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**Abstract**

Why do damages from changes in environmental quality differ across and within countries? Causal investigation of this question has been challenging because differences may stem from heterogeneity in cumulative exposure or differences in socioeconomic factors such as income. We revisit the temperature-violence relationship and show that cash transfers attenuate one-half to two-thirds of the effects of higher same-day temperatures on homicides. Our results not only demonstrate causally that income can explain much of the heterogeneity in the marginal effects of higher temperatures, but also imply that social protection programs can help the poor adapt to rising temperatures.

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# 1 Introduction

Damages from changes in environmental quality such as climate change and air pollution are uneven across space and socioeconomic groups, with profound distributional consequences within and across countries (Deschênes et al., 2009; Dell et al., 2012; Arceo et al., 2016; Burgess et al., 2017; Carleton et al., 2018).[1] Larger marginal effects of environmental changes could arise from higher baseline exposure to environmental insults, or to socioeconomic factors that enable defensive investments and coping mechanisms (Hsiang et al., 2019; Banzhaf et al., 2019). Given that poorer populations tend to have both higher exposure and less capacity to reduce damages, isolating which factor dominates is an open empirical question. The distinction will determine whether reduction in environmental vulnerability among poor communities will be mainly the result of reducing their exposure to environmental hazards directly, or of policies that help these communities adapt to existing environmental hazards.

In this paper, we use the rollout of a large-scale cash transfer program to estimate the causal effect of income on reducing damages from environmental insults, more specifically, for the case of the temperature–homicides relationship. Homicides are a policy-relevant behavioral outcome varying with weather at high temporal frequency, but this class of relationships extends to other crimes and social outcomes that vary with ambient weather and even pollution (Herrnstadt and Muehlegger, 2015; Carleton and Hsiang, 2016; Bondy et al., 2020).[2] Estimating the effects of temperature on an outcome of interest requires sufficient spatial and temporal coverage to allow for location and time fixed effects, thereby identifying the causal effects of temperature deviations from historical location-specific averages (Hsiang, 2016). Our empirical challenge requires a setting with exogenous variation in income, orthogonal to weather, over a sufficiently large spatial and temporal scale to allow the detection of the cross-partial effect of income on the effects of temperature. We address this gap by leveraging daily data on homicides geolocated to the locality (sub-municipal) level from the universe of Mexican death certificates, and combine them with a discontinuity in the locality eligibility criterion for *Progresa*, one of the world's largest conditional cash transfer programs. During our study

[1] Dell et al. (2012) find that 1°C increase in temperature reduces economic growth by 1.3% in poor countries but has comparatively modest effects in rich countries. Burgess et al. (2017) show that higher temperatures by the end of the 21st century will reduce life expectancy by 10.4 years in rural India, 2.82 years in urban India and only 0.28 years in the United States. Arceo et al. (2016) find that marginal increases in carbon monoxide have larger effects on mortality in Mexico relative to the U.S. but the effects of PM2.5 on mortality are similar across the two settings. Chay and Greenstone (2003) and Currie and Walker (2011) find that the effects of air pollution on mortality are greater amongst African Americans than Whites in the United States.

[2] Recent research has documented the effects of higher temperatures on many social and economic outcomes (Carleton and Hsiang, 2016), including income per capita (Dell et al., 2012; Hsiang, 2010), growth rates (Hsiang and Jina, 2014; Burke et al., 2015), agricultural output (Jagnani et al., 2018; D'Agostino and Schlenker, 2016; Mendelsohn et al., 1994),infectious disease (McCord, 2016), labor productivity (Graff Zivin and Neidell, 2014; Somanathan et al., 2015; Masuda et al., 2019; Garg et al., 2019), human capital (Garg et al., 2018; Graff Zivin et al., 2018; Park et al., 2020) and mortality (Deschenes, 2014; Barreca, 2012).

period, *Progresa* accounted for 25% of income of poor rural households.

The temperature–violence relationship has two features that make it ideal for our empirical exercise.[3] First, we find that higher temperatures in prior days have no effect on homicides on a given day. Second, violence is one of the only outcomes that is affected linearly over the temperature distribution (Carleton and Hsiang, 2016). The combination of no displacement and linear effects allows us to estimate the attenuating effects of cash transfers on a single parameter, reducing demands on statistical power in the research design.[4]

We begin by demonstrating that there is a same-day effect of higher temperatures increasing the likelihood of homicides. We estimate the effect of temperature on homicides accounting for time-invariant factors using locality fixed effects, seasonality at the state level through state-by-month-of-year fixed effects, state level annual shocks through state-by-year fixed effects as well as abnormal crime rates during holidays using day-of-year fixed effects (Hsiang et al., 2019; Barreca et al., 2016). Results show that a 1°C increase in temperature raises the risk of a homicide occurring in a locality by 2.1%. While the economics literature has typically studied the weather-violence relationship in a framework of income shocks following Becker (1968),[5] recent evidence suggests that higher temperatures can increase violent behavior over short time horizons through mechanisms other than income.[6] The most important of these mechanisms is that higher ambient temperatures are associated with decreased serotonin levels resulting in increased irritability and aggressive behavior (Kenrick and MacFarlane, 1986; Anderson, 2001; Larrick et al., 2011). In fact, a lab-in-the-field experiment finds that heat increases aggressive behavior while not affecting most other behavioral and cognitive measures including risk-taking, time-inconsistency, patience and trust (Almås et al., 2019).[7]

We then employ difference-in-differences, event study and regression discontinuity designs to show that homicides in localities barely-eligible for *Progresa* were considerably less sensitive to higher temperatures than barely-ineligible localities. *Progresa* could affect the temperature-violence relationship either through the additional income or through effects of its programmatic features such as the health and education conditionalities or empowerment of women (Bobonis et al., 2013). A series of

[3]It should also be noted that violent crime is an alarming problem in Mexico with one of the highest homicide rates in the world at an annual estimated cost of over USD 250 billion (Mexico Peace Index, 2018).

[4]Some temperature–mortality estimations employ a 31-day lagged model; for an example in Mexico, see Cohen and Dechezleprêtre (2017).

[5]A non-exhaustive set of papers includes Collier and Hoeffler (1998); Miguel et al. (2004); Miguel (2005); Mehlum (2015); Sheetal and Storeygard (2011); Axbard (2016); Blakeslee and Fishman (2017); Vanden Eynde (2017); McGuirk and Burke (2017); Khanna et al. (2019a,b).

[6]Jacob et al. (2007) use weekly weather as an instrument to estimate the effect of high crime periods on future crime. Ranson (2014) uses monthly data and finds similar effects to ours. Three concurrent papers also examine the relationship between temperature and violence in Mexico (Baysan et al., 2019; Cohen and Gonzalez, 2018) and India (Blakeslee et al., 2018).

[7]Other plausible mechanisms include increases in interactions with others and increased alcohol use (Cohen and Gonzalez, 2018), although we provide suggestive evidence that these are unlikely to be the dominant mechanisms.

empirical tests reject the possibility that *Progresa*'s programmatic features are the dominant drivers in attenuating the temperature-homicide relationship. The additional income may be going towards households reducing their exposure through defensive investments (for example, cooling technologies (Davis et al., 2014; Barreca et al., 2016; Gertler et al., 2016)), but in fact the additional income could be used in many different ways that affect the sensitivity of violent behavior to higher temperatures and it is infeasible to separately identify the many causal chains.[8] Of specific relevance to violent outcomes is the effect of additional income on reduction in psychological stress, documented in several settings including the case of *Progresa* (Fernald and Gunnar, 2009; Haushofer and Fehr, 2014). Regardless of which mechanism dominates, our focus is to evaluate the effect of a cash transfer program as a relevant economic and policy instrument to reduce vulnerability to temperature extremes.

This paper provides several advances on earlier work. First, we provide causal evidence on the role of income in explaining heterogeneous marginal environmental damages.[9] Previous work has carefully documented an income gradient in the effects of temperature generally and the temperature-violence relationship specifically.[10] Our work builds on these papers by estimating the causal effect of income on the marginal effects of temperature. Since exposure and income are often correlated, our result is important in demonstrating that the income gradient is not primarily due to differences in baseline exposure along a non-linear dose-response function (Hsiang et al., 2019). Our finding is also important in determining the extent of damages as incomes rise, particularly in the developing world which faces greater environmental risks (Greenstone and Jack, 2015; Burke et al., 2015; Barrett et al., 2016).

Second, by showing that cash transfers significantly attenuate the effects of higher temperatures, we demonstrate a readily-available policy tool for adaptation. While quantifying the extent of adaptation is important for understanding net damages from high temperatures (Burke and Emerick, 2016; Shrader, 2017), there has been relatively little work on policy levers that enable adaptation. In the last decade, over 60 countries across 5 continents have implemented some form of a cash transfer program (Parker and Todd, 2017). Indeed, cash transfers may be a scalable adaptation policy, particularly in poor communities of developing countries where credit constraints may inhibit the adoption

[8] A review of the extensive literature on the effects of *Progresa* documents the many effects of the additional income (Parker and Todd, 2017).

[9] Social protection programs have been shown to attenuate the effects of weather-induced agricultural productivity shocks on mortality (Burgess et al., 2017), conflict (Fetzer, forthcoming), educational attainment (Adhvaryu et al., 2018) and test scores (Garg et al., 2018). Relatedly, (Mullins and White, 2019) show that Community Health Centers reduce the impact of extreme temperatures on mortality.

[10] For instance, Heilmann and Kahn (2019) examine the effect of temperature on crime in rich versus poor neighborhoods in Los Angeles and find that richer neighborhoods are less vulnerable to increases in violent crime as a result of higher temperatures. Mares (2013) similarly finds that socially disadvantaged groups in St. Louis, Missouri are more prone to experiencing high levels of violence as a consequence of climatic shocks: 20% of most disadvantaged neighborhoods are predicted to experience 50% of the climate change-related increases in violence.

of defensive investments (Davis et al., 2014).

Finally, we provide estimates of the indirect benefits of cash transfers in reducing vulnerability to environmental extremes. As research and policy experimentation proceeds, conditional and unconditional cash transfers are emerging as popular tools to improve health, educational and labor market outcomes, and a benchmark against which to gauge cost-benefit of other government and aid expenditure (Baird et al., 2011; Haushofer and Shapiro, 2016; Baird et al., 2019). Quantifying the benefits to recipients through reduced vulnerability to environmental insults solves an important omission, potentially altering the cost-benefit analysis. Indeed, our back of the envelope calculations suggest that during our study period, the attenuation of same-day temperature violence relationship due to the program generated social benefits worth 5.7% of total program costs.

# 2 Data

Descriptive statistics on key variables are presented in Appendix Table A.1. The data are compiled at the locality-day level spanning 1998-2012 for every locality with population between 50 and 5,000 persons in accordance with eligibility for *Progresa*. Of these 77,389 localities there are 19,487 localities which experienced at least one homicide over the entire study period.

## 2.1 Homicides

Data on homicides are from the Mexican National Health Information System (SINAIS) (Sistema Nacional de Informacion de Salud, 2016).[11] SINAIS registers daily death certificates in Mexico from 1998 onwards, including information on the cause of death, location (state, municipality, and locality), date and time of occurrence, and the victim's locality of residence, date of birth, sex, occupation, level of education, and weight. We limit deaths to those whose registered cause was 'intentional injury' (excluding 'self-injury', i.e. suicides) to focus on violent behavior against others. During the study period period, there were 232,375 homicides across 19,487 localities. The average number of homicides per locality per day was 0.0017.

[11] The original data was cleaned and death codes standardized with WHO Global Burden of Disease codes by the Center for US-Mexican Studies at the University of California, San Diego.

## 2.2 Weather variables

Temperature and precipitation data come from the Mexican National Meteorology Institute (SMN).[12] The SMN has approximately 5,000 weather stations distributed across Mexico. The original data reports daily minimum and maximum temperatures, as well as daily precipitation for each of these stations. To construct municipal-level weather variables, we calculate the distance from stations to municipal population-weighted centroids, using gridded population data for 2010 from CIESIN (2016). Then, for each municipality we calculate a distance-weighted average of temperature and precipitation for all stations' within 300 km, where the weights are the inverse square distance from centroids to stations. We then calculate the daily average temperature as the average of the daily minimum and maximum temperatures. All localities within the municipality are assigned the same weather values.

## 2.3 Socioeconomic variables

Socioeconomic variables, available at the municipal level, come from the National Population Council (CONAPO), which uses data from the Census and inter-censal surveys (Conteos) of the National Institute of Statistics and Geography (INEGI). This data is available every five years. CONAPO data from 1995 was assigned to 1998-1999 in our sample, the 2000 census was assigned to 2000-2004, the 2005 data was assigned to the years 2005-2009, and the 2010 census data was merged with the 2010-2012 in our data.

The first variable we consider is the Marginality index, which is a measure of lack of access to services in municipalities. CONAPO estimates it using principal components of indicators such as access and quality of education, housing, and other services.[13] A second variable is the percentage of households without electricity, since household adaptation to warmer weather may include using fans or air conditioning (INEGI, 2010). Finally, we also employ CONAPO estimates of municipal income per capita for 2000 available (CONAPO, 2001). These were calculated from 1999 state-level GDP from national accounts, downscaled to the municipal level by using the within-state distribution of municipal incomes in the 2000 census.

In addition to data from CONAPO, we obtain estimates of municipality level Gini coefficients based on the 2000 ENIGH national household survey (CONEVAL, 2000), and data on school enrollments by municipality and age from the 1995 inter-censal survey (Conteo de Poblacion y Vivienda)

[12] Station-level data for daily temperature and precipitation was provided upon direct request to the SMN (http://smn.cna.gob.mx).

[13] The index includes components such as the percentage of illiterate population above 15 years old, people with no primary education, percentage of housing occupants with no access to electricity, piped water, or with dirty floors, percentage of population that live in localities of less than 5,000 inhabitants, and percentage of working population with earnings up to 2 minimum wages. For a detailed explanation of this methodology, see CONAPO (2010).

(INAFED, 1995). These two variables are used to explore heterogeneity in *Progresa*'s effects by baseline inequality levels and educational enrollment.

### 2.4 Hospital Admissions Data

In order to check whether the observed relationship between weather and homicides is reflected in hospital admissions data, we use daily data from hospital admissions spanning 2000-2012 (Dirección General de Información de Salud, 2016). While these data only include public medical facilities, they span the entire country, and patients in the dataset reside in all 32 states, 493 municipalities (around 20% of total), and 12,397 localities (14% of total). Unfortunately, the hospital admissions data does not have locality identifiers prior to 2005 by which time the *Progresa* program had spread through most localities in Mexico. Therefore, we use the hospital admissions data as a robustness check for the effect of same-day temperature on homicides but are unable to use these data in the analysis on the attenuating effects of the cash transfer.

### 2.5 Progresa Data

We define the date of initiation of *Progresa* in each locality as the year of the first disbursement to a family. Date of first disbursement was made available upon request from the Mexican government.[14]

## 3 The Effect of Same-day Temperature on Homicides

In this section, we estimate the same-day effect of temperature on homicides. Results show that a 1°C increase in temperature increases the likelihood of a homicide occurring in that locality on the same day by an effect size of 1.6%. The effect is linear over the temperature distribution (Appendix Figure A.2(a)). We provide a number of robustness checks and suggestive evidence on the underlying mechanism.

### 3.1 Research Design

We follow standard research designs in the literature to estimate the causal effect of temperature on homicides (Hsiang, 2016; Ranson, 2014; Barreca, 2012). The analysis is at the locality-day level covering over 77,000 localities across 14 years. 99.8% of locality-days have no homicides and very few locality-days have more than one homicide. Therefore, we use a binary indicator for whether

[14] We thank Arturo Aguilar (ITAM) for these data.

any homicide took place in a given locality on a given day. We then estimate the following linear probability model:[15]

$$I\left(H_{SMLymd}>0\right)=\alpha+\beta TEMP_{Mymd}+\theta PREC_{Mymd}+\varphi_{L}+\upsilon_{Sy}+\varphi_{Sm}+\lambda_{md}+\varepsilon_{Lymd} \quad (1)$$

where $H_{SMLymd}$ is the number of homicides in locality *L* (within municipality *M* and state *S)* in year *y* in month *m* and day *d*. The dependent variable is binary, equal to 100 if there was at least one homicide on that day.[16] $TEMP_{Mymd}$ is the average temperature in Celsius degrees in a municipality *M* on that day *ymd*; and $PREC_{Mymd}$ is the precipitation (in mm) in municipality *M* on that day. We control for many potential confounds through a battery of fixed effects. Locality fixed effects ($\varphi_L$) account for time-invariant location characteristics that may be correlated to homicide levels and average weather. In the main specification, we also include state-by-year fixed effects ($\upsilon_{Sy}$) to flexibly capture sub-national trends (including the notable increase after 2007 during the "war on drugs"), state-by-month fixed effects ($\varphi_{Sm}$) to capture seasonality, as well as day-of-year (e.g. January 1) fixed effects $\lambda_{md}$ to control for abnormally high or low crime rates during holidays. $\varepsilon_{Lymd}$ is the idiosyncratic error term and we cluster our standard errors at the state level.

The causal effect of same-day temperature on the likelihood of at least one homicide in a locality on that day can be measured as $\beta$ under the standard assumption that conditional on fixed effects, the same-day temperature is exogenous to the likelihood of homicides in that locality (Hsiang, 2016). We break down the distribution of temperature into two-degree "bins" where the independent variable is an array of binary indicators for whether or not the temperature on that day lies in the temperature range codified by the bin. In Appendix Figure A.2(a), we show that this regression supports a linear specification in temperature consistent with previous work on the contemporaneous temperature-violence relationship (Carleton and Hsiang, 2016; Ranson, 2014).

## 3.2 Results

In Table 1, we report results from estimating Equation 1, the effect of same-day temperature on the likelihood of at least one homicide in a locality. On average, a one-degree Celsius increase in temperature leads to a 0.00138 percentage point increase in the likelihood of a homicide occurring on that day. The result is significant at conventional levels with a p-value less than 0.0001. The mean value of

[15]Given that number of homicides in a locality-day is a count variable, an appropriate model could also be a zero-inflated Poisson. However, since identification relies on the use of location fixed effects, the first-stage probit model is susceptible to the incidental parameters problem. In robustness checks we present similar results with a Poisson specification.

[16]We use 100 instead of 1 to allow legibility of estimated coefficients without using scientific notation. Coefficients can be directly interpreted as percentage points. Results described as effect sizes can be interpreted as percentages.

the daily homicide risk in the data is 0.066%, implying that effect size of a one-degree Celsius increase in temperature amounts to a 2.1% increase in daily homicide risk. These results are consistent with earlier work in the United States (Ranson, 2014) and concurrent work in Mexico (Baysan et al., 2019; Cohen and Gonzalez, 2018) and India (Blakeslee et al., 2018). The results are similar when we include locality-by-month-of-year fixed effects (Column 2). We find that the effects are larger in poorer areas (Column 3) and in those with lower than median electrification rate (Column 4). While the lack of electrification may prevent households from being able to invest in adaptive measures such as fans or air conditioning, one cannot infer that policies promoting electrification would attenuate the effect of temperature on homicides. Since electrification penetration is correlated with overall levels of economic development, estimating the effect of electrification on vulnerability to temperature requires a research design that allows for causal identification. This highlights the importance of a research design such as the one we employ on the effect of cash transfers in Section 4. We obtain roughly similar effect sizes when using population weights (Table 1, Column 5) and twice as large effect sizes using alternative data on hospital admissions due to violence (Table 1, Column 6) . In particular, our consistent results across homicides and hospital admissions suggests that violence, regardless of resultant mortality, arises from higher temperatures.

**Validation of Linear Specification:** Figure A.1 plots the non-parametric estimate of the relationship between homicide rates and the same-day temperature, after partialing out locality and time fixed effects, as well as precipitation. The figure suggests the linear model is a good approximation of the overall relationship (some nonlinearity may be present at the largest temperature deviations from that day's mean, but the sparsity of the data leads to noisy estimates). In Figure A.2(a) we show the results from a semi-parametric specification to explore whether different temperatures have different marginal effects, without imposing linearity. The data are in two-degree bins, where the line graphs the effect of moving the day from the omitted category (16-18 degrees) to another bin. The marginal effect of moving across temperature bins is roughly consistent over the range of temperatures in the data, again supporting the use of the linear model.

**Temporal Displacement of Homicides:** A potential challenge in interpretation of the coefficients in Table 1 could be that higher temperatures are simply displacing homicides and not generating additional homicides. In Figure 1 we present the result of a distributed lag/lead model using the daily data of homicides and temperature after controlling for rainfall, locality fixed effects, and the same time fixed effects as above. The fact that subsequent days do not have a negative coefficient suggest that the temperature increase does not lead to a homicide that would have happened anyway in the counterfactual (that is, there is no evidence of "harvesting"). In fact, the sum of the coefficients from day 0 - day 7 shows a net effect almost identical to the same-day coefficient. Additionally, evidence that the risk of homicide is only correlated to the same-day temperature also supports a contemporaneous mechanism between heat and violence as opposed to a mechanism such as an income shock that operates at more aggregate time horizons (for example, agricultural season). The figure also shows that the specification passes the leads test: future temperature deviations do not predict changes in the present-day likelihood of homicides.

**Mechanisms:** The observed effect of same-day temperature on violent behavior (as measured through homicides or hospital admissions due to violence) is consistent with an underlying psychological mechanism whereby increases in ambient temperature correspond to decreases in serotonin levels and consequently increased irritability and aggressive behavior (Anderson, 2001; Larrick et al., 2011; Baysan et al., 2019).

Other plausible mechanisms might lead temperature to affect violence at a daily timescale. One possibility is that higher temperatures lead to increased social interaction as people spend more time outdoors, increasing the opportunities for violence. Another possibility is that alcohol consumption increases from heat exposure, leading to violent behavior. However, it is unlikely that either of these mechanisms is the dominant driver in the same-day temperature homicide relationship. Both of them would lead to temperature having a larger effect on homicides during weekends, under the maintained assumption that individuals have more discretionary time to interact with others and increase their alcohol consumption on weekends. Appendix Table A.2 shows that the effect size of temperature is consistent across weekdays and weekends, suggesting that temperature-driven changes in social interaction or alcohol consumption are not the dominant mechanisms.

**Additional Robustness Checks:** Appendix Table A.3 shows a series of robustness tests. Column (1) indicates that results are robust to limiting the analysis to 1998-2007, thus excluding the years of dramatic increase in homicides due to the Mexican government's scale-up of operations against drug-trafficking organizations.[17] Columns (2) and (3) show that temperature affects homicides in the case of male victims more than female victims, perhaps in part due to the significantly lower frequency of female homicides. Column (4) excludes four states where the accuracy of the death certificate data

[17] Violent deaths in Mexico increased dramatically starting in 2007. The causes are many, though scholars underline President Calderon's 'war on drugs'declared in December 2006, as well as structural factors (weak state capacity in local governments) and the instability of drug cartels agreements (Escalante, 2009, 2011; Guerrero, 2009, 2011; Merino, 2011; Hope, 2013).

may be suspect,[18] and shows that omitting these states does not change the baseline result of how temperature affects daily homicide likelihood. Column (5) shows the same specification as column (1) with municipal-level data, producing a comparable effect size of 1.9%. In Column (6) we replicate Column (5) with a poisson estimator with an effect size of 1.3%

# 4 The Role of Cash Transfers

Empirical evidence shows that the effect of same-day temperature on homicides varies by levels of income and economic development, both across and within countries. Longitudinal studies on this relationship have been conducted, to our knowledge, in three countries - United States, India and Mexico; effects in Mexico and India (Blakeslee et al., 2018) are at least an order of magnitude larger than the effects in the United States (Ranson, 2014). Within Mexico, the effect of temperature on homicides declines as income and levels of electrification rise (Figure 2). Which of these associations is causal, if any, is of first-order importance to public policy. This section estimates the causal effect of income by exploiting the discontinuity in the locality-specific eligibility criterion for *Progresa*, and finds that the program attenuates the temperature-homicide relationship by 50-67%.

## 4.1 Background on Progresa

*Progresa* was one the first large-scale conditional cash transfer (CCT) programs in the world, beginning with an experimental sample in 1997 and scaling up to national coverage over subsequent years.[19] *Progresa*'s primary goal was to improve education and health among poor households. During the rollout years we study (1999-2003), *Progresa* accounted for 25% of poor rural household's income. The program budget was 6.8 billion dollars in 2000 US dollars, nearly half of Mexico's entire anti-poverty budget (Levy, 2007; Alix-Garcia et al., 2013).

Localities had to meet several requirements in order to be eligible for their poor households to receive *Progresa*.[20] First, since the CCT required eligible households to meet certain health and education requirements, *Progresa* was limited to localities with requisite health and education infrastructure facilities.[21] Furthermore, during the first years of national scale-up, very small and very large local-

[18] In states with limited capacity, deaths might not be registered or the information aggregation might be incomplete. We identify states that have large differences between total yearly deaths in the CONAPO death certificate data and the number of deaths calculated by demographic modelers based on the census. The state with the largest discrepancy is Guerrero, followed by Chiapas, San Luis Potosi, and Oaxaca.

[19] Starting as *Progresa* in 1997, it was renamed *Oportunidades* from 2002-2014 and later *Prospera*.

[20] We rely on the locality-level discontinuity in the nationwide expansion of the CCT as opposed to the widely-studied experiment in a smaller subset of localities. See Parker and Todd (2017) for an exhaustive review.

[21] In fact, some of the poorest localities with the highest marginality indices are not covered by the program, due to their remote location leading to an absence of health and education infrastructure and complicating program implementation.

ities were excluded based on the 1995 inter-census survey. Although the program's stated criterion was to limit *Progresa* to localities with population between 50 and 2,500, in practice many localities with population of up to 5,000 were treated (see Appendix Figure A.3). Second, localities were only eligible during the initial years of scale-up if their marginality index from the 1995 census was above a specific cutoff. The intended cutoff was -1.2; Figure A.4 shows that while the discontinuity is not perfectly adhered to, there is a very significant change in the proportion of localities receiving *Progresa* on either side of the threshold. This discontinuity continues until 2003, and disappears thereafter. For the purposes of our analysis on *Progresa*'s effects, we limit our sample to the years 1998-2003 and to localities with populations between 50 and 5,000.[22]

Although the primary instrument of the program was to provide cash transfers to poor households, the conditions that households were subject to – children in the household meeting education and health requirements – means that results should be interpreted as the effect of cash transfers in the context of the conditions of the program. In subsequent sections we provide evidence consistent *Progresa*'s effects occurring due to the direct effects of cash and not to the effects of health and education conditionalities.

## 4.2 Causal Effect of *Progresa* on the Temperature-Homicide Relationship

In order to the test the causal effect of cash transfers on the marginal effect of temperature on homicides, we rely on three strategies: a difference-in-differences design, an event study and finally a regression discontinuity design.

### 4.2.1 Difference-in-Differences

The difference-in-differences specification restricts our sample to localities in the neighborhood of the threshold marginality index value that determines the locality's eligibility for *Progresa*. The estimation equation is the following:

$$\begin{aligned} \mathbb{1} \cdot (H_{SMLymd} > 0) = & \alpha + \beta_0 TEMP_{Mymd} + \beta_1 TEMP_{Mymd} * Treat_{Ly} + \gamma Treat_{Ly} \\ & + \theta_0 PREC_{Mymd} + \theta_1 PREC_{Mymd} * Treat_{Ly} + \varphi_L + \upsilon_{Sy} + \varphi_{Sm} + \lambda_{md} + \varepsilon_{Lymd} \end{aligned} \tag{2}$$

$Treat_{Ly}$ is a binary indicator if locality $L$ received *Progresa* in year $y$. Table 2 shows the results

[22] Results are robust to alternative population thresholds of 3,500 (Appendix Table A.4, Panel A) and 2,500 individuals (Appendix Table A.4, Panel B).

from estimating equation 2. Columns (1) - (6) restrict the bandwidth to $\pm 1$, 0.8, 0.6, 0.5, 0.4 and 0.3 units of the marginality index, respectively, around the discontinuity threshold of -1.2. Our results are robust to varying the bandwidth. Across all 6 specifications, the effect of a 1°C increase in same-day temperature on the likelihood of at least one homicide remains consistent with those in Table 1. All specification show that localities receiving *Progresa* have a significantly smaller coefficient on temperature (a reduction in the effect of temperature by over 75%). Following the logic of regression discontinuity designs, two localities near to but on opposite sides of the threshold should be statistically similar, except for treatment status. This quasi-random assignment allows for causal inference at bandwidths near the threshold. Interestingly, more narrow bandwidths - considering treatment and control communities with approximately similar marginality indices - result in a larger attenuation effect of *Progresa*. One condition for valid inference is that localities did not strategically manipulate their marginality index in order to be eligible to receive *Progresa*. We test whether there is a discontinuous change in the frequency distribution of localities around the threshold, and find no evidence of strategic bunching of localities on the right side of the discontinuity (Appendix Figure A.5). We also show that our results are robust to the use of population weights (Appendix Table A.5). Finally, in Appendix Table A.6, we aggregate the daily data to the monthly level and use the number of homicides as a continuous variable rather than a binary variable. Our results are maintained across these changes. We also test the effect of *Progresa* over the entire temperature distribution in bins of 2°C each by estimating the effect of same-day temperature in a particular bin before and after a locality received *Progresa*. In Appendix Figure A.2(b) we show this attenuation through the flattening of the temperature-homicide curve.

#### 4.2.2 Event-Study Design

We employ an event study design to test whether the effects of *Progresa* demonstrate parallel trends in the pre-treatment period and whether they attenuate over time - an exercise that also serves to account for the fact that different localities have different lengths of exposure to the program (Goodman-Bacon, 2018). We modify equation (1) by including $Treat^{\tau}_{Ly}$ – indicator variables for time period relative to treatment time of $\tau = 0$ – along with their interactions with temperature:

$$
\begin{aligned}
\mathbb{1} \cdot (H_{SMLymd} > 0) =& \alpha + \sum_{\tau=-3}^{3} \beta^{\tau} TEMP_{Mymd} * Treat_{Ly}^{\tau} + \sum_{\tau=-3}^{3} \gamma^{\tau} Treat_{Ly}^{\tau} \\
& + \theta_0 PREC_{Mymd} + \theta_1 PREC_{Mymd} * Treat_{Ly} + \varphi_L + \upsilon_{Sy} + \varphi_{Sm} + \lambda_{md} + \varepsilon_{Lymd}
\end{aligned} \tag{3}
$$

Figure 3 and Appendix Table A.7 show the results. At all bandwidths, the pre-trends are flat suggesting that there is no generalized downward trend in the effect of temperature on homicides which might be confounded with the effect of *Progresa*. Prior to the start of the program, the effect size of 1°C increase in temperature is a 4% increase in the risk of a homicide in a given locality. We show that following the introduction of *Progresa*, the marginal effect of temperature decreases to almost zero. We find no evidence to suggest a reduction in the size of its effect over time. The effects are most prominent for the smallest bandwidth, where localities on either side of the discontinuity are most similar to each other.

#### 4.2.3 Regression Discontinuity Design

Finally, we estimate the marginal effect of temperature on homicides separately for each locality over the study period from 1999-2003. We modify Equation 1 with two changes: (a) we interact the coefficient on temperature with a binary indicator for each locality, (b) we then estimate the following equation separately for each state (to maintain minimal restrictions on the parameters and to reduce computational requirements):

$$
\mathbb{1} \cdot (H_{MLymd} > 0) = \alpha + \sum_{l} \beta^{L} \cdot TEMP_{Mymd} + \theta PREC_{Mymd} + \varphi_L + \upsilon_y + \varphi_m + \lambda_{md} + \varepsilon_{Lymd} \tag{4}
$$

This procedure results in a $\hat{\beta^L}$ for each locality, representing the marginal effect of a 1°C increase in same-day temperature on the likelihood of at least one homicide occurring on that day in locality $L$. Using these estimated coefficients, we employ a regression discontinuity design to estimate the effect of *Progresa* on this marginal effect of temperature. We estimate the following equation:

$$
\hat{\beta_L} = \gamma_0 + \gamma_1 \cdot CCT_L + \gamma_2 (Index_L - T) + \gamma_3 (Index_L - T) \cdot \mathbb{1}(Index_L \geq T) + \epsilon_L \tag{5}
$$

$Index_L$ is the marginalization index of locality $L$, $T$ is the treat threshold in the marginalization index (-1.2). We present results from Equation 5 graphically in Figure 4 for the effect size (Panel A) and coefficient (Panel B). As can be seen, there is a statistically significant difference in the effect size and marginal effect of temperature between eligible and ineligible localities around the cutoff.[23] We show that these results are robust to higher order polynomials (Appendix Figure 4, Appendix Table A.8) and to employing fuzzy discontinuity designs since the cutoff in marginalization index doesn't perfectly predict treatment status (Appendix Table A.9). In each check, the results are equivalent to or stronger than the baseline results.

## 4.3 Why Does *Progresa* Matter?

In this section, we investigate the mechanisms underlying *Progresa*'s attenuation of the same-day temperature-violence relationship. We classify potential mechanisms into two categories: (a) effects due to programmatic features such as the conditionality, designating women as beneficiaries, and reductions in inequality and (b) effects of additional income. We provide evidence suggesting programmatic features are not driving the result. Given that additional income will affect exposure and behavior through a myriad pathways that cannot be separately identified, we rely on a rich literature on *Progresa* and cash transfers more generally to enumerate documented plausible pathways.

### 4.3.1 Programmatic Features

**Changes in Time Use:** The main conditions on *Progresa* recipients were enrollment of children in school, routine health check-ups for all members of the household and a monthly seminar for the beneficiary (typically mothers) (Parker and Todd, 2017). One possibility is that the conditionalities had a large effect on people's time use, which either changed the exposure of potential victims to potential assailants, or changed the value of time of potential assailants with children. Two empirical tests show that the results are not driven by the conditionality. First, we estimate the attenuating effects of *Progresa* at different levels of inframarginality in compliance with educational requirements. In Appendix Table A.10 we show that across the three terciles of pre-*Progresa* school enrollment rates for ages 6-14, the attenuating effects of *Progresa* are similar. The absence of a statistical difference in treatment effects across levels of inframarginality suggest that compliance with program conditions is unlikely to be driving the result. Second, given that compliance with health[24] and education pro-

[23]Since the outcome variable is an estimate itself, the standard errors are likely overstated and provide a conservative upper bound on the confidence intervals.

[24]Time use is unlikely to be much affected by the health conditionality, since the requirements are minimal except for children under the age of 2. Children under the age of 2 are required to attend monthly checkups, children between the ages of 2 and

gram conditions would have a larger effect on time use during the week, we test whether the effects of *Progresa* differ by weekdays versus weekends. Appendix Table A.11 shows *Progresa*'s effects for weekends (Panel A) and weekdays (Panel B). The coefficients across weekdays and weekends and the corresponding effect sizes are not statistically distinguishable, which is inconsistent with *Progresa*'s effect being primarily due to time use changes from the education and health conditionalities. It is also worth noting that while *Progresa* raised educational attainment and improved health over the longer term (Parker and Todd, 2017), the effects on the temperature-homicide relationship are evident immediately upon the start of the program. To the extent that income effects of the cash transfer operate faster than *Progresa*'s benefits through health and education improvements, the immediacy of *Progresa*'s effect on reducing vulnerability to high temperatures further suggests that it operates due to the income transfer itself.

**Empowerment of Women:** *Progresa* beneficiaries were primarily women (Parker and Todd, 2017). It may be, therefore, that the effects of *Progresa* on the relationship between temperature and violence are due not to income effects per se, but to an increase in female empowerment and a subsequent reduction in violence against women (Bobonis et al., 2013). We test for this by measuring the effect of *Progresa* on the temperature-homicide relationship for female victims aged 18-45, who are more likely to be victims of domestic abuse, and comparing it to all other victims. The attenuating effects of *Progresa* among these female victims is relatively modest compared to the effect on the rest of the victims (Appendix Table A.12). The fact that our results are not overturned when we exclude female victims aged 18-45 suggests that the effects of *Progresa* are likely not driven by a female empowerment mechanism.

**Reduction in Inequality:** Since the cash transfers targeted poor households in poor locations (a two-stage targeting criteria), one consequence would be to reduce income inequality. Income inequality can reduce happiness and lead to resentment (Perez-Truglia, 2020), potentially engendering violent behavior. To test the role of income inequality, we estimate heterogeneity in the effects of temperature by terciles of the pre-*Progresa* Gini coefficient in the municipality. In Appendix Figure A.8, we show similar effects of temperature on violence across all levels of Gini suggesting that changes in income inequality are unlikely to explain the effects of *Progresa* on the temperature-violence relationship.

4 are required to attend three check-ups a year, children between the ages of 5 and 16 are required to attend two checkups a year and all other individuals are required to attend one check-up a year.

### 4.3.2 Effects of Additional Income

An expanding literature demonstrates that cash transfers affect many interrelated margins of behavior and economic outcomes (Parker and Todd, 2017). As such, it would be impossible to isolate a single causal chain through which additional income attenuates the temperature-violence relationship. A likely channel is that the additional income reduces exposure to higher temperatures through adoption of cooling technologies (Barreca et al., 2016; Gertler et al., 2016). A second possibility is that the additional income reduces overall stress levels, such that irritability due to higher temperatures is less likely to result in violent behavior. There is a growing body of evidence to suggest that poverty results in psychological stress and higher cortisol levels, while raising income can improve psychological well-being (Chemin et al., 2013; Haushofer and Fehr, 2014; Haushofer and Shapiro, 2016). Evidence from *Progresa* finds the same salubrious effect of the cash transfer on children's cortisol levels (Fernald and Gunnar, 2009).

Documented effects of *Progresa* offer other plausible mechanisms such as decreases in household debt (Angelucci et al., 2012), increased food consumption (Angelucci and De Giorgi, 2009), increases in entrepreneurial activity (Bianchi and Bobba, 2013), and migration (although the effects on migration are documented to be minimial) (Stecklov et al., 2005; Angelucci, 2015).[25] Regardless of which mechanisms operate as *Progresa* affects the temperature-violence relationship, we emphasize that this increasingly common policy lever – cash transfers – has unintended positive effects in reducing vulnerability of the poor to certain environmental damages.

## 4.4 How Large are these Benefits of Social Protection Programs?

To provide a benchmark for the magnitude of the protective effects of *Progresa*, we compute the total number of homicides attributable to daily temperatures above 18°C (the inflection point in Appendix Figure A.2b) in our sample of localities from 1998-2003. We use regression estimates to construct a counterfactual without *Progresa*, and estimate the difference in homicides with and without *Progresa* to be 2,577 homicides in these localities. As a lower bound we consider the value of a statistical life (VSL) in Mexico – USD 210,880 in 2014 dollars or USD 153,393 in 2000 dollars – as the cost of each homicide (de Lima, 2019). In practice, the social cost of a homicide is considerably larger than a VSL due to negative externalities generated from violent behavior (Mexico Peace Index, 2018). Given that the programmatic cost of *Progresa* from 1999-2003 was USD 6.9 billion in 2000 US dollars (Levy, 2007),

[25] We also test for evidence of migration around the program discontinuity and find that total population, percentage male population, percentage male population above 18 years of age and percentage of households with female heads are not different in barely eligible and barely ineligible localities (Appendix Figure A.9).

our estimates imply that the attenuation of the same-day temperature-violence relationship generated social benefits worth 5.7% of total program costs.

# 5 Conclusion

As climate change warms the planet, increases weather variability and makes extreme events more frequent, identifying efficient ways to enhance the ability of households to adapt becomes increasingly important. This is particularly imperative for poorer households in the developing world. Our research leverages the rollout of a conditional cash transfer at large geographic scale, and uniquely identifies the causal effect of the program on reducing vulnerability to weather extremes. Given scarce evidence on the potential for adaptation, the fact that scalable cash transfer programs may not only achieve development outcomes but also facilitate household adaptation to weather extremes opens a new and urgent avenue of research. 60 countries across 5 continents have designed and implemented various cash transfer programs (Parker and Todd, 2017). These well-established programs could be instrumental in fostering adaptation, particularly among the large proportion of extremely poor populations living in low- and middle-income countries (Page and Pande, 2018) who are especially vulnerable to environmental stressors (Barrett et al., 2016; Banzhaf et al., 2019), . This study only looks at the causal effect of income on a very specific portion of the wealth distribution, and only measures the effect on one outcome (homicides). Future work should test the potential of cash transfers (including unconditional ones) to reduce vulnerability at other levels of wealth and across other outcomes affected by climate change.

Our research has important implications for several ongoing debates in the literature. First, an open question remains on whether or not adaptation to climate change is likely. Previous attempts have focused on interventions such as information provision (Shrader, 2017), air-conditioning (Barreca et al., 2016), or on the use of long time series of historical data to measure total adaptation (Burke and Emerick, 2016). We show that adaptation on some margins is likely as incomes rise, and that cash transfers can serve as an important policy lever in adaptation among extremely poor populations.

Second, we contribute to answering an important question on whether heterogeneity in the marginal effects of temperature are driven by a nonlinear dose-response function or differences in income/wealth. Answering this question requires exogenous income variation (driven by factors other than weather) over a large enough geographic area and temporal scale to generate enough temperature variation. Our context is ideal, leveraging the discontinuity in Mexico's national conditional cash transfer program. Cash transfers attenuated over two-thirds of the contemporaneous temperature-violence relationship, suggesting that differences in income can explain a major share of the heterogeneity in marginal effects of higher temperatures.

Finally, an important debate considers whether cash transfers deliver benefits to recipients in the short- and long-run. While short-run evidence has been promising (Baird et al., 2011; Haushofer and Shapiro, 2016), the long-run evidence has been less encouraging (Baird et al., 2019). These important papers have considered a variety of outcomes including household asset ownership, but have not considered the potential benefits of such transfers to reducing household vulnerability to weather extremes. Our findings motivate future research to consider these resilience benefits of cash transfers particularly for long-run programs such as Universal Basic Income (UBI) that are currently being studied (Hoynes and Rothstein, 2019; Banerjee et al., 2019).

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**Ranson, Matthew**, "Crime, weather, and climate change," *Journal of Environmental Economics and Management*, 2014, *67* (3).

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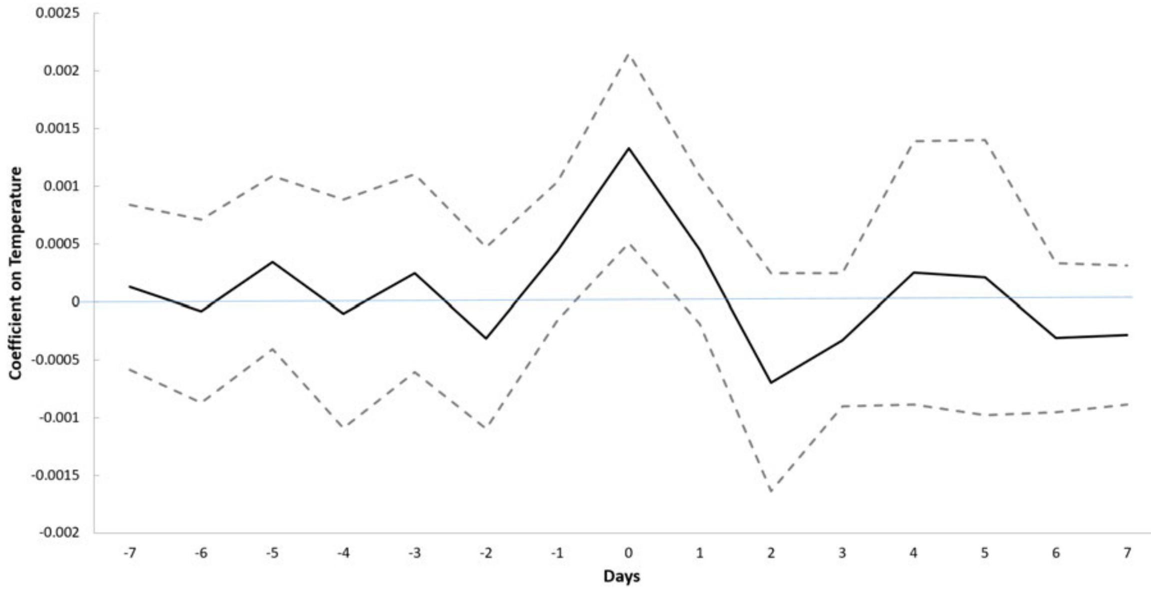
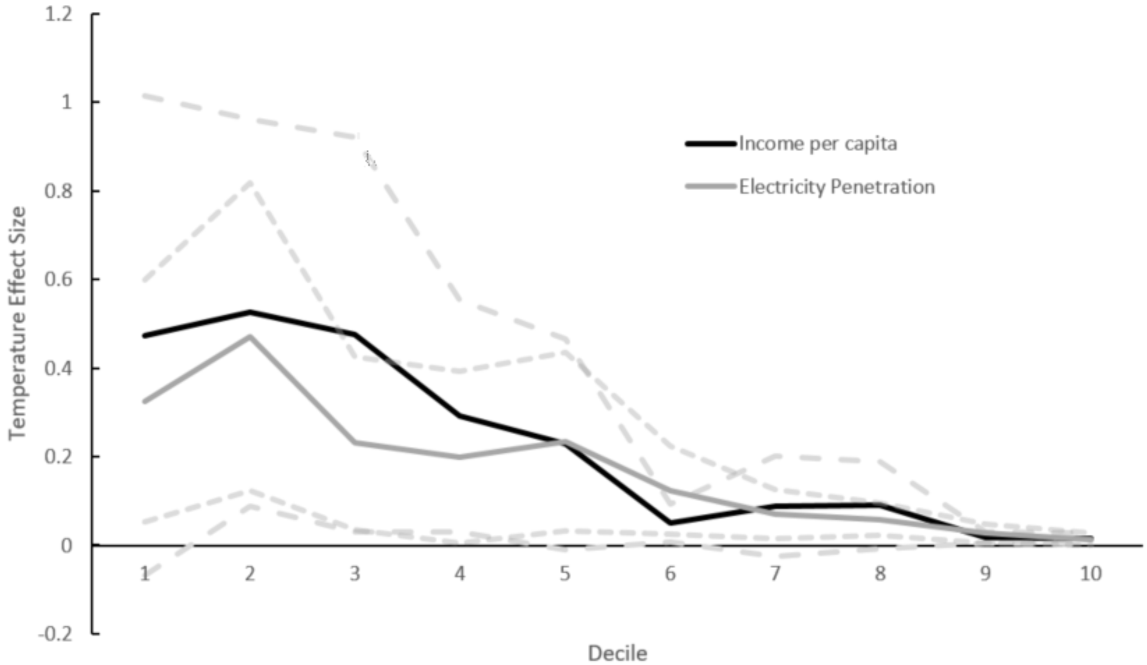
# Figures and Tables

Figure 1: Distributed Lag and Lead Models



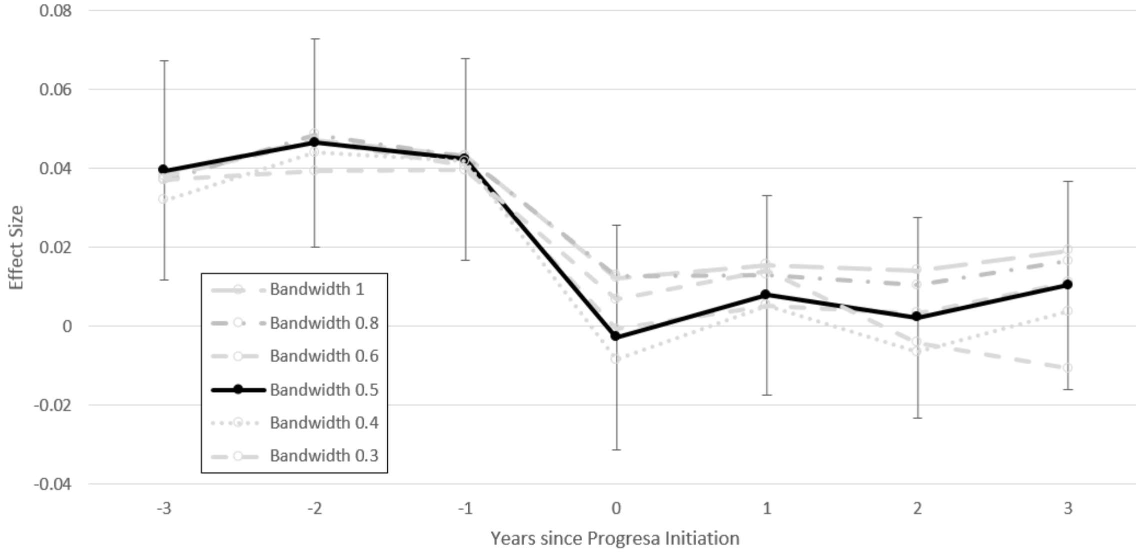

Notes. The figure shows the effects on homicide of temperature on the day of the homicide as well as each of the 7 days before and after. The solid line represents the point estimate of the effect of temperature (°C) whereas the dotted lines represent the 95% confidence intervals with standard errors clustered at the state level. Regressions include locality, state-year, state-month and day-of-year fixed effects.

Figure 2: Same-Day Effect of Temperature on Homicides by Income and Electrification

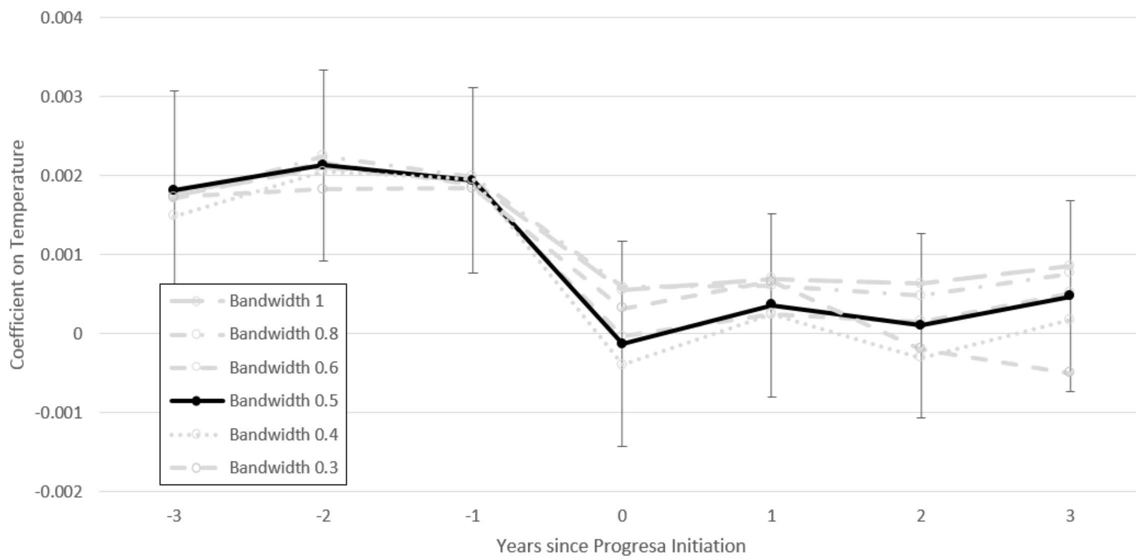



Notes. The figure shows the effect size of one degree higher temperature on same-day homicide likelihood, by deciles of income per capita and electrification in 2000 (by municipality). The solid line represents the point estimate of the effect of temperature (°C) whereas the dotted lines represent the 95% confidence intervals with standard errors clustered at the state level. Regressions include data from 1998-2007 to reduce computation time, for a total of 99,095,877 observations. Specifications include locality, state-year, state-month and day-of-year fixed effects.

Figure 3: Event Study of Progresa on Effect of Temperature at Different Bandwidths



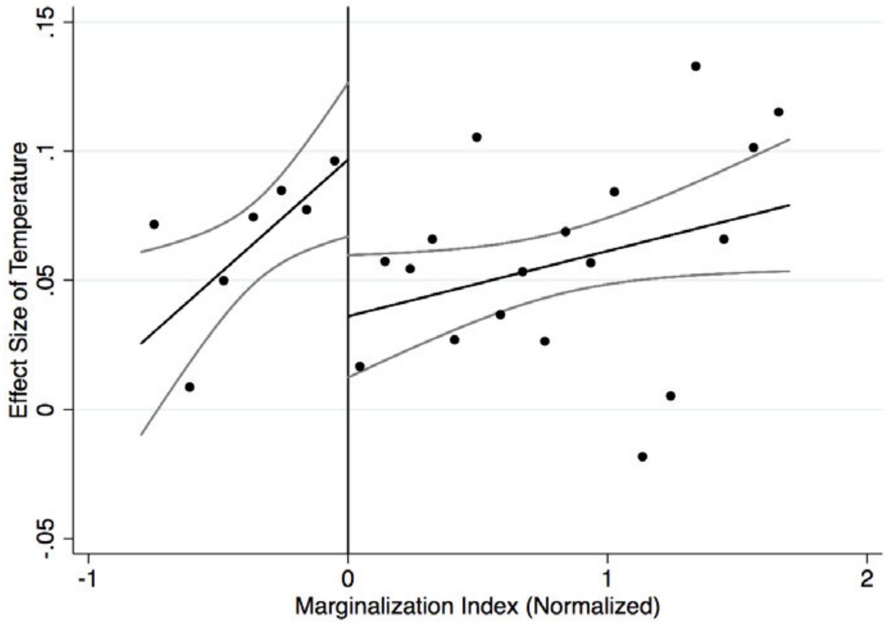

(a) Effect Size



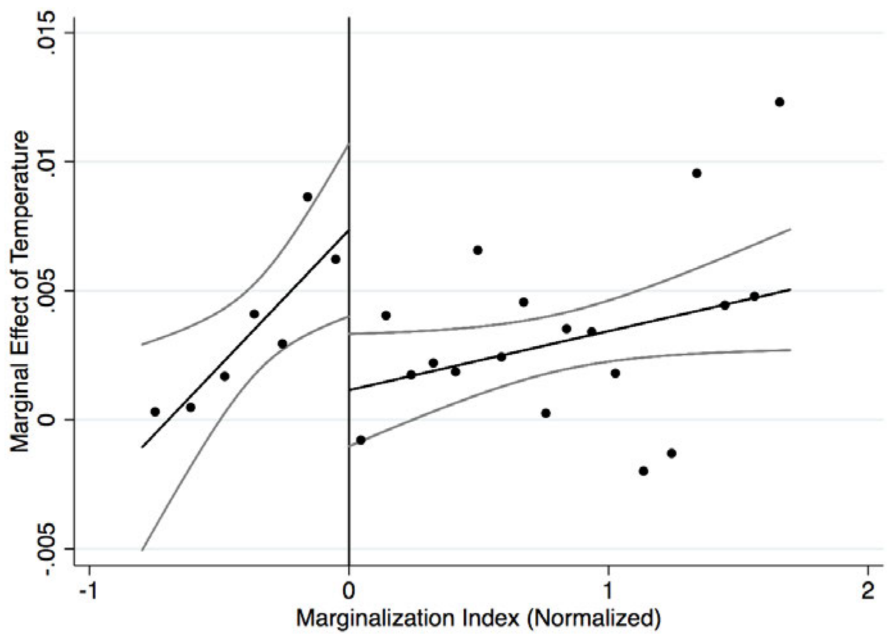

(b) Coefficient

Notes. The figures represents the event study of the effect size (Panel A) and coefficient (Panel B) of temperature (°C) on homicides before and after the introduction of *Progresa* in that locality. Time period 0 represents the start of the program in a locality. Error bars represent the 95% confidence interval for the smallest bandwidth (0.3) with standard errors clustered two way at the municipality and state-by-date level. Regressions include locality, state-year, state-month and day-of-year fixed effects. The event study is robust to regressions weighted by 1995 population (Appendix Figure A.6).

Figure 4: Effect of Temperature across CCT Discontinuity - Effect Sizes and Coefficients




(a) Effect Size: RD Estimate = -0.168 (p-value < 0.01)




(b) Coefficient: RD Estimate = -0.00863 (p-value < 0.01)

Notes. These figures show the regression discontinuity at marginality index normalized at 0 for effect sizes (Panel A) and coefficients (Panel B) of temperature on homicides. To obtain the locality-specific effect size and coefficients, Equation (4) was estimated. The dark line shows the fitted local polynomial using 1995 population weights on either side of the discontinuity. The results are robust to higher order polynomials (Appendix Figure A.7). The lighter line represents the 95% confidence interval.

Table 1: Effect of Same-Day Temperature on Homicides

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | | $\mathbb{1}\cdot$(Homicides > 0) | | | $\mathbb{1}\cdot$(Violent Hospitalizations > 0) |
| Temp (°C) | 0.00138*** | 0.00142*** | 0.000993*** | 0.00102*** | 0.121*** | 0.00484*** |
| | (0.000311) | (0.000339) | (0.000252) | (0.000250) | (0.0350) | (0.00114) |
| X Poor | | | 0.000870*** | | | |
| | | | (0.000257) | | | |
| X Low Electricity | | | | 0.000795** | | |
| | | | | (0.000303) | | |
| Observations | 200,558,224 | 200,558,224 | 200,271,839 | 196,537,093 | 200,181,037 | 37,041,459 |
| R-squared | 0.075 | 0.075 | 0.075 | 0.075 | 0.253 | 0.101 |
| Mean | 0.0661 | 0.0661 | 0.0662 | 0.0674 | 8.067 | 0.225 |
| Weights | None | None | None | None | Population | None |
| *Fixed Effects* | | | | | | |
| Locality | Yes | Yes | Yes | Yes | Yes | No |
| Municipality | No | No | No | No | No | Yes |
| State-Month | Yes | No | Yes | Yes | Yes | Yes |
| Locality-Month | No | Yes | No | No | No | No |
| State-Year | Yes | Yes | Yes | Yes | Yes | Yes |

Notes. Columns 1-5 The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. "Poor" is a binary indicator equal to 1 if the marginalization index of a locality was below median. "Low Electricity" is a binary indicator equal to 1 if a locality had below median levels of electrification. Column 5: The dependent variable is a binary indicator for whether or not a municipality had any hospitalizations due to violence on a given day. For all regressions, standard errors in parenthesis are clustered at the state level. Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2: Effect of Cash Transfers on the Marginal Effect of Temperature

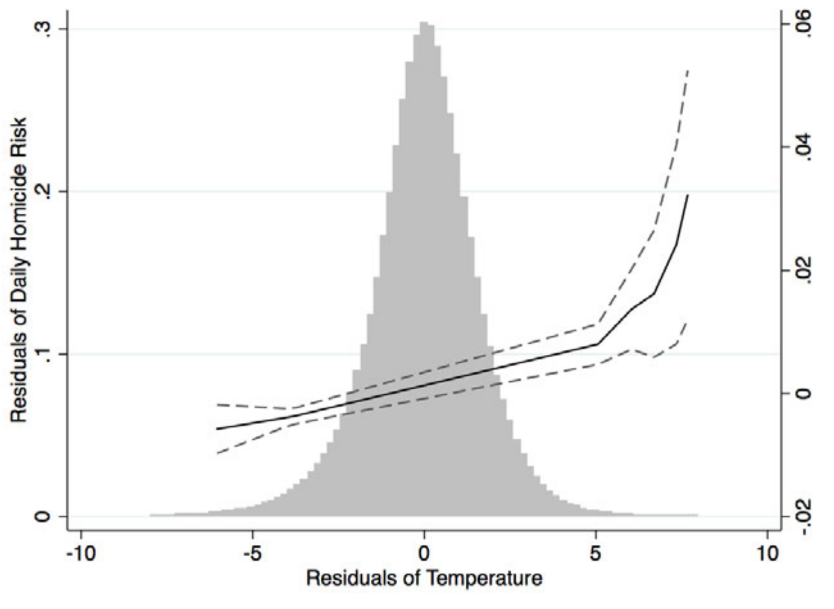
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Bandwidth | 1 | 0.8 | 0.6 | 0.5 | 0.4 | 0.3 |
| Temperature (°C) | 0.00194*** | 0.00199*** | 0.00194*** | 0.00196*** | 0.00183*** | 0.00177** |
| | (0.000473) | (0.000499) | (0.000545) | (0.000590) | (0.000657) | (0.000735) |
| X Treated | -0.00118*** | -0.00132*** | -0.00162*** | -0.00167*** | -0.00183*** | -0.00182** |
| | (0.000447) | (0.000457) | (0.000497) | (0.000541) | (0.000613) | (0.000707) |
| Treated | 0.0234** | 0.0259** | 0.0258** | 0.0231* | 0.0329** | 0.0330** |
| | (0.0106) | (0.0105) | (0.0114) | (0.0126) | (0.0142) | (0.0162) |
| Observations | 18,285,494 | 15,424,986 | 12,060,639 | 10,213,991 | 8,242,445 | 6,118,305 |
| R-squared | 0.005 | 0.006 | 0.006 | 0.005 | 0.005 | 0.005 |

Notes. The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. The population is limited to only those localities that had population in 1995 between 50 and 5,000. Robustness checks for alternative population thresholds are included in Appendix Table A.4. All regressions include locality, state-year, state-month-of-year and month-date fixed effects as well as rainfall with standard errors in parenthesis clustered two ways at the municipality and state-by-date. Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

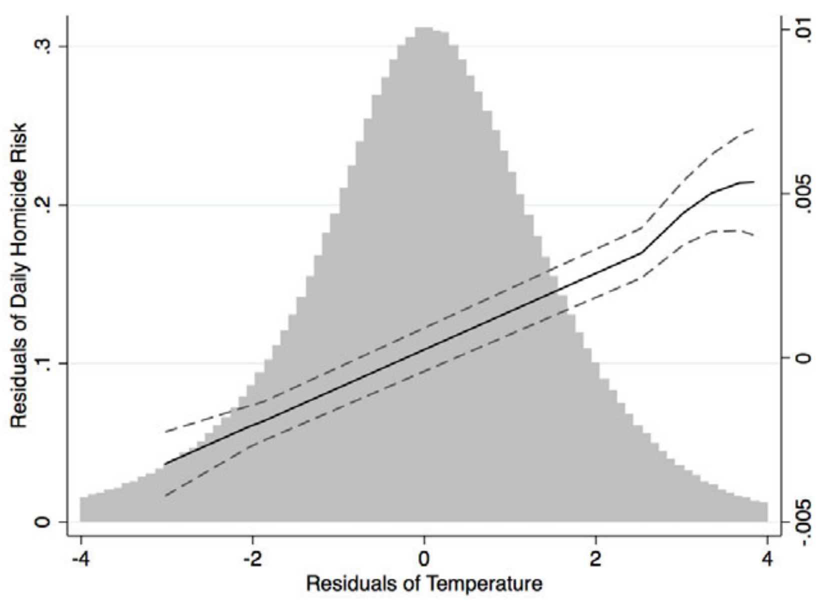
# Appendix (For Online Publication Only)

## Figures

Figure A.1: Daily Temperature Deviations and Same-Day Homicide Likelihood



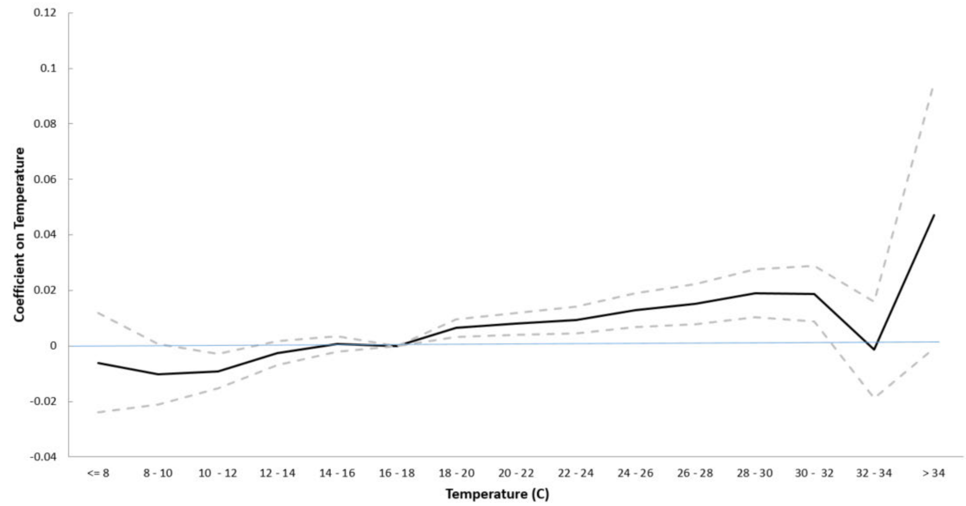

(a) Sample: 8 degree deviations



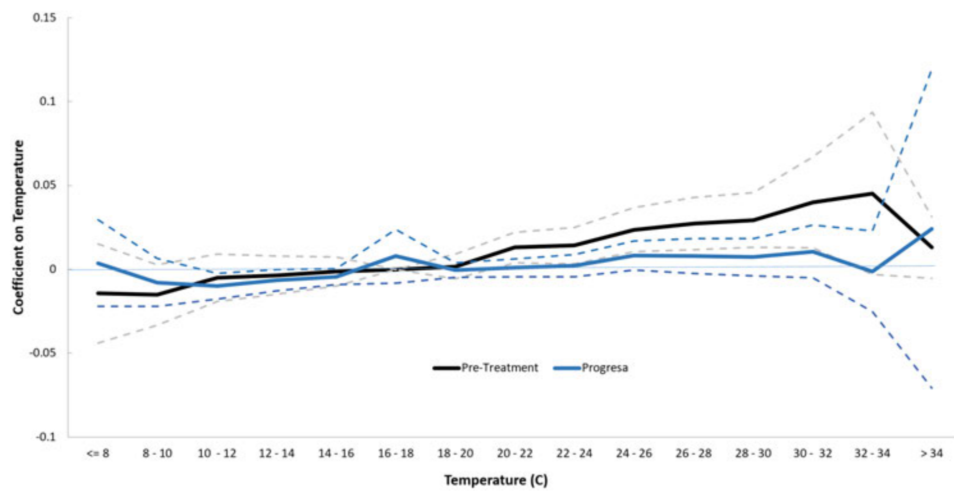

(b) Sample: 4 degree deviations

Notes. This figure shows the relationship between residuals of daily homicide risk and daily temperature after partialing out locality, state-by-year, state-by-month-of-year and day-of-year fixed effects. Panel (a) restricts the sample to up to 8 degree deviations from the mean and panel (b) restricts the sample to up to 4 degree deviations from the mean. These restrictions drop less than 1% and 5% of the observations respectively. The relationship is approximately linear for the entirety of the distribution except at the extremes with minimal density of observations.

Figure A.2: Semiparametric Estimate of Same-Day Temperature Effect



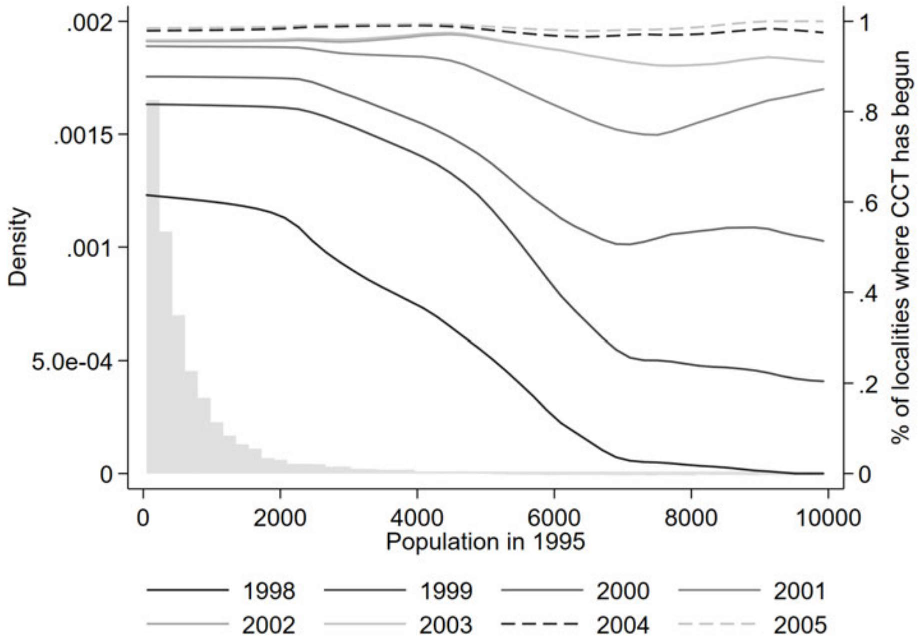

(a) Full Sample




(b) Before/After Progresa

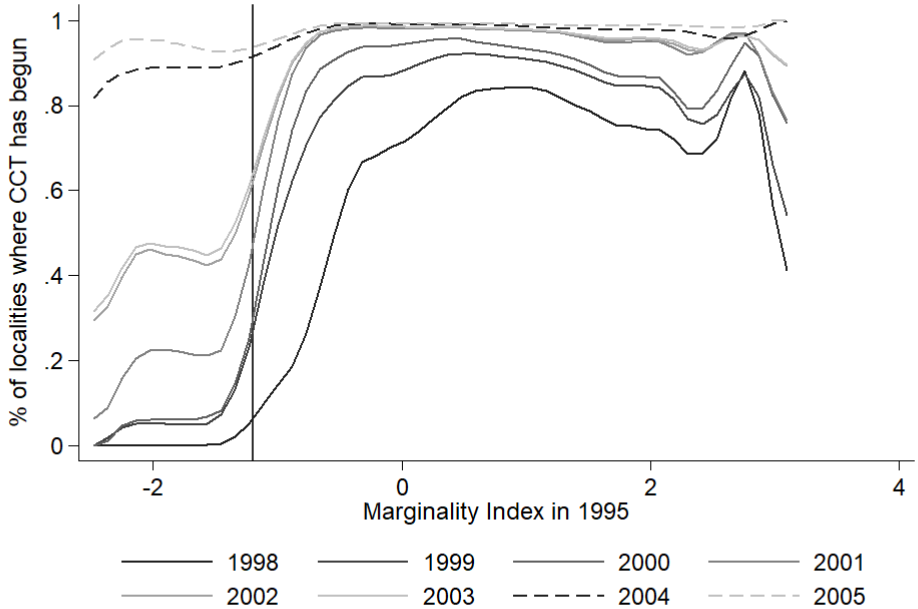
Notes. Panel (a) shows coefficient on 2-degree temperature bins in specification (1), with 95% confidence intervals over the full sample (1998-2012). Regression includes locality, state-by-year, state-by-month-of-year and day-of-year fixed effects. Standard errors clustered at the state level. The relationship is approximately linear for the entirety of the distribution except at the extremes with minimal density of observations. Panel (b) shows the same exercise limiting the sample to the years of the rollout (1999-2003) and to only localities eligible for the program. The black (blue) line denotes the effects across temperature bins before (after) *Progresa*, with corresponding 95% confidence intervals in dotted lines. Standard errors are clustered at the municipality and state-by-date levels.

Figure A.3: Localities treated by CCT Program, by Population



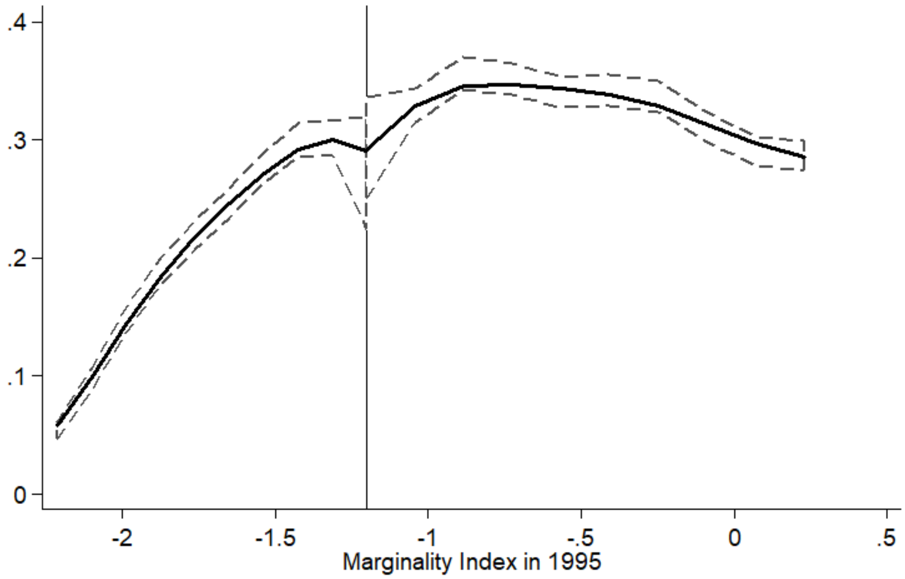

Notes. This figure shows the density of localities at various population levels (left Y-axis) and the percentage of localities that were treated with the *Progresa* program each year from 1998-2005 at various population levels (right Y-axis). We choose the 5,000 population cutoff for the main specification but show that our results are robust to using 3,500 and 2,500 population cutoffs.

Figure A.4: Localities treated by CCT Program, by Marginality Index



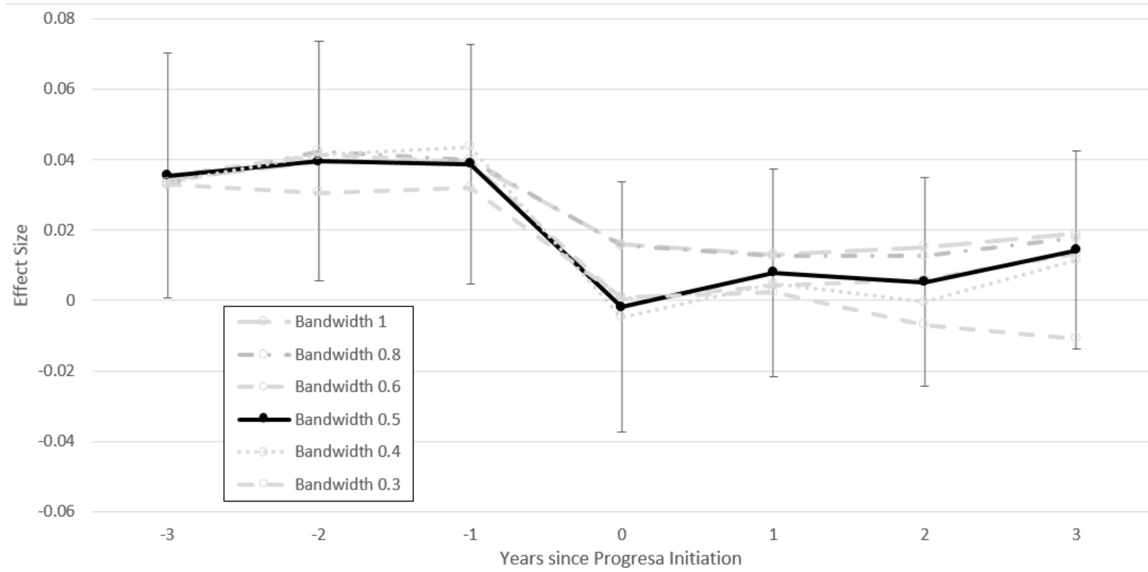

Notes. This figure shows the percentage of localities that were treated with the *Progresa* program each year from 1998-2005. The sample is restricted to localities with populations in 1995 between 50-5,000.

Figure A.5: Density Test on Progresa Eligilibity Discontinuity

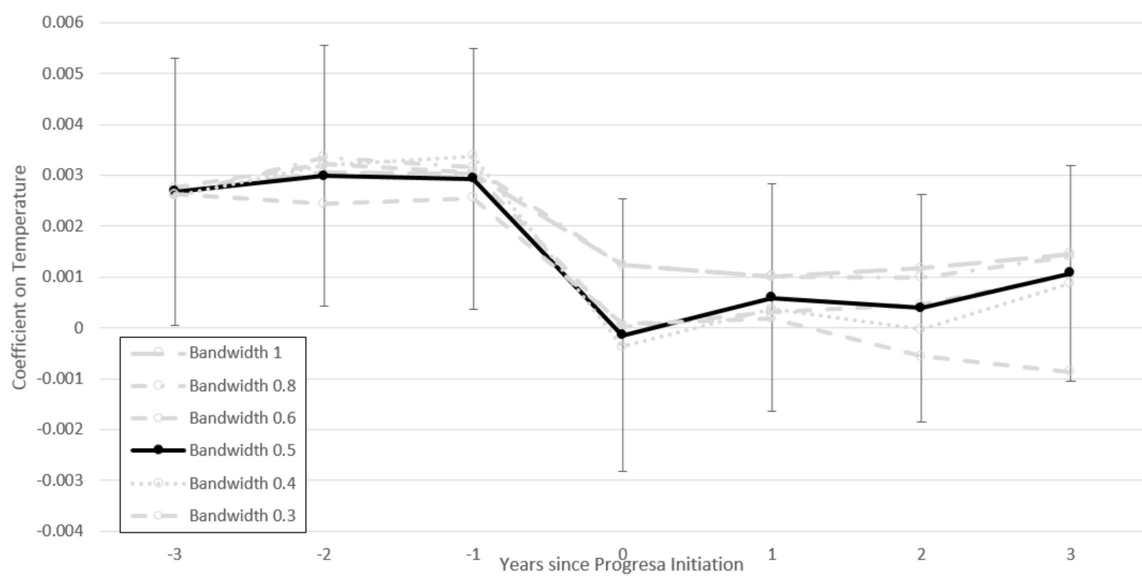



Notes. The figure displays a graph from the regression discontinuity test density test (McCrary, 2008). The X-axis shows marginality index relative to the *Progresa* treatment eligibility threshold. The Y-axis shows a kernel estimate of the density of localities in a given marginality index band. The lines display non-parametric fits to the density function along with 95% confidence intervals.

Figure A.6: Event Study of Progresa on Effect of Temperature at Different Bandwidths (Population Weights)



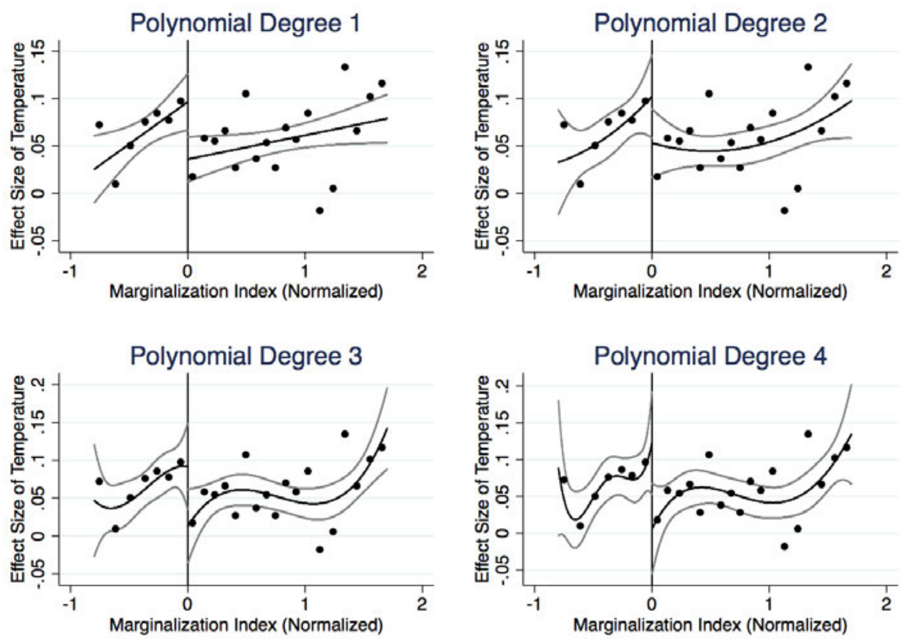

(a) Effect Size



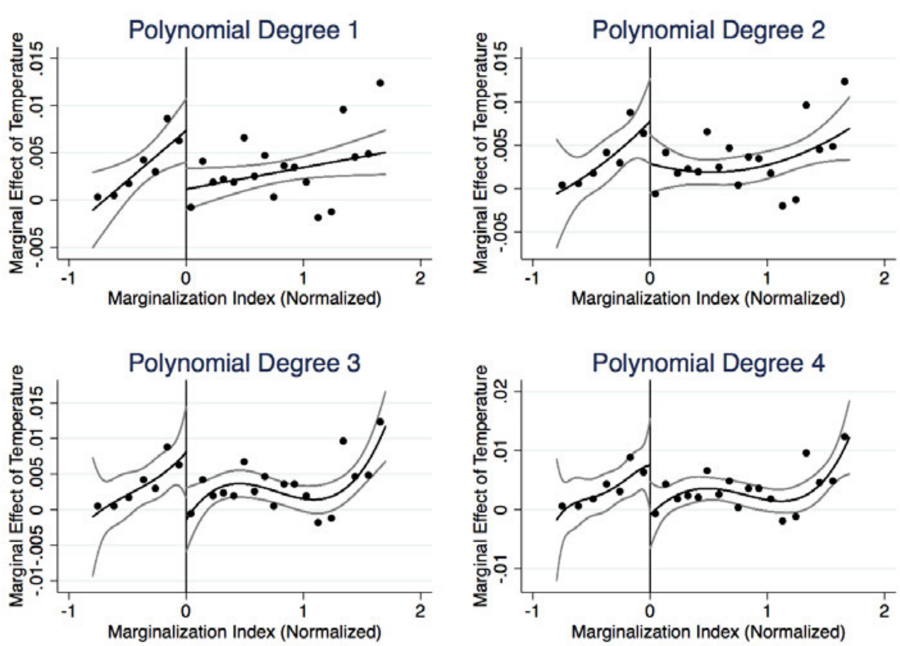

(b) Coefficient

Notes. The figures represents the event study of the effect size (Panel A) and coefficient (Panel B) of temperature (°C) on homicides before and after the introduction of *Progresa* in that locality. Time period 0 represents the start of the program in a locality. Error bars represent the 95% confidence interval for the smallest bandwidth (0.3) with standard errors clustered two way at the municipality and state-by-date level. Regressions include locality, state-year, state-month and day-of-year fixed effects and are weighted by 1995 population.

Figure A.7: Effect of Temperature across CCT Discontinuity - Effect Sizes and Coefficients



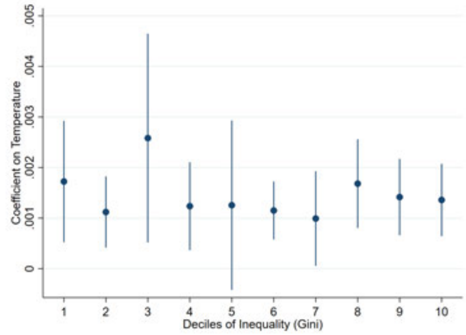

(a) Effect Size



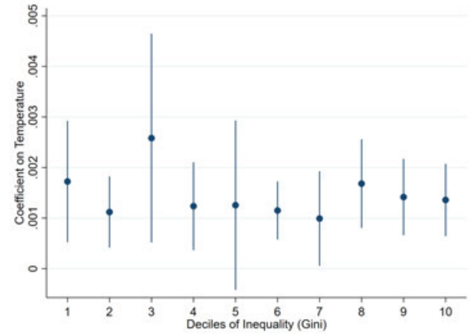

(b) Coefficient

Notes. These figures show the regression discontinuity at marginality index normalized to 0 for effect sizes (Panel A) and coefficients (Panel B) of temperature on homicides. To obtain the locality specific effect size and coefficients, Equation (4) was estimated. The dark line shows the fitted local polynomial using 1995 population weights on either side of the discontinuity. The dotted line represents the 95% confidence interval.

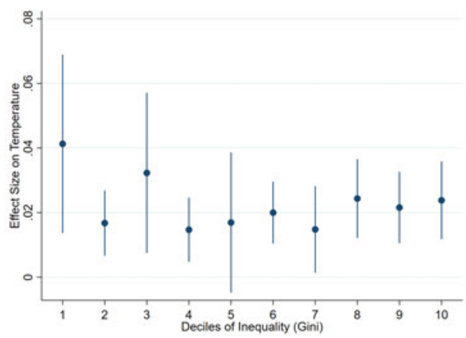
Figure A.8: Effect of Temperature on Homicides by Deciles of Inequality (Gini)



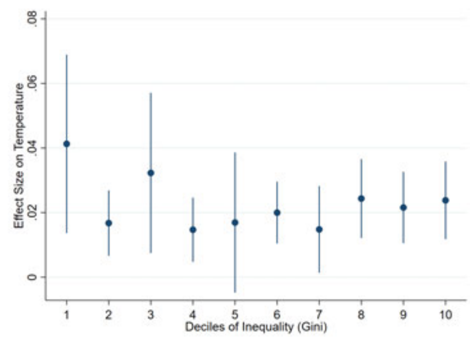

(a) Coefficient (Unweighted)




(b) Coefficient (Population Weighted)



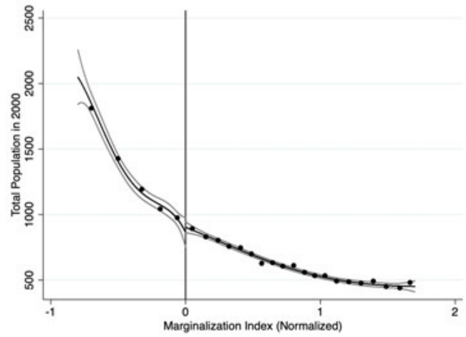

(c) Effect Size (Unweighted)



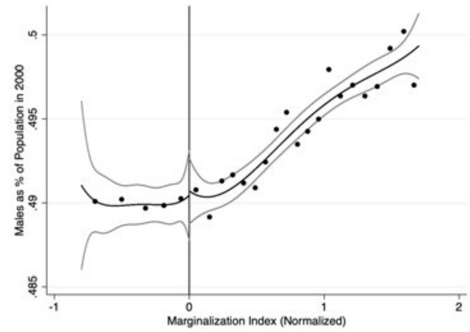

(d) Effect Size (Population Weighted)

Notes. These figures show that the effect of temperature on homicides by deciles of municipality-specific Gini coefficients. Higher deciles of Gini imply greater inequality. Panel (a) shows coefficients without population weights, Panel (b) shows coefficients with 1995 population weights, Panel (c) shows effect sizes without population weights and Panel (d) shows effect sizes with 1995 population weights. The bars represents the 95% confidence interval.

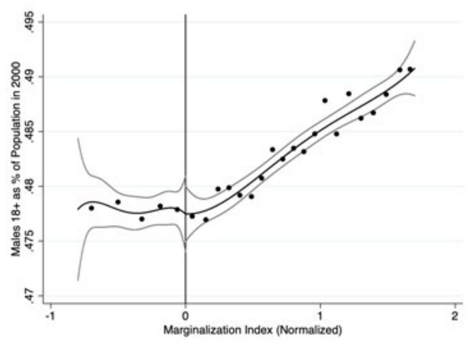
Figure A.9: Effect of Progresa on Male/Female composition



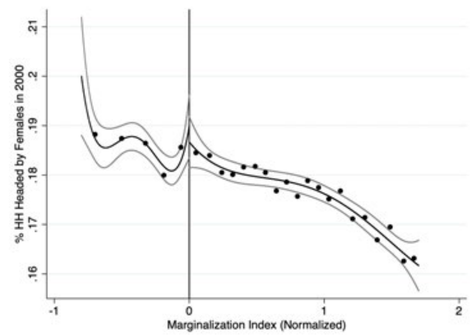

(a) Total Population




(b) Percentage Males




(c) Percentage Males 18+




(d) Percentage HH with Female Heads

Notes. These figures show that the effect of *Progresa* on migration variables: (a) total population in 2000, (b) percentage of population that is male in 2000, (c) percentage of population that is male and over the age of 18 in 2000 and (d) the percentage of households with female heads in 2000. All four variables are obtained from 2000 Census. The plots use a fourth-order polynomial.

## Tables

Table A.1: Descriptive Statistics

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Full Sample | | | | *Progresa* Sample | | | |
| | Mean | Std. Dev. | Min. | Max. | Mean | Std. Dev. | Min. | Max. |
| Homicides | 0.000857 | 0.0454 | 0 | 55 | 0.000419 | 0.0225 | 0 | 26 |
| $\mathbb{1}\cdot$(Homicides > 0) | 0.0661 | 2.570 | 0 | 100 | 0.0391 | 1.978 | 0 | 100 |
| Rainfall (mm) | 3.012 | 7.047 | 0 | 485.3 | 2.932 | 6.516 | 0 | 416.3 |
| Temperature (°C) | 20.57 | 4.58 | -13.56 | 38.98 | 20.47 | 4.60 | -3.91 | 37.34 |
| % No Electricity | 7.398 | 10.33 | 0 | 98.95 | 11.76 | 13.35 | 0 | 98.95 |
| Population (1995) | 2.087 | 27.023 | 0.050 | 1697 | 0.713 | 0.847 | 0.050 | 4.990 |

Notes. This table provides descriptive statistics of key outcome and explanatory variables. Columns (1)-(4) report on the full sample whereas Columns (5) - (8) report on the *Progresa* sample. Homicides is the number of deaths characterized as a homicide on a given day in a locality. $\mathbb{1}\cdot(\text{Homicides} > 0)$ is a binary indicator equal to 0 if there were no homicides in a locality on a given day, 100 otherwise. We use 100 instead of 1 to allow legibility of estimated coefficients without using scientific notation, and so that coefficients can be interpreted as percentage points. Rainfall is municipality-day rainfall in millimeters. Temperature is the municipality-day temperature in °C. % No Electricity denotes the percentage of households per municipality in 1995 that did not have an electricity connection. Population (1995) is the population of the locality in 1995 in 1000s of persons.

Table A.2: Same-Day Temperature on Homicides and Hospital Admissions due to Violence: Effect Sizes by Weekdays v. Weekends

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $\mathbb{1}\cdot$(Homicides>0) | | | $\mathbb{1}\cdot$(Violent Hospitalizations>0) | | |
| | All | Weekdays | Weekends | All | Weekdays | Weekends |
| Temperature (°C) | 0.0209*** | 0.0228*** | 0.0197*** | 0.0215*** | 0.0171*** | 0.0263*** |
| | (0.0047) | (0.0057) | (0.0041) | (0.0051) | (0.0056) | (0.0058) |
| Observations | 200,558,224 | 114,586,394 | 85,971,830 | 37,041,459 | 21,163,885 | 15,877,574 |
| R-squared | 0.075 | 0.073 | 0.078 | 0.101 | 0.096 | 0.109 |
| Unit of Observation | Locality-Day | | | Municipality-Day | | |
| Mean | 0.0661 | 0.0608 | 0.0731 | 0.225 | 0.206 | 0.250 |

Notes. All columns report effect sizes (coefficients divided by within-column mean of the dependent variable). Columns 1-3: The dependent variable is a binary indicator for whether or not a locality had any homicides on a given day. Regressions include rainfall, locality, state-year, state-month-of-year and month-date fixed effects with standard errors in parenthesis clustered at the state level. Column 1 is the full sample, whereas Columns 2 and 3 limit the sample to Monday-Thursday and Friday-Sunday respectively. Columns 4-6: The dependent variable is a binary indicator for whether or not a municipality had any hospitalizations due to family or non-family violence on a given day. Regressions include municipality, state-year, state-month-of-year and month-date fixed effects with standard errors in parenthesis clustered at the state level. Column 4 is the full sample, whereas Columns 5 and 6 limit the sample to Monday-Thursday and Friday-Sunday respectively. Significance levels denoted at conventional levels *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Robustness Checks For Effect of Same-Day Temperature on Homicides

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | | $\mathbb{1}\cdot$(Homicides>0) | | | Homicides |
| Temp (°C) | 0.00144*** | 0.000828*** | 0.000141 | 0.00152*** | 0.0181*** | 0.0129*** |
| | (0.000319) | (0.000215) | (9.02e-05) | (0.000389) | (0.00488) | (0.00378) |
| Observations | 105,298,170 | 200,558,224 | 200,558,224 | 149,868,270 | 12,984,447 | 12,269,643 |
| Specification | LPM | LPM | LPM | LPM | LPM | Poisson |
| Sample | Pre-2008 | Male Victims | Female Victims | Omit 4 states | All (Muni) | All (Muni) |
| Mean | 0.0664 | 0.0386 | 0.0107 | 0.0711 | 0.994 | 0.0140 |

Notes. Columns 1-4: The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. Regressions include locality, state-year, state-month-of-year and month-date fixed effects with standard errors in parenthesis clustered at the state level. Column 1 limits the sample to years before 2008, that is prior to the start of the drug war. Columns 2 and 3 limit the sample to male and female victims respectively. Column 4 omits the four states with known data quality issues. Column 5: The dependent variable is a binary indicator for whether or not a municipality had any homicides on a given day. Columns 1-5 use a linear probability model. Column 6: The dependent variable is a count of the number of homicides on a given day at the municipality level. The estimation employs a poisson regression using pseudo maximum likelihood. Regressions include municipality, state-year, state-month-of-year and month-date fixed effects with standard errors in parenthesis clustered at the state level. Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.4: Effect of Cash Transfers on the Marginal Effect of Temperature (Alternative Population Cutoffs)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Bandwidth | 1 | 0.8 | 0.6 | 0.5 | 0.4 | 0.3 |
| | | | *Panel A: Population Cutoff = 3500* | | | |
| Temperature (°C) | 0.00183*** | 0.00184*** | 0.00185*** | 0.00185*** | 0.00157** | 0.00160** |
| | (0.000446) | (0.000475) | (0.000527) | (0.000568) | (0.000627) | (0.000686) |
| X Treated | -0.00112*** | -0.00121*** | -0.00143*** | -0.00150*** | -0.00157*** | -0.00149** |
| | (0.000418) | (0.000429) | (0.000472) | (0.000519) | (0.000589) | (0.000667) |
| Observations | 17,549,707 | 14,752,736 | 11,545,041 | 9,782,745 | 7,897,378 | 5,848,460 |
| R-squared | 0.006 | 0.006 | 0.006 | 0.005 | 0.005 | 0.005 |
| | | | *Panel B: Population Cutoff = 2500* | | | |
| Temperature (°C) | 0.00185*** | 0.00183*** | 0.00181*** | 0.00188*** | 0.00160*** | 0.00160** |
| | (0.000426) | (0.000450) | (0.000499) | (0.000543) | (0.000599) | (0.000669) |
| X Treated | -0.00118*** | -0.00127*** | -0.00144*** | -0.00156*** | -0.00150*** | -0.00145** |
| | (0.000391) | (0.000405) | (0.000445) | (0.000492) | (0.000566) | (0.000612) |
| Observations | 16,666,448 | 13,968,430 | 10,933,453 | 9,272,297 | 7,479,675 | 5,552,345 |
| R-squared | 0.004 | 0.004 | 0.004 | 0.004 | 0.004 | 0.004 |

Notes. The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. The population is limited to only those localities that had population in 1995 between 50 and 3500 (Panel A) and 2500 (Panel B). All regressions include locality, state-year, state-month-of-year and month-date fixed effects with standard errors in parenthesis clustered two ways at the municipality and state-by-date. Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.5: Effect of Cash Transfers on the Marginal Effect of Temperature (Using Population Weights)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Bandwidth | 1 | 0.8 | 0.6 | 0.5 | 0.4 | 0.3 |
| Temperature (°C) | 0.00296*** | 0.00311*** | 0.00308*** | 0.00293** | 0.00315** | 0.00250 |
| | (0.00102) | (0.00107) | (0.00116) | (0.00126) | (0.00140) | (0.00163) |
| X Treated | -0.00171* | -0.00192** | -0.00253** | -0.00235** | -0.00287** | -0.00281* |
| | (0.000979) | (0.000980) | (0.00107) | (0.00115) | (0.00130) | (0.00156) |
| Treated | 0.0458** | 0.0500** | 0.0542** | 0.0482** | 0.0635** | 0.0607** |
| | (0.0214) | (0.0202) | (0.0220) | (0.0236) | (0.0270) | (0.0299) |
| Observations | 18,285,494 | 15,424,986 | 12,060,639 | 10,213,991 | 8,242,445 | 6,118,305 |
| R-squared | 0.007 | 0.007 | 0.007 | 0.005 | 0.006 | 0.006 |
| State-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| State-Month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Month-Date FE | No | No | No | No | No | No |

Notes. The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. The sample is limited to localities with population in 1995 between 50 and 5,000. All regressions include locality, state-year, state-month-of-year and month-date fixed effects as well as rainfall with standard errors in parenthesis clustered two ways at the municipality and state-by-date. All regressions are weighted by population in 1995. Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.6: Effect of Cash Transfers on the Marginal Effect of Temperature: Monthly Aggregation

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Bandwidth | 1 | 0.8 | 0.6 | 0.5 | 0.4 | 0.3 |
| Temp (°C) | 0.000730*** | 0.000654** | 0.000617* | 0.000707* | 0.000725* | 0.000643 |
| | (0.000278) | (0.000307) | (0.000346) | (0.000365) | (0.000424) | (0.000503) |
| X Treated | -0.000509*** | -0.000534*** | -0.000634*** | -0.000655*** | -0.000727*** | -0.000628** |
| | (0.000192) | (0.000195) | (0.000210) | (0.000221) | (0.000251) | (0.000279) |
| Mean | 0.0146 | 0.0151 | 0.0151 | 0.0149 | 0.0152 | 0.0151 |
| Observations | 601,008 | 506,988 | 396,408 | 335,712 | 270,912 | 201,096 |
| R-squared | 0.136 | 0.141 | 0.150 | 0.124 | 0.132 | 0.132 |

Notes. The dependent variable is a continuous measure for the number of homicides in a locality in a given month. The population is limited to only those localities that had population in 1995 between 50 and 5,000. Regressions include locality, state-year and, state-month-of-year fixed effects. Standard errors in parenthesis clustered two ways at the municipality and state-by-date. Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.7: Event Study of the Effect of Cash Transfers on the Marginal Effect of Temperature

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Bandwidth | 1 | 0.8 | 0.6 | 0.5 | 0.4 | 0.3 |
| | | | *Panel A: No Population Weights* | | | |
| Temperature (°C) | | | | | | |
| X Time = -3 | 0.00175*** | 0.00171*** | 0.00176*** | 0.00181*** | 0.00149** | 0.00173** |
| | (0.000515) | (0.000545) | (0.000610) | (0.000647) | (0.000714) | (0.000740) |
| X Time = -2 | 0.00211*** | 0.00225*** | 0.00217*** | 0.00213*** | 0.00205*** | 0.00183** |
| | (0.000502) | (0.000526) | (0.000567) | (0.000617) | (0.000688) | (0.000760) |
| X Time = -1 | 0.00193*** | 0.00199*** | 0.00188*** | 0.00194*** | 0.00195*** | 0.00184** |
| | (0.000480) | (0.000502) | (0.000546) | (0.000598) | (0.000677) | (0.000792) |
| X Time = 0 | 0.000549 | 0.000593 | -3.15e-05 | -0.000127 | -0.000394 | 0.000319 |
| | (0.000496) | (0.000547) | (0.000616) | (0.000664) | (0.000789) | (0.000787) |
| X Time = 1 | 0.000694 | 0.000602 | 0.000244 | 0.000363 | 0.000249 | 0.000661 |
| | (0.000424) | (0.000468) | (0.000531) | (0.000590) | (0.000700) | (0.000794) |
| X Time = 2 | 0.000634 | 0.000484 | 0.000154 | 0.000104 | -0.000304 | -0.000193 |
| | (0.000410) | (0.000460) | (0.000547) | (0.000593) | (0.000698) | (0.000858) |
| X Time = 3 | 0.000859** | 0.000766* | 0.000512 | 0.000475 | 0.000177 | -0.000498 |
| | (0.000398) | (0.000452) | (0.000540) | (0.000618) | (0.000745) | (0.000987) |
| Treated | 0.0298** | 0.0318** | 0.0346** | 0.0313* | 0.0426** | 0.0214 |
| | (0.0139) | (0.0141) | (0.0161) | (0.0172) | (0.0200) | (0.0190) |
| Observations | 18,285,494 | 15,424,986 | 12,060,639 | 10,213,991 | 8,242,445 | 6,118,305 |
| R-squared | 0.005 | 0.006 | 0.006 | 0.005 | 0.005 | 0.005 |
| | | | *Panel B: 1995 Population Weights* | | | |
| Temperature (°C) | | | | | | |
| X Time = -3 | 0.00267** | 0.00262** | 0.00276** | 0.00268** | 0.00260* | 0.00263 |
| | (0.00107) | (0.00113) | (0.00126) | (0.00134) | (0.00152) | (0.00163) |
| X Time = -2 | 0.00305*** | 0.00336*** | 0.00323*** | 0.00299** | 0.00320** | 0.00244 |
| | (0.00106) | (0.00112) | (0.00120) | (0.00131) | (0.00145) | (0.00167) |
| X Time = -1 | 0.00303*** | 0.00316*** | 0.00306** | 0.00293** | 0.00338** | 0.00255 |
| | (0.00105) | (0.00110) | (0.00121) | (0.00131) | (0.00147) | (0.00174) |
| X Time = 0 | 0.00123 | 0.00124 | 2.75e-05 | -0.000144 | -0.000368 | 7.34e-05 |
| | (0.00103) | (0.00114) | (0.00127) | (0.00137) | (0.00156) | (0.00148) |
| X Time = 1 | 0.00101 | 0.00100 | 0.000319 | 0.000594 | 0.000371 | 0.000185 |
| | (0.000856) | (0.000934) | (0.00104) | (0.00114) | (0.00129) | (0.00142) |
| X Time = 2 | 0.00117 | 0.000995 | 0.000459 | 0.000393 | -3.10e-05 | -0.000555 |
| | (0.000840) | (0.000918) | (0.00105) | (0.00114) | (0.00127) | (0.00151) |
| X Time = 3 | 0.00146* | 0.00140 | 0.00104 | 0.00108 | 0.000877 | -0.000867 |
| | (0.000808) | (0.000888) | (0.00101) | (0.00108) | (0.00123) | (0.00158) |
| Treated | 0.0533* | 0.0582* | 0.0693** | 0.0635* | 0.0826** | 0.0481 |
| | (0.0298) | (0.0301) | (0.0343) | (0.0358) | (0.0418) | (0.0361) |
| Observations | 18,285,494 | 15,424,986 | 12,060,639 | 10,213,991 | 8,242,445 | 6,118,305 |
| R-squared | 0.007 | 0.007 | 0.007 | 0.005 | 0.006 | 0.006 |

Notes. The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. The population is limited to only those localities that had population in 1995 between 50 and 5,000. All regressions include locality, state-year, state-month-of-year and month-date fixed effects as well as rainfall with standard errors in parenthesis clustered two ways at the municipality and state-by-date. In Panel A, regressions are unweighted whereas in Panel B, regressions are weighted by the population of the locality in 1995. Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.8: Effect of Cash Transfers on Effect Size and Marginal Effect of Temperature: Regression Discontinuity

| | (1) Degree 1 | (2) Degree 2 | (3) Degree 3 | (4) Degree 4 |
|---|---|---|---|---|
| | Panel A: Effect Sizes | | | |
| Conventional | -0.168*** | -0.189** | -0.217*** | -0.220** |
| | (0.0616) | (0.0739) | (0.0816) | (0.0860) |
| Bias-corrected | -0.193*** | -0.207*** | -0.227*** | -0.220** |
| | (0.0616) | (0.0739) | (0.0816) | (0.0860) |
| Robust | -0.193*** | -0.207** | -0.227** | -0.220** |
| | (0.0695) | (0.0833) | (0.0903) | (0.0935) |
| Observations | 702 | 1,060 | 1,518 | 2,078 |
| | Panel B: Coefficients | | | |
| Conventional | -0.00863** | -0.0161** | -0.0180** | -0.0189** |
| | (0.00415) | (0.00700) | (0.00749) | (0.00888) |
| Bias-corrected | -0.00948** | -0.0180** | -0.0195*** | -0.0186** |
| | (0.00415) | (0.00700) | (0.00749) | (0.00888) |
| Robust | -0.00948* | -0.0180** | -0.0195** | -0.0186* |
| | (0.00494) | (0.00780) | (0.00818) | (0.00963) |
| Observations | 1,302 | 1,025 | 1,569 | 1,692 |

Notes. This table estimates the sharp regression discontinuity treatment effects of CCT on the effect size (Panel A) and marginal effect (Panel B) of temperature on homicides corresponding to Figure 4. Columns (1)-(4) report conventional and bias-corrected standard estimates with conventional and robust standard errors following Calonico et al. (2014). Significance levels denoted at conventional levels *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: Sharp and Fuzzy Regression Discontinuity Estimates

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | Fuzzy Regression Discontinuity | | | | |
| | Sharp RD | 1999 | 2000 | 2001 | 2002 | 2003 |
| | | | Panel A: Effect Sizes | | | |
| Conventional | -0.168*** | -0.769** | -0.754** | -0.493** | -0.501** | -0.482** |
| | (0.0616) | (0.341) | (0.349) | (0.200) | (0.204) | (0.196) |
| Bias-corrected | -0.193*** | -0.803** | -0.752** | -0.533*** | -0.546*** | -0.530*** |
| | (0.0616) | (0.341) | (0.349) | (0.200) | (0.204) | (0.196) |
| Robust | -0.193*** | -0.803** | -0.752* | -0.533** | -0.546** | -0.530** |
| | (0.0695) | (0.384) | (0.394) | (0.226) | (0.231) | (0.221) |
| Observations | 702 | 709 | 709 | 709 | 709 | 709 |
| | | | Panel B: Coefficients | | | |
| Conventional | -0.00863** | -0.0448* | -0.0476* | -0.0271** | -0.0272** | -0.0257** |
| | (0.00415) | (0.0235) | (0.0257) | (0.0136) | (0.0137) | (0.0128) |
| Bias-corrected | -0.00948** | -0.0484** | -0.0502* | -0.0290** | -0.0280** | -0.0264** |
| | (0.00415) | (0.0235) | (0.0257) | (0.0136) | (0.0137) | (0.0128) |
| Robust | -0.00948* | -0.0484* | -0.0502 | -0.0290* | -0.0280* | -0.0264* |
| | (0.00494) | (0.0281) | (0.0308) | (0.0163) | (0.0164) | (0.0153) |
| Observations | 1,302 | 1,328 | 1,328 | 1,328 | 1,328 | 1,328 |

Notes. This table estimates the sharp and fuzzy regression discontinuity treatment effects of CCT on the effect size (Panel A) and marginal effect (Panel B) of temperature on homicides corresponding to Figure 4. Each column reports conventional and bias-corrected standard estimates with conventional and robust standard errors following Calonico et al. (2014). All specifications use a fourth order polynomial. Column (1) estimates a sharp RD design. Columns (2)-(6) report fuzzy RD estimates with varying treatment years from 1999-2003. Significance levels denoted at conventional levels *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: Effect of Cash Transfers on the Marginal Effect of Temperature by Educational Enrollment

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Bandwidth | 1 | 0.8 | 0.6 | 0.5 | 0.4 | 0.3 |
| | | | *Panel A: Bottom Tercile of Educational Attainment* | | | |
| Temperature (°C) | 0.00184** | 0.00195** | 0.00247** | 0.00229* | 0.00207 | 0.00188 |
| | (0.000881) | (0.000955) | (0.00113) | (0.00126) | (0.00142) | (0.00172) |
| X Treated | -0.00125 | -0.00144* | -0.00203** | -0.00185* | -0.00186 | -0.00196 |
| | (0.000799) | (0.000843) | (0.000947) | (0.00108) | (0.00124) | (0.00154) |
| Observations | 5,356,082 | 4,445,523 | 3,413,389 | 2,854,779 | 2,287,042 | 1,702,874 |
| R-squared | 0.008 | 0.009 | 0.010 | 0.006 | 0.006 | 0.006 |
| | | | *Panel B: Middle Tercile of Educational Attainment* | | | |
| Temperature (°C) | 0.00135** | 0.00137** | 0.00163** | 0.00139* | 0.00133 | 0.00108 |
| | (0.000573) | (0.000613) | (0.000699) | (0.000764) | (0.000859) | (0.000958) |
| X Treated | -0.000833 | -0.000976* | -0.00159** | -0.00146** | -0.00150* | -0.00119 |
| | (0.000549) | (0.000572) | (0.000627) | (0.000701) | (0.000799) | (0.000928) |
| Observations | 11,799,292 | 9,911,735 | 7,741,611 | 6,528,384 | 5,249,061 | 3,897,092 |
| R-squared | 0.006 | 0.006 | 0.007 | 0.005 | 0.005 | 0.005 |
| | | | *Panel C: Top Tercile of Educational Attainment* | | | |
| Temperature (°C) | 0.00237*** | 0.00244*** | 0.00235*** | 0.00253*** | 0.00221*** | 0.00231** |
| | (0.000583) | (0.000617) | (0.000672) | (0.000730) | (0.000806) | (0.000942) |
| X Treated | -0.00143*** | -0.00157*** | -0.00172*** | -0.00181*** | -0.00194*** | -0.00219** |
| | (0.000534) | (0.000545) | (0.000590) | (0.000637) | (0.000713) | (0.000854) |
| Observations | 11,900,700 | 10,014,999 | 7,780,609 | 6,583,467 | 5,318,396 | 3,948,914 |
| R-squared | 0.006 | 0.007 | 0.007 | 0.005 | 0.006 | 0.006 |

Notes. The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. The sample is limited to localities with population in 1995 between 50 and 5,000. Panel A, B and C limit the samples to localities in the bottom, middle and top terciles of educational enrollment in 2000. All regressions include locality, state-year and state-month-of-year fixed effects with standard errors in parenthesis clustered two ways at the municipality and state-by-date. Significance levels denoted at conventional levels *** p<0.01, ** p<0.05, * p<0.1.

Table A.11: Effect of Cash Transfers on the Marginal Effect of Temperature (Weekends v. Weekdays)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Bandwidth | 1 | 0.8 | 0.6 | 0.5 | 0.4 | 0.3 |
| | | | *Panel A: Weekends* | | | |
| Temp (°C) | 0.00190** | 0.00191** | 0.00207** | 0.00233** | 0.00168 | 0.00104 |
| | (0.000752) | (0.000794) | (0.000872) | (0.000907) | (0.00104) | (0.00121) |
| X Treated | -0.00151** | -0.00159** | -0.00198*** | -0.00211*** | -0.00217** | -0.00213* |
| | (0.000663) | (0.000681) | (0.000751) | (0.000813) | (0.000943) | (0.00114) |
| Mean | 0.0505 | 0.0524 | 0.0522 | 0.0515 | 0.0525 | 0.0528 |
| Observations | 7,827,038 | 6,602,677 | 5,162,628 | 4,372,192 | 3,528,211 | 2,618,965 |
| R-squared | 0.007 | 0.007 | 0.008 | 0.007 | 0.007 | 0.007 |
| | | | *Panel B: Weekdays* | | | |
| Temp (°C) | 0.00222*** | 0.00231*** | 0.00216*** | 0.00210*** | 0.00240*** | 0.00253*** |
| | (0.000570) | (0.000611) | (0.000671) | (0.000707) | (0.000807) | (0.000884) |
| X Treated | -0.00131*** | -0.00154*** | -0.00168*** | -0.00172*** | -0.00194*** | -0.00194*** |
| | (0.000498) | (0.000519) | (0.000569) | (0.000609) | (0.000681) | (0.000720) |
| Mean | 0.0403 | 0.0415 | 0.0418 | 0.0415 | 0.0422 | 0.0418 |
| Observations | 10,458,456 | 8,822,309 | 6,898,011 | 5,841,799 | 4,714,234 | 3,499,340 |
| R-squared | 0.006 | 0.006 | 0.006 | 0.005 | 0.005 | 0.005 |

Notes. The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. The sample is limited to localities with population in 1995 between 50 and 5,000. Panel A further limits the sample to weekends (Friday-Sunday) and Panel B limits the sample to weekdays (Monday-Thursday). All regressions include locality, state-year, state-month-of-year and month-date fixed effects with standard errors in parenthesis clustered two ways at the municipality and state-by-date. All regressions are weighted by population in 1995. Significance levels denoted at conventional levels *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: Effect of Cash Transfers on the Marginal Effect of Temperature: Females of Marriage Age

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Bandwidth | 1 | 1 | 0.8 | 0.8 | 0.6 | 0.6 | 0.5 | 0.5 | 0.4 | 0.4 | 0.3 | 0.3 |
| Temp (°C) | 6.32e-05 | 0.00169*** | 7.47e-05 | 0.00172*** | 0.000114 | 0.00163*** | 0.000115 | 0.00162*** | 0.000178 | 0.00146** | 0.000127 | 0.00138** |
| | (0.000105) | (0.000450) | (0.000113) | (0.000472) | (0.000127) | (0.000509) | (0.000143) | (0.000544) | (0.000164) | (0.000606) | (0.000187) | (0.000671) |
| X Treated | -4.72e-05 | -0.00102** | -7.05e-05 | -0.00112** | -0.000138 | -0.00134*** | -7.95e-05 | -0.00142*** | -0.000159 | -0.00154*** | -0.000159 | -0.00150** |
| | (8.99e-05) | (0.000432) | (9.44e-05) | (0.000445) | (0.000101) | (0.000482) | (0.000107) | (0.000523) | (0.000120) | (0.000589) | (0.000138) | (0.000680) |
| Treated | 0.00139 | 0.0201** | 0.00171 | 0.0220** | 0.00248 | 0.0207* | 0.000408 | 0.0193 | 0.00252 | 0.0271** | 0.00299 | 0.0261* |
| | (0.00199) | (0.0101) | (0.00206) | (0.0101) | (0.00223) | (0.0110) | (0.00235) | (0.0119) | (0.00248) | (0.0132) | (0.00282) | (0.0152) |
| Observations | 18,285,494 | 18,285,494 | 15,424,986 | 15,424,986 | 12,060,639 | 12,060,639 | 10,213,991 | 10,213,991 | 8,242,445 | 8,242,445 | 6,118,305 | 6,118,305 |
| R-squared | 0.001 | 0.005 | 0.001 | 0.005 | 0.001 | 0.006 | 0.001 | 0.005 | 0.001 | 0.005 | 0.001 | 0.005 |
| Sample | FMA | Others | FMA | Others | FMA | Others | FMA | Others | FMA | Others | FMA | Others |
| Mean | 0.00220 | 0.0422 | 0.00231 | 0.0436 | 0.00238 | 0.0435 | 0.00238 | 0.0430 | 0.00235 | 0.0438 | 0.00245 | 0.0437 |

Notes. The dependent variable is a binary indicator (coded 0 or 100) for whether or not a locality had any homicides on a given day. The population is limited to only those localities that had population in 1995 between 50 and 5,000. Odd numbered columns with sample "FMA" limit sample to female victims of marriageable age (18-45). Even numbered columns with sample "Others" limit the sample to all other victims excluding female victims of marriageable age. All regressions include locality, state-year, state-month-of-year and month-date fixed effects as well as rainfall with standard errors in parenthesis clustered two ways at the municipality and state-by-date. Significance levels denoted at conventional levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$.