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Economic Effects of Environmental Crises: Evidence from Flint, Michigan†

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In April 2014 Flint, Michigan switched its drinking water supply from the Detroit water system to the Flint River as a temporary means to save $5 million. Over 18 months it was revealed that the switch exposed residents to dangerous levels of lead, culminating in an emergency declaration in October 2015. This paper examines the impact of this crisis on the Flint housing market. The value of Flint's housing stock has fallen by $520 million to $559 million despite over $400 million in remediation spending. Home prices remain depressed through August 2019, 16 months after the water was declared safe for consumption. (*JEL* H12, I12, Q25, Q51, Q53, R31)

On April 15, 2014 Flint, Michigan switched its drinking water supply from the Detroit water system to the Flint River as a temporary measure to save the city $5 million over two years (Guyette 2018). The more corrosive Flint River, combined with administrative failures, caused toxic lead to contaminate the city's water

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supply. These events triggered one of the most notorious public health crises in modern US history. In only a short period, the Flint crisis has become the subject of intense presidential debates, documentary films, and extensive news coverage. This paper uses the unfortunate events surrounding the Flint crisis to understand how housing markets respond to a major public policy failure and subsequent efforts to address it.

At every level of government, policies are designed to safeguard human health and well-being. This covers the gamut of human activities including food, occupational and transportation safety, fire and crime, and the provision of safe drinking water and clean air. The United States spends tens of billions of dollars on these programs each year, and most federal regulations are thought to pass a cost-benefit test (Office of Management and Budget 2013; Keiser, Kling, and Shapiro 2019). Benefit estimates used to assess these policies often arise from studies of marginal changes in risk, environmental quality, or other measures of well-being. The events in Flint provide a unique opportunity to assess the consequences of a disastrous, nonmarginal policy failure. These types of catastrophic events have played an important role in understanding the policy responses to natural disasters and climate change (Nordhaus 2011; Gallagher 2014; Weitzman 2014; Martin and Pindyck 2015; Gallagher and Hartley 2017; Deryugina, Kawano, and Levitt 2018; Beatty, Shimshack, and Volpe 2019). Arguably, they are poorly understood in many other public policy contexts. What happens when a basic good like safe drinking water is no longer available? How do households and housing markets respond to uncertain information about the length and severity of large crises? How do the economic impacts of this type of event inform current policy decisions? This paper seeks to shed light on these questions.

We focus on two periods of the Flint crisis. The first—the switch—spans April 2014 to September 2015, the period after the city changed its water supply but before any public health emergency declarations. Flint residents were inundated with conflicting information about the quality of their drinking water during this time. Many residents noticed a stark difference in their water quality, and the city issued several boil advisories due to safe drinking water violations unrelated to lead. At the same time, residents were continually reassured by local, state, and federal officials that their water was safe for consumption. The mayor of Flint, Dayne Walling, infamously drank a cup of tap water on TV to assure citizens that their water was safe (WNEM News 2015).

A concerted effort by outside parties revealed the extent of the crisis in late 2015. These revelations led public officials to declare a public health emergency, admitting that Flint's water was unsafe. This declaration was followed relatively quickly by city, state, and federal declarations. These events mark the start of our second period, the crisis, which began in October 2015. In response to the crisis, government and private groups led efforts to identify homes with lead service lines, test lead concentrations in homes throughout Flint, and distribute bottled drinking water and water filters to homes. Six years later, these efforts are nearly complete. The water was deemed safe to drink again, and the state's free bottled water program ended in April 2018 (Baptiste 2018). Efforts to replace lead service lines are in their final stages. Prior to COVID-19, the program was approximately 85 percent complete (Carmody 2020).

This paper studies the housing market impacts of the Flint water crisis. We first use Google search behavior and avoidance purchasing behaviors to study awareness of possible pollution exposure from after the switch to the Flint River until the emergency declarations. The analysis reveals some concern about water pollution from April 2014 through late 2015. These measures of awareness spiked dramatically after the emergency declarations. We then use housing market data through August 2019, four years since the switch to the Flint River and 16 months after the water was deemed safe, to study the impacts of these events on home prices. We leverage the insights of these behavioral responses to inform two separate research designs that study the impact of the switch and subsequent emergency declarations on the Flint housing market.

Our first empirical strategy matches Flint to a set of control cities based on preswitch socioeconomic characteristics including income, population, racial composition, the share of vacant homes, and the unemployment rate. We identify three cities that closely match Flint on these dimensions: Youngstown, Ohio; Pontiac, Michigan; and Camden, New Jersey. We then examine the evolution of home prices in Flint relative to these control cities in calendar time to measure how the Flint housing market responded to the switch and emergency declarations. We refer to this empirical strategy as the matched-city research design.

Our second empirical strategy addresses the fact that the crisis was precipitated by underlying financial instability in Flint. Flint was placed into financial receivership in December 2011, and the state appointed an emergency manager to oversee the city's finances. The switch to the Flint River was one of the actions taken by the emergency manager to improve the city's finances. Flint remained under the control of an emergency manager until April 2015, a year after the switch, but before the emergency declarations. A concern with our matched-city design is that the control cities did not experience financial receivership from December 2011 through March 2015. If housing prices in Flint evolve differentially from the matched cities due to this intervention, we would not causally identify the impact of the crisis. Our second empirical strategy constructs a counterfactual using five other Michigan cities where the state similarly appointed an emergency manager. We compare Flint's home prices to this set of emergency manager cities leading up to, through, and after control of a state-appointed emergency manager in event time, or time since the emergency management period. We refer to this empirical strategy as the emergency manager design.

We find that average home values in Flint have declined by approximately 27 percent to 39 percent since the switch to the Flint River using our matched-city design. Approximately 67 percent to 72 percent of this decline occurred since the public health declarations. Our results are similar (though slightly higher) for the emergency manager design, where we find average home value declines of 36 percent to 43 percent. Approximately 69 percent to 80 percent of the decline occurred after the public health declarations. The two designs indicate that home values have fallen by about \$27,400 to \$29,400 per home. Further, both designs show little or no evidence of recovery in home values even after the state spent over \$400 million remediating the crisis. There has been no clear rebound in the 16 months since the water was declared safe for human consumption. Our estimates suggest a \$520 million to \$559 million loss of value to a large portion of Flint's housing stock.

We also explore heterogeneous impacts of the crisis within Flint. We group homes in Flint into 18 areas based on census tracts and estimate differential impacts of the crisis after the emergency declarations. We find some evidence that a few areas experienced larger losses than others. Most areas experienced losses of 20 percent or greater. We examine correlations between these price impacts and 2014 census demographic data, data on the city's lead service line inspection and replacement program, and approximately 33,000 in-home lead test results. We find some evidence of larger impacts in areas with a greater fraction of nonvacant homes and in poorer areas of the city. Lead line inspection and replacement intensity are not strongly correlated with changes in housing values, while we see smaller impacts in areas with higher in-home lead test concentrations. Overall, while some heterogeneity exists, the analysis reveals that the impacts of the crisis were felt across the city.

Finally, we explore changes in sales, mortgage characteristics, migration, and population changes. The analysis reveals no major changes to sales volume or migration since the public health declarations. Since this period of the crisis was most salient, these results suggest that the crisis impacted the housing market primarily through prices.

Our study contributes to several strands of literature. First, we build on a robust literature that examines the economic consequences of disasters (Gallagher 2014; Gallagher and Hartley 2017; Deryugina, Kawano, and Levitt 2018; Beatty, Shimshack, and Volpe 2019; Gibson and Mullins 2020). Flint is unique from these previous studies in that the events considered here were due to human error. This difference provides a unique opportunity to study the consequences of a significant public policy failure in which trust in government plays an important role.

Second, we build on a large literature that uses housing price impacts and averting expenditures to estimate social benefits from local environmental quality improvements (Chay and Greenstone 2005; Greenstone and Gallagher 2008; Bayer, Keohane, and Timmins 2009; Graff Zivin and Neidell 2009; Neidell 2009; Currie et al. 2015; Deschenes, Greenstone, and Shapiro 2017; Keiser and Shapiro 2019a; Williams and Phaneuf 2019; Ito and Zhang 2020). A key empirical challenge in this literature involves properly characterizing the extent to which consumers are informed about pollution risk (Shimshack, Ward, and Beatty 2007; Pope 2008; Davis 2011; Barwick et al. 2019). We use averting behavior data and Google Trends analyses to study Flint citizens' awareness of the pollution crisis. The findings from these tests guide our empirical study of housing market impacts. Our results suggest that even limited signals of pollution risk can trigger corresponding responses in housing prices. The largest housing market impacts, however, are observed when the extent of the crisis was widely known. Using averting expenditure data, we also characterize the persistence of behavioral effects even in the wake of costly remediation expenditures. Bottled water sales spiked after the state ended the free bottled water program in April 2018. This finding suggests that the home price impacts we estimate may persist in part because of public mistrust in the quality of Flint's drinking water.

Third, we contribute to a large literature studying health and economic damages from lead pollution and a growing literature examining the causes and consequences of the Flint crisis. Lead exposure, particularly in early childhood, has been shown to increase suspensions and juvenile detentions in schools (Aizer and Currie 2019), lower test scores (Aizer et al. 2018; Hollingsworth and Rudik 2021), increase violent criminal behavior (Feigenbaum and Muller 2016), decrease fertility (Clay, Portnykh, and Severini 2021), and increase mortality (Clay, Troesken, and Haines 2014; Hollingsworth and Rudik 2021). Researchers have already found convincing evidence of acute health impacts from the Flint crisis due to lead and legionella exposure (Zahran, McElmurry, and Sadler 2017; Grossman and Slusky 2020; Danagoulian and Jenkin 2021). Our estimated housing market impacts demonstrate that the economic damages extend beyond direct human health impacts. These results contribute to a growing literature that has estimated significant benefits from lead pipe replacement (Theising 2019) and lead paint remediation (Billings and Schnepel 2017; Gazze 2019).

Finally, we contribute to a growing literature on the importance of government action to maintain safe drinking water in the context of aging infrastructure. In the United States, this work has focused on the health impacts of providing basic treatment (Cutler and Miller 2005; Alsan and Goldin 2019; Anderson, Charles, and Rees 2022), perceptions of risk and avoidance behavior (Graff Zivin, Neidell, and Schlenker 2011; Wrenn, Klaiber, and Jaenicke 2016), and public water system behavior (Bennear and Olmstead 2008; Bennear, Jessoe, and Olmstead 2009; Allaire, Wu, and Lall 2018). Our work contributes in at least two important ways to these studies. First, our results on housing price effects complement current work that estimates direct health impacts from the Flint crisis, which are important for developing comprehensive estimates of the benefits of avoiding widespread water pollution events. Evidence on these values is thin, and costs are large (Keiser and Shapiro 2019b). We are not aware of any available estimates that consider the benefit of avoiding a system-wide contamination crisis. Second, our averting analysis provides a unique setting to explore how these behaviors respond to conflicting information signals over a long period, even after drinking water has been deemed safe by authorities. These results are important for informing both benefit estimates of providing clean water and the potential health consequences of drinking contaminated water.

The paper proceeds as follows. We first provide historical context for our study and discuss key events leading up to, during, and after the Flint water crisis (Section I). This section also considers Google Trends and averting behavior data to understand better when Flint residents knew about the crisis. Next, we discuss our two estimation strategies (Sections II and III) and data (Section IV). Section V presents our results and citywide damage estimates. Section VI discusses the implications of our results regarding the value of safe drinking water and water infrastructure investments.

I. Background

A. *Economic Conditions in Flint*

Flint, Michigan was among America's first and foremost automobile towns. General Motors (GM) was founded in the city in 1908, and Flint had one of the highest per capita incomes in the United States in the early twentieth century (Clark 2018). Both the city's wealth and population grew rapidly through the 1950s, along with the demand for automobiles. Since 1960 the city has experienced large population losses, mainly due to the closure of manufacturing plants in the 1980s and 1990s. Along with the large population decreases, economic conditions in Flint have deteriorated dramatically since the 1980s.

Economic conditions deteriorated further with the Great Recession. Unemployment rates increased from between 8 percent and 10 percent in the mid-2000s to a high of 16.9 percent in July 2009. Between 2007 and 2010, employment levels fell from 195,000 to around 160,000. Unemployment gradually declined from postrecession highs during 2011 and 2012 but remained between 8.3 percent and 11 percent through 2013 (U.S. Bureau of Labor Statistics 2020a). The percentage of residents below the poverty level during this period exceeded 40 percent (United States Census Bureau 2020).

A 2011 Michigan Treasury Department review of Flint's ability to meet its short-term financial obligations revealed severe structural deficits in the city's finances (Fraser et al. 2011). In response, the governor placed Flint in financial receivership in November 2011, appointing Michael Brown as the city's emergency manager. Under Michigan law, emergency managers have broad power to make unilateral decisions regarding city finances. The system was initially established to resolve systemic financial deficits accrued by cities. Emergency managers' powers were broadened in 1990 and again in 2011 so that they could handle all financial matters of the cities placed under receivership.[1] Between 2011 and 2014, the governor appointed four successive emergency managers in Flint. All had broad authority to, for example, lay off government workers and renegotiate city labor contracts to stabilize the city's finances. They also had the power to alter municipal utility operations and bills.

B. *A Timeline of the Flint Water Crisis*

From the 1960s until 2014, Flint received treated drinking water from the Detroit Water and Sewer Department (DWSD). Flint's water rates were (and remain) among the highest in the country due to the high costs of maintaining the city's aging water system, the increasing costs of acquiring water from DWSD, and a declining population (Highsmith 2015). From 2003 to 2014, DWSD raised rates by 6.2 percent, on average, annually (Flint Water Advisory Task Force 2016).

In spring 2013, Emergency Manager Ed Kurtz and the Flint City Council adopted resolutions to end the city's long-standing water purchase agreement with DWSD. Instead, the city would partner with other regional governments, entering into a long-term contract with the Karegnondi Water Authority (KWA) to construct a regional pipeline network to receive water from Lake Huron (Kurtz 2013). The move was projected to save the city around $200 million over 25 years. DWSD subsequently notified the city it would terminate its contract in spring 2014. As it waited for the KWA pipeline network to be built, the city government decided to use and

[1] The year 2011 was not the first time Flint was assigned an emergency financial manager. Flint was also assigned an emergency financial manager from 2002 to 2004 following a debt crisis in the early 2000s.

treat water from the Flint River for two years as a temporary water source (Adams 2014). The decision required renovating the city's water treatment plant, which had not been used as a primary water supply source since the city switched to DWSD water in 1967 (Flint Water Advisory Task Force 2016).

On April 25, 2014, despite warnings from county officials that the plant was not ready to treat water from the Flint River, the city closed off the water supply from DWSD and turned on its water treatment plant (Fleming 2018). Soon after, residents began to complain of the color, taste, and smell of their water even as state officials assured residents the water quality issues were related only to the relative hardness of the Flint River water (Fonger 2014f). By early June 2014, residents reported that they were supplementing their drinking water with bottled water (Clark 2018). In early fall 2014, the city issued its first of three boil advisories for elevated fecal coliform levels in the drinking water (Fonger 2014c, e). In October, a local GM plant switched its water supply to the neighboring Flint Township because increased corrosion from Flint's water caused rust to form on engine parts (Fonger 2014d).[2] Despite these concerns, officials continued to assure residents their tap water was safe to drink. For example, in an interview in June 2014, Mayor Dayne Walling said, "I think people are wasting their precious money buying bottled water" (Fonger 2014a).

Early 2015 saw additional water quality violations, with Flint's drinking water exceeding allowable levels of a disinfection byproduct (Fonger 2015a). These events were followed by a raucous town hall meeting where residents brought orange, discolored water from their home taps, demanding the city return to DWSD water. Jerry Ambrose, the emergency manager at the time, insisted that the move would be too costly, and Michigan Department of Environmental Quality (MDEQ) officials assured residents that the water was safe (Fonger 2015b). Ambrose subsequently vetoed a vote by the Flint City Council to reconnect to DWSD in March 2015.

Independent water quality and blood lead testing began in February 2015 and continued through September 2015. Researchers from Virginia Polytechnic Institute and State University released first-round results from a large-scale lead testing effort in early September 2015 (Edwards 2015; Edwards, Roy, and Rhoads 2015). Shortly after this, a team lead by Dr. Mona Hanna-Attisha released results from a study showing elevated blood lead levels in Flint children (Hanna-Attisha et al. 2016). These efforts revealed that residents were exposed to amounts of lead in water that far exceeded levels deemed safe by the Environmental Protection Agency (EPA). The Genesee County Board of Commissioners subsequently declared a public health emergency on October 1, 2015 (Genesee County Board of Commissioners 2015) followed by city, state, and federal health emergency declarations in December 2015 and January 2016 (City of Flint 2015b; Snyder 2016; White House Office of the Press Secretary 2016). The city reconnected to the DWSD system shortly after the first public health emergency declaration (City of Flint 2015a).

A series of institutional failures and federal regulatory violations caused and worsened the Flint water crisis (US Environmental Protection Agency 2018). First,

[2] The move reduced water pressure and increased the time water stayed in the city's water mains before reaching households, worsening lead concentrations in households' tap water (Clark 2018).

the city failed to implement a corrosion control program when it switched to the Flint River.[3] The city did not implement a proper corrosion control program until August 31, 2015. Second, the city failed to adequately monitor water lead levels per EPA's Lead and Copper Rule (LCR). The LCR requires in-home drinking water sampling to be conducted by the MDEQ. An independent task force found that MDEQ guidance and the city's water monitoring efforts were deeply flawed (Davis et al. 2016).

Since the crisis declarations local, state, and federal authorities, private citizens, and nongovernmental organizations have mobilized tremendous resources to minimize the public health impacts of the crisis and restore residents' access to safe drinking water through the public water system. The largest funding source is the state of Michigan. As of June 2020, the state reports spending over $400 million on the Flint water emergency.[4] The US EPA awarded the MDEQ $100 million in March 2017 to upgrade the city's drinking water infrastructure. The largest portion of these funds has provided free bottled water, water filters, and filter cartridges to Flint residents and funds to inspect and replace all noncopper lines through the city's Fast Start program.

City and state officials made steady progress reducing lead in water in Flint after the emergency declarations. Water tests revealed that the city's water tested below EPA action levels beginning in mid-2016. In April 2018, the state announced the end of the city's free bottled water and water filter program, as water lead levels continued to test far below EPA action limits (State of Michigan 2018). The city also made steady progress replacing lead service lines. Since 2016, the city has excavated over 26,000 service lines and replaced over 9,750 noncopper service lines.[5] Most progress was made during 2017–2018. Only a few hundred home inspections remain outstanding.

C. *What Did Flint Residents Know? And When?*

The timeline above highlights many public events indicating some local knowledge of water quality issues in the city before the public health emergencies. Reports suggest some residents and local officials began to meet and complain about water quality issues as early as June 2014 (Fonger 2014b). The subsequent water quality violations, events like the local GM plant switching water sources, and town hall meetings suggest some residents had significant concerns. At the same time residents were continually reassured that their water was safe to drink. As such, the timeline and news events highlighted above do not reveal the extent of residents' knowledge. Further, they do not tell us whether and when knowledge about lead contamination was widespread. This information is critical to developing our empirical strategy.

[3] All community water systems are subject to corrosion control requirements under the Optimal Corrosion Control Treatment (OCCT) provision of the Safe Drinking Water Act (SDWA). The Michigan Department of Water Quality misinterpreted their compliance requirements and made corrosion control dependent on a six-month monitoring period for lead and copper in homes (Davis et al. 2016). At the end of the monitoring period, the ninetieth percentile lead test was six parts per billion, disqualifying Flint from the (previously misinterpreted) exemption from the OCCT (City of Flint Water Plant 2014). The MDEQ, however, failed to advise the Water Treatment Plant of their obligation at that time.

[4] The state spending tracker is available online at https://www.michigan.gov/budget/0,9357,7-379-88613_88628_96845—-,00.html (last accessed December 26, 2020).

[5] See https://www.cityofflint.com/gettheleadout/.

We use three indicators to understand awareness of the extent of the water crisis among residents. First, we examine changes in the demand for information as a proxy of knowledge about drinking water concerns in Flint using monthly search intensity from Google Trends for residents living in the area around Flint from January 2006 to August 2019.[6] Second, we compile monthly bottled water and Pur water filter sales at stores that serve Flint residents from January 2006 to August 2019 from the Nielsen Retail Scanner and Consumer Panel data from the Kilts Center for Marketing at the University of Chicago. We standardize all the data and graph each z-score metric over time.

Figure 1 shows the three series along with seven of the events described in the previous section. Panel A graphs Google Trends queries. Search intensity was slightly elevated ($\approx +0.6$ standard deviations) in the months immediately following the switch to the Flint River (event A) and the three boil advisories (event B) in 2014, but not noticeably above previous trends. There was a more sustained increase in search activity following the total trihalomethanes (TTHM) violations and town hall meeting in January 2015 (event C). Interest waned in the period from April through August 2015. The index increases to $+1.4$ and $+1.6$ standard deviations in August and October 2015 with the release of the Edwards and Hanna-Attisha reports and Genesee emergency declarations (event D). The intensity of search behavior in January and February 2016 is far greater than all previous and subsequent months. The index increases to $+8.1$ and $+4.9$, respectively, after the state and federal emergency declarations (event E). Since 2016, the index has fluctuated between 0 and $+1$, increasing around notable events like the end of the state and federal public health emergencies (event F) and the free bottled water program (event G).

Bottled water and water filter purchases suggest a similar story, with a few notable exceptions. Bottled water sales increased between $+0.4$ and $+0.95$ standard deviations in the months immediately following the switch and the fall boil advisories. The increases, however, were not notably higher than historical summertime increases in demand. Bottled water sales increased notably ($+2$ to $+2.6$ standard deviations) after the TTHM violations and town hall meeting in January 2016 (event C) and remained elevated until the emergency declarations. The finding confirms previous research showing averting expenditure spikes following water quality violations (Graff Zivin, Neidell, and Schlenker 2011). Sales saw their largest swing, from $+2.9$ to -0.15 standard deviations, following the emergency declarations. This is not surprising given that bottled water and water filters were made available for free to residents in Flint. Sales increased $+1$ to $+2$ standard deviations above historical averages after the free bottled water program ended (event G). This accords with our finding of a sustained price reduction in housing markets. Water filter sales show no substantive response to any events until the Genesee County public health emergency (event D) and subsequent federal and state emergencies. Filter sales increased $+10$ standard deviations in June 2016, shortly before the end

[6] Specifically, we query monthly search intensity for the terms "Flint water," "brown water," "drinking water," "water testing," "lead," "boil advisory," or "Legionella." Online Appendix A.1 contains more details on the Google Trends data and Nielsen grocery store sales data. We also compare Google Trends search intensity in Flint to other cities used in our subsequent analysis. Search intensity in Flint far exceeds that in other areas, even around the emergency declarations.

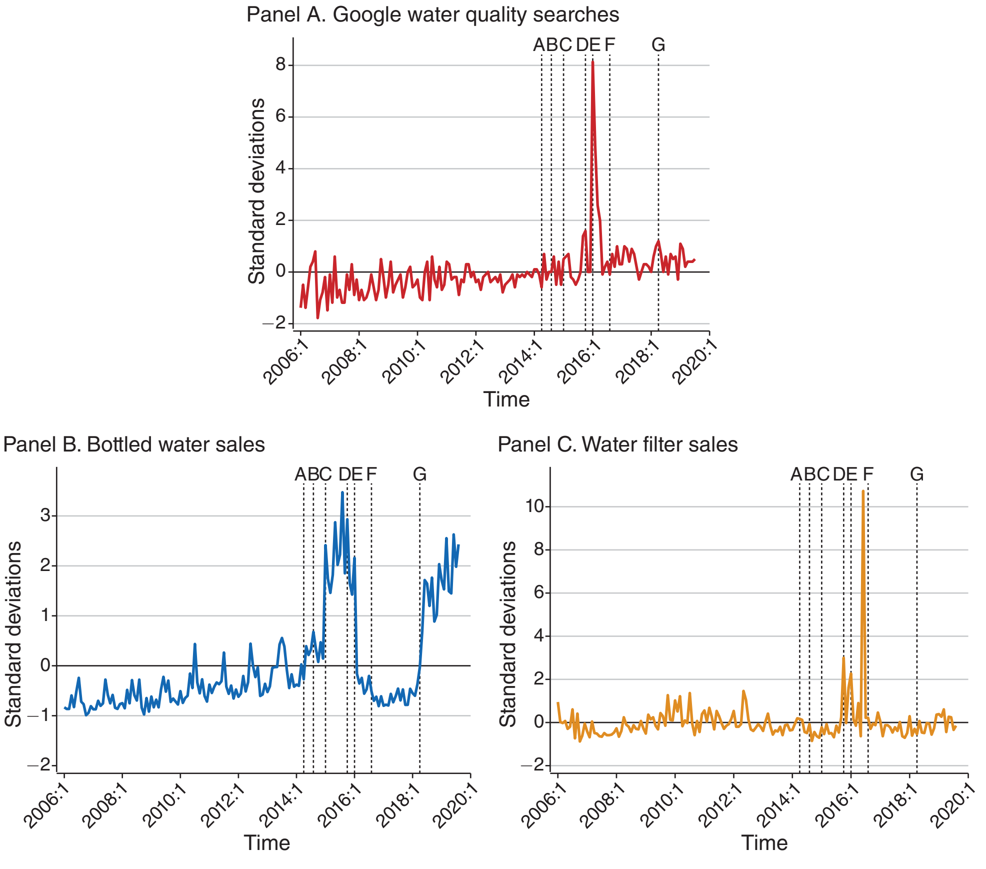


Figure 1. Search and Purchasing Behavior in Flint

Notes: Panel A graphs monthly Google search behavior for water quality–related searches in Flint from January 2006 through August 2019. Panel B and panel C graph monthly bottled water and Pur water filter sales at stores that serve Flint, Michigan residents from January 2006 through August 2019. All values are standardized to have mean zero and standard deviation one. A indicates the month of the switch to the Flint River; B indicates the first of three boil advisories in 2014; C indicates TTHM violations and the town hall meeting; D indicates the Genesee Public Health Emergency; E indicates the state and federal public health emergencies; F indicates the end of the federal public health emergency and beginning of the lead line replacements; and G indicates the end of the free bottled water program.

of the federal health emergency. That month, public health officials declared that Flint's tap water was safe for human consumption so long as it was filtered (Ridley 2016).

The three metrics provide consistent evidence that Flint residents had some awareness of water quality issues in the period following the switch to the Flint River. This awareness, however, only appears to have become widespread after the emergency declarations, where we see the largest changes in every metric. As such, both of our empirical strategies use a difference-in-differences approach, evaluating housing market impacts separately by the switch period (April 2014 to September 2015) and the postemergency period (October 2015 to August 2019).

II. Empirical Strategy One: Matched-City Design

Flint is distinct from most US cities. The city stands out for its high proportion of low-income households as well as high poverty rates, crime, and the percentage of the population that is Black or African American. Similar to other papers studying the impacts of disasters in unique cities (e.g., Deryugina, Kawano, and Levitt 2018), our first empirical strategy uses a matched-city difference-in-differences design, comparing housing prices in Flint to cities with similar demographic and economic characteristics. We first describe the construction of our control group and then discuss our empirical approach.

A. *Control Group Construction*

We collect population, labor force, and demographic data from the US census and American Community Survey (ACS) to construct our first control group (Ruggles et al. 2015). We limit the pool of potential control cities to those whose 2010 population is greater than 50,000 and less than 200,000 so that any matched cities are similar in size to Flint. We then match cities based on the level and long-run changes of key economic characteristics. From the 2010 census, we match cities on population, the percentage of vacant homes, and the percentage of the population that is Black or African American. From the 2012 ACS, we match on median household income and the unemployment rate. We also match on median household income changes from 1980 to 2012 and population changes from 1970 to 2012.[7]

Our primary analysis uses the top three matched cities from this exercise.[8] The top three matched control cities, in order of match proximity, are Youngstown, Ohio; Pontiac, Michigan; and Camden, New Jersey. To varying degrees, each city shares a history of postwar economic prosperity due to large manufacturing growth followed by substantial population declines and only modest income growth over the last several decades. In 2012, when Flint was placed into financial receivership, all cities shared low median incomes, high unemployment rates, a large share of vacant houses, and high population shares that were Black or African American (Table A.1).

B. *Estimation Strategy*

Having defined the sample of control cities, our main estimation strategy uses the following difference-in-differences model:

$$
\begin{aligned}
(1)\quad y_{ijt} = {} & \gamma_1\big[Receivership_t \times Flint_i\big] + \gamma_2\big[Switch_t \times Flint_i\big] + \gamma_3\big[Emergency_t \times Flint_i\big] \\
& + \mu_1\big[Receivership_t\big] + \mu_2\big[Switch_t\big] + \mu_3\big[Emergency_t\big] \\
& + \mathbf{X}_{ijt}'\beta + \delta_j + \delta_t + \epsilon_{ijt}.
\end{aligned}
$$

[7] City-level median income statistics are unavailable for 1970. Online Appendix A.2 details our matching procedure, variables, and summary statistics for the matched cities.

[8] We explore the sensitivity of our results to using other groupings (top, top two, top five, and top ten) in online Appendix C.1.

Our outcome of interest is the natural log of the transaction price for property i in census block group j on date t. The variables $[Receivership_t]$, $[Switch_t]$, and $[Emergency_t]$ are indicators that equal one if date t is after Flint was placed in financial receivership, the city switched its water supply to the Flint River, and the city's first emergency declaration, respectively.[9] While our matched control cities are similar to Flint along many observable dimensions, time-varying differences likely remain and impact our estimated impacts of the water crisis. As such, in our main regressions we include census block group fixed effects δ_j, temporal fixed effects δ_t, and other controls $\mathbf{X}_{ijt}$. We discuss the specific temporal fixed effects and controls when we present our results.

Our coefficients of interest are γ_2 and γ_3. The coefficients measure the average change in outcome y_{ijt} in Flint relative to control cities. We include $[Receivership_t]$ in all specifications to control and test for possible differential pre-trends in Flint before the switch.

We also estimate a flexible difference-in-differences model, comparing impacts of the crisis over time as

$$(2) \quad y_{ijt} = \sum_{\tau} \mu_\tau \mathbf{1}[t = \tau] + \sum_{\tau} \gamma_\tau \big(\mathbf{1}[t = \tau] \times Flint_i\big) + \mathbf{X}'_{ijt}\beta + \delta_j + \delta_t + \epsilon_{ijt}.$$

$\mathbf{1}[t = \tau]$ is an indicator for whether date t falls in year-quarter τ, and $Flint_i$ is an indicator for whether observation i is in Flint. All other variables are defined as before.

Estimating equation (2) has three key benefits. First, the estimates allow us to examine the speed with which home prices responded to the events following the crisis as detailed in Section IB. Second, examining the path of γ_τ's before the switch to the Flint River provides a test of differential pre-trends between Flint and our control cities. Last, equation (2) allows us to leverage new work by Rambachan and Roth (2021) and examine the robustness of our results to allowing for potential linear differential pre-trends.

III. Empirical Strategy Two: Emergency Manager Design

We use a second research design to account for the possibility that financial receivership and a state-appointed emergency manager influenced Flint home prices leading up to the switch to the Flint River. For this, we compare Flint to five other Michigan cities that were also placed under an emergency manager after 2000. We compare home prices in Flint to those in these other emergency manager cities in event time, or the time since the emergency manager period.

[9] An alternative approach would follow Davis (2004) and use differential risk metrics for Flint and a set of control cities. We do not pursue this approach here given the mixed evidence of awareness during the switch period and sudden increase in awareness around the emergency declarations. Nevertheless, regressions of housing prices on the Google Trends data yield similar price impact estimates to our difference-in-differences approach.

A. *A Brief History of Emergency Management in Michigan*

Michigan's Public Act 101 of 1988 established the state's role for intervening in cases of financial distress. The act was passed in response to a 1986 court order that placed the city of Ecorse in receivership (Michigan State University Extension 2017). The law outlined steps for reviewing a city's finances and the appointment of an emergency financial manager. A few years later, Public Act 72 of 1990 strengthened the law and gave emergency financial managers broad powers over a city's finances. Following the Great Recession, the law was strengthened once more. Public Act 4 of 2011 renamed the emergency financial manager position as an emergency manager. The emergency manager was given greater control over the city's finances, including the ability to renegotiate all contracts on behalf of the city (Michigan Radio Newsroom 2011; Michigan State University Extension 2017). Emergency managers retain control over a city until the underlying financial issues have been resolved. Powers are then restored to local government officials. The state can appoint a Receivership Transition Advisory Board (RTAB) to oversee this transition of power back to the city. Flint's emergency manager period ended in April 2015 and an RTAB was appointed. Flint was fully released from receivership in April 2018.[10]

Emergency managers were readily used to resolve cities' financial troubles in Michigan following the Great Recession. From 2009 to 2016 18 towns and school districts were deemed to be in financial distress.[11] It is from this pool of cities that we draw our second control group.

B. *Control Group Construction*

We select five Michigan cities from the pool of cities placed under emergency management since 2000. The cities were selected based on the availability of both pre– and post–emergency manager period observations. Specifically, we observe the same length of the pre–emergency manager period (2.75 years) and post–emergency manager period (4.25 years) for all five control cities and for Flint. We exclude cities where an emergency manager was appointed solely for a school district (Highland Park Schools, Muskegon Heights Schools), where consent agreements reduced the emergency manager period to one or two months (Highland Park, Inkster, River Rouge, Royal Oak Township, Wayne County), or where we do not observe the same post–emergency manager period as Flint (Lincoln Park). We exclude Detroit, since it also experienced bankruptcy during its emergency manager period. One of our control cities, Hamtramck, experienced two emergency manager periods. The first was from 2000 to 2007; the second was from July 2013 to December 2014. We use the second emergency manager period and exclude observations from the first.

[10] RTABs were appointed in each of our control cities, and each city has been released from their control ("Resolving Financial Emergencies in Michigan").

[11] To identify the potential set of emergency manager cities, we use a public timeline of financial emergencies from the state of Michigan ("Resolving Financial Emergencies in Michigan"), an extension document from Michigan State University (Michigan State University Extension, 2017), and a Wikipedia page on Michigan's emergency managers (https://en.wikipedia.org/wiki/Financial_emergency_in_Michigan).

The cities used in our analysis are Allen Park, Benton Harbor, Ecorse, Hamtramck, and Pontiac. Allen Park, Ecorse, Hamtramck, and Pontiac are in the greater Detroit metropolitan area. Benton Harbor abuts Lake Michigan on the western side of the state. On average, each city spent approximately three years under emergency manager control. The shortest period was 1.5 years (Hamtramck) and the longest was 4.5 years (Pontiac). Flint was under emergency manager control for just over three years.

C. *Estimation Strategy*

Our estimation strategy for the emergency manager design is similar to the matched-city design. Instead of comparing home prices in Flint to those in control cities in calendar time, however, we examine them in event time. Specifically, we compare Flint housing prices following the city's start and release from emergency manager oversight to average housing prices in other emergency manager cities upon their start and release. We estimate

$$
\begin{aligned}
(3) \qquad y_{ijt} = {} & \gamma_1 \left[EMStarts_t \times Flint_i\right] + \gamma_2 \left[EMStarts_t \times Flint_i \times Switch_t\right] \\
& + \gamma_3 \left[EMEnds_t \times Flint_i\right] + \mu_1 \left[EMStarts_t\right] + \mu_2 \left[EMEnds_t\right] \\
& + \mathbf{X}'_{ijt}\beta + \delta_j + \delta_t + \epsilon_{ijt}.
\end{aligned}
$$

As with the matched-cities model, our outcome of interest, y_{ijt}, is the natural log of the transaction price for property i in census block j on date t. We control for two event periods related to the emergency manager. $\left[EMStarts_t\right]$ is an indicator that equals one after a city is placed under the control of an emergency manager (EM). $\left[EMEnds_t\right]$ is an indicator that equals one if date t is after a city exits the control of an emergency manager. We interact each of these time indicators with an indicator for the city of Flint. Additionally, since Flint was under the control of an emergency manager when the drinking water supply was switched, we add an interaction for Flint for the period after the switch while the city was under the control of an emergency manager. As before, in our main regressions we include census block group fixed effects, δ_j, temporal fixed effects, δ_t, and other controls, $\mathbf{X}_{ijt}$. We discuss the specific temporal fixed effects and controls used when we present our results.

Our coefficients of interest are γ_2 and γ_3. The coefficients measure differential housing price responses in Flint relative to the control cities after the switch to the Flint River and once the emergency management period ended.[12] Similar to our previous strategy, γ_1 provides an indirect test of the differential pre-trends assumption by testing for differences in Flint housing prices at the outset of the emergency management period but before the switch to the Flint River, relative to other cities entering emergency management.

[12] Interpreting the coefficient γ_3 is slightly nuanced since there is a five-month period between the end of emergency management and the public health emergency. Overall price impacts are similar if we include an additional Flint–by–pre-Emergency Declaration interaction term in equation (3).

We also plot quarterly estimates of prices in Flint relative to control cities using a flexible version of the difference-in-differences model in equation (2). The model allows us to visualize the change in Flint's housing prices relative to our control cities in the year-quarters leading up to and after emergency management. Our reference category includes the entire emergency manager period. We do this because each city was under the control of an emergency manager for varying lengths of time.

IV. Data and Summary Statistics

A. *Data Sources*

Housing Sales Data.—We use transaction-level data on housing prices and home characteristics covering the universe of transactions in Flint and our control cities from 2006 through August 2019 from the Zillow Transactions and Assessment Dataset (ZTRAX). Zillow aggregates these data from several sources, including county recorder and assessor offices.[13] All prices are adjusted to 2019 dollars using the U.S. Bureau of Labor Statistics Consumer Price Index for All Urban Consumers: Housing in US City Average. We restrict the data in several ways. First, we follow Currie et al. (2015) and restrict our sample to homes that sold for between $25,000 and $10 million.[14] Second, we remove intrafamily transfers and tax-exempt transactions. Third, we remove transactions for multifamily properties, undeveloped land, and mixed-use or commercial properties. Fourth, we remove all transaction types that do not involve deed transfers (e.g., assessments, affidavits of trust). Fifth, we remove properties flagged as non–arm's length or homes sold for tax purposes. Additionally, we remove potential outlier transactions that sold more than once per year in our study period and homes that sold for more than ten times or less than one-tenth of the average two-year rolling average home price within their census block groups.

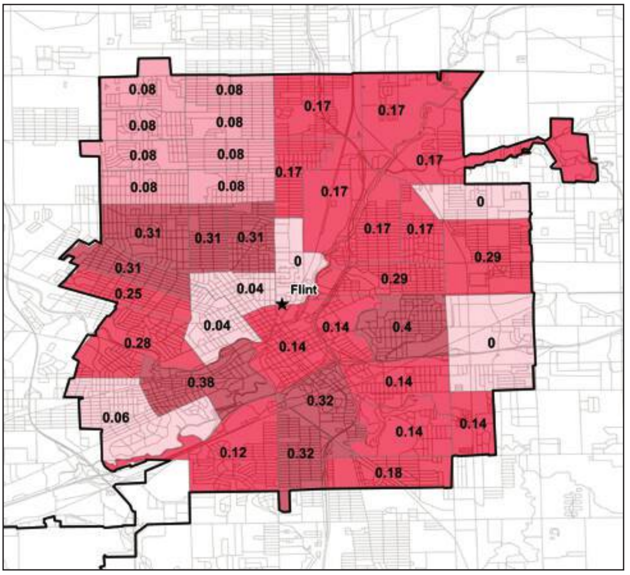
Water Infrastructure and Lead Test Data.—We collect data from several sources to examine whether the crisis had heterogeneous impacts based on lead exposure and risk. First, we collect tract-level data for August 2016 through July 2018 from Flint's FAST Start lead line inspection and replacement program. The data include the number of lines inspected and replaced, as well as lines that were cut and capped.[15] Figure 2 maps the spatial distribution of lines replaced across Flint during this period. These lines are distributed relatively evenly throughout the city,

[13] ZTRAX data are available at https://www.zillow.com/research/ztrax/.

[14] Flint has a large number of vacant homes and lots. Many are purchased for very low prices by neighboring property owners or land banks. Before restricting the data, the lowest nonzero transaction price in the data is $101; 25 percent of transactions sell for less than $4,500, and the median home sold for $(2019)12,350. We restrict our main sample to higher-valued homes, choosing a minimum value common in the literature, to ensure our data reflect arm's-length housing transactions. Given the large number of transactions, we still present and discuss impacts on lower-valued homes in our results section and the online Appendix.

[15] We thank Shawn McElmurry (Wayne State University) for providing the data. The data include counts of the number of lines inspected but not replaced, the number of lines where only the private portion of the service line was replaced, the number of lines where only the public portion of the service line was replaced, and the number of lines where the public and private portions of the service line were replaced. Cut and capped lines reflect abandoned homes.

Panel A. Service lines



Panel B. Lead test results

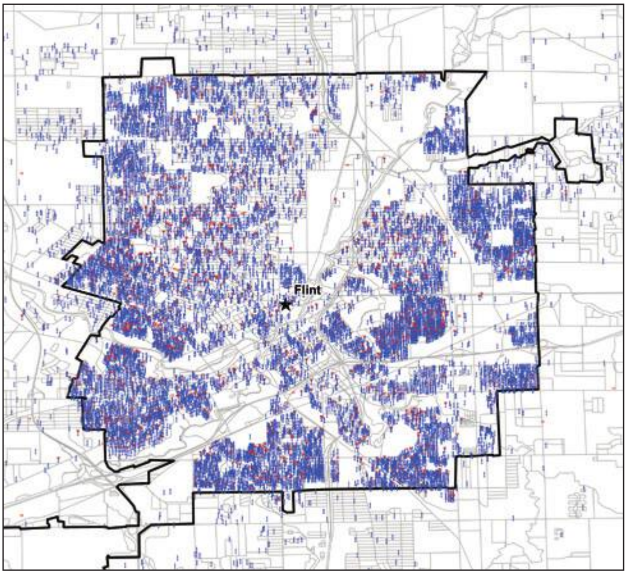


Figure 2. Flint Service Line Replacements and Lead Tests

Notes: Panel A displays the average ratio of service lines replaced to the number of occupied housing units by census tract areas in Flint. Replacement ratios are grouped by quintile. Darker red indicates a higher fraction. Panel B presents lead testing results in and around the city. Results violating safe drinking water standards are shown as red squares, and those below safe drinking water standards are blue circles. The City of Flint boundary is delineated by a dark black line. Census block groups are delineated by gray lines. The maps exclude the Flint airport, in the southwestern section of the city.

with some areas in the center and outskirts of the city showing little presence of lead lines.

We also collect data on average in-home lead test results within Flint. The state of Michigan made a concerted effort to release lead risk and exposure information after the emergency declaration. The state created an online database of aggregate blood test results, lead and copper test results at several Flint water main locations and schools, and residential- and establishment-level lead test results data.[16] We observe around 33,000 in-home lead test results covering a large portion of Flint's housing stock. The data include the date the sample was submitted, the home address, the concentration of lead and copper detected in a one-liter sample, and the date the results were released online. We use the data to construct spatial measures of lead concentrations that we match to our ZTRAX data. Figure 2, panel B maps the lead test results through September 5, 2019. Tests that violate EPA safe drinking water standards are shown as red squares, while those with low lead levels are blue circles. Areas with missing tests are mainly nonresidential areas. There is no clear visual evidence of hot spots. Some pockets with and without violations, however, stand out. For example, areas in the northwest corner of the city have few violations. The western part of Flint has fewer but more clustered violations.

Labor Force Controls and Weather Data.—We use the U.S. Bureau of Labor Statistics' monthly county-level estimate of the labor force (employed and unemployed persons) as an additional control variable in some specifications. The data are meant to control for time-varying economic conditions in Flint relative to control cities. We also include county average monthly temperature and precipitation data from PRISM Climate Data to control for seasonal and weather differences across Flint and the control cities.

Census Bureau Demographic and Housing Data.—We use data from the American Community Survey (ACS) to determine baseline socioeconomic and housing data for the preswitch period in Flint and our control cities (Ruggles et al. 2015). Most data are from the five-year, 2010-14 ACS. These data include tract-level median home values, the fraction of nonvacant homes, the fraction of owner-occupied homes, median home age, median income, the fraction of the population above the poverty level, and the fraction of non-White residents. We use housing stock data from the one-year 2014 ACS to assess city-level home value losses from the crisis.

B. *Summary Statistics*

Table 1 presents baseline summary statistics for Flint and compares Flint to the matched control cities (column 2) and emergency manager cities (column 3).[17] Panel A shows city-level demographic statistics from the 1970, 1980, and 2010 censuses and the 2010–2012 American Community Survey. Flint's population in 2010

[16] See https://www.michigan.gov/flintwater/.

[17] Columns 2 and 3 present coefficients from separate regressions of each statistic on an indicator for the relevant control group. We use quantile regression to estimate differences in median home prices.

Table 1—Summary Statistics: Baseline Characteristics

	Flint (1)	Flint versus matched cities (2)	Flint versus emergency manager cities (3)
Panel A. City demographics			
Population (2010)	102,434.00	−34,490.67	−76,494.40
		(10,339.40)	(22,366.64)
Population change (1970–2010)	−90,883.00	49,630.00	79,369.20
		(31,536.64)	(9,277.94)
Median income (2010)	26,339.00	311.33	5,186.60
		(2,543.24)	(16,209.79)
Median income change (1980–2010)	9,158.00	4,523.33	6,269.40
		(3,859.84)	(9,002.64)
Housing vacancy rate (percent, 2010)	0.21	−0.04	−0.05
		(0.03)	(0.07)
Unemployment rate (percent, 2010)	0.25	−0.04	−0.03
		(0.03)	(0.07)
African American or Black population (percent, 2010)	0.57	−0.08	−0.15
		(0.04)	(0.37)
Panel B. Housing market (2006–2013)			
Average home price (\$2019)	71,030.01	23,107.72	61,835.65
		(2,869.91)	(2,769.17)
Median home price (\$2019)	63,775.83	21,299.24	39,488.10
		(621.05)	(414.46)

Notes: The table presents summary statistics for the city of Flint in the first column and coefficients from a regression of the outcome on an indicator variable for the respective control group (top three matched control cities or emergency manager control cities). Panel A presents statistics from the 1970, 1980, and 2010 censuses and 2010–2012 American Community Survey. Panel B presents average and median home prices (in 2019 dollars). The latter estimates a quantile regression model.

was just over 102,000, down 91,000 from 1970. Flint's median income in 2010 was just \$26,000 and grew by only \$9,000 since 1980. Recent housing vacancy and unemployment rates both exceed 20 percent, and the city has a large African American or Black population (nearly 60 percent of all residents). Panel B shows ZTRAX housing market characteristics from 2006 to 2013. The average (median) price home in our main sample sold for around \$71,000 (\$64,000).

Our matched control cities are similar to Flint along many of these dimensions. The cities are smaller, though they saw similarly large population declines exceeding 40,000 people since 1970, on average. Median incomes in 2010 are very similar in Flint and the matched control cities, though the matched control cities saw greater income growth from 1980. Vacancy and unemployment rates as well as population demographics are very similar. Average and median home prices in the matched control cities are about \$23,000 higher than in Flint.

The emergency manager control cities are significantly smaller than Flint and have higher average and median home prices, though they share similar socioeconomic characteristics and demographics. The cities experienced population declines since 1970 and have relatively low median incomes, high home vacancy, and unemployment rates and a large share of the population that is African American or Black.

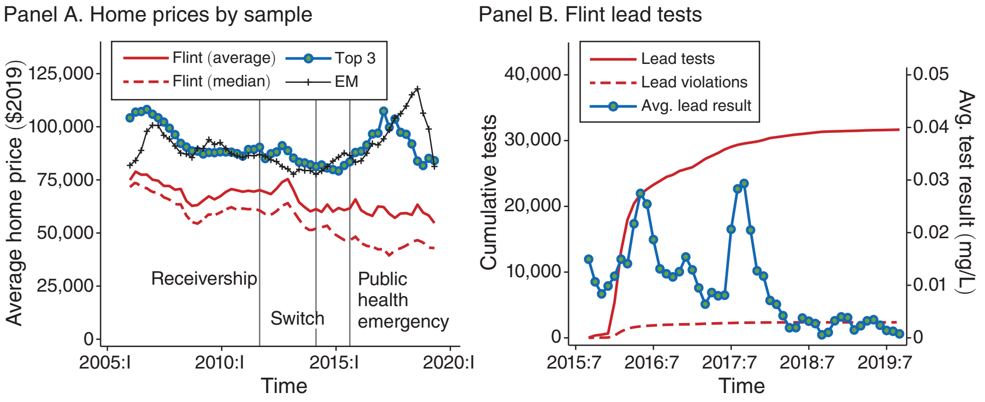


FIGURE 3. HOUSING MARKETS AND LEAD SUMMARY STATISTICS

Notes: Panel A graphs smoothed average housing prices in Flint, the top three matched control group cities, and the emergency manager control cities. Panel B graphs the cumulative number of lead tests and tests where lead concentrations violated EPA health standards over time (left axis) as well as the average lead test result in homes (right axis).

Figure 3 graphs average and median housing prices in Flint and average housing prices in the matched control cities and the emergency manager cities over our study period.[18] Housing prices in both Flint and our control cities exhibit a gradual but steady decline from 2006 through 2014–2015. Since 2015, Flint transactions indicate a slight decrease in average home prices and a decline in median home prices. In contrast, average prices in both control groups increased through 2018–2019, after which they decreased.[19] Neither average nor median housing prices in Flint collapsed after either the switch to the Flint River or the emergency declaration. Instead, Flint prices remained flat during a period largely characterized by growth in the control cities.[20]

Figure 3 graphs cumulative lead tests and lead violations as well as smoothed average lead test results from September 2015 through September 2019. Early testing revealed dangerously high lead levels, suggesting the homes tested early on had particularly acute water quality problems. The number of lead tests in Flint jumped from just a handful to over 20,000 by April 2016. Total tests exceeded 33,000 by late 2019. A non-negligible number of test results violated EPA standards in late 2015 through mid-2016. Average test results remained near or above EPA health standards (0.015 milligrams per liter) through 2017 but have remained low since the latter part of 2018. By 2018, very few homes showed additional lead violations, reflecting the success of the city's remediation efforts.

[18] We smooth house prices using a two-quarter moving average to limit the influence of seasonality and better illustrate long-term trends in the data.

[19] The price increases in control cities are not an isolated trend. In both the United States and Michigan, median home prices have increased steadily since the Great Recession.

[20] Comparing Flint to the emergency manager control cities in Figure 3 is slightly harder because equation (3) compares Flint to the cities in event time rather than calendar time.

Table 2—Flint Housing Price Impacts: Matched-City Design

	(1)	(2)	(3)
Emergency × Flint	−0.179	−0.194	−0.257
	(0.039)	(0.039)	(0.055)
Water supply switch × Flint	−0.090	−0.074	−0.128
	(0.032)	(0.032)	(0.034)
Receivership × Flint	0.127	0.043	0.013
	(0.028)	(0.027)	(0.034)
Observations	37,318	37,318	37,318
N (houses)	28,309	28,309	28,309
Census block group fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
Labor force controls	No	Yes	No
City-year linear trends	No	No	Yes

Notes: The dependent variable is the log of home sales prices ($2019) in Flint and the top three matched control cities from January 2006 to August 2019. All regressions include census block fixed effects, year fixed effects, month fixed effects, and weather controls. Labor force controls are the log of each city's monthly labor force. Standard errors are clustered at the census block group.

V. Results

We first examine the impact of the water crisis on home prices using our matched-city research design. We then examine the results for our emergency manager design. Last, we explore heterogeneous impacts of the water crisis using demographic data from the United States Census Bureau and our service line and lead test data.

A. *Matched-City Design*

Table 2 reports results from estimating equation (1) for the matched-city design. All specifications include census block group fixed effects, year fixed effects, month-of-year fixed effects, and weather controls.[21] Column 1 includes only these baseline controls. Column 2 adds the log of each city's monthly labor force to control for time-varying, differential economic conditions in Flint relative to our control cities. A potential downside of using labor force as a control is that the crisis may have impacted labor markets in the city. As an alternative, column 3 adds city-specific linear time trends to control for time-varying unobservables that affect housing prices linearly over time.[22]

Across all specifications, we find a statistically significant decline in home prices after the water supply switch of 7.4 percent to 12.8 percent, with a further reduction after the emergency declaration between 17.9 percent and 25.7 percent. Overall, total impacts since the switch range from –26.9 percent to −38.5 percent.

[21] Census block groups contain between 600 and 3,000 people. We observe sales in 133 Flint block groups.

[22] Prior research finds that unit-specific linear time trends can attenuate estimated treatment effects when the true effects grow over time (Lee and Solon 2011; Meer and West 2016). We find similar magnitudes with and without the time trends, and results are robust to city-level quadratic trends. This suggests that the public health emergencies, in particular, had a sharp impact on prices.

Comparing specifications, the coefficient on $[Receivership \times Flint]$ is positive and significant in column 1, suggesting potential differential pre-trends leading into April 2014. Our estimates would be biased if Flint's receivership status affected home prices in ways that our model does not control. When we include a control for the city labor force in column 2, the magnitude of the $[Receivership \times Flint]$ coefficient drops by two-thirds and is no longer statistically significant. In column 3, the receivership coefficient is small and not statistically different from zero. Nonetheless, concerns may remain—particularly, for our specification in column 1—that housing prices were increasing in Flint relative to the control cities before the switch to the Flint River. For this reason, we focus on results using our specification in column 3 in much of the remaining results. Further, as we confirm in our honest difference-in-differences analysis in online Appendix D.2, accounting for potential pre-trends in column 1 implies our results are attenuated.

Figure 4 plots quarterly estimates from estimating equation (2). We use the same controls as specification (3) in Table 2. We observe a modest decline in home prices after the switch. Price impacts are not statistically significant in the first two quarters but then fall 16 to 17 percent below second-quarter 2014 levels, where they mostly remain until the public health emergency. The initial decrease corresponds to the timing of increased awareness indicated by the uptick in bottled water purchases (Figure 1). Prices fall further after the public health emergency declarations, settling between -25 percent and -45 percent below levels at the time of the switch. The average magnitude remains relatively stable at around -30 percent for nearly four years, including after the water was deemed safe in mid-2018, suggesting the crisis had long-term, negative impacts on home values in the city.[23]

Figure 4 provides another test of differential pre-trends. No year-quarter combination is statistically different from zero at the 5 percent level. Figure 4 adds coefficient estimates from the flexible difference-in-differences model with the same controls as column 1 for comparison. Recall that results in column 1 show the strongest presence of potential pre-trends. Consistent with this, we see suggestive evidence of a linear pre-trend in prices ending around the timing of the receivership. Even in these specifications, however, the estimated impacts of the switch and subsequent crisis are nearly identical to our preferred estimates.[24] Further, online Appendix D.2 presents results from an honest difference-in-differences exercise following recent work by Rambachan and Roth (2021). Using the specification in column 1, we account for potential linear pre-trends in home prices leading up the switch. The exercise suggests our preferred estimates are conservative.

Table 4, panel A shows the sensitivity of our results to including lower-priced homes in our specification with city linear trends.[25] Columns 1–5 present results

[23] We formally test for price recovery after April 2018 in online Appendix D.1 and find no clear evidence of a rebound after officials declared the water safe for human consumption.

[24] Figure C.1a similarly compares our preferred estimates to those using labor force controls.

[25] Online Appendix C.1 reports results for all specifications with a price threshold \$500 lower. We also include results where we limit the upper-price threshold to \$1 million instead of \$10 million and where we consider only low-priced homes (\$500 to \$25,000). Our results are not sensitive to our upper-bound price choice, but all specifications are sensitive to choosing a very low (\$500) price threshold.

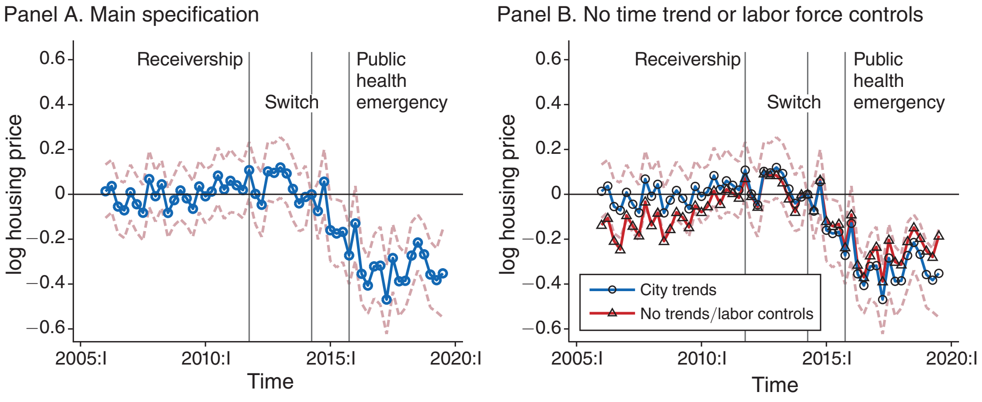


Figure 4. Home Price Event Studies: Matched-City Design

Notes: Panel A graphs year-quarter coefficients from the top-three matched control city design. The estimates plot the differential change in home prices in Flint versus the control cities leading up to and after the switch to the Flint River. The specification includes census block group fixed effects, year-quarter fixed effects, city–linear time trends, month fixed effects, and weather controls. Dashed lines show 95 percent confidence intervals. Panel B displays how the corresponding coefficient estimates differ for a specification with no labor force controls or city–linear time trends.

as we include homes that sold for more than \$20,000, \$15,000, \$10,000, \$5,000, and \$500, respectively. In all cases, the upper price threshold remains \$10 million. Results using a \$20,000 threshold are similar to results in column 3 of Table 2. A general pattern emerges where the coefficient on $[Emergency \times Flint]$ decreases and the coefficient on $[Switch \times Flint]$ increases as we include lower-priced houses down to \$5,000. The coefficient on $[Receivership \times Flint]$ increases as we lower the price threshold, however, suggesting that our research design may not be well suited for assessing impacts of the events in Flint on very low-valued homes. The most dramatic change occurs when we lower the price threshold to \$500. While the receivership coefficient is indistinguishable from zero in this specification, we see the largest losses after the water supply switch, with much smaller losses after the emergency. As we show in C.4, average total home value losses of the crisis, the sum of the coefficients, remains similar to our main sample. We remain most confident, however, following the prior literature and using our main price sample.

Online Appendix C.1 explores the sensitivity of our results to the use of different control cities and control variables. Results are similar and show no differential pre-trends in any specification when we use only the top matched control city (Youngstown, Ohio). Results are also similar for the top two matched cities. The overall magnitude of the crisis decreases as we include broader control city groupings (top five and ten), though differential pre-trends remain a concern in these alternative control samples. Our primary results are robust to including controls for home characteristics, year-month fixed effects, and quadratic city trends. We also present results from a property-fixed-effects (or repeat-sales) model. While the property-fixed-effects model controls for time-invariant unobservables at the house

Table 3—Flint Housing Price Impacts: Emergency Manager Design

	(1)	(2)	(3)
EM ends × Flint	−0.275	−0.346	−0.250
	(0.041)	(0.043)	(0.046)
EM starts × Flint × switch	−0.123	−0.087	−0.114
	(0.032)	(0.030)	(0.035)
EM starts × Flint	0.062	−0.035	−0.004
	(0.026)	(0.025)	(0.032)
EM ends	−0.027	0.012	−0.008
	(0.020)	(0.019)	(0.020)
EM starts	0.086	0.123	0.055
	(0.024)	(0.023)	(0.023)
Observations	28,058	28,058	28,058
N (houses)	20,608	20,608	20,608
Census block group fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
Labor force controls	No	Yes	No
City–event time linear trends	No	No	Yes

Notes: The dependent variable is the log of home sale prices ($2019) in Flint and the emergency manager control cities for the years leading up to, through, and after the emergency manager period. All regressions include census block fixed effects, year fixed effects, month fixed effects, and weather controls. Labor force controls are the log of each city's monthly labor force. Standard errors are clustered at the census block group.

level, the sample size is substantially smaller. Nonetheless, overall impacts from the property-fixed-effects model are similar to those described here.

B. *Emergency Manager Design*

Table 3 presents our results from estimating equation (3) for the emergency manager design. All specifications include census block group fixed effects, year fixed effects, and month fixed effects. As in our matched-city design, column 1 includes only these baseline controls, column 2 controls for the monthly labor force of a city, and column 3 replaces the labor force control with city–event time linear trends that control for differences in time-varying unobservables (in event time) for each city.

Beginning with the bottom row with $[EMStarts]$, home prices in all cities increase on average after an emergency manager period starts. The coefficient $[EMStarts \times Flint]$ tests whether Flint housing responded differentially to being placed under emergency management. In column 1, we see that Flint's home prices are approximately 6.2 percent higher after the emergency manager starts. In columns 2 and 3 this differential change no longer exists, suggesting that when controlling for labor force differences or city-specific linear trends Flint's housing market responded similarly to the start of receivership and the introduction of an emergency manager, supporting the validity of this second research design.[26]

[26] Similar to the matched-city design, the differential pre-trends in column 1 suggest that home prices in Flint were increasing relative to other emergency manager cities before the switch to the Flint River. As supported by our

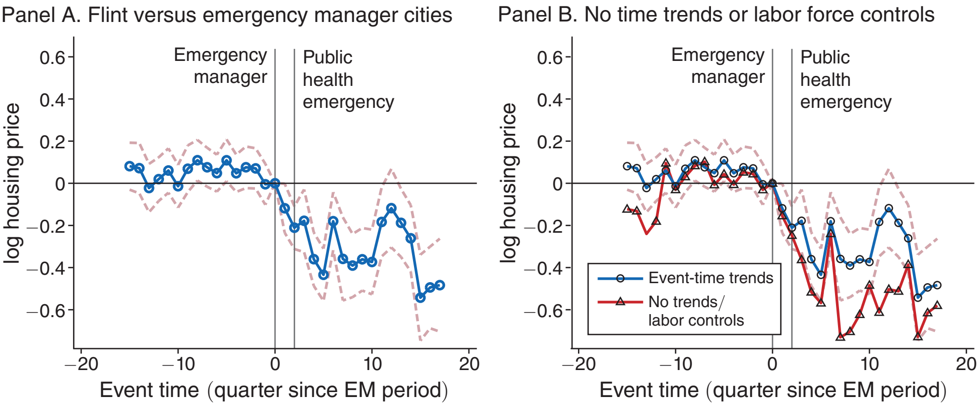


Figure 5. Home Price Event Studies: Emergency Manager Design

Notes: Panel A graphs year-quarter coefficients from the emergency manager empirical design. The estimates plot the differential change in home prices in Flint versus the control cities leading up to and after an emergency manager period. The specification includes census block group fixed effects, event time fixed effects, year fixed effects, month fixed effects, city–event time linear trends, and weather controls. Dashed lines show 95 percent confidence intervals. Panel B displays how the corresponding coefficient estimates differ for a specification with no labor force controls or city–linear time trends.

The coefficients $[EMStarts \times Flint \times Switch]$ and $[EMEnds \times Flint]$ estimate how the switch and public health emergencies, respectively, impacted Flint. After Flint switched its drinking water supply, home prices fell by 8.7 percent to 12.3 percent. The estimate is highly significant and nearly identical to those from the matched-city design. Flint's home prices fell an additional 25.0 percent to 34.6 percent relative to the control cities after the end of the emergency manager period. Overall total impacts since the switch range from −36 percent to −43 percent.[27]

Figure 5 plots quarterly estimates from a regression using the same controls as in column 3. Consistent with Table 3, we find no differential change in Flint home prices relative to our control cities leading up to the emergency manager period. We observe a steady decline in Flint home prices after the emergency manager period ends, which accelerates after the public health emergency declarations. While there is a temporary increase in prices three years after the public health emergency declarations, prices remain depressed up to fifteen quarters after the first public health emergency declaration. Figure 5 compares our main estimates to those using the same controls as column 1. The results suggest similar quarter-by-quarter changes in home prices in Flint relative to the control cities, though we generally find greater home price impacts from the crisis.[28] As before, there is some (though less stark) evidence of differential pre-trends in models with no event time trends or labor force

honest difference-in-differences analysis in online Appendix D.2, positive differential pre-trends would attenuate our results.

[27] The emergency manager indicator $[EMEnds \times Flint]$ includes five months before the first public health emergency. The overall estimates are similar, though larger, if we account separately for the periods after the end of emergency management and before versus after the first emergency declaration.

[28] Online Appendix Figure C.1b compares the year-quarter estimates to those when we include labor controls. Pre-event estimates are nearly identical to those in Figure 5, and we find larger home price impacts in later quarters after the public health emergency.

Table 4—Flint Housing Price Impacts: Alternative Price Thresholds

	(1)	(2)	(3)	(4)	(5)
Panel A. Matched-city design					
Emergency × Flint	−0.275	−0.265	−0.242	−0.229	−0.086
	(0.054)	(0.053)	(0.049)	(0.044)	(0.047)
Water supply switch × Flint	−0.153	−0.184	−0.206	−0.224	−0.423
	(0.039)	(0.041)	(0.040)	(0.037)	(0.050)
Receivership × Flint	−0.042	−0.113	−0.183	−0.237	−0.031
	(0.034)	(0.035)	(0.035)	(0.037)	(0.044)
Price lower bound	$20,000	$15,000	$10,000	$5,000	$500
Observations	40,924	45,522	51,525	59,444	69,765
N (houses)	30,182	32,360	34,833	37,894	41,855
Census block group fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Labor force controls	No	No	No	No	No
City-year linear trends	Yes	Yes	Yes	Yes	Yes
Panel B. Emergency manager design					
EM ends × Flint	−0.246	−0.266	−0.301	−0.341	−0.177
	(0.043)	(0.041)	(0.038)	(0.040)	(0.053)
EM starts × Flint × switch	−0.125	−0.138	−0.123	−0.151	−0.314
	(0.036)	(0.036)	(0.037)	(0.035)	(0.050)
EM starts × Flint	−0.063	−0.110	−0.163	−0.238	−0.155
	(0.033)	(0.035)	(0.035)	(0.038)	(0.056)
EM ends	−0.015	0.032	0.064	0.099	0.054
	(0.020)	(0.020)	(0.022)	(0.028)	(0.042)
EM starts	0.077	0.045	0.061	0.089	0.120
	(0.022)	(0.023)	(0.025)	(0.027)	(0.038)
Price lower bound	$20,000	$15,000	$10,000	$5,000	$500
Observations	31,191	35,257	40,893	48,680	59,884
N (houses)	22,188	24,113	26,473	29,831	34,637
Census block group fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Labor force controls	No	No	No	No	No
City–event time linear trends	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the log of home sale prices ($2019) in Flint and the specified control cities. All regressions include census block fixed effects, year fixed effects, month fixed effects, and weather controls. Standard errors are clustered at the census block group.

controls. Online Appendix D.2 graphs honest difference-in-differences bounds for these specifications. As with the matched-city design, the honest bounds suggest our preferred treatment effects may be attenuated.

Table 4, panel B shows the sensitivity of our results to including lower-priced homes in our specification with city–event time linear trends. The coefficients on $[EMStarts \times Flint \times Switch]$ and $[EMEnds \times Flint]$ are more stable than in the matched-city design as we decrease the lower price bound to $5,000 from columns 1 to 4. As in the matched-city design, including very low-priced homes ($500 to $10,000) leads to a higher coefficient on $[EMStarts \times Flint \times Switch]$, suggesting larger, earlier impacts of the events in Flint. The coefficient on $[EM\ Starts \times Flint]$, however, is large and statistically significant in all models except column 1. This again suggests low-priced homes in Flint may follow differential pre-trends than those in our chosen control cities. Online Appendix C.2 reports the results from other robustness tests. We examine the sensitivity of the results to alternative emergency manager–controlled cities and including alternative control variables. As with the matched-city design, results are robust across these different specifications. Notably, the magnitude of overall price impacts is very similar for the property fixed effects (repeat sales) model and those in Table 3: -38 percent to -40 percent compared to -36 percent to -43 percent.

C. *Heterogeneity Analysis*

This section explores price heterogeneity of the water crisis within the city of Flint. To conduct these analyses, we group Flint census tracts into 18 tract areas.[29] We then interact indicators for each tract area with the $[Emergency \times Flint]$ and $[EMEnds \times Flint]$ indicators in equations (1) and (3), respectively.[30] We focus here on results from regressions that include city–linear time trends, though results are similar across other specifications.

Figure 6 graphs the tract area coefficient estimates for the matched-city and emergency manager designs. All estimates are negative. For the matched-city design, the estimates range from -14 percent to -43 percent. For the emergency manager design, the estimates range from -11 percent to -39 percent. Tract area estimates are highly correlated across the two designs.[31] The maps indicate that most areas of the city experienced a 15 percent to 35 percent decline in prices after the emergency declarations. No single tract area or group of tract areas appear to drive the results on impacts. All areas experienced declines, and most declines are in the range of our main estimates. Nonetheless, we explore whether the price heterogeneity correlates with tract area demographic or lead risk and remediation metrics.

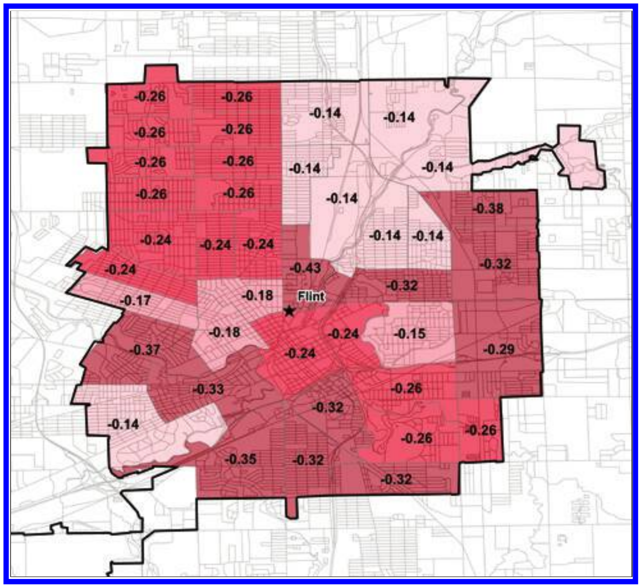
Demographic Characteristics.—Online Appendix Figures D.2a and D.3a compare the tract price coefficients to preswitch demographics from the 2010–2014 American Community Survey. We include scatterplots that examine relationships between our estimates and tract area median home values, the fraction of nonvacant homes, the fraction of owner-occupied homes, median home ages, median incomes, percentage of the population above the poverty level, and share of the population that is non-White. For the matched-city design coefficients, we find a significant, negative relationship between the tract area effects and the fraction of nonvacant homes and the median home age. Both correlations appear to be driven by one tract. No demographic characteristics show a statistically significant relationship with the tract area price effects for the emergency manager design. When we regress the home price impacts on all characteristics, we find that none significantly predict the tract area effect size except perhaps a positive correlation with the area's median income. These findings together suggest that areas of the city with

[29] We construct the tract areas as either individual tracts or neighboring tract groups with a minimum of around 30 sales after the emergency health declarations. See Online Appendix A.4 for more details.

[30] We focus on the postemergency period since, should it exist, we would expect to find the greatest heterogeneity after the emergency declarations.

[31] The correlation coefficient between the two estimates is 0.8.

Panel A. Matched-city design



Panel B. Emergency manager design

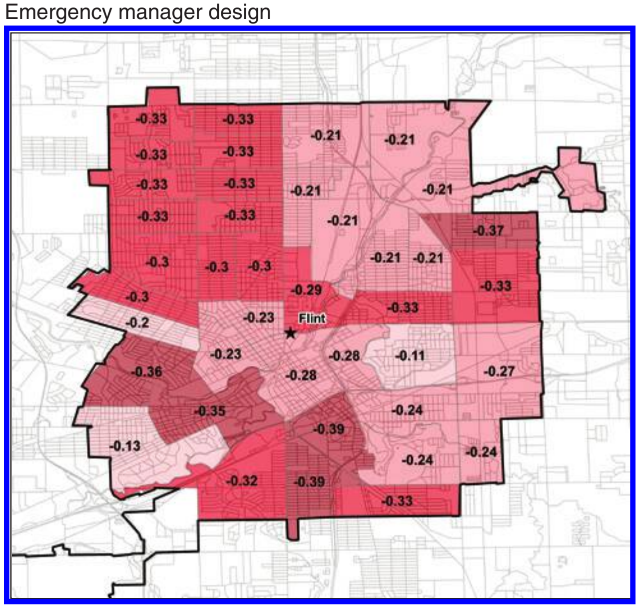


FIGURE 6. PRICE HETEROGENEITY BY CENSUS TRACT

Notes: Figure 6 displays the results of tract-level interactions for Flint after the public health emergency declarations (panel A) and after the emergency manager period ends (panel B). Price impacts are grouped by quintile. Darker red indicates larger impacts. Coefficient estimates correspond to the respective tract area. The City of Flint boundary is delineated by a dark black line. Census block groups are delineated by gray lines. The maps exclude the Flint airport, in the southwestern section of the city.

a higher fraction of nonvacant homes and lower incomes may have experienced greater impacts from the crisis.

Lead Risk.—We conduct a similar analysis to examine whether our estimated price impacts correlate with lead remediation efforts or lead risks. Figures D.2b and D.3b graph scatter plots of the tract area price effects with the fraction of occupied homes where lines were inspected, replaced, and not replaced. We also examine correlations between tract area impacts and the fraction of homes where the entire service line was replaced, only the private section of the line was replaced, only the public portion of the line was replaced, and the average tract area lead concentration. We find some evidence of greater impacts in areas with more service line replacements and a positive correlation between tract area impacts and average lead concentrations. When all lead-related characteristics are included in a single regression, the average lead concentration is still positively and significantly related to housing market impacts. One explanation is that the results may suggest smaller impacts in high-lead areas in anticipation of remediation, yet we find little evidence of this market response to other measures of actual lead line replacement.[32]

A primary takeaway from this analysis is that the crisis impacted the single-family housing market in Flint as a whole. While we find some evidence that suggests differential price impacts across the city, both research designs suggest that the magnitudes of these differences are small, and we find little evidence that the differential effects correlate in an important way with demographic or lead risk and remediation characteristics.

D. *Home Sales Impacts*

A natural question is whether the Flint water crisis also affected housing sales. Figure plots smoothed quarterly home sales in Flint and our control cities. Figure 7, panel A compares Flint to the matched control cities. Flint saw a large decline in housing sales from 2006 through mid-2015. Sales flattened or even increased through the end of our sample and were relatively stable around our events of interest. The matched control cities exhibit similar trends through 2012 but have seen steady growth since then. Figure 7, panel B compares Flint sales to those in the emergency management control group. Sales in emergency manager control cities fluctuated between 2006 and 2012, became relatively constant between 2012 and 2018, and dropped toward the end of the study period. We formally test for home sales impacts of the crisis in online Appendix D.4. After the switch, the probability of observing sales in a Flint census block group relative to either a matched city or emergency manager city block group decreased between 8 percent and 14 percent. We see no further declines in the likelihood of a sale in Flint after the emergency

[32] One may expect home value losses to vary more systematically based on lead risk and exposure. Lead line locations, however, were not fully known over much of our sample period. Efforts to digitize lead line locations did not begin until after the emergency declarations, and the city has excavated far more service lines than it has replaced due to imperfect information. Further, others have found evidence that a range of household characteristics (including service line type) only weakly predict in-home lead test results, suggesting other factors also affect in-home lead concentrations (Chojnacki et al. 2017).

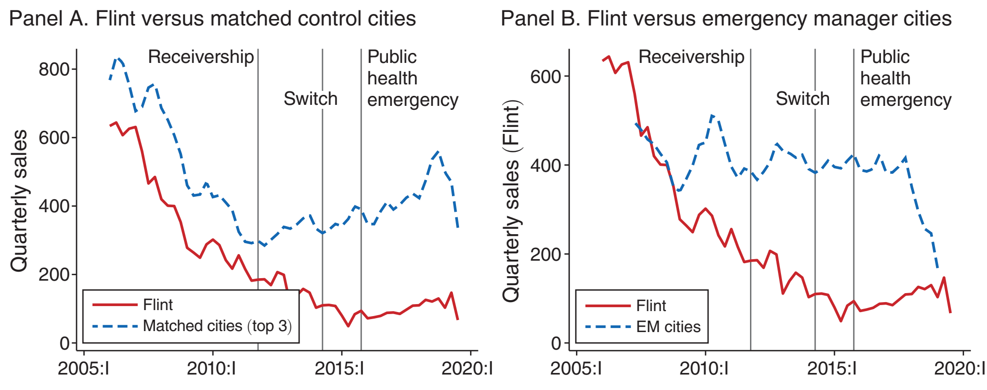


Figure 7. Home Sales: Flint versus Control Cities

Notes: Panel A graphs quarterly home sales for Flint and the top three matched control cities. Panel B graphs quarterly sales volumes for Flint and the emergency manager cities. All sales are a moving average including the previous two quarters to smooth seasonal fluctuations in home sales.

declarations. The analysis confirms our findings in Figure 7. Sales in Flint were relatively stable in the period where we estimate the largest price impacts.

We also test for compositional changes in home sales in online Appendix D.4. Specifically, we test whether the probability of a sale varied across Flint census block groups and examine correlations with within-Flint heterogeneity in the same characteristics considered in Section VC. As with the price regressions, we find limited evidence of heterogeneity in the sales volume with respect to demographics and lead risk in our scatterplots. There is one exception. In the matched-city design, areas with higher median home values were more likely to sell after the public emergency declarations. If true, our estimated price impacts would be attenuated. In a regression with all variables included, this relationship between home value and sales probability no longer holds. Instead, we find a small, negative relationship between owner-occupied housing and sales probability. Estimating the same heterogeneous sales responses using our extended home price range ($500 to $10 million) finds stronger correlations with tract-area economic well-being and whether a tract saw an increase in home sales after the emergency declarations. Specifically, tract area treatment effects are positively correlated with median income, owner-occupied housing, nonvacant homes, and households above the poverty level. The result highlights further concerns with this broader home sample.

We use three additional data sources to further explore possible changes in home sales and composition. Online Appendix D uses data from the Home Mortgage Disclosure Act to examine whether the characteristics of individuals applying for mortgages changed over time in Flint. We find evidence of an increase in mortgage applications from high-income households after 2014, but no other significant departures from underlying trends in the characteristics of individuals applying for mortgages. Online Appendix D.5 uses data from the United States Census Bureau to examine whether the overall population in Flint fell since 2014 and data from InfoUSA to identify homeowner and renter out-migration patterns from Flint. We find some evidence that population fell slightly in the city. The magnitude of population changes, however, is unable to explain the price impacts we estimate above. Last, we explore compositional changes in high-value versus low-value home sales in online Appendix D.4. Low-value home sales increase at the beginning of our sample but have remained relatively steady from 2010 through the end of our sample. Low-value home sales do not show notable increases or decreases around our events of interest. In summary, housing prices fell sharply while other aspects of the market remained (relatively) stable.

E. *Aggregate Housing Value Losses*

The consistency of estimates of housing price impacts across the two different research designs is striking. Our preferred specification of column 3 of Table 2 suggests a decline in home prices of 38.5 percent since the switch. This is nearly identical to the 36.4 percent decline in column 3 of Table 3. Further, in the heterogeneity analysis, the correlation coefficient in the tract area impacts across the two separate designs is 0.8, suggesting similar estimates both within Flint and at the city level. The fact that two separate research designs yield similar counterfactuals provides compelling evidence that our estimates identify the causal impacts of the water crisis.[33,34]

We use both research designs to estimate the change in the total value of the housing stock in Flint. We adjust the relevant coefficients from specifications (3) of Tables 2 and 3 following Kennedy (1981) to obtain estimates of the percentage change in housing values. We then multiply these estimates of the percentage change in housing values by the average home value in our sample. We compute standard errors using the delta method. In the matched-city design, we find that average Flint home prices declined by \$29,412 (SE = \$4,655) from the switch to August 2019. The emergency manager design suggests a similar decline of \$27,397 (SE = \$3,816). According to the 2014 one-year American Community Survey, Flint had 39,125 occupied housing units in 2014. Of those, 79.1 percent were detached, one-unit homes. Owner-occupied homes with values greater than \$25,000 constitute 61.4 percent of the housing stock in Flint.[35] Using these figures, we estimate total housing market losses of \$559 million (SE = \$88 million) using the matched-city

[33] Further, Figures 4 and 5 both indicate that Flint's housing prices evolved in parallel to each respective counterfactual group of cities before the crisis. The fact that different research designs suggest similar counterfactual trends four years after the crisis provides additional support for the assumption that there were no contemporaneous events occurring in control cities that could confound our difference-in-differences estimates.

[34] An important caveat remains. Our primary analysis limits attention to homes priced over \$25,000. While these represent the majority of the value of the Flint housing stock, we exclude many properties with this price restriction. Our choice of lower bound is common in the literature. Although we include and discuss results from broader price ranges, we remain cautious in interpreting them as causal since many low-priced homes are unlikely to be arm's-length transactions and several specifications suggest the homes follow differential pre-trends. We leave determining impacts of the Flint water crisis and other important environmental crises on low-value homes to future research.

[35] Using data on rent payments in Flint suggests a similar or even larger proportion of renter-occupied homes in the city are valued at \$25,000 or higher. Further, while homes in our main sample make up just over 61 percent of the number of homes in Flint, 2014 ACS data suggests they make up nearly 90 percent of the value of homes in the city. As such, we view our preferred citywide home value loss estimates for single-unit detached homes as conservative.

design. The emergency manager design yields total housing market losses of \$520 million (SE = \$73 million).[36]

How do our damage estimates compare to the value of the total housing stock in Flint? While the ACS does not provide exact home value estimates, it tracks the count of owner-occupied units in different home value ranges and the count of renter-occupied units in different cash rental price ranges. Using the midpoint of these ranges, we estimate Flint's housing stock was worth around \$1.82 trillion in 2014.[37] Adjusting for inflation, our estimates suggest a 27 percent to 29 percent loss in the aggregate value of occupied housing units in the city.

VI. Discussion and Conclusion

A few important features of this setting are worth noting. First, our estimates capture the housing market impacts from a crisis resulting from a system-wide loss of access to safe drinking water. As such, they do not solely reflect consumers' valuation of clean water but instead highlight how disastrous events like those observed in Flint can have large and lasting negative impacts on communities. In Flint, there were likely other significant costs, including human health and well-being, not captured by these housing value losses. Further, these housing impacts occurred despite more than \$400 million in state and federal spending on remediation efforts.[38]

Our results raise a critical question. Are damages from the Flint crisis permanent? The impacts that we identify at the onset of the emergency declarations have changed little over time despite the fact that the water has been declared safe for human consumption and nearly all residential lead service lines have been replaced. When the city ended its free bottled water program, private expenditures on bottled water increased to the same high levels seen after the switch in the drinking water supply.

Several potential explanations exist. Depressed housing values may reflect, in part, significant damage to public trust. Articles in the popular press highlight continued frustration and mistrust of government officials (Smith, Bosman, and Davey 2019). Bottled water and water filter sales suggest persistent mistrust in the safety of the city's tap water. In the wake of institutional failures that repeatedly affect disadvantaged communities, public mistrust can persist and further affect long-run out-

[36] Tables C.9 and C.10 present coefficient and damage estimates for all three specifications for the main and extended price ranges. Average home damage estimates are around \$8,000 lower in both models when we consider the extended price range, but citywide losses are \$66 million to \$102 million larger since the specifications cover all single-family detached homes.

[37] We impute renter-occupied home values by multiplying the midpoint cash rental price by the ratio of the average home value in Flint to the average cash rental price.

[38] We find it helpful to contextualize our home value losses. Our preferred estimates suggest a decline of 36 percent to 39 percent in housing values since the switch. This is larger than a 22 percent decrease for a toxic algae bloom (Wolf and Klaiber 2017), a 14 to 16 percent decline for a cancer cluster (Davis 2004), a 15 percent increase for brownfield remediation (Haninger, Ma, and Timmins 2017), a 14 percent decrease for the siting of a well pad for shale gas (Muehlenbachs, Spiller, and Timmins, 2015), an 11 percent decrease for the opening of a toxic plant (Currie et al. 2015), and an 8 percent decrease for the discovery of hazardous waste (Mendelsohn et al. 1992). Our estimates are roughly equivalent to a recent study of the value of lead paint remediation. Billings and Schnepel (2017) estimate a 32 percent increase in home values from remediation outside the context of a spike in lead exposures, though the authors note that a significant fraction may be due to other property improvements associated with remediation.

comes (Alsan and Wanamaker 2018). Other reasons may include long-run changes in stigma associated with living in Flint (McClelland, Schulze, and Hurd 1990; McCluskey and Rausser 2003) and the damage done to homes' internal plumbing and appliances.[39] While housing values may increase in the long run as trust is restored or as outstanding damages are addressed, our results indicate that the impacts of economic shocks like the Flint water crisis can last long after remediation.[40]

The magnitude of the housing market impact naturally begs the question of what, if any, changes should be made to the way we provide and regulate public services like safe drinking water. On the one hand, one could argue that the regulations in place are sufficient, as the crisis was caused by failures to adhere to existing rules. Perhaps all that is needed are more significant safeguards to prevent similar cases of wrongdoing on the part of state and local officials. On the other hand, the rule does not account for the possibility of system-wide failures.

The original Lead and Copper Rule of 1991 placed a heavy emphasis on corrosion control, which had favorable cost-benefit ratios. This seems rational at first glance. If Flint had adhered to the rule, the switch likely would not have triggered a crisis. The crisis, however, occurred in part because of the existence of lead service lines and plumbing throughout the city. The EPA did not include a major program to replace lead service lines in the 1991 regulation because the cost-benefit ratio of a nationwide program was deemed unfavorable ("Lead and Copper Rule" 1991). The original analysis of the benefits of such a program were based on studies estimating health benefits from changes in lead exposure before and after service line replacement. This type of cost-benefit analysis does not account for a potential crisis that results in system-wide losses of safe drinking water, the associated health impacts of such an event, or the persistent impacts of such an event on public trust. While it is difficult, if not impossible, to gauge the likelihood of an event like the Flint water crisis occurring again, our study highlights a key limitation of the current framework used by regulators in assessing benefits of critical infrastructure investment programs.

Indeed, the Flint water crisis has been cited as a significant reason why the recently signed Infrastructure Bill allocates \$15 billion in federal funding to replace lead service lines (West 2021). Given that the Flint crisis resulted in over \$500 million in damages to housing values alone, such spending may be economically efficient. Yet, the cost-benefit analysis for a recently revised Lead and Copper Rule still ignores the possibility of system-wide failures. The revisions add a range of new testing requirements and require community water systems to create and provide public inventories of lead service lines (US Environmental Protection Agency 2020). These revisions, however, have been criticized for extending the length of time that systems in violation of the rule have to replace lead service lines (Kaplan

[39] After the crisis, households learned that the switch to the Flint River not only damaged the service lines but also damaged pipes and appliances. State and federal funds have been allocated to pay for service line replacements but do not cover damaged appliances or pipes within homes. According to *Money* magazine, replacing in-home pipes and fixtures will cost an average homeowner around \$7,200 (Talty 2017).

[40] Our sample ends in August 2019, just months before COVID-19 was first reported in North America, complicating potential long-run studies in this setting.

and Dennis 2020). As with the 1991 rule, systemic failures like those found in Flint were not considered in the accompanying economic analysis.

The Flint water crisis resulted from a series of institutional failures and violations of federal regulatory requirements. We believe the resulting housing market losses are relevant for understanding the benefits of current and future federal drinking water regulations such as the Safe Drinking Water Act and the Lead and Copper Rule. In particular, our results suggest the need to revisit how these rules account for the possibility of major, system-wide impacts that can have widespread economic consequences and lasting impacts on public trust.

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