Last, I also find that Fonden only reduces the mortality of adults 50 years or older. This age gradient in the reduction of mortality is also consistent with the mechanism because older adults have a higher prevalence of non-communicable conditions for which access to basic health services can considerably curtail mortality.

Building on the estimates for adults 50 or older, I use a back-of-the-envelope calculation to show that Fonden saves 18,104 lives per year, at a cost of $43,366 (constant 2010 international dollars) per life. To put this cost figure in perspective, I then show, using the most conservative estimate of the value of a statistical life in Mexico ($210,880), that the benefit of Fonden is at least 4.9 times larger than its cost. I also compare Fonden's cost to the cost of saving lives through other disaster preparedness interventions reviewed by Tengs et al. (1995) and conclude that Fonden is fairly cost-effective.

My findings are closely related to the literature on the health effects of natural disasters. This literature describes the relationship between hazards and deaths and documents how the intensity of the hazard or the country's characteristics (e.g., income level, democracy, institutional quality, income inequality) moderate this relationship (Toya and Skidmore, 2007; Kahn, 2005; Strömberg, 2007; Kellenberg and Mobarak, 2008; Yang, 2008; Bakkensen and Mendelsohn, 2016). This paper makes four contributions to this literature. First, the paper provides the first causal estimates of a pre-arranged disaster aid program on mortality. It thereby moves the discussion forward from analyzing the determinants of mortality risk from natural disasters to quantifying the effects of a replicable and scalable policy capable of considerably mitigating this risk. Second, the paper shows that even in an upper-middle-income country, heavy rainfall events can exert a significant death toll in the absence of disaster relief. Third, the paper provides supporting evidence identifying disruptions in

access to health services as one of the primary mechanisms for disaster-related deaths. This result is in line with the findings of Kishore et al. (2018), who shows that one-third of the deaths caused by Hurricane Maria in Puerto Rico resulted from delayed or interrupted access to health care. It is also consistent with recent work in Mexico showing that access to public health insurance can considerably reduced deaths from temperature shocks (Cohen and Dechezleprêtre, forthcoming). Fourth, the paper highlights that the risk from disaster-related death varies with age in the absence of disaster relief and reveals that those 50 or older are at the highest risk. This finding extends those of Cohen and Dechezleprêtre (forthcoming) who finds that adults 55 and older are at the highest risk of death from temperature shocks by showing that this group is also at the highest risk from hydro-meteorological shocks.

My findings also speak to the literature on the provision and effectiveness of disaster transfers. Earlier work in this literature has shown that developing economies routinely fail to provide timely transfers (Noy and Nualsri, 2011), and highlights how the largely discretionary nature of the transfers makes their provision dependent on media coverage and democratic institutions (e.g., Besley and Burgess, 2002; Eisensee and Strömberg, 2007).

A more recent strand of the literature shows that pre-arranged disaster aid programs offer a promising model for the provision of disaster relief. In the case of cash grants provided to households immediately in the aftermath of disasters, a series of studies find that the grants reduce food insecurity and reliance on costly shock coping strategies, such as the selling of productive assets (Pople et al., 2021; Gros et al., 2019, 2020).

In the case of transfers used to accelerate the reconstruction of public infrastructure, the evidence is more scarce but points to significant returns to rapid rebuilding. Specifically, a closely related paper, Del Valle, de Janvry and Sadoulet (2020) uses the same research design as this study to investigate the impact of Fonden on economic recovery as measured by night lights. The paper finds that Fonden enables an accelerated economic recovery that lasts for one year. It also shows that while the benefit of the economic recovery is transitory, its value is large enough to offset the program's costs. Notably, the paper also finds that Fonden is shoring up shortcomings in the provision of public infrastructure. Specifically, the paper shows that worst-off municipalities that lack the infrastructure necessary to mitigate damage from extreme weather, such as storm drains, benefit disproportionately more from Fonden.

This paper makes two substantive contributions that extend the previous findings on Fonden. First, I show that the primary benefit of Fonden is saving lives despite the program not being designed for this objective. A conservative back-of-the-envelope calculation reveals that the value of the lives saved is several times greater than Fonden's benefit from economic recovery. Moreover, unlike Fonden's transitory effect on economic growth, its benefit from

saving lives is largely permanent over the two-year window that I study.

Second, the paper reveals that the ability of Fonden to curb disaster-related mortality hinges on its complementarity with public health infrastructure. This finding is important because it documents the mechanism through which Fonden operates and because it extends the literature on disaster resilience and the interaction of social safety nets programs. Recent work by Deryugina (2017) shows that resilience to tropical cyclones in the US stems from programs such as unemployment and disability insurance making up the shortfall in disaster relief. This paper broadens the literature by documenting a different type of interaction where increased resilience results from disaster relief enabling programs like public health insurance to continue operating after a disaster.

Another reason why the previous finding is important is that it highlights that the distribution of Fonden benefits depends on its interaction with public health insurance, which in Mexico is skewed towards generally worst-off municipalities. Accordingly, this finding also implies that more disadvantaged municipalities will benefit disproportionally more from Fonden. Taken together, this finding and the findings of earlier work on Fonden's economic benefits, bolster the idea that pre-arranged disaster aid can play an influential role in reducing health and economic inequalities.

The paper is organized as follows. Section 1 summarizes the relevant institutional details of Fonden. Section 2 describes the data. Section 3 presents the identification strategy and results. Section 4 provides supporting evidence on the identification assumptions and presents robustness checks. Section 5 concludes by discussing the findings and their possible policy implications.

1 Disaster Response in Mexico

The federal government created FONDEN in 1996 to insure public infrastructure and low-income housing against geophysical and hydrometeorological hazards. The primary type of infrastructure insured by Fonden included roads, hydraulic infrastructure (safe water), low-income housing, and educational and medical infrastructure. Fonden ceased operations in 2020 when the government diverted its funds to pandemic relief efforts. Since then, the responsibility of disaster response has devolved to state governments.

Two design features made Fonden distinct from other disaster response programs. First, Fonden relied on a financial plan to guarantee fund availability. The plan used the Fonden budget and reserves (0.4 percent of the federal budget ≈$800 million) to pay for small, frequent claims and relied on risk transfer instruments to pay for large, infrequent claims. These instruments included a $400 million excess loss reinsurance policy on yearly payouts

over $1 billion and several tropical cyclone and earthquake catastrophe bonds. In the unlikely event that expenditures exceeded the limits of the financial plan, the program could continue operating through an exceptional budget allocation from Mexico's Oil Surplus Fund (World Bank, 2012).

The second design feature was a set of rules for the verification and payment of claims. These rules resembled a parametric insurance contract that allowed for damage assessment when triggered. The Fonden process begins with a request for municipal-level disaster verification. State governments or line ministries with affected assets make these requests (lists of plausibly affected municipalities). In the case of hydrometeorological events, the requests are sent to Conagua (the National Water Authority). To verify eligibility, Conagua relies primarily on the heavy rainfall rule. This rule, introduced in 2004, establishes that a municipality is eligible for Fonden if, at any of the municipality's representative weather stations, daily rainfall is greater or equal to the percentile 90 of maximum daily rainfall for the month in which the event took place. In addition to the heavy rainfall rule, Conagua also uses the flooding and tropical cyclone criteria. Specifically, Conagua verifies the occurrence of flooding if it could corroborate that water was pooled in an area not normally submerged or that a body of water overflowed past its normal limits. Tropical cyclones are verified when sustained winds over 80 km/h are recorded.[1]

After the Conagua verification process is complete, the agency reports to SEGOB (Ministry of the Interior). SEGOB then verifies among the eligible that the disaster exceeded the local response capacity (almost always the case) and issues a disaster declaration in the Federal Register. The disaster declaration lists the municipalities that requested verification and the subset eligible for Fonden.

In the next step, a damage assessment committee, comprised of federal and state representatives, visits the municipalities listed as eligible in the disaster declaration. The committee issues a damage report that includes itemized reconstruction costs, geocoded photographic evidence of damages, and proposed reconstruction improvements to harden infrastructure. Fonden then goes on to verify that there is no duplication of efforts, that resources are not used to repair preexisting damage, and that cost-sharing provisions are being enforced.[2] The SHCP (the Ministry of Finance) is notified, and the disbursement of resources beings. In my sample, the time between disaster and disbursements has a mean of 75 days and a median of 38 days. In 2009, Fonden expedited the reconstruction process by approving disbursements for the reconstruction of critical infrastructure (including medical infrastructure)

[1] My weather dataset does not include Conagua's hydrological models or wind speed records, I am therefore only able to replicate the verification process using the heavy rainfall thresholds.

[2] State owned assets are subject to a cost-sharing provision by which Fonden provides only partial coverage (50 percent in most cases).

immediately after Conagua verified the occurrence of a qualifying event.

In the last step, several agencies, including federal agencies such as the SCT (the Ministry of Communication and Transportation), assume the responsibility of designing and contracting the reconstruction work. While these agencies must provide Fonden with regular progress reports, they can follow their operating procedures and hire third-party providers as needed. Planned reconstruction times have a mean of 149 days and a median of 137 days. In sum, the information on disbursement and planed reconstruction times indicate that, even without expedited disbursements, Fonden reconstruction projects should be completed on average within 7 to 8 months of the disaster.

In municipalities ineligible for Fonden, the reconstruction process depends on the discretionary efforts of state and municipal governments. Several pieces of evidence suggest that the reconstruction process was slower in these municipalities. First, consistent with the Fiscal Coordination Law, which regulates federal transfers, Del Valle, de Janvry and Sadoulet (2020) find no evidence of ineligible municipalities receiving additional resources from the federal government. In the absence of additional transfers, local governments were unlikely to have reconstruction resources. Anecdotally, interviews with Federal and State officials indicate that these municipalities relied on postdisaster financing. In particular local governments commonly relied on budget reallocations, which involve a lengthy administrative process. Second, the disbursement of reconstruction resources may have been further slowed down by other institutional features like the Public Works and Related Services Law, which limits direct procurement and requires some projects to be executed through a public tendering process. Third, consistent with these institutional features Del Valle, de Janvry and Sadoulet (2020) shows that the economic recovery of ineligible municipalities (relying on discretionary postdisaster financed reconstruction) lagged for over one year relative to the economic recovery experienced by Fonden eligible municipalities.

2 Data

To study the impact of Fonden on excess deaths for up to two years after a disaster, I calculate the annualized mortality rate difference (AMRD) at four-month intervals. Specifically, I compute the difference in the mortality rate 4 months after a disaster with the same 4 month period's mortality rate two years before the disaster. I then repeat this exercise using windows of 8, 12, 16, 20, and 24 months.

I construct the mortality dataset in three steps. In the first step, I aggregate the 2000-2017 mortality records from Mexico's national statistical institute (INEGI, 2017) to compute the count of deaths by municipality of residency and date of death (month-year). The counts

include all certified deaths, even if recorded with a five-year lag or in a place different from the deceased municipality of residency. All deaths are likely to be recorded. Mexico follows World Health Organization reporting standards since the 1950s and routinely ranks in the top 20 countries with high-quality mortality records (Mathers et al., 2005).

In the second step, I sum the number of deaths in the subsequent 4, 8, 12, 16, 20, and 24 months for every municipality and month. I then use these counts to calculate all-cause annualized mortality *rates* (AMR) by dividing the counts by the number of person-years at risk. In the cases where the population is at risk of dying for a duration different from one year, the denominator of the mortality rate is given by the municipal-year population multiplied by the fraction of the year that the population was at risk. For example, in the case of the 4-month AMR, the denominator is equal to the municipal-year population times one-third. The population data comes from Mexico's Population Council municipal-year projections (CONAPO, 2012, 2019). All rates are reported as deaths per 1,000 person-years.

In the third step, for every AMR window (4, 8, 12, 16, 20, and 24 months), I calculate the annualized mortality rate *difference* (AMRD) by subtracting the corresponding AMR (same window) from two years before. This setup guarantees, for all window lengths, that the baseline (subtrahend) AMR is computed using only predisaster data. Another attractive feature of this setup is that it allows me to calculate the AMRD from AMRs that occur in the same calendar months. This feature of the calculation is important because deaths in Mexico display seasonal but stable trends. Because it is also common practice to define the baseline AMR as the mean of the corresponding AMR's (same window) over several predisaster years, I show in section 4 that the paper's findings are not sensitive to the choice of baseline AMR.

Next, using an analogous three-step procedure, I calculate AMRD's that are cause-specific, age-specific, sex-specific, and age-sex adjusted (standardized). The cause-specific categories computed include transport injuries, self-harm or interpersonal violence, communicable, non-communicable, non-communicable amenable, and non-communicable amenable covered by public health insurance. The mapping between these categories and the 10th revision of the International Statistical Classification of Diseases and Related Health Problems (ICD-10) codes reported in the mortality records are given by GDB (2018). The definition of non-communicable amenable conditions is given by Kruk et al. (2018). The definition of conditions covered by public health insurance is provided by Ministry of Health (2007). To calculate cause-specific AMRD's, I modify step 1 and sum over the deaths attributable to the ICD-10 conditions that match each category. All other steps are as previously described. To calculate age-specific and sex-specific AMRD's, I modify steps 1 and 2 so that the calculation only includes information from a given age or gender group. The age groups used

are 0-14, 15-49, and 50 or older. To calculate the standardized AMRD, I modify step 2 and calculate a weighted average of the previously derived age-sex-specific AMR. The weights correspond to the proportion of the age-sex group in the 2000 population.

To perform falsification exercises, I modify step 3 and recalculate all the AMRD's previously described using only pre-disaster data. Specifically, I take the difference between the AMR's windows (4, 8, 12, 16, 20, and 24 months) from two years before the disaster and subtract the corresponding AMR (same window) from four years before.

Next, I use the municipal month-year dataset assembled by Boudreau (2015), from disaster declarations published in Mexico's Federal Registry between 2004 and 2012. This dataset contains all municipalities plausibly affected by hydrometeorological events (heavy-rainfall, flooding, and tropical cyclones), that is, all municipalities that requested Fonden and the subset that became eligible. I then replicate for this sample the Fonden verification process under the heavy rainfall rule. To this end, I use weather station identifiers to combine daily rainfall records from Conagua (2015*a*), with the thresholds used for verification Conagua (2015*b*), and the mapping between municipalities and weather stations (Conagua, 2015*c*). Using the resulting merged dataset, I then calculate the normalized running variable by subtracting the Fonden thresholds from observed rainfall. Municipalities where rainfall minus threshold is greater or equal to zero are eligible for Fonden under the heavy rainfall rule. Because the heavy rainfall rule is satisfied by crossing the threshold at any weather station on any day, I report the running variable's maximum in cases where municipalities have multiple weather stations or when events span multiple days.

Last, using municipal and month-year identifiers, I merge the dataset previously derived with the mortality dataset. The resulting dataset includes 2,742 municipal-month-year requests for Fonden, of which 1,949 were deemed eligible for funding.[3] To provide supporting evidence for the identifying assumptions and to explore Fonden's channels, I merge this dataset with several other municipal-year level datasets. These include data on public medical infrastructure from the SSA (Ministry of Health), Fonden administrative records, census, and other datasets from INEGI (the National Institute of Statistics and Geography). The complete list of additional datasets and sources is presented in table A6.

Table 1 provides the summary statistics of key variables. The main outcome variable is the all-cause AMRD. I present results for the 8-month AMRD because, as discussed in the next section, Fonden leads to the largest reduction in mortality during this period. The table shows that on average there are 0.35 excess deaths per 1,000 person-years after a disaster, and that the events in the sample are of a considerable magnitude. According to the

[3] I exclude from the analysis 229 observations where the weather station was out of service and 16 observations for which I have no mortality information.

United Nations World Meteorological Organization, heavy rainfall occurs when precipitation exceeds 50 millimeters (mm) over 24 hours. The events in the sample generally fall under the international heavy rainfall definition with an average of 82 mm or approximately 3.2 inches (in) over 24 hours. The table also shows that the Fonden thresholds have an average of 89 mm, and the support of the running variable spans from -219 to 297 mm. Municipalities where the running variable is greater or equal to zero qualify for Fonden under the heavy rainfall rule. In the sample, 36 percent of the municipalities that request Fonden qualify under the heavy rainfall rule, and 71 percent qualify through one of the three rules (heavy rainfall, flooding, tropical cyclone).

3 Results

To estimate the causal impact of Fonden on mortality, I use a fuzzy regression discontinuity design (FRD) that exploits the discontinuous change in Fonden assignment at the heavy rainfall threshold. As previously mentioned, I use a fuzzy design because Fonden requires the disaster to exceed local response capacity (which is almost always the case) and because the flooding and tropical cyclone criteria (which I do not observe) are also used to verify eligibility.

This design allows me to estimate Fonden's ITT under the assumption that the characteristics of municipalities (observed and unobserved) vary smoothly with the running variable at the threshold (continuity assumption). In subsection 4.1, I provide supporting evidence for the continuity assumption by showing that a wide range of predetermined municipal characteristics are continuous at the threshold.

The FRD design also allows me to estimate Fonden's LATE under three additional assumptions (Hahn, Todd and Van der Klaauw, 2001). These assumptions include (i) monotonicity (ruling out local defiers) which given the Fonden allocation process seems plausible; (ii) the existence of a first stage, which is demonstrated in this section; (iii) and local independence (LI). Dong (2018) has recently shown that the local smoothness (LS) assumption can be used instead of the LI assumption. In my application, both the LI and the LS assumptions are likely to hold. Following Dong (2018) I provide supporting evidence for both assumptions. Specifically, in subsection 3.3, I provide supporting evidence for LI by showing that the treatment effect derivative is small and statistically indistinguishable from zero. In subsection 4.1, I provide supporting evidence for LS by showing that the empirical density of the running variable is continuous at the threshold.

3.1 Fonden reduces post-disaster excess mortality

Figure 1 illustrates the FRD design. Panel A plots the first stage, and panel B the ITT. In each panel, the solid blue lines are fourth-order global polynomials fits. These lines are estimated separately on each threshold side and provide a smooth representation of the underlying conditional expectation. The red circles represent the local mean of the outcome over disjoint bins of the running variable. The green error bars represent 95 percent confidence intervals for the local means. Given the relative sparsity of observations far from the threshold, I use quantile space bins constructed to have a similar number of observations. The number of bins is chosen to minimize the integrated mean square error of the underlying regression function as described in Calonico, Cattaneo and Titiunik (2015).

In Panel A, the outcome is the probability of receiving Fonden. The figure reveals a clear first stage, with the likelihood of receiving Fonden increasing from 0.65 just below the threshold to 0.87 just above. This sharp jump implies that Fonden's LATE will be approximately 4.5 times larger than the ITT. In Panel B, the outcome is the 8-month AMRD, that is, the difference between the 8-month AMR post-disaster and the same rate computed two years before the disaster. AMRD values greater than zero indicate that a municipality experienced excess deaths after a disaster. I use the 8-month AMRD for the figure because, as shown in table 2 this is the window when Fonden leads to the most considerable reduction in excess mortality. The figure reveals a clear downward jump at the threshold and highlights that municipalities ineligible for Fonden (under the heavy rainfall rule) experience progressively larger increases in post-disaster excess mortality, with municipalities immediately to the left of the threshold experiencing roughly 0.79 excess deaths per 1,000 person-years. By comparison, municipalities eligible for Fonden, immediately to the left of the threshold, don't experience excess deaths.

Next, I derive estimates of the first stage, the ITT, and the LATE. Specifically, I estimate the following equation:

$$\mathbf{Y}_{mt} = \alpha + \beta Above_{mt} + g(R_{mt}) + \varepsilon_{mt}, \tag{1}$$

where Y_{mt} is the outcome variable observed in municipality m at disaster time t. When estimating the first stage, the outcome is a binary variable equal to 1 when a municipality receives Fonden and 0 otherwise. When estimating the ITT, the outcome is one of the AMRD. The function $g(R_{mt})$ models the relationship between the outcome and the running variable R_{mt} (rainfall minus threshold). $Above_{mt}$ is an indicator variable equal to 1 when the running variable is greater or equal than zero. In each case, the parameter of interest is β. I also compute the ratio of the ITT to the first stage to recover Fonden's LATE.

Equation 1 is estimated using nonparametric local polynomial methods. Unless otherwise noted, I use the h_{MSE} bandwidth selection algorithm, which minimizes the asymptotic mean squared error. This algorithm is optimal for point estimation (Calonico et al., 2019). Consistent with this choice, I use a triangular kernel because it provides optimal weights for the h_{MSE} (Cattaneo, Titiunik and Vazquez-Bare, 2017). Last, I follow the recommendations of Gelman and Imbens (2019) and use linear and quadratic local polynomials. In the appendix, I show that my results are robust to the choice of these tuning parameters. In all cases, the bandwidth selection algorithm and the inference of standard errors and confidence intervals are adjusted for clustering at the municipal level.

Table 2 shows the impact of Fonden on the AMRD at 4-month intervals over two years following a disaster. The results highlight that Fonden led to a considerable and largely permanent reduction in post-disaster excess mortality. Panel A reports the first stage estimates. Consistent with the graphic depiction of the first stage, the estimates reveal that being just above the threshold increases the probability of receiving Fonden by 22 to 23 percentage points relative to the municipalities just below. In all cases, the coefficients are statistically significant at the one percent level.

Panel B columns 1 to 6 reports estimates of equation 1 when the dependent variable is one of the AMRD computed at 4, 8, 12, 16, 20, or 24 months. The ITT estimates highlight that Fonden leads to a swoosh-shaped decline in post-disaster excess mortality, with the most significant decrease observed for the 8-month AMRD. Specifically, in column 1, I find that the point estimate for the 4-month AMRD is suggestive of a Fonden led reduction in mortality. However, the coefficient is noisily estimated, and I cannot reject the null hypothesis that it is equal to zero.[4] In column 2, I find that for the 8-month AMRD, Fonden leads to a reduction of 0.82 excess deaths per 1,000 person-years. This reduction is substantial because the control mean (the no program counterfactual, left-hand-side prediction of the nonparametric regression at the threshold) is 0.79. Together these estimates indicate that eight months after a disaster, Fonden enables municipalities just above the threshold to reduce post-disaster excess mortality relative to those just below fully. The timing of this effect coincides well with the Fonden administrative records, which show that, on average, reconstruction projects are completed within 7 to 8 months of the disaster. Next, in columns 3 to 6, I find that the reduction in the AMRD is largely permanent, as I find limited evidence

[4]To see whether Fonden has an immediate effect, I also compute 1, 2, and 3 month AMRD's. I find no evidence of Fonden affecting any of these outcomes. The ITT estimates are: for the 1-month AMRD 0.09 (p-value 0.879), for the 2-month AMRD -0.19 (p-value 0.709), and for the 3-month AMRD -0.37 (p-value 0.35). The lack of an immediate effect is reasonable because, even in the case of expedited disbursements (approved immediately after verification), the reconstruction of damaged infrastructure may take several months.

of temporal death displacement in the two years after a disaster. To see why Fonden is not merely changing the timing of deaths, note, for example, that the 24-month AMRD includes all deaths reported between the disaster and the subsequent 24 months. If a compensating increase in fatalities in Fonden eligible municipalities followed the initial reduction observed at 8-months, I would fail to find an effect of Fonden when using the 24-month AMRD as the outcome. Instead, as shown in column 6, I find that at 24-months, Fonden led to a reduction of 0.33 excess death per 1,000 person-years, or a 75 percent reduction relative to the control mean.

Panel C reports estimates of the LATE. Columns 1 to 6 show that Fonden led to a reduction of between 2.3 and 3.7 excess deaths per 1,000 person-years among complier municipalities at the threshold. As in the case of the estimates of panel B, with the exception of the coefficient for the 4-month AMRD, all coefficients are statistically different from zero at the five percent level. Next, to provide supporting evidence for the validity of the FRD design, I test whether a null effect is recovered when I use AMRD outcomes that are unaffected by Fonden because they are computed using only pre-disaster data. The placebo row reports the p-value when the LATE is estimated using these outcomes. Reassuringly, in all cases, I find that the coefficients are statistically indistinguishable from zero at conventional levels. Overall, these results provide robust evidence of the capability of Fonden to reduce excess mortality in the aftermath of a disaster.

3.2 Channels

Given the type of reconstruction projects that Fonden undertakes and its capability to accelerate economic recovery (Del Valle, de Janvry and Sadoulet, 2020), there are several mechanisms through which the program is likely to operate. To begin pinning down the channels table 3 presents estimates of the impact of Fonden on 8-month cause-specific AMRD's. I focus on the AMRD's measured at eight months because Fonden's largest effect is observed at this time. First, given Fonden's focus on road reconstruction and the prevalence of transport injuries (deaths of drivers, riders, cyclists, and pedestrians), I test whether Fonden affects this type of death. As shown in column 1, Panels B and C, I find no evidence to support a Fonden led reduction in deaths due to transport injuries. Specifically, the control mean reveals no postdisaster uptick in this type of death among municipalities ineligible to Fonden. Moreover, Fonden's ITT and LATE estimates for this type of death are small and statistically indistinguishable from zero.

Second, I follow the literature on the impact of natural disasters on mental health (Satcher, Friel and Bell, 2007) and conflict (Xu et al., 2016) and test whether the restoration

of infrastructure and plausibly government presence can reduce deaths due to self-harm and interpersonal violence. As shown in column 2, I also fail to find evidence to support this mechanism.[5]

Third, given the risk of increased water-borne and vector-borne diseases in the aftermath of flooding (Ahern et al., 2005) and Fonden's support for the reconstruction of hydraulic infrastructure, I test whether Fonden reduces deaths caused by communicable diseases. Column 3 reports the results of this exercise. As in the previous cases, I find Fonden impact estimates that are small and statistically indistinguishable from zero. The lack of support for this channel is not surprising as subsequent work on epidemics following natural disasters (e.g., Watson, Gayer and Connolly, 2007) suggests that the risk of outbreaks is low unless the affected population is permanently displaced, which is unlikely in this context.

Fourth, I study whether Fonden's accelerated reconstruction of infrastructure (e.g., roads, safe water, and medical infrastructure) reduces deaths by helping restore access to health services. Because delays in diagnosis and treatment adversely affect survival from non-communicable conditions (Di Cesare et al., 2013), I begin by testing Fonden's impact on these conditions (Column 4). Consistent with this mechanism, panel B shows that municipalities eligible to Fonden considerably reduce non-communicable postdisaster excess deaths relative to ineligible municipalities.

Because some non-communicable conditions require advanced medical technology, which is not available everywhere, I study in column 5 the impact of Fonden on a subset of 34 non-communicable conditions that only require access to basic medical services for their diagnosis or treatment. These conditions are defined by Kruk et al. (2018), and I will refer to them as amenable conditions from here on. These conditions include, for example, ischemic heart disease and diabetes, which are leading causes of death in Mexico. The results from this exercise show that Fonden can fully mitigate postdisaster amenable excess deaths. Specifically, I find an uptick of roughly 0.5 postdisaster excess amenable deaths per 1,000 person-years among ineligible municipalities and a corresponding Fonden led decrease of the same magnitude among the Fonden eligible municipalities (ITT).

To pin down the mechanism, in column 6, I study Fonden's impact on a subset of 20 non-communicable amenable conditions whose medical diagnoses and treatment (including medications) are covered for free by Mexico's public health insurance (PHI) system. I will refer to these conditions as PHI-covered conditions from here on. For deaths caused by these conditions, I also find a sharply estimated reduction of 0.3 postdisaster excess deaths per 1,000 person-years or an 83 percent reduction relative to the control mean. This finding bolsters the idea that Fonden operates by restoring access to health services because access for

[5] I also fail to find evidence of impact when I test self-harm and interpersonal violence separately.

PHI-covered conditions depends only on the availability of services and not on the capacity of households to pay for these services (also possibly affected by the Fonden led economic recovery).

Next, to provide further evidence for the mechanism, I take advantage of the idea that if Fonden operates by restoring access to health services, it should disproportionately benefit municipalities with a higher prevalence of public medical services. To see why this is the case, consider that Fonden is not adding new public health infrastructure but merely restoring access in areas where it is already available. Moreover, because public health infrastructure in Mexico is disproportionately provided to disadvantaged areas, it is likely that in these generally worse-off municipalities, disruptions to health services will lead to considerably worst health outcomes and plausibly to larger benefits from quickly restoring access.

To explore how the effect of Fonden varies with the level of preexisting public medical infrastructure, I use DGIS (2003) data on the number of public doctors per capita. I then split the sample at the median (0.45) and estimate the impact of Fonden in each sub-sample.[6] Importantly, for the results causal interpretation, the number of public doctors is measured in 2003 and is shown not to change discontinuously at the threshold (figure 3).

Table 4 columns 1 and 2 present results from this exercise when the outcome is the amenable 8-month AMRD, columns 3 and 4 present the results when the outcome is the PHI-covered 8-month AMRD. The table shows that the Fonden led reduction in mortality is driven by municipalities where public medical infrastructure is widely available. For municipalities with above-median public doctors (columns 1 and 3), the control mean (panel B) reveals a clear uptick in postdisaster excess deaths. Estimates of the ITT for this same group further show that Fonden leads to a 0.98 amenable and 0.5 PHI-covered postdisaster excess deaths per 1,000 person-years. Taken together, these two estimates highlight that Fonden can fully offset the increase in excess deaths observed among ineligible municipalities. By comparison, for municipalities with below-median public doctors (columns 2 and 4), I find no evidence of Fonden's impacting postdisaster excess mortality. I additionally formally test for differential Fonden effects using the estimates of Fonden's LATE (Panel C). For both outcomes, I reject the null hypothesis of equal Fonden effects between municipalities with above and below-median public doctors per capita (p-values 0.008 and 0.031).

The finding of Fonden's effects concentrated on amenable and PHI-covered deaths, and the additional evidence indicating that areas with more public medical infrastructure drive these effects are consistent with the idea that Fonden operates by restoring access to health

[6] I also test for differential Fonden effects using other measures of medical infrastructure such as the number of health centers, doctor offices, or the number of beds. In all cases, I find very similar results suggesting that municipalities with above-median public health infrastructure benefit disproportionately more from Fonden.

services. Nonetheless, given Fonden's documented impact on economic recovery (Del Valle, de Janvry and Sadoulet, 2020), and the multiple channels through which economic growth can improve health outcomes (Cutler, Deaton and Lleras-Muney, 2006), I further study whether Fonden may operate through an income channel.

I conduct two additional exercises to see whether an income channel better explains the previous results. First, I test whether municipalities that drive the reduction in mortality (above-median public doctors) experience a differential economic recovery than those that don't experience a Fonden led decrease in mortality (below-median public doctors). Evidence of municipalities with above-median public doctors experiencing a more pronounced economic recovery would be indicative of Fonden also operating through an income channel. Because I don't observe economic activity at the municipal level, I follow Del Valle, de Janvry and Sadoulet (2020) and proxy the change in economic activity using night lights. Table A1 in the appendix columns 1 and 2 reports the results from this exercise. The setup and sample are identical to that of table 4 except for the use of the log difference in night lights as the dependent variable. Contrary to the idea that Fonden operates through an income channel, panels B and C show that Fonden has nearly identical effects on night lights for these two groups of municipalities. Accordingly, I fail to reject the null hypothesis of equal Fonden LATE's on economic recovery between municipalities with above and below-median public doctors per capita (p-value 0.799).

The second exercise tests whether municipalities that experience a more pronounced economic recovery experience a differential reduction in amenable and PHI-covered deaths than those that experience a more muted economic recovery. Specifically, I take advantage of Del Valle, de Janvry and Sadoulet (2020) finding that municipalities with below-median storm drain coverage, which lack the infrastructure to mitigate the damage from extreme weather events, experience a Fonden led economic recovery and order of magnitude larger than above-median municipalities. If Fonden reduces mortality through an income channel, I should also observe a differential reduction in excess mortality between municipalities with above and below-median storm drain coverage.

Following Del Valle, de Janvry and Sadoulet (2020) I measure storm drain coverage using census data on the percentage of dwellings connected to the sewer.[7] I then split the sample at the median (71.85) and estimate the impact of Fonden on each subsample. As in the previous case, the resulting estimates have a causal interpretation because storm drain coverage is measured before the disaster and because it does not change discontinuously at the threshold (figure 3). Table A1 in the appendix columns 3 and 4 reports the results from this exercise

[7]This variable is a good proxy because sewage and storm drains are generally constructed simultaneously and because combined sewage serves both purposes.

when the outcome is the amenable 8-month AMRD, and columns 5 and 6 report the results when the outcome is the PHI-covered 8-month AMRD. While the LATE point estimates are larger for the below-median sample, I find no evidence to support the idea of differential Fonden effects. Specifically, for both outcomes, I cannot reject the null hypothesis of equal Fonden's LATE's between municipalities with above and below-median storm drain coverage (p-values 0.741 and 0.413).

Overall, the evidence presented in this section indicates that the most likely channel for the observed reduction in mortality is the restoration of access to health services through the accelerated restoration of safe water and the reconstruction of roads and medical infrastructure. These findings are important because they highlight that investments in social safety nets programs such as disaster relief and public health insurance are complementary and together have the potential to curtail postdisaster mortality substantially.

3.3 Who's life is saved and at what cost?

To provide a more accurate valuation of Fonden's benefits, I study whether the program effects vary by gender and age. Specifically, I conduct analogous exercises this time using sex-specific and age-specific 8-month AMRD's as the dependent variables. The result are reported on Table 5. Columns 1 to 2 study the impact of Fonden by gender. The estimates reported in panels B and C reveal that Fonden reduces post-disaster excess deaths for males and females. While the point estimates tentatively suggest larger reductions for males, I cannot reject the null hypothesis of no differential gender effects (p-value 0.41) given the wide confidence intervals.

Columns 3 to 5 study the impact of Fonden among three age groups 0-14, 15-49, and 50 or older. The results in columns 3 to 4 show no effect of Fonden among younger groups. By comparison, the results in column 5 show that those 50 or older benefit disproportionately from Fonden. Using the estimates for the LATE, I further test and reject the null hypotheses of no differential Fonden effects between those 50 or older and both of the younger age groups (p-values 0.002 and 0.006). Because a higher prevalence of amenable conditions also characterizes those 50 or older, the finding of Fonden effects concentrated in this age group provides further supporting evidence for the mechanism described in the previous section.

The estimates of column 5 also highlight that the reduction in annual deaths created by Fonden is of a meaningful magnitude. Among complier municipalities at the threshold, panel C shows that Fonden leads to a 15.1 per 1,000 person-years reduction in post-disaster excess deaths for those 50 or older. To get a better sense of how many lives Fonden saves, I compute counterfactual lives lost in the absence of the program. I begin the back-of-the-envelope

calculation by restricting the sample to municipalities that received Fonden resources. I then calculate the lives saved for every year in the sample by multiplying Fonden's LATE (table 5 column 5) by the municipal population 50 years or older and summing over the municipalities that received Fonden in each year. To express the results as lives saved, I multiply by minus one. Taking the mean over all the years in the sample, I find that Fonden saves on average 18,104 lives per year.

Next, for every year, I sum Fonden's reconstruction and administrative costs and divide them by the number of lives saved computed in the previous exercise. I find that the average cost of saving a life is $43,366 (constant 2010 international dollars). These back-of-the-envelope calculations implicitly assume that Fonden's treatment effect is homogeneous and that the LATE is informative about the program's impact away from the threshold. The latter assumption is plausible in this setting because I also find that the ITT and the first stage estimates are stable and, thus, more likely to have external validity. Specifically, I follow Cerulli et al. (2017) and estimate the treatment effect derivative (TED) and the complier probability derivative (CPD). In both cases, I find estimates of the TED and the CPD that are small and statistically indistinguishable from zero (p-values 0.55 and 0.97).[8]

To put the average cost of saving a life in perspective, I perform two additional exercises. First, I derive a benefit-cost ratio by dividing the value of a statistical life (VSL) by the average cost. The extensive VSL literature offers several estimates that for Mexico fall in the $210,880 to $1,537,323 (constant 2010 international dollars) range (Hammitt and Ibarraran, 2006; de Lima, 2020; Viscusi and Masterman, 2017). Following Banzhaf (2021) recent meta-meta analysis of VSL estimates, I also compute an income adjusted extrapolation from US VSL estimates. This calculation suggests that a reasonable VSL for Mexico would be at the top of the range presented in the literature ($1,392,619). Nonetheless, to provide the most conservative benefit-cost ratio, I use the lower bound VSL estimate of $210,880 and find that Fonden's benefit-cost ratio is at least 4.9.[9] The purpose of this exercise is not to provide a comprehensive benefit-cost analysis, as this would require accounting for the economic benefits of Fonden, for the costs of providing medical services, and for the dead-weight loss of funding the program with taxes. Nonetheless, the calculation clearly illustrates

[8]Consistent with these findings, exercises where the back-of-the-envelope is calculated using only observations that fall within the estimating bandwidth (or a smaller bandwidth) yield very similar estimates of the average cost of saving a life.

[9]While the back-of-the-envelope is calculated for the population 50 years or older, further adjusting the VSL estimates by age would have no bearing on the results. Specifically, Aldy and Viscusi (2008) shows that VSL has an inverted-U shape relationship with age, with the VSL peaking just before age 50. Importantly, as Aldy and Smyth (2014) has further shown, even the VSL at age 80 is roughly one-third of the VSL at age 50. This finding implies that I would recover benefit-cost ratios considerably larger than one even if I performed the calculation assuming an age 80 VSL for the entire sample.

that the first-order benefit of Fonden is saving lives. By comparison, a similar benefit-cost ratio calculation of Fonden economic benefits finds a ratio close to one (Del Valle, de Janvry and Sadoulet, 2020).

The second exercise compares Fonden's cost of $43,366 per life saved to the cost of saving a life through an alternative disaster preparedness intervention. These interventions range from those without direct costs like banning construction in high-risk areas to those with costs in the order of tens of millions, like retrofitting infrastructure. Tengs et al. (1995) reviews several of these disaster preparedness interventions and finds that the median cost is $1.5 million per life-year saved or roughly $44 million for a 50-year-old with a 79.6-year life expectancy.[10] Accordingly, by this metric, Fonden is also shown to be worthwhile as it provides a fairly cost-effective way of saving lives.

4 Validation and Falsification of the FRD Design

4.1 Other resource allocations and manipulation of the running variable

There are two key threats to the validity of the FRD design. The first is the possibility that program thresholds affect mortality through non-Fonden mechanisms. For example, if other government agencies used Fonden thresholds for resource allocation, I could erroneously attribute these resources' impact to Fonden. While I cannot rule out this possibility, an extensive review of resource allocation regulations and interviews with federal and state officials failed to uncover any instance in which non-Fonden financial or in-kind transfers were allocated using Fonden thresholds. Moreover, as shown by Del Valle, de Janvry and Sadoulet (2020) per capita federal transfers to local governments (including discretionary funds) do not change discontinuously at the threshold.

The second threat is the possibility that municipalities may try to manipulate the running variable in the hope of becoming eligible for Fonden. Gaming the verification process is difficult because the thresholds and the subset of weather stations used for verification are only known to Conagua and because rainfall is reported nearly in real-time.

Nonetheless, to investigate whether manipulation could have taken place, I conduct three exercises. First, I test whether the running variable's density is continuous at the threshold. In this setting, observing many more municipalities barely qualifying for Fonden (right of the threshold) than barely failing to qualify would be indicative of manipulation. Figure 2 panel A plots the histogram of rainfall minus threshold and provides the first indication that

[10] The source for life expectancy in Mexico at age 50 is WHO (2000).

there is no apparent excess density to the right of the threshold (dashed green line). Next, I use the local polynomial density estimator and test the statistic of Cattaneo, Jansson and Ma (2018). Figure 2 panel B plots the estimated empirical density. The figure confirms that there is no excess density just to the right of the threshold and that the running variable's density is continuous at the threshold. The results from the test static corroborate this finding as I am unable to reject the null hypothesis that the running variable's density is continuous at the threshold (p-value 0.57).[11]

Second, I take advantage of the idea that if rainfall records were manipulated, the observations closest to the threshold would be those where tampering is most likely to occur. The test, therefore, consists in checking the sensitivity of the ITT (table 2 column 2) to excluding observations that are within 5 mm of the threshold (2.5 mm radius). The results reported in table A2 in the appendix show that in all cases, the estimates of the ITT are of a similar magnitude and remain statistically significant at conventional levels.

Third, if some municipalities can game the verification process, I should observe that those just above the threshold are systematically different from those just below. To test whether the predetermined characteristics of municipalities change discontinuously at the threshold, I compiled 26 variables from the census and administrative records. These variables can be grouped into four categories. These categories include state capacity and provision of public goods, local governments' financial capacity, geographic and demographic features, and public medical infrastructure. Unless otherwise stated, all variables are measured in the most recent year that predates a natural disaster. To begin the analysis, I use the methods described in subsection 3.1 to plot each of these variables relative to the running variable. The resulting regression discontinuity plots are presented in figures A1 to A5 in the appendix. In all cases, the figures fail to reveal any discontinuity at the threshold. Next, to further test whether the predetermined characteristics of municipalities change discontinuously, I estimate equation 1 using as outcome each of the 26 variables. Before estimation, I transform the variables into standard deviation units to facilitate comparisons. Figure 3 present the results from this exercise. Consistent with the idea that there are no systematic differences between municipalities, I find that for all 26 outcomes, the resulting point estimates are small and that the 95 percent confidence intervals include zero.

Overall, the results from these three exercises provide robust evidence that municipal governments were unable to sort around the threshold. The absence of manipulation is important because, as previously mentioned, it provides supporting evidence for the identi-

[11] I also use an alternative version of the test statistic. This test statistic increases power by assuming that the cumulative density function and higher-order derivatives are the same for both groups around the threshold. With this test, I also fail to reject the null hypothesis that the running variable's density is continuous at the threshold (p-value = 0.45).

fication assumptions (continuity and local smoothness).

4.2 Falsification exercises and multiple inference adjustments

To further test the validity of the FRD design, I perform a falsification exercise for every outcome tested in the paper. As explained in the data section, I compute for each outcome equivalent AMRD's using only pre-disaster data. I then use these placebo outcomes to estimate Fonden's LATE. I report the p-values from these exercises in the last row of Panel C of every table. Out of 41 placebo exercises, I find only 3 cases where a Fonden placebo LATE is statistically different from zero at the ten percent level (table 2 column 6, table 5 column 4, and table A5 column 7).

Because the paper tests how Fonden affects several outcomes and significant coefficients may be estimated by chance even if Fonden has no effect, I also compute adjusted p-values that account for multiple inference. Specifically, I follow Anderson (2008) and compute sharpened q-values that control for the false discovery rate (FDR). The family of tests includes all of Fonden's LATE estimates (real and placebos) from all tables (2-5, A1, A4, A5), as well as the tests of differences between the LATE estimates reported in the table notes.

In the case of the (real) estimates of Fonden's LATE and the test of differences between these coefficients, the paper results remain unchanged when interpreting the findings using q-values that control for the FDR at level q=0.10 instead of p-values. Specifically, I find that all estimates where the null hypothesis of no Fonden effect are rejected at conventional levels using p-values are also rejected when using q-values. Similarly, for estimates where I fail to reject the null hypothesis of no Fonden effects with p-values, I also fail to reject using q-values.

By comparison, in the case of the placebo estimates of Fonden's LATE, the q-values now indicate that all coefficients are statistically indistinguishable from zero. These findings suggest that after accounting for multiple inference, there is no evidence of Fonden having any effect on any outcome measured in the pre-disaster period.

Next, I conduct an additionally falsification exercise that tests whether there are program effects at placebo thresholds. Specifically, table A3 in the appendix presents estimates of the impact of Fonden on the 8-month AMRD both at the true (zero) threshold and at several placebo thresholds. To carry out this test, I restrict the sample to nonnegative values of the running variable (excluding the true threshold) and estimate Fonden's ITT using the first five deciles of the running variable as placebo thresholds. I then repeat the previous exercise, this time restricting the sample to negative values of the running variable.

Consistent with the idea that the 8-month AMRD only changes discontinuously at the true (zero) Fonden threshold, I find that the ITT estimates for placebo thresholds are statistically indistinguishable from zero.

4.3 Robustness checks

Given that the estimates presented in tables 2 to 5 require a choice of bandwidth selection algorithm, local polynomial degree, and kernel, I begin by showing that the results of the paper are not sensitive to the choice of tuning parameters. In particular, I focus on the key result of Fonden's impact on the 8-months AMRD (table 2 column 2). Table A4 in the appendix presents the results from these robustness checks. In column 1, I show estimates that use a bandwidth selection algorithm that minimizes the asymptotic coverage error rate (h_{CER}) and provides optimal inference of confidence intervals. Next, in columns 2 and 3, I use h_{MSE} and h_{CER} selection algorithms that allow for different bandwidth lengths on each side of the threshold. In all cases, I find estimates of the first stage, the ITT, and the LATE that are of very similar magnitude and that remain statistically significant at the one-percent level. In columns 4 and 5, I use a local quadratic polynomial instead of a local linear polynomial. As in the previous case, I find very similar results. Next, in columns 6 and 7, I present estimates that use a uniform and an epanechnikov kernel instead of a triangular kernel. Once again, I find very similar results. Last, in figure A6 in the appendix, I provide further evidence that the estimates are not sensitive to the choice of bandwidth. Specifically, I show that I can recover very similar estimates over a wide range of bandwidths. I consider bandwidths between 0.5 and 1.5 times the optimal h_{MSE} bandwidth (25.72 to 77.17 mm). I then partition this range into ten and estimate Fonden's ITT using each of the ten bandwidths. The circles represent point estimates. The error bars 95 percent confidence intervals. The figure reveals the standard trade-off between bias and variance, with estimates that use a smaller bandwidth displaying larger confidence intervals and vice-versa. Notably, despite the wide range of bandwidths, I recover estimates of Fonden's ITT that remain statistically significant at the five-percent level and that are of a very similar magnitude. Taking these findings together, I conclude that the tuning parameters have no impact on the paper's findings.

Next, in table A5 I show that the estimate of Fonden impact is robust to various issues. I begin with issues related to the construction of the AMRD. Because the crude mortality rate does not account for the population's age and sex distribution, it may not be well suited for comparisons across municipalities or over time. To address this shortcoming, I compute an 8-month AMRD that is age-sex-adjusted using as reference the age and sex distribution of

Mexico in 2000. The result of this exercise is reported in column 1. Consistent with the idea that there are no systematic demographic differences among municipalities just above and just below the threshold, I find that standardization makes little difference. For example, while the estimates from table 2 column 2 indicate that Fonden fully reduces postdisaster excess deaths, the equivalent results using the standardized outcome (table A5 column 1) reveal a comparable reduction of 87%.

Another issue in the computation of the AMRD is the choice of baseline AMR. Throughout the paper, I have used a baseline AMR with the same window length as the postdisaster AMR but measured two years before the disaster. An alternative approach is to use the mean of the corresponding AMR (same window length) over several predisaster years. In the case of the 8-month AMRD, I can compute the AMRD using as baseline AMR the mean of the 8-month AMR over four predisaster years.[12] The results from using this alternative definition of the 8-month AMRD are presented in column 2. As in the previous case, consistent with the idea that this choice in the computation of the AMRD creates no systematic differences among municipalities just above and below the threshold, I find very similar estimates of Fonden impact. For example, the ITT estimate (panel B) indicates that Fonden can reduce 0.63 postdisaster excess deaths per 1,000 person-years or a 94 percent reduction relative to the control mean.

Next, while the AMRD is computed from AMR's that occur in the same calendar months, I further address seasonality by including calendar month fixed effects to specification 1. As reported in column 3, I find estimates that are of a very similar magnitude and that remain statistically significant at the one-percent level. Turning to other issues, in columns 4 and 5, I test the sensitivity of the results to the exclusion of municipalities that received Fonden on consecutive years. Specifically, in column 4, I exclude municipalities that received Fonden in the year before or after a request. In column 5, I expand the window to two years before and after. In columns 6 and 7, I test whether the results are driven by municipalities that experience the most extreme weather shocks. In column 6, I exclude the municipalities that experience the largest events (top decile of rainfall). In column 7, I exclude the municipalities that recurrently experiencing the most extreme rain (top decile of Fonden thresholds). In all cases, I find similar estimates of program impact that remain statistically significant at the five percent level.

Last, I investigate whether Fonden's effect on the 8-month AMRD varies with the level of the Fonden thresholds by following the methodology proposed by Cattaneo et al. (2016)

[12]Because in my dataset, disaster declarations began in 2004 and mortality in 2000, I have four years of predisaster data for all municipal-year observations.

for continuous and noncumulative thresholds.[13] I begin the analysis by examining the distribution of the Fonden heavy rainfall thresholds. The top panel of Figure A7 in the appendix plots the histogram. The figure reveals that the bulk of the thresholds fall in the 0 to 200 mm range. Next, I select five equally spaced points in this range: 40, 80, 120, 160, and 200. For each of the 5 points, I then select the 400 observations with Fonden thresholds nearest to the point and use that subsample to estimate Fonden's ITT. The bottom panel of figure A7 in the appendix plots the resulting five thresholds-specific treatment effects. Circles represent point estimates and error bars the 95 percent confidence intervals. I also plot a quadratic polynomial fit estimated from the five thresholds-specific treatment effects (dashed red line). To compare these estimates against the ITT estimate derived when normalizing and pooling the Fonden thresholds, the figure also plots the estimate from table 2 column 2 panel B (solid blue line). The figure reveals that Fonden's effect is homogeneous with the point estimates and quadratic polynomial fit closely tracking the normalized and pooled ITT estimate. The smaller sample sizes of the threshold-specific treatment effects, however, lead to wider confidence intervals that include zero in all cases. By comparison, Fonden's ITT from the normalized and pooled approach is statistically significant at the one percent level. Overall, these findings highlight that in this application, normalizing and pooling the thresholds lead to estimates that do a good job of summarizing the impact of Fonden while also increasing statistical power by aggregating the sample across thresholds.

5 Conclusion

Reducing disaster-related deaths is an increasingly urgent challenge, given the expected intensification of hydrometeorological events caused by climate change (IPCC, 2021). In this paper, I study Fonden, Mexico's pre-arranged disaster aid fund. Using a fuzzy regression discontinuity design that exploits the discontinuity created by Fonden's eligibility rules, I show that the program led to a substantial and largely permanent reduction in post-disaster excess mortality. I also provide supporting evidence showing that Fonden saves lives by restoring access to health services.

The findings of this paper are important for policymakers because they highlight that a potential first-order benefit or redesigning disaster relief programs around a pre-arranged disaster aid model is the reduction in disaster-related mortality. As illustrated by the back-of-the-envelope calculations, Fonden's cost of saving a life compares favorably to the value of a statistical life and the cost of saving a life through other policy interventions. Moreover, Fonden's benefit of mortality risk reduction is accrued in the short run. By comparison,

[13] The thresholds are continuous, and their level is unrelated to the amount of Fonden resources disbursed.

other interventions such as incentivizing the reallocation of assets and people away from high-risk areas or the hardening of structures through building codes are likely to operate over longer time horizons.

Another important implication of the paper's findings is that Fonden's capability to save lives stems from its complementary with public health insurance. The past decade's remarkable push for universal health coverage and the adoption of universal health insurance as a Sustainable Development Goal indicates that there is an increasing number of countries that are well prepared to curb disaster-related mortality risk by making complementary investments in disaster relief.

Finally, the case for Fonden like programs is compounded by previous research findings, which show that Fonden yields the additional benefit of accelerating post-disaster economic recovery.

Despite Fonden's many advantages, two design features in this type of program require attention and should be further refined. First, the program protects taxpayer resources by conditioning payouts to the triggering of the index and the subsequent damage assessment. This setup eliminates upside basis risk (experiencing no damage and receiving a payout) but allows for downside basis risk (experiencing damage without receiving a payout). To reduce downside basis risk, the literature on agricultural index insurance provides several paths forward. One alternative is to improve the correlation between the index and damages, for example, by increasing the number of weather stations or by using information from remote sensing platforms. Another alternative is to redesign the program's rules to introduce an appeal process verified by an audit rule or by a secondary index.

Second, the disbursement rules could be adapted to further reduce deaths by incentivizing the retreat from high-risk areas and allowing the use of resources to harden structures against disasters. In this respect, Fonden's evolution also provides a valuable role model. Over time, the program adapted, offering resources to rebuild infrastructure in safer areas and, under the build back better principle, providing funds above replacement costs to build more resilient structures.

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6 Tables and Figures

Table 1: Summary statistics

	Mean	Std. Dev.	Min	Max
8-month AMRD	0.35	2.07	-11.77	22.75
Rainfall (mm)	81.62	80.36	0	391
Fonden threshold (mm)	89.60	44.02	2	237
Rainfall minus threshold (mm)	-7.97	76.22	-219	297
Above threshold=1	0.36	0.48	0	1
Fonden=1	0.71	0.45	0	1

Note: The sample is composed of municipalities that requested Fonden between 2004 and 2012. There are 2,742 municipal-year observations in the sample. The 8-month AMRD is the difference between the annualized mortality rate eight months after a disaster with the same eight-month period annualized mortality rate two years before the disaster. Above threshold is an indicator variable equal to one when the rainfall is equal to or above the threshold. The Fonden variable is equal to one when the Federal Register reports that a municipality is eligible for Fonden resources.

Table 2: Evolution of Fonden Impact on all-cause AMRD

	4 months (1)	8 months (2)	12 months (3)	16 months (4)	20 months (5)	24 months (6)
Panel A. *First Stage*	0.220	0.222	0.223	0.226	0.232	0.239
p-value	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
CI 95 percent	[0.11,0.28]	[0.12,0.29]	[0.12,0.28]	[0.12,0.29]	[0.13,0.28]	[0.14,0.29]
Panel B. *ITT*	-0.502	-0.816	-0.491	-0.412	-0.334	-0.335
p-value	0.187	0.002	0.022	0.039	0.039	0.013
CI 95 percent	[-1.40,0.27]	[-1.48,-0.34]	[-1.00,-0.08]	[-0.85,-0.02]	[-0.73,-0.02]	[-0.67,-0.08]
Control Mean	0.618	0.794	0.529	0.505	0.445	0.447
Panel C. *LATE*	-2.277	-3.676	-2.203	-1.822	-1.440	-1.400
p-value	0.161	0.003	0.019	0.032	0.030	0.009
CI 95 percent	[-6.68,1.11]	[-7.36,-1.48]	[-4.83,-0.43]	[-4.03,-0.18]	[-3.37,-0.17]	[-3.04,-0.44]
Placebo (p-value)	0.658	0.155	0.105	0.175	0.288	0.072
Bandwidth	55.2	51.4	55.6	56.1	62.8	69.7
Obs(left|right)	986|515	944|488	1008|523	1012|525	1136|559	1236|592

Note: Panel A and B present estimates of equation 1 using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. In panel A the dependent variable is an indicator for receiving Fonden. In panel B, the dependent variables are the annualized all-cause mortality rate difference for the window listed in the column title. The control mean is the left-hand-side prediction of the nonparametric regression at the threshold. Panel C reports the LATE computed as the ratio of the ITT to the first stage. The Placebo row reports the LATE p-value when the outcome is the AMRD computed using only pre-disaster data. The p-values and 95 percent confidence intervals reported are constructed using robust bias correction and clustering at the municipal level.

Table 3: Impact of Fonden on cause specific AMRD's

	Transport injuries	Self-harm inter-personal violence	Commu-nicable	Non-commu-nicable	Amenable	PHI covered
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. *FS*	0.222	0.224	0.223	0.228	0.223	0.226
p-value	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
CI 95 percent	[0.12,0.29]	[0.12,0.28]	[0.12,0.30]	[0.13,0.31]	[0.12,0.29]	[0.12,0.30]
Panel B. *ITT*	-0.040	-0.040	0.026	-0.464	-0.508	-0.299
p-value	0.215	0.244	0.322	0.026	0.002	0.037
CI 95 percent	[-0.13,0.03]	[-0.11,0.03]	[-0.03,0.10]	[-0.96,-0.06]	[-0.94,-0.21]	[-0.69,-0.02]
Control Mean	0.028	0.046	-0.031	0.392	0.501	0.361
Panel C. *LATE*	-0.181	-0.177	0.115	-2.032	-2.276	-1.326
p-value	0.188	0.205	0.299	0.035	0.003	0.037
CI 95 percent	[-0.62,0.12]	[-0.52,0.11]	[-0.14,0.47]	[-4.46,-0.16]	[-4.59,-0.93]	[-3.23,-0.10]
Placebo (p-value)	0.103	0.423	0.169	0.146	0.353	0.198
Bandwidth	53.9	60.9	49.6	44.0	49.2	46.1
Obs(left|right)	968|513	1106|550	923|471	818|425	918|470	865|437

Note: Panel A and B present estimates of equation 1 using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. In panel A the dependent variable is an indicator for receiving Fonden. In panel B, the dependent variable is the cause-specific 8-month AMRD listed in the column title. Amenable refers to non-communicable conditions responsive to basic medical care. PHI covered refers to non-communicable amenable conditions for which interventions and medications are covered for free by public health insurance. The control mean is the left-hand-side prediction of the nonparametric regression at the threshold. Panel C reports the LATE computed as the ratio of the ITT to the first stage. The Placebo row reports the LATE p-value when the outcome is the AMRD computed using only pre-disaster data. The p-values and 95 percent confidence intervals reported are constructed using robust bias correction and clustering at the municipal level.

Table 4: Impact of Fonden on cause specific AMRD's by preexisting level of public medical infrastructure

Dep. variable:	Amenable AMRD		PHI covered AMRD	
Sample split:	Public doctors (pc)			
	Above Median (1)	Below Median (2)	Above Median (3)	Below Median (4)
Panel A. *FS*	0.207	0.224	0.208	0.225
p-value	0.005	0.001	0.004	0.001
CI 95 percent	[0.06,0.32]	[0.09,0.32]	[0.06,0.31]	[0.09,0.32]
Panel B. *ITT*	-0.985	0.014	-0.505	0.055
p-value	0.001	0.872	0.030	0.566
CI 95 percent	[-1.73,-0.47]	[-0.24,0.29]	[-1.13,-0.06]	[-0.18,0.34]
Control Mean	0.710	0.217	0.476	0.180
Panel C. *LATE*	-4.764	0.064	-2.426	0.244
p-value	0.007	0.864	0.037	0.543
CI 95 percent	[-9.94,-1.57]	[-1.08,1.29]	[-6.05,-0.18]	[-0.81,1.53]
Placebo (*p*-value)	0.279	0.944	0.131	0.866
Bandwidth	46.4	57.2	51.6	56.1
Obs(left|right)	481|243	465|240	519|267	458|240

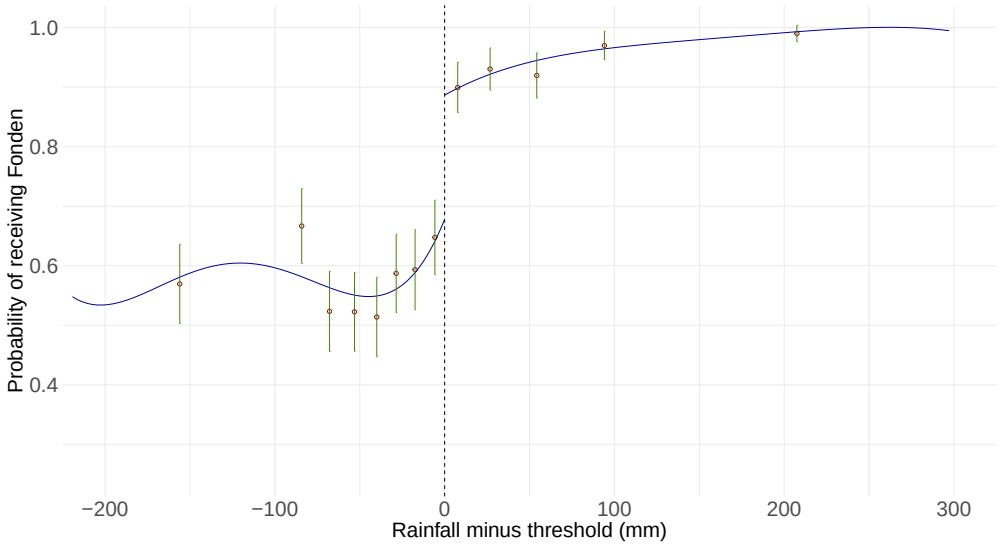
Note: Panel A and B present estimates of equation 1 using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. In panel A the dependent variable is an indicator for receiving Fonden. In panel B, the dependent variable is the cause-specific 8-month AMRD listed in the column title. Amenable refers to non-communicable conditions responsive to basic medical care. PHI covered refers to non-communicable amenable conditions for which interventions and medications are covered for free by public health insurance. The control mean is the left-hand-side prediction of the nonparametric regression at the threshold. Panel C reports the LATE computed as the ratio of the ITT to the first stage. The Placebo row reports the LATE p-value when the outcome is the AMRD computed using only pre-disaster data. The p-values and 95 percent confidence intervals reported are constructed using robust bias correction and clustering at the municipal level. In columns 1 and 3, the sample is restricted to areas with above-median public doctors per capita (high health supply). In columns 2 and 4, the sample is restricted to areas with below-median public doctors per capita (low health supply). For amenable conditions (columns 1 and 2), the p-value for the null hypothesis that the LATE for high and low health supply areas is equal is 0.008. For PHI-covered conditions (columns 3 and 4), the p-value for the null hypothesis that the LATE for high and low health supply areas is equal is 0.031.

Table 5: Impact of Fonden on sex and age specific AMRD's

	Male (1)	Female (2)	0-14 (3)	15-49 (4)	50 or older (5)
Panel A. *FS*	0.225	0.229	0.228	0.236	0.223
p-value	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
CI 95 percent	[0.12,0.30]	[0.12,0.28]	[0.13,0.30]	[0.13,0.28]	[0.12,0.29]
Panel B. *ITT*	-1.055	-0.665	0.172	-0.177	-3.376
p-value	0.006	0.048	0.263	0.324	0.002
CI 95 percent	[-2.08,-0.34]	[-1.42,-0.01]	[-0.18,0.65]	[-0.63,0.21]	[-6.22,-1.38]
Control Mean	0.865	0.742	-0.170	0.231	3.357
Panel C. *LATE*	-4.686	-2.900	0.753	-0.751	-15.109
p-value	0.009	0.037	0.247	0.278	0.003
CI 95 percent	[-9.97,-1.45]	[-6.71,-0.21]	[-0.74,2.88]	[-2.79,0.80]	[-30.35,-5.99]
Placebo (*p*-value)	0.919	0.264	0.868	0.052	0.509
Bandwidth	46.4	63.5	43.7	70.1	48.7
Obs(left|right)	871|437	1148|558	808|423	1237|594	901|464

Note: Panel A and B present estimates of equation 1 using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. In panel A the dependent variable is an indicator for receiving Fonden. In panel B, the dependent variable corresponds to the age or gender-specific 8-month AMRD listed in the column title. The control mean is the left-hand-side prediction of the non-parametric regression at the threshold. Panel C reports the LATE computed as the ratio of the ITT to the first stage. The Placebo row reports the LATE p-value when the outcome is the AMRD computed using only pre-disaster data. The p-values and 95 percent confidence intervals reported are constructed using robust bias correction and clustering at the municipal level. The p-value for the null hypothesis that the LATE by gender is equal is 0.41. The p-values for the null hypotheses that the LATE for those 50 or older is equal to each of the younger groups is 0.002 and 0.006.

(a) First Stage



(b) Intention-to-Treat

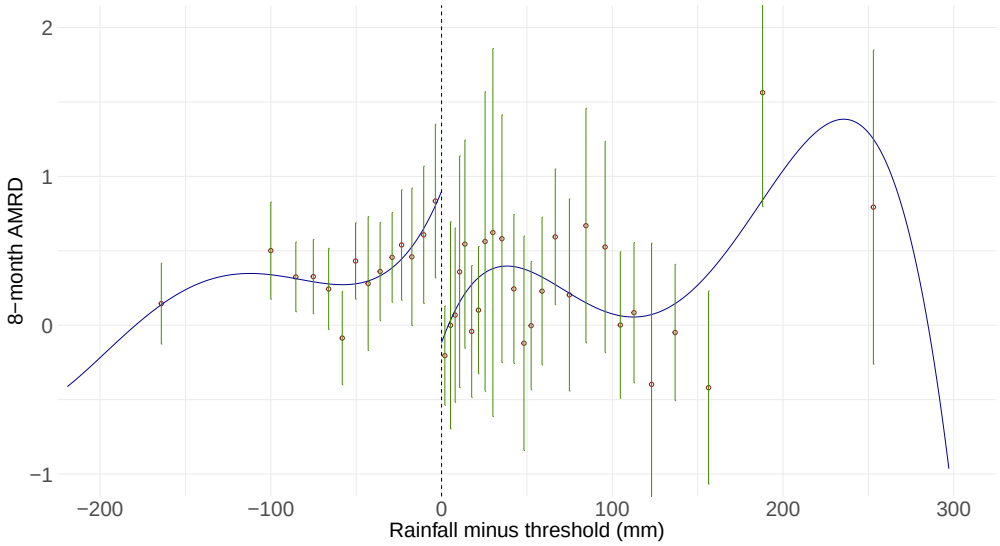
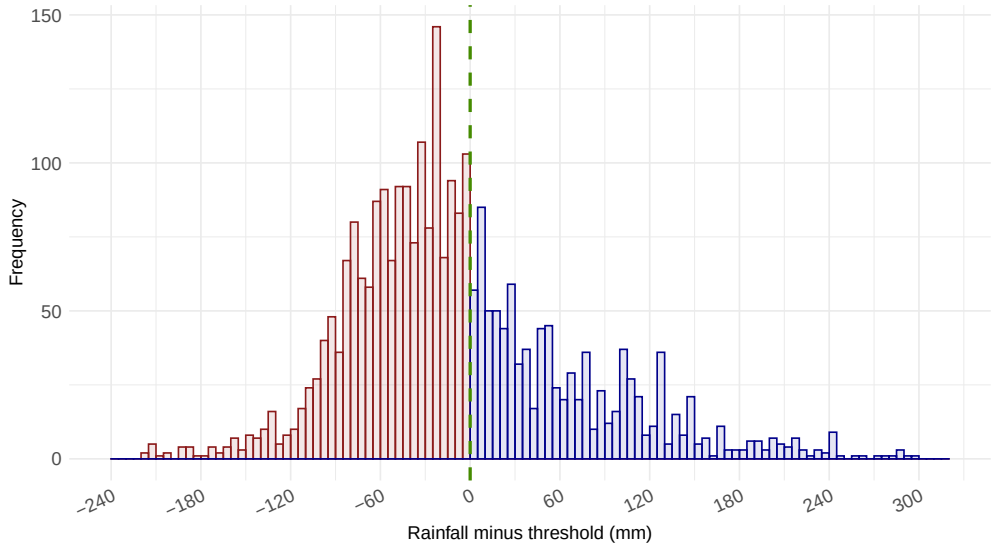


Figure 1: First Stage and Intention-to-Treat

Note: Each graph plots the outcome (probability of receiving Fonden or eight-month annualized mortality rate difference) as a function of the running variable (rainfall minus threshold). In each graph, the support of the running variable has been partitioned into disjoint bins of roughly the same number of observations. The number of bins is selected to minimize the integrated mean square error of the underlying regression function, as described in Calonico, Cattaneo and Titiunik (2015). The circles plot the local mean of the outcome at the mid-point of each bin. The error bars are the 95 percent confidence intervals for the local means. The solid lines are fourth-order global polynomials fits (estimated separately from the raw data on each side of the threshold). Observations to the right of the vertical dashed line are eligible for Fonden.

(a) Histogram



(b) Estimated Density

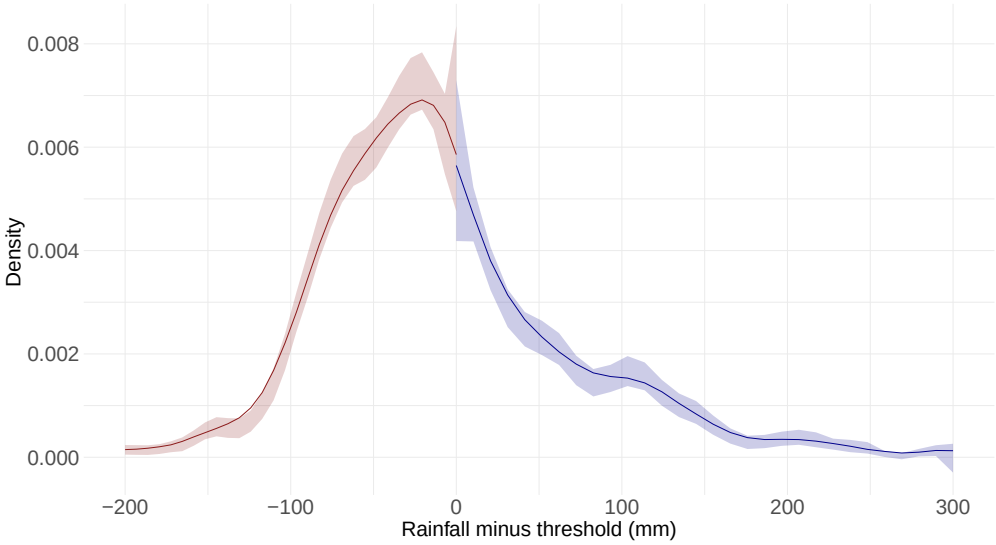


Figure 2: Histogram and estimated density of rainfall minus threshold

Note: Panel A plots the histogram, and panel B plots the empirical density of the running variable (rainfall minus threshold). The p-value for the null hypothesis that the density of the running variable is continuous at the threshold is 0.56. For details on the test statistic see subsection 4.1 and Cattaneo, Jansson and Ma (2018).

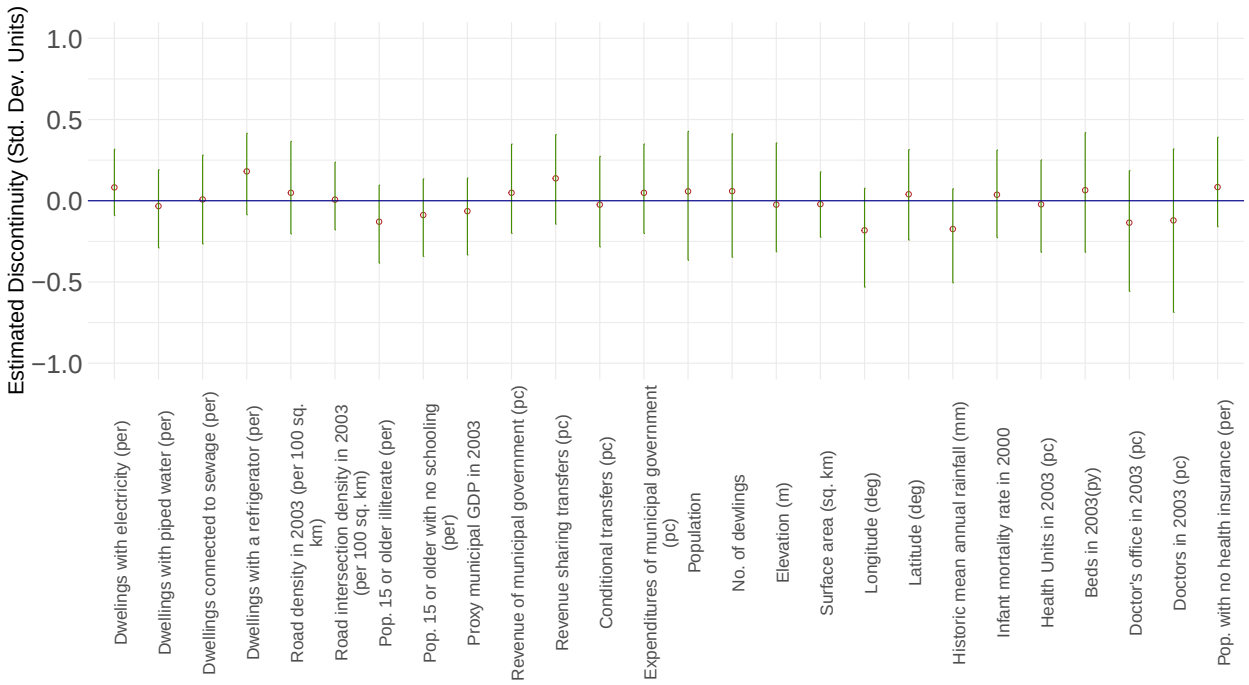


Figure 3: Balance of predetermined covariates

Note: The figure plots estimates of equation 1 using as outcome each of the variables listed. The abbreviation (per) denotes percent, (pc) denotes per capita. Unless otherwise stated in the label, all variables are measured in the most recent year available that predates a natural disaster used to request Fonden verification. Variables are standardized to facilitate comparison. The estimates use a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. The circles represent point estimates. The error bars represent robust 95 percent confidence intervals. The confidence intervals are constructed using robust bias correction and clustering at the municipal level.

APPENDIX - FOR ONLINE PUBLICATION

A Supplementary Tables and Figures

Table A1: Does Fonden reduce mortality through and income channel?

Dep. variable:	Log difference night lights		Amenable AMRD		PHI Covered AMRD	
Sample split:	Public doctors (pc)		Storm drain coverage (per)			
	Above Median (1)	Below Median (2)	Above Median (3)	Below Median (4)	Above Median (5)	Below Median (6)
Panel A. *FS*	0.233	0.211	0.229	0.254	0.238	0.236
p-value	0.001	0.002	0.001	$p < 0.001$	0.001	0.001
CI 95 percent	[0.08,0.34]	[0.07,0.29]	[0.08,0.31]	[0.12,0.41]	[0.08,0.31]	[0.10,0.38]
Panel B. *ITT*	0.057	0.056	-0.350	-0.670	-0.096	-0.432
p-value	0.146	0.070	0.108	0.009	0.494	0.038
CI 95 percent	[-0.02,0.14]	[-0.01,0.14]	[-1.01,0.10]	[-1.29,-0.18]	[,0.31]	[-0.94,-0.03]
Control Mean	-0.077	-0.061	0.427	0.563	0.251	0.452
Panel C. *LATE*	0.244	0.265	-1.528	-2.637	-0.404	-1.828
p-value	0.143	0.059	0.088	0.043	0.454	0.075
CI 95 percent	[-0.10,0.66]	[-0.01,0.71]	[-4.74,0.33]	[-5.58,-0.10]	[-2.80,1.25]	[-4.27,0.20]
Placebo (*p*-value)	0.528	0.844	0.991	0.167	0.631	0.177
Bandwidth	55.9	58.8	53.3	42.7	57.6	47.1
Obs(left|right)	542|277	474|245	459|261	405|196	499|283	457|205

Note: Panel A and B present estimates of equation 1 using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. In panel A the dependent variable is an indicator for receiving Fonden. In panel B, columns 1 and 2, the dependent variable is the log difference night lights between the 12 months before and the 12 months after a disaster. In panel B, columns 3 to 6, the dependent variable is the cause-specific 8-month AMRD listed in the column title. Amenable refers to non-communicable conditions responsive to basic medical care. PHI covered refers to non-communicable amenable conditions for which interventions and medications are covered for free by public health insurance. The control mean is the left-hand-side prediction of the nonparametric regression at the threshold. Panel C reports the LATE computed as the ratio of the ITT to the first stage. The Placebo row reports the p-value of the LATE when the outcome is the dependent variable calculated using only pre-disaster data. The p-values and 95 percent confidence intervals reported use robust bias correction and clustering at the municipal level. Public doctors per capita are observed in 2003; its median value is 0.45. To proxy storm drain coverage, I use the percentage of dwellings connected to sewage as measured in the most recent census that predates the natural disaster; its median value is 0.71. The sample in columns 1 and 2 is the same as the one used for the analysis presented in table 4. Del Valle, de Janvry and Sadoulet (2020) documents no impact of Fonden on night lights for the subsample used in columns 3 and 5. The same paper documents the largest Fonden led growth expansion for the subsample used in columns 4 and 6. I find no evidence of differential Fonden effects in any of the outcomes. The p-value for the null hypothesis that the LATE on night lights by the level of medical infrastructure is equal is 0.799. The p-value for the null hypotheses that the LATE on the amenable AMRD by the level of storm drain coverage is equal is 0.741. The p-value for the null hypotheses that the LATE on the PHI covered AMRD by the level of storm drain coverage is equal is 0.413.

Table A2: Robustness donut-hole analysis

Donut-hole Radius (1)	Intention to Treat (2)	p-value (3)	CI 95% (4)	Bandwidth (5)	Obs (6)	Excluded Obs Left (7)	Excluded Obs Right (8)
0.0	-0.816	0.002	[-1.48,-0.34]	51.4	1432	0	0
0.5	-0.711	0.006	[-1.36,-0.23]	55.5	1520	2	7
1.0	-0.945	0.001	[-1.70,-0.43]	49.6	1355	29	10
1.5	-0.830	0.001	[-1.51,-0.42]	61.8	1626	33	13
2.0	-1.067	0.001	[-1.89,-0.52]	48.1	1291	40	21
2.5	-0.570	0.057	[-1.28,0.02]	50.4	1337	61	26

Note: Robustness of the ITT estimate to the exclusion of observations that are within 2.5 mm on each side of the threshold. The dependent variable is the eight-month annualized all-cause mortality rate difference. Column 1 reports the excluded radius in mm, columns 7 and 8 report the number of observations excluded at each side of the threshold. Estimates are derived using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. The p-values and 95% confidence intervals reported are constructed using robust bias correction and clustering at the municipal level.

Table A3: Robustness placebo thresholds

Alternative cutoff (1)	ITT (2)	p-value (3)	CI 95% (4)	Bandwidth (5)	Obs Left (6)	Obs Right (7)
-46.2	-0.211	0.427	[-1.2,0.5]	18.4	307	329
-36.3	-0.068	0.855	[-0.9,0.8]	16.8	260	360
-27.1	-0.110	0.802	[-0.8,0.6]	12.5	220	261
-20.0	0.328	0.509	[-1.0,2.0]	8.6	200	135
-9.2	0.168	0.367	[-1.3,3.4]	10.1	157	173
0.0	-0.816	0.002	[-1.5,-0.3]	51.4	944	488
8.0	-0.102	0.801	[-1.7,1.3]	27.2	99	282
15.4	-0.760	0.269	[-2.1,0.6]	20.1	198	183
26.2	0.705	0.389	[-1.0,2.6]	22.7	261	167
38.1	-0.491	0.622	[-2.4,1.4]	15.7	129	115
52.2	-0.125	0.768	[-1.2,0.9]	15.9	115	82

Note: The dependent variable is the eight-month annualized all-cause mortality rate difference. The table presents estimates of the ITT at the true zero threshold and at various placebo thresholds. The sample in the first five rows is restricted to negative values of the running variable. The placebo thresholds are given by the first five deciles. The sample in the last five rows is restricted to non-negative values of the running variable. The placebo thresholds are determined in an analogous manner. Estimates are derived using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. The p-values and 95 percent confidence intervals reported are constructed using robust bias correction and clustering at the municipal level.

Table A4: Robustness tuning parameters

	Alternative bandwidths			Local Polynomial Degree		Kernel	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *First Stage*	0.233	0.221	0.242	0.209	0.250	0.213	0.219
p-value	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
CI 95 percent	[0.13,0.32]	[0.13,0.30]	[0.14,0.34]	[0.11,0.30]	[0.14,0.36]	[0.11,0.26]	[0.11,0.29]
Panel B. *ITT*	-0.948	-0.879	-0.958	-0.929	-1.102	-0.636	-0.839
p-value	0.002	0.002	0.002	0.003	0.002	0.008	0.002
CI 95 percent	[-1.62,-0.37]	[-1.57,-0.36]	[-1.63,-0.37]	[-1.63,-0.32]	[-1.83,-0.41]	[-1.26,-0.18]	[-1.52,-0.34]
Control Mean	0.838	0.795	0.839	0.834	0.877	0.765	0.793
Panel C. *LATE*	-4.063	-3.972	-3.964	-4.445	-4.414	-2.982	-3.822
p-value	0.005	0.006	0.007	0.015	0.011	0.007	0.003
CI 95 percent	[-7.56,-1.31]	[-7.62,-1.27]	[-7.26,-1.12]	[-8.43,-0.91]	[-7.93,-1.04]	[-6.53,-1.01]	[-7.65,-1.52]
Placebo (p-value)	0.172	0.169	0.179	0.282	0.219	0.142	0.165
Bandwidth (left|right)	35.5|35.5	50.9|41.2	35.1|28.4	79.5|79.5	52.0|52.0	48.8|48.8	46.5|46.5
Obs (left|right)	683|381	942|417	679|328	1364|642	951|497	908|467	875|438
Bandwidth selection	$\hat{h}_{CER}$	$\hat{h}_{MSE2}$	$\hat{h}_{CER2}$	$\hat{h}_{CER}$	$\hat{h}_{MSE}$	$\hat{h}_{MSE}$	$\hat{h}_{MSE}$
Local Polynomial Degree	1	1	1	2	2	1	1

Note: Panel A and B present estimates of equation 1. In panel A the dependent variable is an indicator for receiving Fonden. In panel B, the dependent variable is the eight-month all-cause annualized mortality rate difference. Estimates are derived using a triangular kernel except for columns 6 (epanechnikov) and 7 (uniform). The local polynomial degree and optimal bandwidth selection algorithm used are indicated in each column. The $\hat{h}_{MSE}$ bandwidth selection algorithm is optimal for point estimation; the $\hat{h}_{CER}$ selection algorithm is optimal for inference of confidence intervals. The subscript 2 in the description of the bandwidth selection algorithm denotes that different bandwidth lengths have been selected on each side of the threshold. The control mean is the left-hand-side prediction of the nonparametric regression at the threshold. Panel C reports the LATE computed as the ratio of the ITT to the first stage. The Placebo row reports the LATE p-value when the outcome is the AMRD computed using only pre-disaster data. The p-values and 95 percent confidence intervals reported are constructed using robust bias correction and clustering at the municipal level.

Table A5: Robustness various issues

	Alternative AMRD definitions		Calendar Month FE	Multiple exposure to Fonden		Extreme events	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *First Stage*	0.222	0.223	0.238	0.267	0.289	0.248	0.210
p-value	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
CI 95 percent	[0.12,0.28]	[0.12,0.29]	[0.14,0.29]	[0.15,0.36]	[0.16,0.40]	[0.15,0.38]	[0.09,0.25]
Panel B. *ITT*	-0.469	-0.639	-0.695	-0.900	-1.062	-0.984	-0.706
p-value	0.010	0.002	0.002	0.010	0.006	0.003	0.005
CI 95 percent	[-0.90,-0.12]	[-1.16,-0.27]	[-1.32,-0.30]	[-1.77,-0.24]	[-2.00,-0.34]	[-1.81,-0.36]	[-1.38,-0.25]
Control Mean	0.541	0.678	0.782	0.917	1.061	0.876	0.854
Panel C. *LATE*	-2.115	-2.862	-2.925	-3.370	-3.683	-3.963	-3.365
p-value	0.009	0.002	0.002	0.015	0.011	0.020	0.003
CI 95%	[-4.44,-0.62]	[-5.68,-1.22]	[-6.00,-1.33]	[-7.12,-0.78]	[-7.41,-0.95]	[-7.63,-0.66]	[-7.57,-1.59]
Placebo (p-value)	0.100	.	0.222	0.217	0.247	0.233	0.066
Bandwidth	57.9	50.3	65.3	46.5	49.3	27.8	65.7
Obs (left|right)	1046|530	937|486	1185|562	663|314	497|252	519|323	1101|532

Note: Panel A and B present estimates of equation 1 using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. In panel A the dependent variable is an indicator for receiving Fonden. In panel B, column 1, the dependent variable is the 8-month annualized all-cause standardized (age-sex adjusted) mortality rate difference. In panel B, column 2, the dependent variable is the 8-month annualized all-cause crude mortality rate difference, where the baseline period is given by the average of the 8-month annualized mortality rate observed in the four years before a disaster. In panel B, columns 3 to 7, the dependent variable is the 8-month annualized all-cause crude mortality rate difference used throughout the paper. The specification in column 3 includes calendar month fixed effects. Column 4 excludes observations with Fonden in the year before or after. Column 5 excludes observations with Fonden two years before or after. Column 6 excludes the rainfall events that rank in the top decile. Column 7 excludes observations in the top decile of Fonden thresholds. The control mean is the left-hand-side prediction of the nonparametric regression at the threshold. Panel C reports the LATE computed as the ratio of the ITT to the first stage. The Placebo row reports the p-value of the LATE when the outcome is the dependent variable calculated using only pre-disaster data. No placebo p-value is reported in column 2 because I use all the available pre-disaster information to compute the baseline annualized mortality rate. The p-values and 95 percent confidence intervals reported are constructed using robust bias correction and clustering at the municipal level.

Table A6: Additional datasets and Sources

Variable	**Source**
INEGI (The National Institute of Statistics and Geography)	
Population	
No. of dwellings	
Pop. with no health insurance	
Pop. 15 or older illiterate	Census
Pop. 15 or older with no schooling	INEGI (2000)
Dwellings with electricity	INEGI (2005)
Dwellings with piped water	INEGI (2010)
Dwellings connected to sewage	
Dwellings with a refrigerator	
Elevation	
Revenue of municipal government	Public finances of municipalities
Expenditures of municipal government	INEGI (2014)
Total transfers	
Revenue sharing transfers	
Conditional transfers	
Municipal surface area	Municipal boundaries
Centroid longitude	INEGI (2013*a*)
Centroid latitude	
State level GDP	INEGI (2013*b*)
Fund for Natural Disasters Fonden	
Fonden expenditures	Fonden administrative record
Fonden disbursement times	Fonden (2015)
Fonden planned reconstruction times	
Public Medical infrastructure	
Health units per 1,000 persons	
Hospital beds per 1,000 persons	DGIS (2003)
Doctors offices per 1,000 persons	
Doctors per 1,000 persons	
Other Datasets	
Life expectancy by age and gender	WHO (2000)
Historic mean annual rainfall (20 years before Fonden)	Conagua (2015*a*)
Infant mortality rate	CONAPO (2005)
Road intersection density (per 100 sq. km)	USGS (2003)
Road density (per 100 sq. km)	
GDP deflator	World Bank (2010*a*)
PPP exchange rates	World Bank (2010*b*)
Night lights	NOAA (2015)

Note: The terms of use of the INEGI datasets used can be found in `http://en.www.inegi.org.mx/inegi/terminos.html`

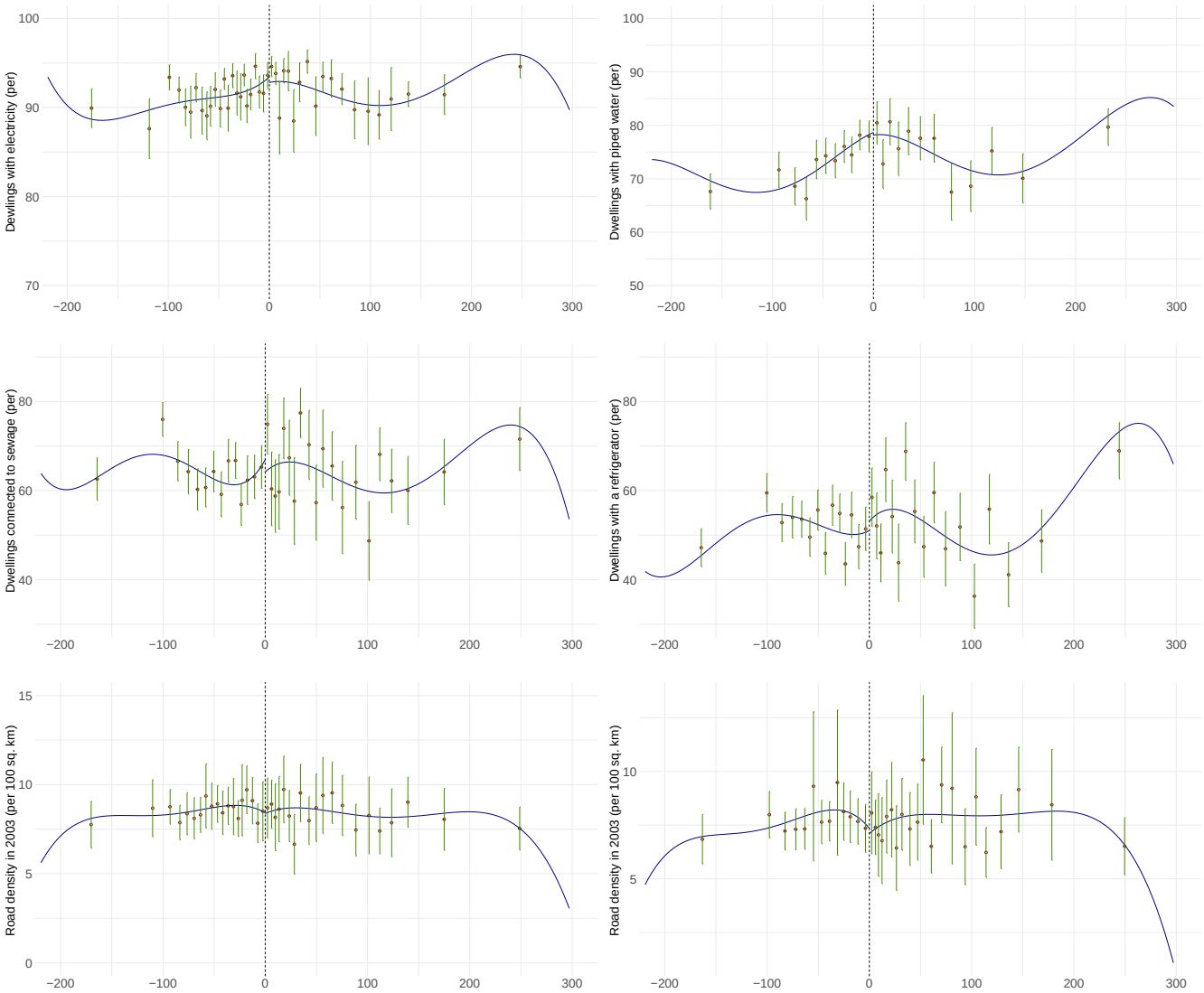


Figure A1: Predetermined Covariates I

Note: The abbreviation (per) denotes percent. Each graph plots the outcome listed on the axis label as a function of the running variable (rainfall minus threshold). In each graph, the support of the running variable has been partitioned into disjoint bins of roughly the same number of observations. The number of bins is selected to minimize the integrated mean square error of the underlying regression function, as described in Calonico, Cattaneo and Titiunik (2015). The circles plot the local mean of the outcome at the mid-point of each bin. The error bars are the 95 percent confidence intervals for the local means. The solid lines are fourth-order global polynomials fits (estimated separately on each side of the threshold). Observations to the right of the vertical dashed line are eligible for Fonden under the heavy rainfall criteria.

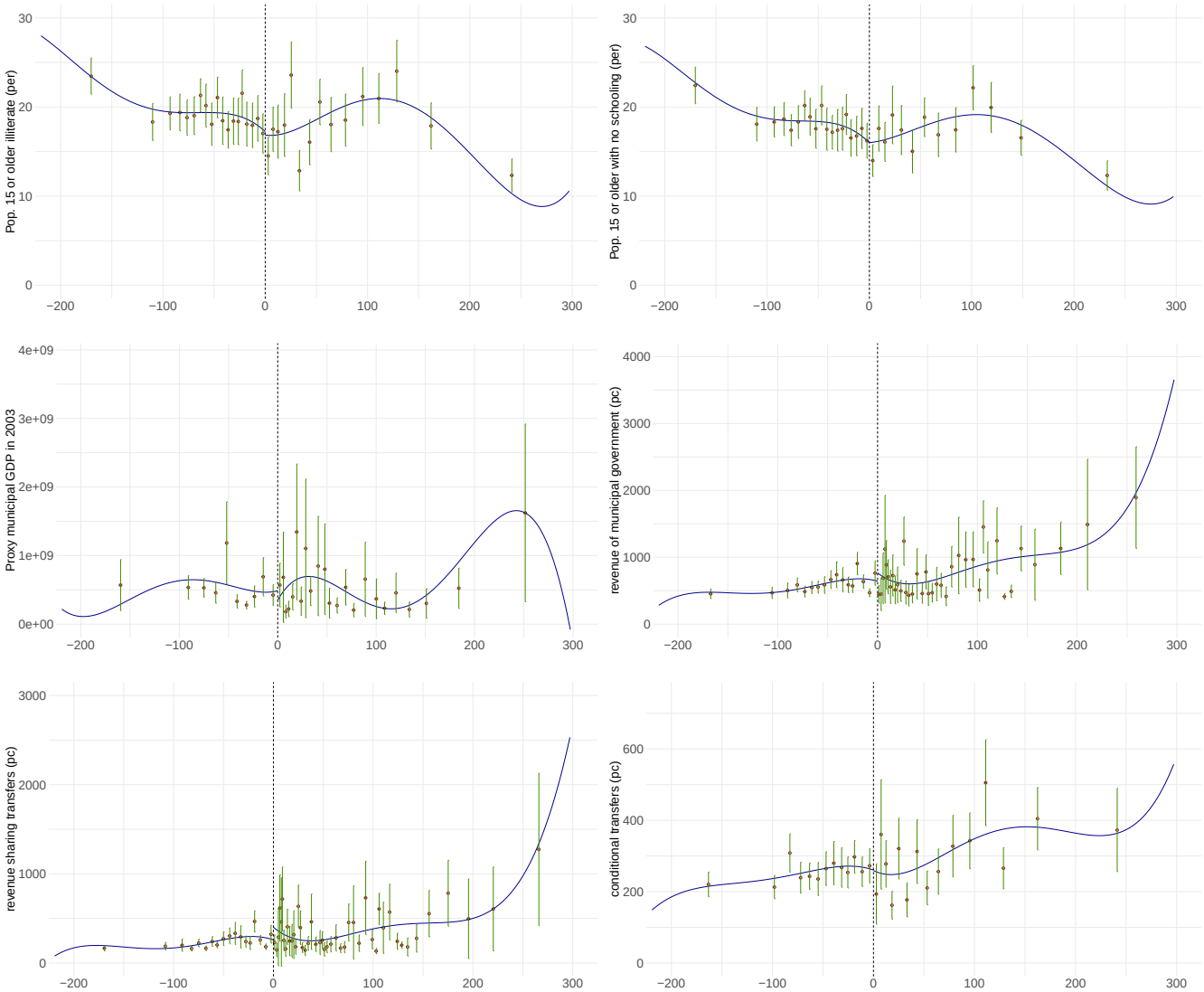


Figure A2: Predetermined Covariates II

Note: The abbreviation (per) denotes percent, and (pc) denotes per capita. Each graph plots the outcome listed on the axis label as a function of the running variable (rainfall minus threshold). In each graph, the support of the running variable has been partitioned into disjoint bins of roughly the same number of observations. The number of bins is selected to minimize the integrated mean square error of the underlying regression function, as described in Calonico, Cattaneo and Titiunik (2015). The circles plot the local mean of the outcome at the mid-point of each bin. The error bars are the 95 percent confidence intervals for the local means. The solid lines are fourth-order global polynomials fits (estimated separately on each side of the threshold). Observations to the right of the vertical dashed line are eligible for Fonden under the heavy rainfall criteria.

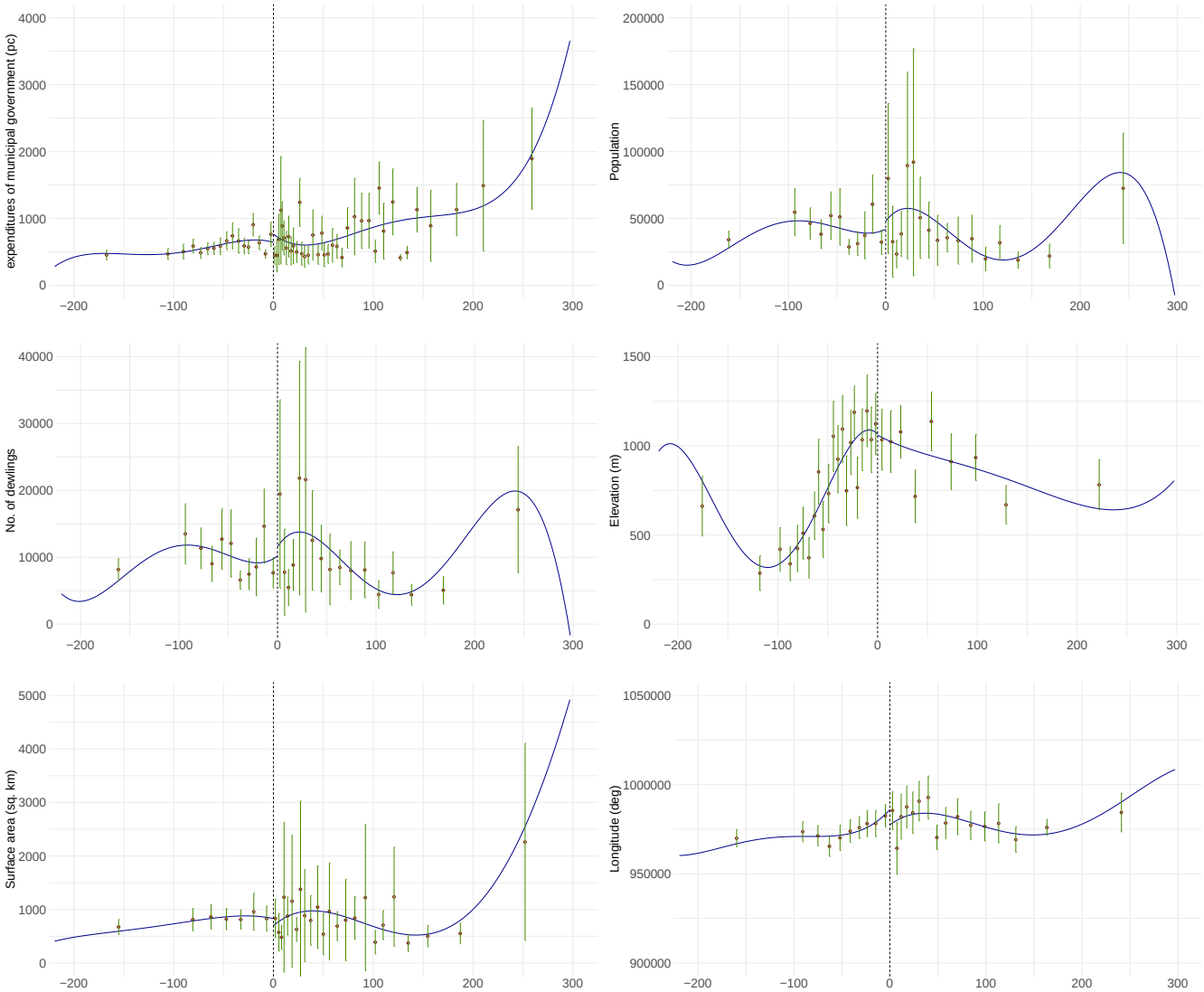


Figure A3: Predetermined Covariates III

Note: The abbreviation (pc) denotes per capita, and (m) denotes meters. Each graph plots the outcome listed on the axis label as a function of the running variable (rainfall minus threshold). In each graph, the support of the running variable has been partitioned into disjoint bins of roughly the same number of observations. The number of bins is selected to minimize the integrated mean square error of the underlying regression function, as described in Calonico, Cattaneo and Titiunik (2015). The circles plot the local mean of the outcome at the mid-point of each bin. The error bars are the 95 percent confidence intervals for the local means. The solid lines are fourth-order global polynomials fits (estimated separately on each side of the threshold). Observations to the right of the vertical dashed line are eligible for Fonden under the heavy rainfall criteria.

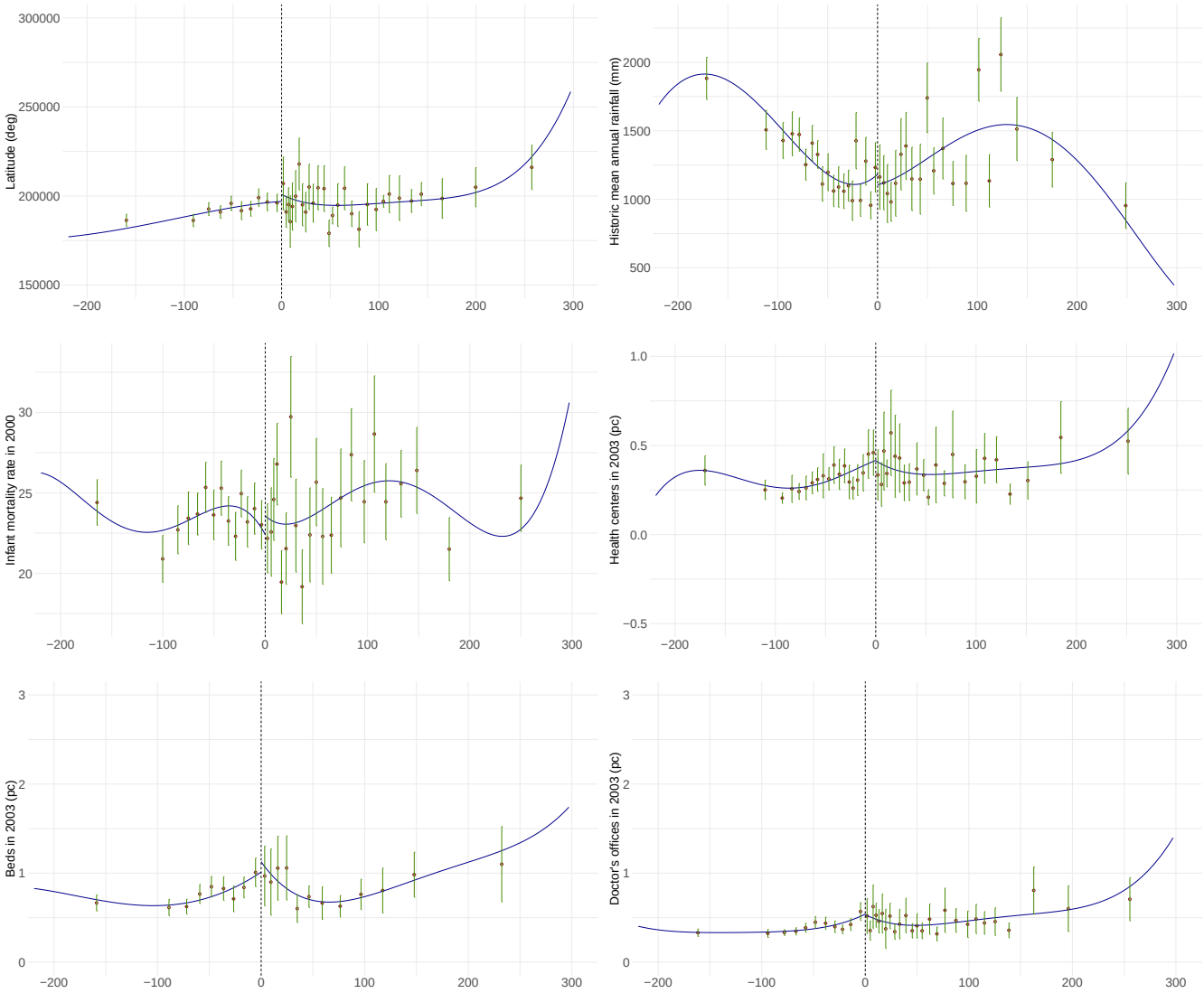


Figure A4: Predetermined Covariates IV

Note: The abbreviation (mm) denotes millimeters, and (pc) denotes per capita. Each graph plots the outcome listed on the axis label as a function of the running variable (rainfall minus threshold). In each graph, the support of the running variable has been partitioned into disjoint bins of roughly the same number of observations. The number of bins is selected to minimize the integrated mean square error of the underlying regression function, as described in Calonico, Cattaneo and Titiunik (2015). The circles plot the local mean of the outcome at the mid-point of each bin. The error bars are the 95 percent confidence intervals for the local means. The solid lines are fourth-order global polynomials fits (estimated separately on each side of the threshold). Observations to the right of the vertical dashed line are eligible for Fonden under the heavy rainfall criteria.

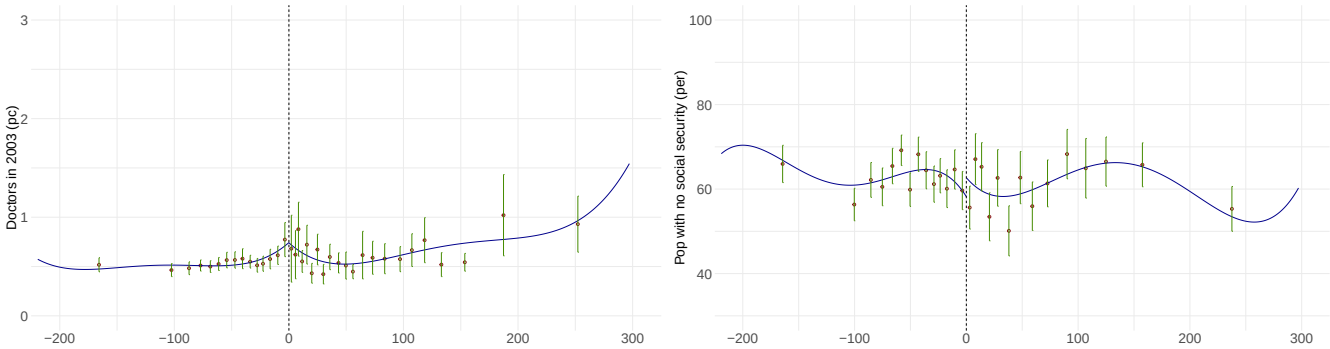


Figure A5: Predetermined Covariates V

Note: The abbreviation (per) denotes percent, and (pc) denotes per capita. Each graph plots the outcome listed on the axis label as a function of the running variable (rainfall minus threshold). In each graph, the support of the running variable has been partitioned into disjoint bins of roughly the same number of observations. The number of bins is selected to minimize the integrated mean square error of the underlying regression function, as described in Calonico, Cattaneo and Titiunik (2015). The circles plot the local mean of the outcome at the mid-point of each bin. The error bars are the 95 percent confidence intervals for the local means. The solid lines are fourth-order global polynomials fits (estimated separately on each side of the threshold). Observations to the right of the vertical dashed line are eligible for Fonden under the heavy rainfall criteria.

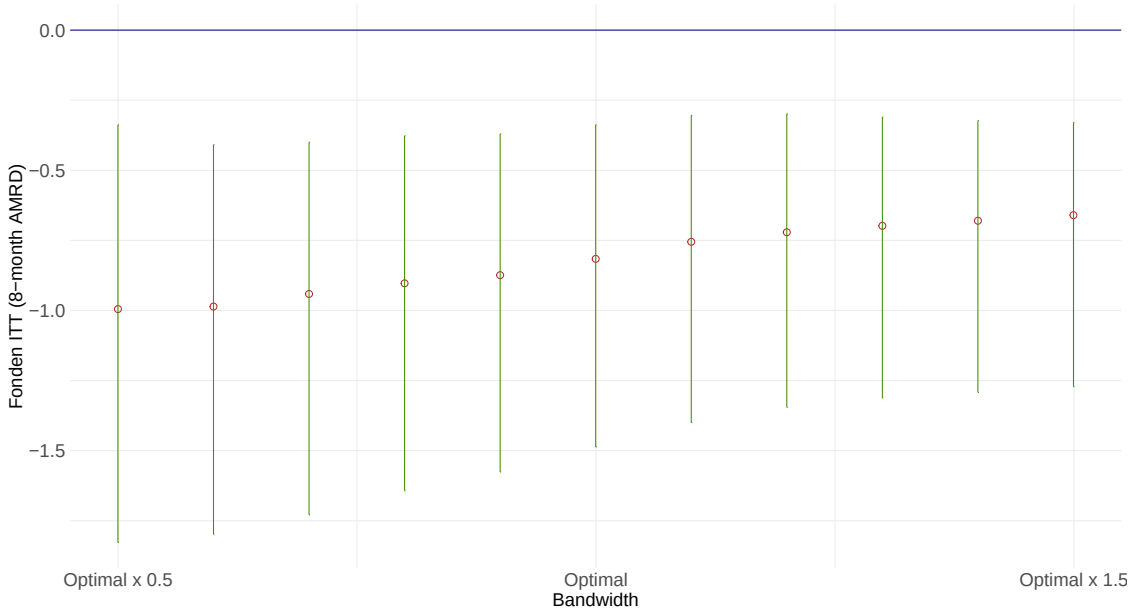


Figure A6: Fonden impact at various bandwidths

Note: Estimates of Fonden ITT at ten evenly spaced bandwidths. The smallest bandwidth, 25.72 mm, is 50 percent smaller than the optimal h_{MSE} bandwidth, the largest, 77.17 mm, is 50 percent larger than the optimal h_{MSE}. The circles represent point estimates constructed using a triangular kernel, a local linear polynomial, and the bandwidth indicated in the x-axis. The error bars represent robust 95 percent confidence intervals.

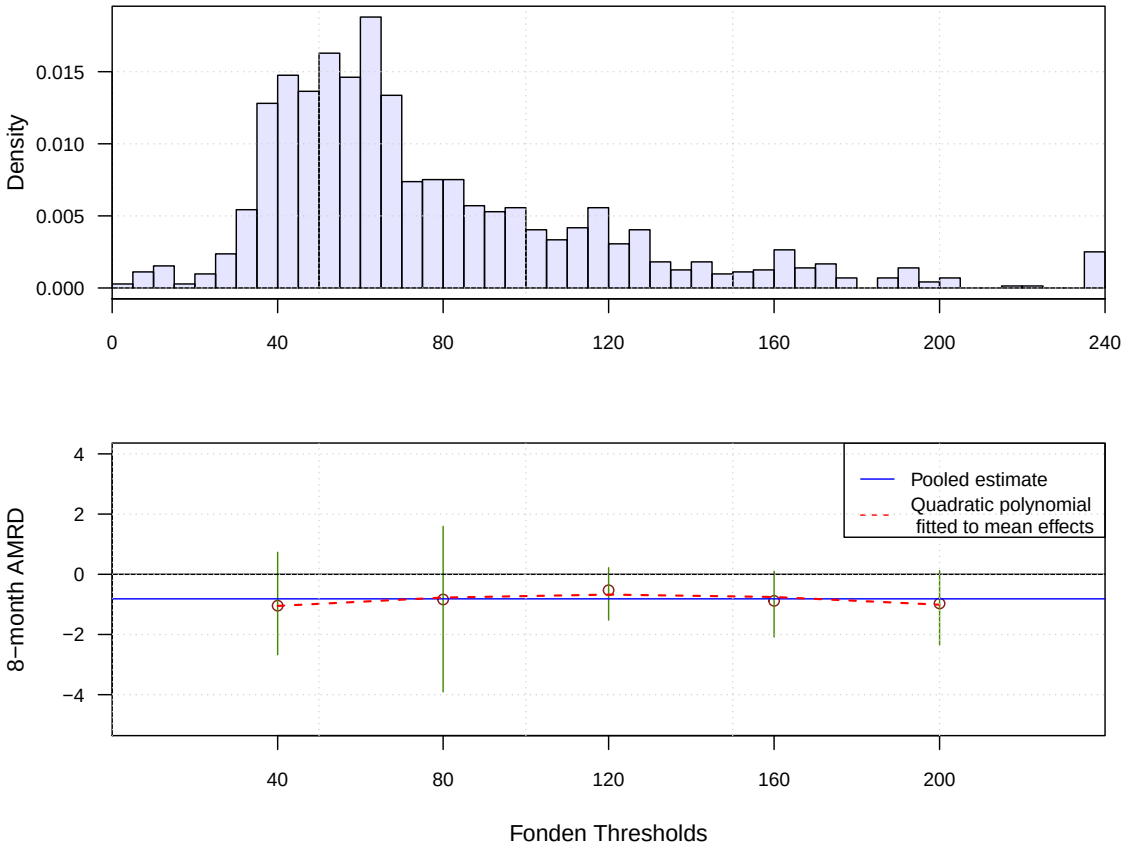


Figure A7: Fonden treatment effect curve

Note: The top panel plots the histogram of Fonden's heavy rainfall thresholds. The bottom panel plots thresholds-specific estimates of Fonden's ITT. Specifically, I estimate equation 1 using a triangular kernel, a local linear polynomial, and a h_{MSE} optimal bandwidth. The dependent variable is the 8-month all-cause annualized mortality rate difference. For each of the five threshold-specific estimates, the sample is given by the 400 observations with the closest Fonden thresholds. Point estimates are represented by red circles and 95 percent confidence intervals by green error bars). The reported confidence intervals are constructed using robust bias correction and clustering at the municipal level. The red dashed line is a quadratic polynomial fit of the five thresholds-specific estimates of Fonden's ITT. The solid blue line is Fonden's ITT estimate from table 2 column 2. This estimate is derived by normalizing and pooling the Fonden thresholds.