It's the Smell:
How Resolving Uncertainty about Local Disamenities Can Affect the Housing Market

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Abstract

This study examines how the housing market responds to closing a major environmental disamenity nearby, particularly when the credibility of local policy is uncertain. Fresh Kills Landfill (NY) provides an empirical setting to examine this question across multiple distinct events with varying credibility signals. Results from a difference-in-differences analysis show that market prices and volumes respond sharply to credible actions (i.e., capping the landfill and park transitioning) rather than policy announcements. The findings suggest resolving uncertainty can have a powerful supply effect for housing markets, applying downward pressure on prices in the short run, thereby overshadowing plausibly positive demand effects.

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"The wind blows across Fresh Kills, bearing away odors that would look, if visible, like a fractal stain petroleum leaves in water." - Verlyn Klinkenborg in "Elegy to a Dumpscape," New York Times (October 10, 1999)

"I hate this place. This zoo. This prison. This reality, whatever you want to call it, I can't stand it any longer. It's the smell; if there is such a thing. I feel saturated by it." - Agent Smith (from the 1999 film *The Matrix* – probably not referring to Staten Island specifically)

1 Introduction

Since at least the onset of the Industrial Revolution, homeowners have coexisted among a wide array of environmental hazards that lower quality of life for those who live nearby – from pollutant-emitting factories to odorous landfills to swine farms. Property markets generally price in the proximity to these sites, as it is usually cheaper to buy or rent properties closer to nuisances to compensate for the negative external effects or "disamenities" that make a particular locale less desirable (Smith and Desvousges, 1986).[1] For instance, homes near landfills typically sell for a substantial discount compared to similar homes further away in order to price in the environmental hazards and noxious smell.[2] However, these disamenities are not always permanent. In some instances they move, close, or, in the case of landfills, are eventually converted to an amenity like a park or nature preserve. In this paper, we study such a reversal: when a major landfill closes, particularly one whose closure has been shrouded in uncertainty for decades, how does the housing market respond to this 'shock'? And, is this a simple story of a positive change in local demand; or, is there a more nuanced story regarding *supply* and liquidity in the housing market, too?

To answer these questions, we examine the closing of Fresh Kills Landfill on Staten Island, New York, finding that the data fit a more nuanced story – a liquidity/supply response can overshadow an unambiguously positive demand shock to the housing market. At one point Fresh Kills was largest landfill in the world covering nearly $2,200$ acres and receiving nearly $30,000$ tons of waste per day. Beyond its size and notoriety, the history of Fresh Kills provides a useful setting for understanding how housing markets respond to policy announcements when the market has experienced tremendous uncertainty about the timing of implementation. This setting also allows us to contrast the housing market's reaction to the news of a seemingly credible announcement to close Fresh Kills versus tangible steps toward implementation (i.e., the actual capping of the landfill). From these comparisons, the results shed light on how context can matter when measuring the impact of an environmental (dis)amenity. Fresh Kills' circumstances suggest the credibility of a policy change plays a critical role in the housing market's response.

An important element of this context stems from Fresh Kills' local history, where uncertainty shrouding the timing of the landfill's actual closure originated from decades of experience to doubt policymaker promises. Fresh Kills began its principal operations in 1948 and became the primary destination for New York City trash disposal soon thereafter. While the initial plan for the site was for the landfill to be operational for only a few years, its closure was delayed decade after decade. As other local landfills proceeded to close, trash intake at Fresh Kills grew exponentially through the mid-1980s. Many closure announcements and subsequent delays to its closure generated substantial uncertainty among Staten Island residents as to when exactly the site would be capped and transitioned to parkland. In the spring of 1996, state and local policymakers intended to resolve this with a final "surprise announcement" of a deal that would

[1] The housing market is a critical part of the U.S. economy and household balance sheet. Housing is typically about 10-13% of GDP, and residential housing made up 49% of private fixed assets in 2023 (U.S. BEA, Table 1.1. Current-Cost Net Stock of Fixed Assets and Consumer Durable Goods - FAAt101).

[2] See, for example, Nelson et al. (1992); Hite et al. (2001); Kinnaman (2009); Ready (2010); Ham et al. (2013), and numerous other related papers in this literature that estimate the effect of landfills on nearby home values.

permanently close the landfill (Melosi, 2020).[3] The key question for the housing market becomes: do potential buyers and sellers actually believe this and respond as a simple supply and demand model might predict when the news is credible? Or, does the market respond when they see substantial evidence of the landfill closing, like when the mounds are being capped and tangible progress is made toward its transition?

Methodologically, we exploit the timing of both the surprise announcement and its implementation as quasi-natural experiments to help shed light on how housing markets respond to these events. Our analysis draws on detailed data from the U.S. housing market, containing detailed information on home transactions going back decades. Although we use only a subset of national data in our analysis, a key benefit of national microdata is that we can observe critical details about the Staten Island housing market during the 1990s around the time of these events along with other markets near landfills across the U.S. during the same period. This allows us to compare thousands of home sales on Staten Island (i.e., those closest to the landfill ("treated") versus homes further away ("control")), both before and after these events using a difference-in-differences method. The data also allow us to contrast the treated market to housing markets outside of New York near other major landfills during the same time period as additional control markets.

The results from our analysis support two main takeaways. First, we find the local housing market had no significant reaction to the surprise policy announcement, despite the fact that the governor, mayor, and local bureau president grabbed local headlines with a seemingly definitive deal to close Fresh Kills once and for all (catalyzing legislation merely days later to codify the closing). Yet, when large portions of the landfill had been capped during the spring following the announcement, only then do we observe the housing market move sharply in response. Initially, this result seems to stand in contrast to studies like Moulton et al. (2018)), which found housing markets responded immediately to a policy change resulting from an election (i.e., prior to its implementation).[4] Yet, one through-line between our study and Moulton et al. (2018) is that credibility matters, where housing markets are reacting to *credible* information signals about a local policy change. The main difference in the Fresh Kills setting is that credibility coincided with the policy's actual implementation.

A second takeaway is that, while prices of homes on Staten Island generally showed significant price appreciation in the months and years that followed Fresh Kills' closing, home prices nearest to the landfill actually dropped sharply (about 8% just after capping) relative to the control group. At first, the direction of the price result nearest to the landfill seems counterintuitive. Implementation of the landfill's closure would appear to be good news for homes most affected by the landfill's externalities, thereby raising the expected value of these assets. In fact, the evidence is still consistent with a standard prediction of a boost in demand, but with the important caveat that the supply of existing homes on the market also shifted dramatically. This is precisely what we observe – a sharp spike in transaction volume in the treated market just after the policy implementation (capping). Homes nearest to the site, which were relatively illiquid due to this shroud of uncertainty, suddenly became much more liquid. The data show a spike in transaction volume in the months following capping (about 26% higher), which remained persistently higher for years after the shroud was finally lifted. Indeed, these were not necessarily new, recently built homes; rather, these were primarily existing, older homes that owners were likely hesitant to list on the market prior to uncertainty being credibly resolved.

Overall, our findings underscore the possibility that a local shock like this can change both demand *and* supply conditions for existing homes in the short run. Homeowners rushing to list

[3] Melosi (2020) documents a comprehensive history of Fresh Kills, including a chapter devoted to the events surrounding its closure. Under the subheading "The Surprise Announcement" in the closure chapter, Melosi described the event in 1996: "Storm clouds cleared at a momentous press conference held on May 28 at the borough president's office on Staten Island. Mayor Guiliani, Governor Pataki, and Borough President Molinari appeared together. They announced what Staten Islanders had been hoping for years: Fresh Kills would be closed to future shipments of refuse on December 31, 2001." At the event, the governor described it as "put[ting] an end to one of the worst environmental nightmares this state and city have ever witnessed" (p. 418).

[4] See, for example, a detailed discussion in Moulton et al. (2018) or Moulton et al. (2022) on how the housing market and other markets respond quickly to information shocks and policy announcements.

their homes can substantially overwhelm whatever boost to demand in the area might have been. In the longer run, prices of properties nearest to the landfill steadily ticked back up toward the control group (*i.e.,* those homes located a bit further away), but would not fully recover for years thereafter. When a new park was completed in 2012, we do observe a boost in prices (about 2.2%), which is consistent with a more conventional positive demand shock as a result of Fresh Kills' first transition to a functioning park.

We perform variety of additional tests to explore whether the effects we observe are properly identified. First, we exploit placebo treatments to explore whether the change in prices and volume we observe are not merely a relic of seasonal patterns or national trends in this market. We accomplish this by examining markets in other states to evaluate whether properties located near other large landfills experienced the same market dynamics around the time of the Staten Island events in order to rule out confounding national trends that somehow changed the prices of these types of homes elsewhere in the same direction (absent a similar local policy announcement near their local landfills).[5] Second, we verify that treated homes trended similarly to those of the comparison group (*i.e.,* parallel trends) and that physical composition of these sales (*e.g.,* the characteristics of the properties themselves) were not significantly different across the relevant time period. Finally, the results are generally robust across alternative specifications and methods, including a regression discontinuity in time (RDiT) methodology where each event is treated as a discrete information discontinuity.

Our findings contribute to several strands of literature. First, the results highlight an often neglected channel, liquidity, through which local disamenities studied in environmental economics,[6] real estate,[7] and urban economics[8] can affect property markets. Papers in these areas often focus on the extent that disamenities (environmental or otherwise) are capitalized into home prices, including landfills and other waste sites. When disamenities are reversed or cleaned up, Greenstone and Gallagher (2008) and others have pointed out the common prediction in the literature that, "the improvement at the site should lead to increases in the demand and supply of local housing and, in turn, increases in the prices and quantities of houses" p. 953. The predicted increase in price assumes the positive demand shock is greater than the positive supply shock. However, our results show a shock to liquidity can be quite substantial, mitigating or even surpassing demand pressures on prices. Overlooking this possibility could mean that one might initially dismiss the evidence for a demand shock as underwhelming, where the price may not appear to fully capitalize the shock. A similar phenomenon in other studies may simply manifest in a subdued positive price effect, zero effect, or even a negative price effect in other studies. We should note that this takeaway is not particularly sophisticated or clever; it merely underscores a basic principle taught in an introductory microeconomics course, that shocks to a market necessitate an evaluation of both price *and* quantity. Thus, we join a budding literature emphasizing that researchers examining home price dynamics should more seriously consider measures of quantity and liquidity, like market sales volume or (when the authors have listing data) time on market for listings, when examining local disamenity effects on the housing

[5] Specifically, we compare the New York experience to two major landfill sites in Illinois and California and many smaller sites in nearby New Jersey. Mallard Lake (outside of Chicago) closed in the same era as Fresh Kills (in 1999), but had announced its closure much sooner, in 1992. Puente Hills (outside of Los Angeles) became the largest landfill in the U.S. after Fresh Kills closed and remained open through most of 2013. As we discuss more below, we use these as placebo tests, whereas none of these landfills experienced a similar policy shock (treatment) at the time of the Fresh Kills announcement.

[6] For example see Clark and Nieves (1994), Dale et al. (1999),Deaton and Hoehn (2004),Garrod and Willis (1998),Ham et al. (2013),Havlicek et al. (1971),Ihlanfeldt and Taylor (2004),Kiel and McClain (1995),Kiel (1995),Li and Li (2018),McCluskey and Rausser (2003a), and Smith and Desvousges (1986). For a literature review and meta-analysis, see also Braden et al. (2011).

[7] See, for example, Bleich et al. (1991), Bouvier et al. (2000), Hite et al. (2001), Kiel and Zabel (2001),Kohlhase (1991),McCluskey and Rausser (2001),Nelson et al. (1992),Ready (2010),Reichert et al. (1992),Smolen et al. (1992), and Thayer et al. (1992).

[8] See also Brasington and Hite (2005), Din et al. (2001), Kiel and Williams (2007), Michaels and Smith (1990), Seok Lim and Missios (2007), Nelson et al. (1997), and Owusu et al. (2014).

market.[9]

Second, our findings contribute to a broader literature in accounting,[10] economics,[11] and finance[12] that document how uncertainty has significant, real effects on markets. One unintended consequence of decades of delaying the closure of Fresh Kills Landfill was that neither prospective buyers nor sellers of properties nearest to the landfill could know when the site would finally be converted to parkland and a nature preserve as promised, likely leading to gaps in willingness to pay (WTP) and willingness to accept (WTA) by buyers and sellers, respectively.[13] Indeed, as one might expect, these properties experienced persistently low volume of transactions prior to the actual initiation of its closure, as policymakers failed to outline clear, credible timelines for closure that the public could trust (Melosi, 2020). One takeaway from the literature on policy uncertainty is that by creating immense uncertainty, policymakers can drive wedges between buyers' and sellers' expectations. Resolving such uncertainty so late in the process can cause further damage to sellers by unintentionally creating a dash to the exits, so-to-speak. Indeed, other studies of disamenities (like Superfund sites) with varying degrees of uncertainty may want to carefully consider the role of uncertainty when measuring capitalization. We return to this point in the concluding section below.

Finally, from an empirical standpoint, this research highlights the importance of credibility in the identification of policy shocks. Surprise announcements or exogenous information events may have immediate and potent effects on housing markets (e.g., Moulton et al. (2022)) and a variety of other markets more generally, as we often observe in stock markets when firms experience shocks to their valuation as a result of news. Yet, all news is not created equal. If policymakers cannot credibly convince the public of their intentions when they make a policy announcement, a decades-long literature in monetary economics (e.g., Taylor (1982), McCallum (1984), Blackburn and Christensen (1989), Du et al. (2020)) has shown the announcement may not have its intended effect. The findings from our paper add to the empirical literature underscoring the role of credibility for local policy announcements in moving markets; whereas, in absence of credibility, it is actions that convince markets.[14]

2 Background – A Brief History of Fresh Kills Landfill and Staten Island

To understand the context of the closing and transitioning of Fresh Kills, it is critical to understand its history - from its origins through the decades of delays and broken promises. Located approximately five miles south of Lower Manhattan, Staten Island had developed on a course

[9] See, for example, Bian et al. (2021), Brastow et al. (2018), Wong et al. (2012), Wentland et al. (2012) that emphasize liquidity and time on market as important for understanding the impact of the (dis)amenities and information asymmetry in the housing market. More broadly, Benefield et al. (2014) review the literature from prior decades that analyze both home prices and time-on-market jointly as outcomes of interest. See also Carrillo and Williams (2019) and others who emphasize the measures of home liquidity are essential for understanding the housing market more generally.

[10] See Drake et al. (2022), Jacob et al. (2021) or Hanlon et al. (2017) for how tax uncertainty, for example, affects firms' decisions.

[11] There is a sizable and growing literature on the economic effects of policy uncertainty. See Baker et al. (2016), Brogaard and Detzel (2015), Kang et al. (2014), Wu et al. (2020) among numerous other recent examples in this literature.

[12] See Julio and Yook (2012), Gulen and Ion (2016), Jens (2017), Drobetz et al. (2018), and Guceri and Albinowski (2021) for some recent examples. See also a related strand of literature (e.g., Baldauf et al. (2020) and Bernstein et al. (2022)) exploring how idiosyncratic expectations and beliefs of market participants matter for how real estate markets react to environmental information.

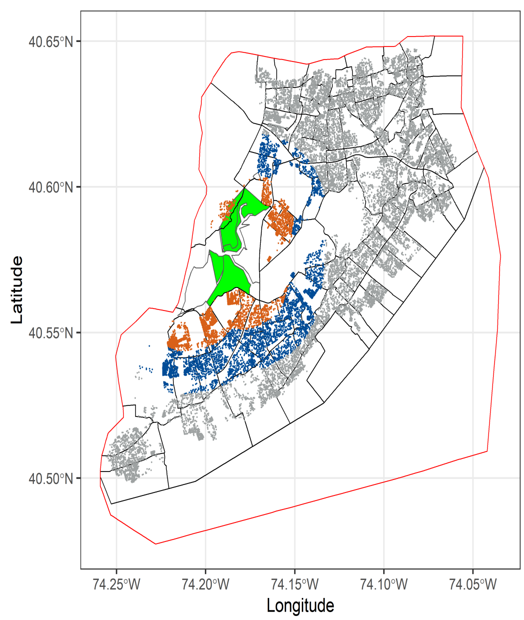
[13] A number of papers discuss this bargaining process in more depth, including in the face of environmental disamenities. For example, see Smith and Desvousges (1986), McCluskey and Rausser (2003b), and Hite (1998).

[14] In a discussion on the credibility of monetary policy, Friedman (1982) argued that once the credibility of monetary policymakers had eroded, only actions mattered: "[S]hort-term swings in monetary growth do no great harm if they are not only actually reversed but also widely expected to be reversed. But there's the rub. The wide short-term swings, partly due to lagged reserve accounting, have eroded the credibility of Federal Reserve policy statements and that *credibility can only be restored by actions.*" p.113, emphasis added in italics

that is distinct from the other New York City boroughs. Until the early 1960's, the Island was largely rural and agricultural with some urban presence in the northern shore of the island. Commuting options for residents were once constrained by the lack of a direct crossing by bridge, restricted to the Staten Island Ferry in order to travel directly to Brooklyn and Manhattan. The opening of the Verrazano-Narrows Bridge in 1964 vastly improved mobility, thereby making Staten Island a viable option as a commuter neighborhood. A wave of real estate development occurred in response to the bridge's opening, which included major amenities such as the Staten Island Mall and nearby housing developments. The first major expressways were introduced in the 1970's and the region began to shed its rural character, growing into the suburban region that is part of the Island's reputation today. Notable levels of development occurred along parts of the South Shore in which three quarters of the current housing stock was constructed in the period after the bridge's opening (NYSOSC, 2005). Figure 1a depicts these major landmarks and the orientation of Fresh Kills in relation to the rest of Staten Island. In Figure 1 we detail the properties sold during our examined period and identify those which are considered treated (orange) by the landfill's closure, those within one mile of the landfill border, and those we consider to be control observations, greater than one mile but less than two from the border.

Figure 1: Maps of Staten Island

(a)



(b)

Note: Figure 1a outlines Staten Island, New York and some of its various landmarks including the area of Fresh Kills. In Figure we have plotted the individual properties appearing in the ZTRAX data set over the examined period. We have shaded those in the treatment area (≤ 1 mile) in Orange and those in the control group ($1 \leq 2$ miles) in blue. Staten Island properties excluded in the main analysis are shaded in gray.

Though Staten Island had undergone significant development throughout the second half of the 20th century, Fresh Kills had gained a position of prominence in the Island's physical landscape and notoriety. Great Kills Landfill, the predecessor of Fresh Kills, located on the eastern coast of Staten Island, was scheduled to stop receiving garbage by 1948. In search of an effective solution to the city's growing garbage concern, the City of New York looked towards another "wasteland" known as Fresh Kills.[15]. In 1947, Robert Moses, commissioner of the NYC Department of Parks, opened Fresh Kills on a pledge that the landfill would be in operation for only *three years*; and, after closure a new expressway would be built on the landfill site (Purnick, 2002). In the first eight months of operations in 1948, Fresh Kills accepted 15% of the city's waste volume, promptly increasing to 28% in year two and stabilized at 33% in year three, which surpasses the 18% and 21% share of waste that Great Kills accepted in 1946 and 1947 respectively. In 1951, operations at Fresh Kills were extended by city officials for an additional 15 to 20 years (DSNY, 1951).[16] In 1967, Sanitation Commissioner Samuel Kearing Jr. warned that Fresh Kills would reach capacity by 1977 (Bird, 1967); however, in 1970, the Department of City Planning advocated for "mounding" of garbage such that capacity could be extended to 1986 (Burks, 1970). By 1989, mounding operations had been implemented and the landfill was projected to grow to a height of 505 feet by 2005 (Severo, 1989).

In the appendix, Figure A.1, we plotted the annual unloaded tons of garbage at Fresh Kills between 1948 and 2001. Tonnage gradually increased until the late 1980's when private garbage carters left the sanitation market, which required the DSNY to increase acceptance of the waste stream. However, this increase was only temporary due to the implementation of waste reduction programs, recycling programs, new avenues of waste disposal, and updated land-filling practices.[17]

During the 1980s and 1990s, public pressure mounted to close the landfill. The conditions around the landfill were so bad that there were even movements for Staten Island to secede from New York City after years of failing to persuade policymakers to shut it down once and for all. In a chapter on these secession efforts, historian Martin Meolsi remarked that, "[Fresh Kills] was too big of an area to be ignored on the relatively small island – its odor, its rats and seagulls, and its leachate and methane were constant reminders of its existence. The uncertainty of New York City's disposal policy in the early 1990s only made Fresh Kills stand out more as a contentious issue" (Melosi (2020), p. 373). In the spring of 1996, local policymakers along with the New York governor, George Pataki, formally announced a plan to phase out and ultimately close the landfill by 2001.[18] Without a definitive plan in place to handle the diverted garbage at the time of the announcement,[19] the City of New York began to rely on exportation of garbage out of the city as landfill operations phased out over the next several years (as we see in the drop-off depicted in Figure A.1 in the appendix). Two of the four mounds, including Main Mound, were capped by 1997, signifying tangibly permanent steps toward closing, beginning its transition to parkland (Allen and Howe (2016)).[20] The City of New York's Department of Sanitation's "Fresh Kills Landfill Post-Closure Monitoring and Maintenance Manual" (2002) had cited April 9, 1997 as the date where financial assurance requirements were based on "closing the largest active portion of the Landfill ever requiring a final cover" and for post-closure care and monitoring to

[15]The 1946 Annual Report of the Department of Sanitation (DSNY) justifies the selection of the to-be Fresh Kills site: "The area to be reclaimed for park purposes consists of 1500 acres and should provide marine unloading disposal facilities for at least 10 years to come" (DSNY, 1946)

[16]Incinerators were considered to be an alternative disposal method during the 1950s and 1960s; however, they fell into disfavor as new emissions standards were passed and made operating such facilities increasingly infeasible (Walsh, 1991).

[17]A recurring concern through the landfill's operation was its impact on public health. In 1998 and again in 2000, the Agency for Toxic Substances and Disease Registry (ATSDR) conducted studies to determine if the landfill indeed had an impact, and the findings of the 1998 Heath Consultation and both the 1998 and 2000 Public Health Assessments did not find sufficient evidence of the landfill's influence (ATSDR, 1998, 2000b,a).

[18]A distinction should be made here about the terminology. Closure or closure construction refers to the process of remediating or preparing the landfill after the end of filling operations. This process may include capping the landfill with impermeable material, filling with sand and other materials to re-contour the land, install landfill gas collection system, lining the site to contain leachate (liquid garbage by-product), and monitoring for environmental concerns. The act of ending operations is a distinctly different action in which the site ceases to receive garbage for filling purposes.

[19]Melosi (2020) notes that "[t]he closure announcement did not, in and of itself, secure the termination of Fresh Kills" and the NY State Assembly swiftly passed legislation in the days following in order to cement this deal to close Fresh Kills once and for all by 2001 (Melosi (2020), p. 423-424.).

[20]Capping efforts were clearly visible to residents of Staten Island by the spring of 1997, but may have been recognized as early as late 1996 with the South mound capping beginning earlier.

commence thereafter (p. 7-1). By March 22, 2001, the last barge load of garbage arrived at the site. The site was reopened briefly in late 2001, shortly after September 11, 2001, as debris and remains from the attacks on the World Trade Center were sorted and buried there.[21] It was not until October 2012, however, that the first park project was completed, which would become the entrance of the North Park.[22]

In our analysis below, we examine four distinct events: 1) the May 1996 surprise announcement to close Fresh Kills once and for all, 2) the 1997 capping of the North Mound completed by April, 3) the final barge of trash received in March 2001, and 4) the completion and opening of the first park project in Fresh Kills, Schmul Park, in October 2012. Though there are numerous potential events to examine related to Fresh Kills' closing, we chose these events to analyze given how they represent a variety of different information shocks with potentially distinct effects. The first is an official announcement with questionable credibility and thus a high degree of uncertainty for housing market participants to price in. In contrast, the second and third are resolving uncertainty to varying degrees via executing the landfill's closing. As discussed above, by the spring of 1997, most of the landfill had been capped, finally reducing the smell and main externalities associated with the landfill for the nearby community, which is why we consider this to be the most distinguishing event for the housing market. By the time the last barge came in 2001, there was only a small remote portion of the landfill yet to be capped, thus allaying a small but potentially very real uncertainty about its closing once and for all. Finally, because the construction of parks and transforming the landscape can take decades (with the initial timeline spanning 30 years), the 2012 park opening signifies a tangible event toward resolving uncertainty about the Fresh Kills park transition.

3 Data

A thorough investigation of the research questions described above requires a somewhat unique set of data. At a minimum, we would need to observe housing market transactions on Staten Island going back decades such that the data covers the time period corresponding to the events at Fresh Kills. More ideally, we would need to observe housing market transactions around other landfills across the country, preferably in multiple states around the same time period, to assess whether national trends or other confounding factors coincide with market dynamics we find on Staten Island. The Zillow Transaction and Assessment Dataset (ZTRAX) dataset contains exactly this kind of data. It contains detailed information about market transactions going back decades (for most states, including New York) as well as a large set of individual property characteristics for sales recorded in local tax assessors' data. The raw data initially contain more than 374 million detailed transaction records across the United States. Specifically, the data include information on each transacted home's sale price, sale date, mortgage information, foreclosure status, and other information commonly disclosed by a local tax assessor's office for each real estate transaction. While our data cover a large portion of the country, some states do not require disclosure of sale prices, so the price data in particular have some gaps in its national

[21] The site is comprised of four sections: Section 1/9 which occupies approximately 401 acres, Section 2/8 at 139 acres, Section 6/7 at 305 acres, and Section 3/4 at 142 acres. By 1993, the two smaller sections of Fresh Kills (3/4 and 2/8) ended filling operations and completed closure construction in 1996 and in 1997, respectively (NYCDPR, 2008). Section 6/7 began closure construction after it ended filling operations on June 18, 1999 (Bellew, 2011). Not long after, the largest section, Section 1/9, ended filling operations on March 22, 2001. In the wake of the World Trade Center attacks on September 11, 2001, Section 1/9 was designated the main receiving grounds for the debris. Approximately 1.3 million tons of debris were placed in Section 1/9 between September 2001 and June 2002.

[22] Schmul Park was a capital project that included renovating a playground, creating new sports facilities and recreational areas in the north portion of Fresh Kills near the Travis Neighborhood. It would serve as the entrance to a 233-acre North Park, as described by a 2012 New York Times article, titled: "Awaiting a 'Trash to Treasure' Moment". For more information on the park's transition timeline, see: "Freshkills Park Timeline".

coverage.[23] The New York portion of the dataset, however, does not have this limitation.

The ZTRAX dataset, however, has other important limitations. First, the dataset does not contain all information from Zillow's website, like Zillow's automated valuation model's estimates of a home's value (Zestimates), but is comprised of only the raw data from their original sources (mainly, tax assessors of local municipalities). Second, the raw data require cleaning and demands careful attention to detail from its users, as documented by Nolte et al. (2024) and discussed in greater detail below. Third, the ZTRAX program had initially given this dataset to researchers and institutions for specific research projects, but Zillow had sunset the program in 2023 and thus the data is no longer available for new projects. While our initial analysis was conducted on ZTRAX data prior to the sunset period, this version incorporates new data purchased by our institution from Black Knight (now ICE) in 2024. This includes transactions and assessment data like the ZTRAX dataset, but also includes additional data (e.g., property information from Multiple Listing Services in Black Knight's Value Range Express national dataset) that can be used for further analysis.

To construct our sample, we first link each transaction to each property's physical characteristics into a single dataset such that each observation contains both transaction-specific information (sale price, sale date, etc..) and time-invariant property information. The time-invariant assessment dataset generally includes a number of characteristics commonly found on Zillow's website or a local tax assessor's website describing a property: the size of the structure on the property (in square feet), lot size (in acres), number of rooms, bedrooms and bathrooms, year built, and various other characteristics. This data also contain detailed information about each property's location (address and latitude-longitude), which we use to measure distance from the landfill site to each property. This allows us to finely group individual homes spatially into "treatment" and "control" groups based on distance to a particular site, which is essential to our analysis. The granularity of this kind of data provides a more accurate measurement of the treatment and control groups than more aggregated data (like county-level or zip code level data), which could mask potential treatment effects by coarsely lumping control properties with the treatment properties (e.g., a zip code nearest to a given landfill may have properties on one side of the geographical area that are reasonably close and others that are not, potentially diluting the effect or generating noise in the estimates). We retain only residential properties as indicated by their land use type.

We scrutinize missing data and extreme values as part of our initial culling of outliers and general cleaning following lessons from Nolte et al. (2024) and others. Some outliers may be distressed sales or non-arm's length transactions (which we omit using variables such as the document type), but others are typos in the source data (e.g., where a municipality records the number of bathrooms as 30 instead of 3), or the property itself is unusual enough that it would not be an appropriate fit for a model (e.g., if the home did, in fact, have 30 bathrooms, it is unlikely that each bathroom is valued in the same way as other, more typical properties). Or, this might signal a misclassification of a property, where a building with 30 bathrooms may actually be a commercial office building or apartment complex. Thus, we cull extreme values for price and home characteristics for our analysis, which is a common practice for recent research using this particular data.[24]

We first remove extreme outlier properties from the sample, like those listed as a structure with less than 50 square feet and a sale price lower than $1,000. We then winsorize price at the 1st and 99th percentile by year and culled homes with square footage (a home's living area) above the 99th percentile and year built below the 1st percentile. Although the Zillow dataset contains a vast number of property characteristics, we primarily rely on the variables that have the most

[23] Because some states do not require mandatory disclosure of the sale price, ZTRAX does not have adequate price data for the following states: Idaho, Indiana, Kansas, Mississippi, Montana, New Mexico, North Dakota, South Dakota, Texas, Utah, and Wyoming.

[24] See Nolte et al. (2024) for a broad discussion of best practices using the Zillow ZTRAX data, which cites some of BEA's prior work using this data (*e.g.,* Gindelsky et al. (2019)). This is a very useful guide to using the Zillow data, so while some of the precise thresholds and cutoffs we use here may differ, we follow many of their suggestions. Our code is available upon request.

coverage. Unfortunately, for the New York market we are examining, the ZTRAX dataset does have limitations on the available hedonic elements for each property. For example, while lot size, square-footage, and the number of stories are available for nearly every property in the New York data, there is little to no information about the number of bedrooms or bathrooms for Staten Island in ZTRAX. On the margin, bedrooms and bathrooms are certainly valuable, and our inability to adjust for these commonly observable characteristics is potentially a limitation. However, later in the paper, we use additional data from Black Knight to examine whether results are sensitive to excluding these property characteristics for landfills more generally.[25] Because the number of bedrooms or bathrooms in a home is often correlated, in many cases highly so, with the square footage of the home, the marginal importance of additional property characteristics tends to diminish with each additional variable and does not substantively alter the treatment coefficient (i.e., the coefficient estimate on the treatment - whether a home is near a landfill). We return to this point in our discussion of the placebo results in the Results section below.

Table 1: Summary Statistics: Staten Island Sample

		25th Percentile	Median	Mean	75th Percentile	Std. Dev.
		Full Sample: 11,740 observations				
	Price	138,500.00	186,000.00	201,254.00	243,500.00	116020.28
	Lot Size (Acres)	0.04	0.07	0.08	0.10	0.07
	Square Footage	1440.00	1894.50	1993.13	2352.00	762.18
	Number of Stories	2.00	2.00	2.15	2.50	0.56
	Age	1.00	10.00	14.03	24.00	14.05
		Treated Homes (< 1 mile): 5,244 observations				
	Price	118,000.00	162,000.00	172,933.70	210,000.00	100657.24
	Lot Size (Acres)	0.03	0.06	0.06	0.08	0.05
Staten Island	Square Footage	1354.00	1690.00	1820.70	2206.50	618.03
	Number of Stories	2.00	2.00	2.33	3.00	0.53
	Age	1.00	8.00	10.70	18.00	9.94
		Control Homes (> 1 mile but < 2 mile): 6,496 observations				
	Price	160,000.00	212,000.00	224,115.90	262,625.00	122391.26
	Lot Size (Acres)	0.06	0.09	0.10	0.11	0.07
	Square Footage	1568.00	2040.00	2132.33	2485.75	835.6
	Number of Stories	2.00	2.00	2.00	2.00	0.53
	Age	1.00	12.00	17.01	28.00	17.41

Note: Summary statistics for Staten Island include houses within two miles of the landfill. Those closest to the landfill (less than 1 mile) are considered treated by the closure while those farther away are considered control observations.

Table 1 shows the summary statistics for the subsets of the ZTRAX data we use for our analysis of Staten Island. Specifically, we tabulate the means, medians, interquartile ranges, and standard deviations for sale price and property characteristics for the main sample of the Staten Island market we study. The summary statistics for the full DiD sample appear at the top of the table, while breaking out treated (homes less than a mile from the landfill) and control (homes greater than a mile but less than 2 miles) groups in the rows below.

We show the same statistics for two counterfactual housing markets near landfills in other states: properties within two miles of Mallard Lake Landfill in Illinois, Puente Hills Landfill in California, and with three miles of large landfills (> 35 acres) in neighboring New Jersey in the Appendix (A.1). Consistent with the literature, the summary statistics show that homes near landfills generally sell for less, where the control group is composed of somewhat larger homes in all samples. This observation motivates the use of such controls in quasi-experimental research

[25] As noted above, Black Knight data contain additional information from Multiple Listing Services (MLS) data, which allowed us to conduct a sensitivity analysis. Unfortunately, this data does not contain additional variables for Staten Island, but it does for most of the rest of the country. We thus conducted an analysis using data from California (which contains the Puente Hills landfill - one of our placebo landfills). As we discuss in more detail later in the paper, supplementing the transactions data with MLS data shows that incorporating additional property characteristics beyond what we control for in our primary analysis does not substantially change the estimates for properties near the Puente Hills landfill.

designs to account for composition differences across properties.[26]

4 Methodology

Prior research on landfills and home prices primarily relies on the cross-sectional variation in home values to estimate how these disamenities impact nearby home valuation, typically using a hedonic model or related methodology.[27] If researchers know the distance from a given home to the landfill and other relevant property characteristics, a hedonic regression can estimate the marginal value of the proximity to a landfill on a home's price by effectively comparing homes with similar (observable) characteristics that sell for different prices further away from the disamenity (Nelson et al., 1992). Researchers can estimate a continuous price gradient or coefficients on discrete variables indicating distance thresholds (e.g., within 1, 2, or 3 miles of a landfill centroid or border) using this approach.

Researchers have employed some variation of the hedonic approach for several decades following Rosen (1974). One pitfall of using cross-sectional variation alone to value (dis)amenity effects is that the estimates might be biased in an important way. If the effect is endogenous or there are important omitted variables (e.g., unobservables like the aesthetic beauty of the home, quality of the structure, and other attributes not readily available in the data), the estimate of the effect of a landfill's proximity on a home price could be biased or overstated. For example, homes near landfills could be both odorous *and* ugly, where we may have a proxy for the former with distance to the landfill but not the latter. Or, perhaps homes nearest to the landfill are more likely to be in disrepair. Indeed, if the size of the bias is large, hedonic estimates relying on a cross-sectional variation alone may grossly misstate the value of a disamenity to a given home.

Research in more recent decades has paired hedonic models with techniques that leverage natural experiments or quasi-natural experiments for causal inference applications (Parmeter and Pope, 2013).[28] In this study, we employ this approach to answer a somewhat different question than much of the prior literature. Rather than pin down a precise magnitude for estimating the marginal value of a landfill's proximity in a cross-sectional sense, we focus on assessing the directional *change* in value following a particular shock. Methodologically, we employ a difference-in-differences (DiD) research design to analyze the impact of multiple events associated with closing Fresh Kills to evaluate how housing markets respond to new information. Later in the paper, we explore a regression discontinuity in time (RDiT) design. While these approaches are not immune from omitted variables bias for a given cross-sectional result, we leverage the distance from the disamenity and the timing of each event for identification, evaluating the change in value that coincides with these shocks. Utilizing the timing of the shock for identification assumes that the omitted variable (e.g., the ugliness of a facade of a home near a landfill) does not also suddenly change at the same time as an exogenous event, side-stepping a key concern in a cross-sectional analysis. Following our main results below, we return to these questions and empirically examine other issues (i.e., omitted variables) and assumptions of these methods (i.e., parallel trends and contemporaneous effects).

[26] In quasi-experimental research designs, even when the controls are likely uncorrelated with the treatment, (Angrist and Pischke, 2009) recommend including controls that "reduce the residual variance, which in turn lowers the standard error of regression estimates" (p.24). This is common practice for papers pairing hedonic methods with quasi-experimental research designs like diff-in-diff.

[27] See Jackson (2001), Farber (1998),and Boyle and Kiel (2001) for a thorough review of the literature prior to the late 1990s, including the classic cross-sectional empirical setup briefly described here.

[28] The movement toward causal inference methods is not at all unique to urban economics, as similar calls (e.g., Greenstone and Gayer (2009)) in environmental economics and other applied micro fields have coincided with a rise in quasi-experimental techniques in recent decades. We emphasize this shift because many of the papers quantifying landfill effects on the housing market preceded this movement. To be clear, we are also not the first to use these methods for analysis of environmental disamenities - we return to this point in the conclusion in a discussion of Superfund site cleanup and how our results may fit into this literature where these methods are commonly employed.

4.1 Difference-in-differences (DiD)

Our primary analysis follows a common functional form that combines a hedonic model with a difference-in-differences method for a pooled cross-section of home transactions:

$$\ln(\text{Price}) = \beta_0 + \beta_1 \text{Treat} + \beta_2 \big(\text{Post} \times \text{Treat}\big) + \sum \beta_k X_i + \alpha_i + \epsilon \tag{1}$$

where ln(Price) is the natural log of a home's transaction price, Post is an indicator for whether the sale took place directly after one of the four events discussed at the end of Section 2 above (i.e., 1. the closure announcement (May 28, 1996); 2. the implementation and completion of capping most of Fresh Kills (April 9, 1997); 3. the receipt of the final barge of trash (March 22, 2001); 4. the completion and opening of the first park, Schmul Park (October 4, 2012)), Treat is an indicator for whether the property was located within one mile of the landfill's border, and $\big(\text{Post} \times \text{Treat}\big)$ is our DiD estimator of interest.[29] Given that property characteristics are not identical across the treatment and control group, we further incorporate controls in our DiD specification, where X_i includes property characteristics such as logged lot size in acres, square footage of the homes living area, number of stories of the property, and the property's age at the time of sale. Because lot size and square footage might have a nonlinear effect on the outcome, we also include quadratic terms of these parameters in the model. Finally, we incorporate a set of fixed effects for zip code and property type (i.e., land use code in the ZTRAX data, which differentiates between types of properties like single family detatched versus attached housing like condos and townhomes). We further incorporate year-by-month indicators that correspond to the month the property sale closed, which is analogous to a two-way fixed effects model (albeit using a pooled-cross section of data, not a balanced panel).[30] Note also that in this specification, we drop the standalone Post term, instead incorporating year-by-month fixed effects to account for time-specific heterogeneity.[31] Our standard errors are clustered by time (year-by-month) and space (zip code) in the default specification, but our findings are generally robust to alternative clustering approaches.

We limit each event sample of the Staten Island data spatially and temporally to improve the plausibility of our counterfactual. For all events, we only include homes within a two mile distance of the Fresh Kills border to maintain a comparable control group. Manhattan, for example, would not be a plausible control group for this housing market. We acknowledge at the outset that this precise distance is somewhat arbitrary. This choice follows one the seminal papers in this literature that found adverse price effects of landfills declined with distance and were negligible beyond 2 miles (Nelson et al., 1992). However, for robustness, we vary this distance threshold later in the paper, finding similar results and a decay of the effect size with

[29] It is important to note that, in the case of real estate prices, there are likely violations of the Stable Unit Treatment Value Assumption (SUTVA) due to the backward looking appraisal process. Appraisals, which are generally required for mortgage financing, are done with comparable properties over smaller but not necessarily geopolitical confined geographies like zip codes or census tracts. This may produce some spatial dependence between treated and control units, especially on the border of the treatment/control boundary. This means that not only will there likely be spillovers in observed market prices for homes in a geographic area, there may also be spatial heterogeneity which is not accounted for by spatial fixed effects. Later in the paper, we alter our boundary for robustness and explore sensitivity to the boundary choice. However, we leave it to future research to examine this issue further by incorporating a spatial weight matrix or neighbor-based analysis to account for these spatial dependence issues. See for example, Cornwall and Sauley (2021). See also Anselin and Arribas-Bel (2013); LeSage and Chih (2018); Cornwall and Parent (2017) for the impact of spatial heterogeneity and spatial fixed effects.

[30] It is important to note that while we are employing a specification analogous to the two-way fixed effects model this is a simple 2×2 difference-in-differences design and does not have heterogeneity in treatment timing like staggered implementation models.

[31] In a prior draft, we tabulated multiple variations this diff-in-diff specification (e.g., including coarser fixed effects), and the main findings were generally robust to a host of common specifications used in the urban economics literature. Also, for brevity, we do not estimate a simultaneous system of price and liquidity (time on market) - see, for example, Turnbull et al. (2019), for an instructive example combining this approach with a diff-in-diff method examining the impact of externalities.

distance.[32] For each event, we keep home transactions that closed within four quarters prior the treatment quarter and four quarters after the treatment quarter (effectively, a 2.25 year window for each event).[33] To analyze the effect of the closure of the Fresh Kills landfill on the liquidity of the housing market, we collapse the pooled cross-sectional dataset into monthly counts of property transactions, estimating the following regression:

$$\text{volume} = \beta_0 + \beta_1 \text{Post} + \beta_2 \text{Treat} + \beta_3 \big(\text{Post} \times \text{Treat}\big) + \epsilon \tag{2}$$

where volume is the number of sales per month, and the DiD parameters are consistent with those described above.

We employ the same DiD models to analyze both the Staten Island sample and a set of counterfactual samples (neighboring New Jersey landfills along with large landfills in California and Illinois). Two key assumptions of DiD are central to our analysis in this paper: 1) both the treatment group and the control group were trending similarly before the announcement (*i.e.,* parallel trends) and 2) some broader, possibly national shock had not contemporaneously occurred at the time of policy shock (i.e., no contemporaneous effects). We thus conduct internal validity tests to determine whether evidence supports these assumptions and therefore causal inferences from these models. We discuss results from these tests in Section 5.

5 Results

5.1 Housing Market Trends on Staten Island – What does the raw data show?

Before we discuss the regression results, it is useful to begin with a look at the data in a more raw form, where much of the story unfolds in a couple visuals. Figure 2a simply plots median (logged) home prices by month over a 20 year period on Staten Island, where the treatment series (homes nearest to the Fresh Kills landfill - within 1 mile of its boundaries) is in orange and the control group (homes further away - 1-2 miles from its boundaries) is represented by the blue series. Figure 2b presents a quarterly series of seasonally adjusted counts of transactions over the same 20 year period. Both panels which zoom in to each of the four events we analyze.

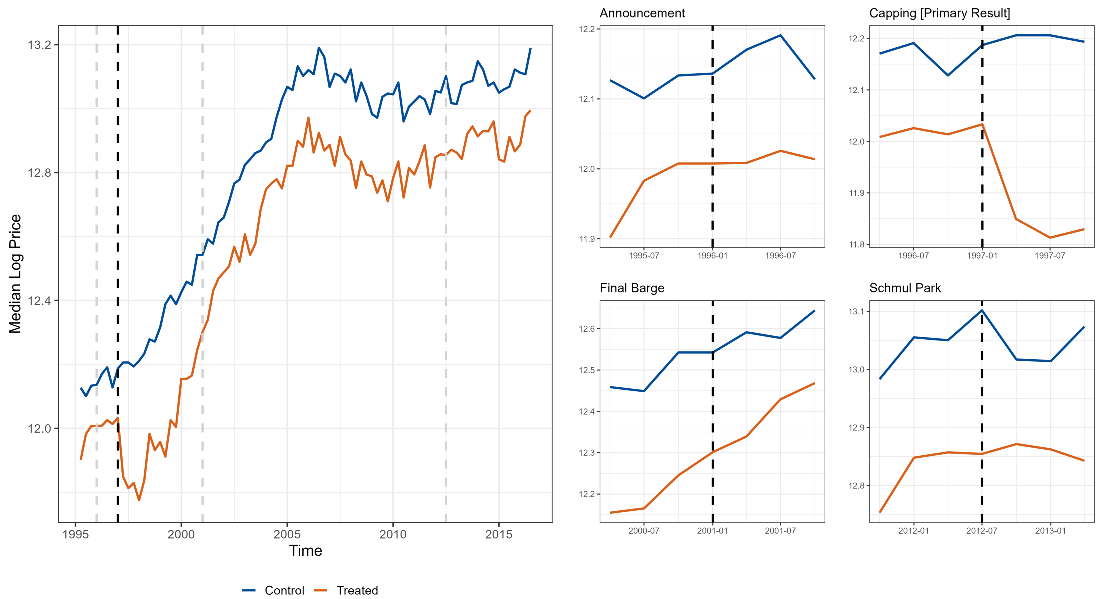
There are a few initial takeaways from the raw price data. First, homes nearest to the landfill clearly and consistently sell for less than those further away over the long time series, consistent with basic intuition, prior literature, and the summary statistics presented in the Data section above. Staten Island also experienced a similar boom in the real estate market that most markets around the U.S. experienced in the late 1990s through the mid-2000s. Second, there is a highly visible dip in prices for the treatment group that coincides with the completion of capping Fresh Kills main mound in 1997. This stands in contrast to little, if any, visible change in 1996 at the time of the closure announcement or the final barge in 2001 (when comparing the treatment and control series). Third, there is little change in the price of the treatment group around the

[32] While many studies use a continuous distance parameter to estimate a price gradient, we use a binary treatment here for ease of interpretation in a diff-in-diff framework, where the treatment is most often (although not exclusively) binary in nature. In untabulated tests, however, we extend this radius out to three miles, finding that extending the control group further does not substantively change the results. We also vary the distance for the treatment in later analysis to show that the results are not sensitive to the choice of 1 mile specifically, and the treatment effect does decay with distance. Other studies like Tanaka and Zabel (2018) use distance buffers from a potential negative externality (nuclear power plants) in their main analysis, but for robustness, they too explore various distances from the externality to show a decaying price gradient.

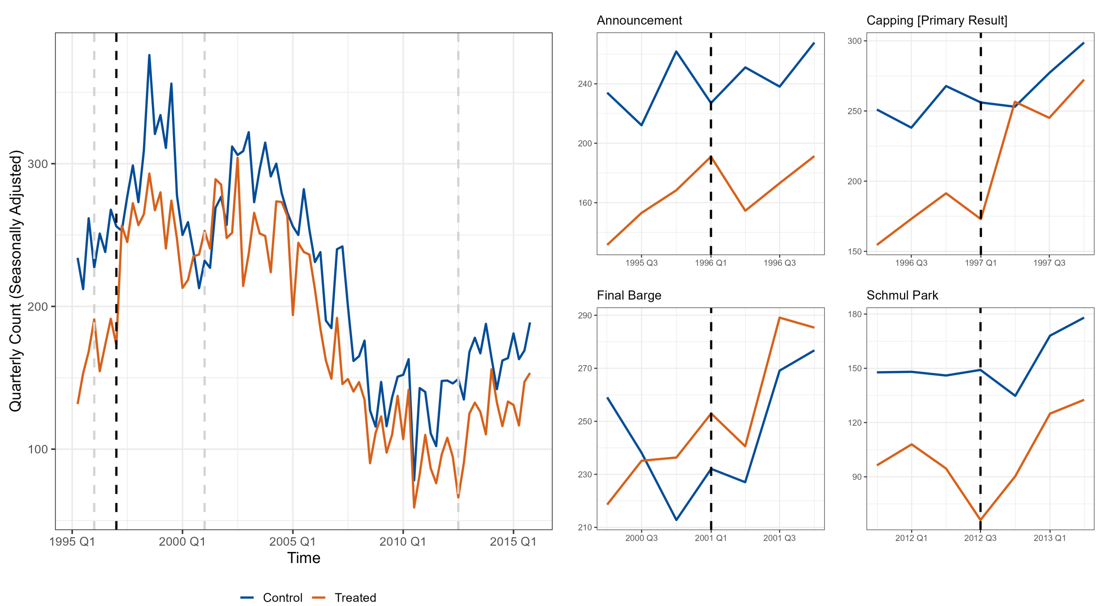
[33] Transactions data on Staten Island for ZTRAX begin in the second quarter of 1995, which truncate the pre-period for this event, but this is not an issue for other events. If we truncate the window similarly for each event, our results are not much changed. This truncation, coupled with the generally lower volume of transactions in the treatment group, explains why the number of observations in the price regression for the surprise announcement is significantly smaller than the other columns in Table 2 below.

timing of the Schmul Park opening, but this stands in contrast to a more general dip in prices for the control group during the 2012 period.

Figure 2: Home Prices and Quantities on Staten Island (1995-2015)



(a) **Note:** Here we plot monthly log median price over the entire time span of data available to us on Staten Island. Each date of interest: announcement, capping, final barge, park ground breaking,and park opening, is identified by the vertical dashed lines. The treatment group is homes that are one mile or less from the border of the Fresh Kills landfill while those in the control group are between one mile and two miles from the border.



(b) **Note:** We plot the seasonally adjusted quarterly sales count over the entire time span of data available to us on Staten Island. Each date of interest: announcement, capping, final barge, park ground breaking,and park opening, is identified by the vertical dashed lines. The treatment group is homes that are one mile or less from the border of the Fresh Kills landfill while those in the control group are between one mile and two miles.

We observe similar phenomena in Panel B, where one sub-market (treated) has persistently lower transaction volume in comparison to the other (control) prior to the capping of Fresh Kills. However, after the capping policy was finally implemented, we see a large increase in the volume of transactions for the treated group (along with a small but noisy bump for the control group). This large rise in transaction volume for homes nearest to the landfill persisted for years, although both groups experienced some mean reversion in the later periods. Taken together, these two figures provide powerful initial evidence that we are observing a shock to liquidity or even a liquidity glut, which put initial downward pressure on prices around the landfill in 1997. The remaining results will provide more formal estimates of these effects, while also attempting to poke holes into this story to ensure the events and findings presented are, in fact, causally related.

5.2 Primary Results

The diff-in-diff results from Tables 2 and 3 comport with the core takeaways from the visual evidence presented above. In both tables, we estimate four diff-in-diff specifications using housing transactions data for windows around the four events discussed above (i.e., the announcement of the Fresh Kills closing in 1996, capping of the main mound in 1997, final barge received in 2001, and the first new park project, Schmul Park, opening in 2012). Table 2 tabulates the coefficients of interest for our analysis of (logged) prices in the Staten Island housing markets. Generally, the (Treatment) coefficients are directionally consistent with the summary statistics, showing that homes nearest to the landfill (within 1 mile of its boundaries) sell for relatively less compared similar homes further away (greater than 1 mile but within 2 miles of the site). The main result, however, is that while there is a noisy, statistically insignificant effect after the 1996 announcement to close the landfill (Treatment $\times$ Post in the first column), the discount changes significantly after the capping was implemented in 1997 (second column). Specifically, prices fell after the capping event by about 7.6 percent for homes near Fresh Kills (relative to similar homes that sold 1-2 miles away).[34] In contrast, the "last barge" closing event in 2001 (third column) had a positive, but imprecisely estimated impact;[35] and, the opening of Schmul Park (fourth column) had a small and statistically significant boost in prices of about 2.2 percent.

To better understand the housing market dynamics, Table 3 helps fill in more of the story, which focuses on the quantity of transactions per month. Generally, the housing market nearest to the landfill (Treatment) experiences a lower volume of sales relative to the neighborhoods 1-2 miles from Fresh Kills in all specifications. On average, the flow of sales in the treated area was about 1/3 lower than the control group initially. After an event (Post), the results show a significant pick-up in market flow for the control group (except the Final Barge event), which is also generally consistent with the volume trends we observe in the raw data over time. Most notably, only after the 1997 capping event, the volume of home transactions for the group nearest to Fresh Kills (Treatment $\times$ Post) increased significantly – by about 14.4 transactions, on average – relative to the control group. This is about 26 percent higher relative to the pre-event treatment baseline (of 55.5 transactions per month), or about 65 percent higher in absolute terms (if one factors in the Post effect). This effect is statistically significant ($p<.05$), while the other columns show that no other event had a significant bump in transaction quantity relative to the control group. Overall, the transaction activity suggests that the 1996 announcement, while important *ex post* for catalyzing the landfill's eventual closings, did not seem to credibly move the market near Fresh Kills in its immediate wake. Instead, transaction activity only spiked after the market observed a tangible, credible action – the landfill's capping in the spring of 1997.

[34] Following Oster (2019) we calculate a bias-adjusted treatment effect with confidence intervals produced via the bootstrap. The mean, adjusted beta -0.0735, is consistent with our main effect in column two of Table 2. Constructing the bootstrap over $1,000$ simulations with replacement provides a 95% confidence interval of $(-0.144, -0.003)$, a result directionally consistent with the main finding. We return to the topic of the role of omitted property characteristics in the next subsection.

[35] The last barge may have been anticipated by the housing market, given the substantial progress on capping to that point and the reduction in trash intake, as plotted in Appendix Figure A.1. To the extent this event was credibly anticipated, the estimates for the final barge effect may be imprecise and/or biased.

Table 2: Price Response to Notable Events - Staten Island Housing Market

Dependent Variable:	Log Sales Price			
Model:	Announcement	Capping	Final Barge	Schmul Park
Treatment	-0.060	-0.046*	-0.081*	-0.043**
	(0.043)	(0.021)	(0.040)	(0.014)
Treatment × Post	-0.044	-0.076*	0.073	0.022**
	(0.064)	(0.035)	(0.044)	(0.005)
Home Characteristics	✓	✓	✓	✓
Zip Code Fixed Effects	✓	✓	✓	✓
Land Use Fixed Effects	✓	✓	✓	✓
Year-Month Fixed Effects	✓	✓	✓	✓
Observations	1,728	7,176	8,334	5,014
Within R^2	0.068	0.107	0.174	0.157
R^2	0.100	0.166	0.315	0.239
Adjusted R^2	0.086	0.159	0.313	0.223
Log-Likelihood	-1,875.1	-6,586.0	-6,069.5	-3,553.1

Note: This table estimates the parameters from our primary diff-in-diff specification (eq(1)) for homes within 2 miles of the Fresh Kills Landfill. All variables and specification details are defined in the Methodology section above. The results from the second column (Capping) indicate a significant fall in price for homes (about 7.6 percent) within one mile of the landfill after it was capped in 1997. The final column shows a small increase (2.2 percent) in price for treated homes after Schmul Park had opened in 2012. The diff-in-diff estimator in the first and third column is not precisely estimated for either event. *Two-way clustered standard errors (Year & Zip Code) in parentheses : ***: 0.01, **: 0.05, *: 0.1*

The statistically insignificant coefficients of interest (Treatment × Post) in the final two columns of Table 3, coupled with the results from the corresponding columns in Table 2, suggest that the events post-capping were perhaps not surprising to the market (and thus attenuating the measured effect toward zero if capitalized prior to the event) or they were only modestly important to buyers and sellers. This is consistent with the narrative history documented by Melosi (2020), where the trash intake into the small portion of the landfill in 2001 was minimal just prior to the final barge arriving. The mitigation of the noxious smell and landfill activity had already been capitalized by the housing market. Schmul Park, on the other hand, may represent a more typical or "normal" demand shock than the capping event. In this case, a new amenity like a park may have had some uncertainty regarding when it would finally be completed; but, once completed, the modest boost in demand for homes nearby the park was not accompanied by some large shock to liquidity like the capping. In this regard, the events of Fresh Kills' closing offer a variety of shocks that stand in sharp contrast with one another, but underscore the capping event as having the most dramatic impact on the housing market. For this reason, the remaining tests will focus on the 1997 capping event in the proceeding sections.

5.3 Parallel Trends, Robustness and Placebo Tests

A key assumption for making causal inferences from a diff-in-diff analysis is that the treatment and control groups trended similarly prior to the timing of the treatment (i.e., parallel trends). In Figure 3, we plot trends in home prices (Panel a) and transaction flows (Panel b) around the capping event, depicting the linear trends with 95 percent confidence intervals through raw monthly averages of these variables. A dashed vertical line corresponds to the spring 1997 capping, which we also use to split the series of trends, allowing a trend line for each group before the policy implementation and a different one for post-implementation. The visual evidence

Table 3: Market Flow (Quantity of Transactions) Response - Staten Island Housing Market

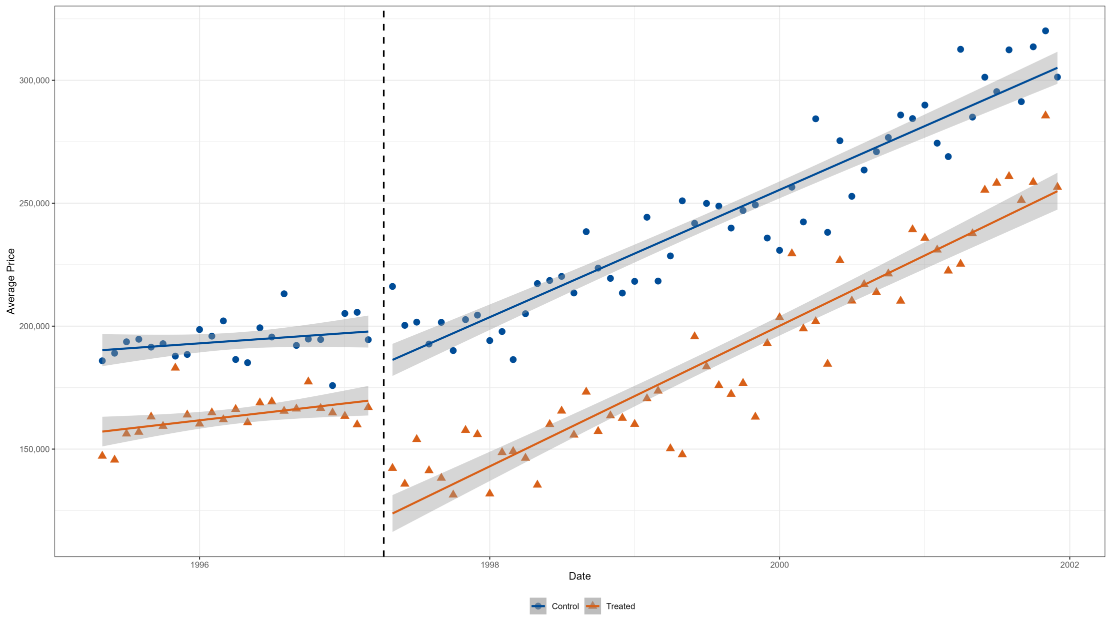
Model:	Announcement	Capping	Final Barge	Schmul Park
Intercept	76.8***	81.2***	90.9***	45.1***
	(3.52)	(2.91)	(3.93)	(1.92)
Treatment	-24.0***	-25.7***	-10.6*	-13.8***
	(4.48)	(3.56)	(5.58)	(2.73)
Post Period	16.2***	21.7***	3.89	9.46***
	(5.83)	(5.56)	(6.10)	(3.35)
Treatment × Post	-4.29	14.4**	4.43	1.28
	(6.64)	(6.60)	(8.31)	(4.07)
Earliest Period	04-1995	01-1995	01-1999	01-2010
Latest Period	12-1997	12-1998	12-2002	12-2014
Period Treated Count	1192	4090	4355	2126
Period Control Count	1726	3221	3951	2908
Squared Correlation	0.652	0.592	0.070	0.372
R^2	0.652	0.592	0.070	0.372
Adjusted R^2	0.625	0.578	0.039	0.355
Log-Likelihood	-158.3	-369.4	-413.1	-448.4

Note: This table estimates the parameters from the diff-in-diff specification (eq(2)) for the flow of home transactions within 2 miles of the Fresh Kills Landfill. All variables and specification details are defined in the Methodology section above. Market Flows are summarized by total sales per month in both treatment (≤ 1 miles from boundary) and control (1 <= 2 miles from boundary). The diff-in-diff estimator in the second column (Capping) shows a large and statistically significant boost in the quantity of home transactions after the capping of Fresh Kills in 1997, with no significant corresponding increase in market flow for the other events. *Clustered robust standard errors in parentheses: *** 0.01, ** 0.05, * 0.1*

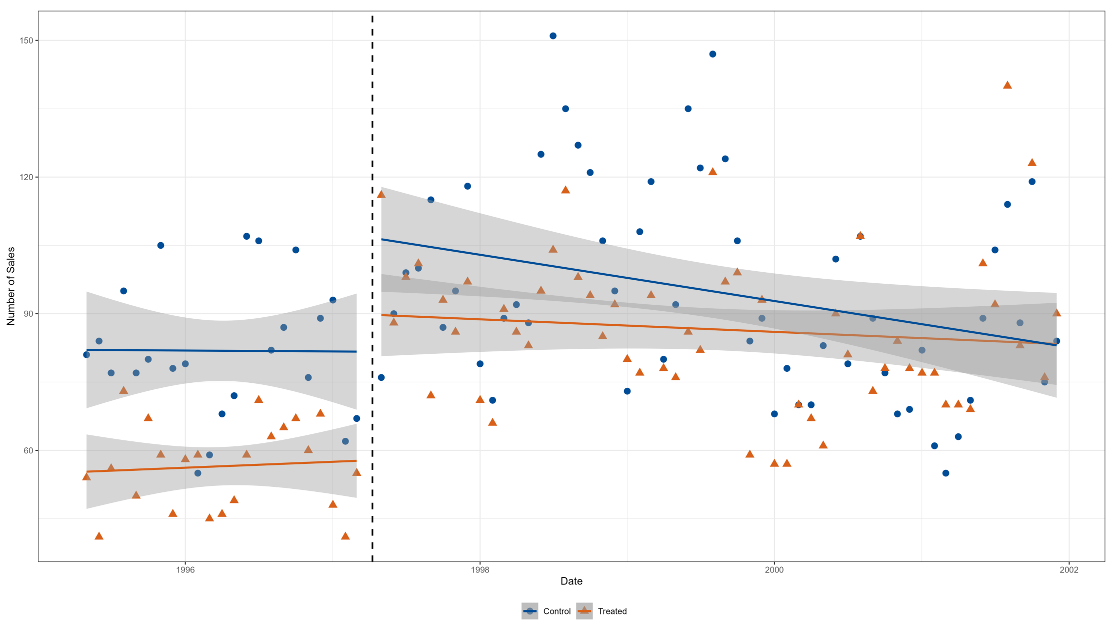
in Figure 3 shows that, while prices and volume were lower in the housing market nearest to Fresh Kills, both markets had nearly identical trends prior to the capping event. Though Figure 3 plots unconditioned prices and quantities, we plot the results from an "event study" in Appendix Figure A.3, which conditions on the covariates from our diff-in-diff model described above. Specifically, we plot the DiD effect by quarter in Figure A.3, showing the (conditioned) effect is not significantly different from zero in the pre-event periods, as we would expect. Taken together, both the unconditioned and conditioned results are consistent with parallel trends, suggesting that the effect coincides with the timing of the capping and a slope change in the trend prior to the policy implementation is unlikely to be a confounder.

A second key assumption of our diff-in-diff analysis is that there are no plausible confounding effects that explain the chain of causality. The main finding presumes the capping's completion caused the price and quantity dynamics we observe. Indeed, one possibility is that there was some spurious regional or national confounding factor that had an impact on housing markets near landfills in the US during the same period. For example, perhaps this could be a seasonal effect or it was an uncharacteristically hot season in the region (or in the US more generally) that coincided with the spring 1997 housing market, making the hot garbage smell particularly potent for all landfills in the region, causing the fall in prices nearby. Alternatively, maybe there was some change to national law or regulations regarding landfills at the same time, confounding this timing as an alternative explanation. In either case, a spurious national or regional effect should affect landfills in the neighboring state, New Jersey, along with the landfills' surrounding

Figure 3: Trends in Home Prices and Quantities around the 1997 Capping of the Main Mound



(a)



(b)

Note: This figure plots home prices and quantities of transactions surrounding capping event in April of 1997. Panel (a) shows average monthly prices for treated and control units, while Panel (b) depicts market flow as measured by number of sales in a given month. Linear trends are fitted with 95 percent confidence intervals for each series, before and after the capping event.

housing markets.[36] Or, if there was a national trend near other extremely large landfills, this confounding factor should have also affected large landfills outside other major US cities, like Mallard Lake (outside Chicago, IL) and Puente Hills (outside Los Angeles, CA) landfills.

To test this possibility, we conduct a similar set of analysis as we had for Fresh Kills, but for placebo housing markets near extremely large landfills in California and Illinois and for 108 of the largest landfills in the state of New Jersey, each of which are greater than thirty-five acres in size, respectively. We similarly designate "treated" properties as those within one mile of a placebo landfill, using the default specification from Table 2 for Panel A of Table 4. While there are notable differences among these markets, the most important result for the purposes of our analysis is that the DiD estimator (Treatment × Post) is not statistically different from zero in each specification in Table 4. If anything, there is a noisy positive coefficient for the DiD estimator in the Puente Hills specification (opposite of the Fresh Kills capping result), but we see no clear pattern from the raw data that this is robust.

As a brief aside, in Panel B, we replicate the Puente Hills results with MLS data from Black Knight, tabulating three specifications: 1) without home characteristics, 2) with the same (limited) characteristics as ZTRAX, and 3) with additional characteristics as controls.[37] The main takeaway from Panel B of Table 4 is that when we incorporate additional variables such as bedrooms, bathrooms, structure condition, presence of a pool or garage, and provision of water/sewer services (col. 3), the coefficient on the price discount associated with being nearby a landfill is little changed from the limited characteristics specification (col. 2). In absence of this data for Staten Island, which, to be sure, would be the more ideal, Panel B of Table 4 provides some evidence of a relatively small marginal contribution of additional property characteristics for analysis of markets around large landfills.[38]

Table 4 also shows that neither Mallard Lake nor New Jersey housing markets near landfills show a significant price effect over the 1997 event period either. Overall, there appears to be no significant evidence of a contemporaneous "general landfill shock" or seasonal effect that impacted housing markets in the same way and at the same time of the Fresh Kills closing announcement. That is, by ruling out plausibly confounding factors, the placebo results provide support that the main price effect is properly identified and distinct from some national or regional shock.

As an additional robustness test, we also consider whether our selection of one mile as a threshold for designating the treatment is arbitrary. Statistically, whenever there is an arbitrary threshold chosen, it is possible that the researchers simply selected (accidentally or not) the one treatment buffer out of many plausible options that provided a spurious relationship with the announcement and outcomes of interest. While unlikely, given all the evidence thus far, it is a natural concern for any study that must choose a threshold for a given spatial treatment. In Appendix Figure A.2, we plot coefficients for the diff-in-diff estimator (Treatment × Post) when we incrementally increase the threshold from as close as 0.5 miles to 3 miles. Consistently, we find a statistically significant negative effect on our DiD estimator for homes near Fresh Kills. The largest effect occurs at shorter distance thresholds and decays with more distance, as we should expect intuitively. A steep decay begins to flatten out after a mile. We have explored using 1-3 miles and further distances from the landfill as control groups, which yielded similar results, but at some point a trade-off emerges that these homes are in a sufficiently different

[36] Conveniently, the state of New Jersey provides a lot of detailed information about current and past landfills via https://njogis-newjersey.opendata.arcgis.com/datasets/2b4eab598df94ffabaa8d92e3e46deb4/explore?location=40.042274%2C-74.754800%2C9.00. And, because it is the closest locale to Staten Island outside of New York, we begin our placebo analysis with this state.

[37] The sample is somewhat smaller, given that we require all three columns to have all of the extended property characteristics.

[38] There may be other important omitted variables not in our data. For example, whether a home has a major renovation is not available for our Staten Island dataset either. Most plausibly, an amenity is likely to encourage renovation activity and investment for homes nearby, generating an upward or positive bias on price. Since the primary price effect in this study is negative, the estimated effect may be somewhat conservative compared to the 'true beta'. Measurement error associated with the property characteristics information being recorded more recently than the sales information may also add noise and attenuate the results toward zero.

Table 4: Placebo Response to Capping of Fresh Kills

Panel A: Placebo Response Using ZTRAX Data

Dependent Variable:	Log Sales Price		
Model:	Puente Hills (CA)	Mallard Lake (IL)	New Jersey
Treated	-0.084***	-0.080	0.024
	(0.014)	(0.072)	(0.020)
Treated × Post	0.092	-0.024	-0.008
	(0.085)	(0.014)	(0.019)
Home Characteristics	✓	✓	✓
Zip Code Fixed Effects	✓	✓	✓
Land Use Fixed Effects	✓	✓	✓
N-Parameters	107	105	399
Observations	8,861	11,320	213,020
R^2	0.314	0.558	0.482
Adjusted R^2	0.306	0.554	0.481
Log-Likelihood	-6194.0	-823.8	-158,779.0

Panel B: Puente Hills - Alternative Data Source

Dependent Variable:	Log Sales Price		
Model:	(1)	(2)	(3)
Treatment	-0.033	-0.073*	-0.066*
	(0.044)	(0.041)	(0.037)
Treatment × Post	0.083	0.069	0.071
	(0.067)	(0.060)	(0.058)
Home Characteristics		✓	✓
Extended Characteristics			✓
Zip Code	✓	✓	✓
Land Use	✓	✓	✓
Year-Month	✓	✓	✓
N-Parameters	96	102	114
Observations	6,545	6,545	6,545
R^2	0.215	0.454	0.470
Within R^2	0.002	0.306	0.327
Adjusted R^2	0.203	0.446	0.461
Log-Likelihood	-4,212.4	-3021.6	-2925.6

Note: This table presents DiD results from housing markets near 'placebo landfils' during the same time window as the Fresh Kills capping event. The bottom panel includes additional specifications for the housing market near Puente Hills using MLS data provided by Black Knight, which includes additional property characteristics not in the ZTRAX dataset. Clustered (Year-Month & Zip Code) standard-errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

location as to not be comparable along that dimension (or may not even be on Staten Island anymore). Nevertheless, the key takeaway from this exercise is that we find a negative effect on home prices across a wide range of plausible thresholds to determine treatment, which intuitively decays with distance as we might expect if the effect is properly identified.

5.4 Alternative Method: RDiT

In the final set of analysis, we re-examine the main capping result using an alternative methodology, sometimes referred to as a regression discontinuity in time (RDiT) or an interrupted time series (ITS). One of the key differences between DiD and RDiT methods is the counterfactual. On one hand, in the DiD method, it is assumed that the treatment group would have evolved over time like the control group; hence, the interpretation of the treatment effect is relative to another set of properties located further away. In the RDiT method, on the other hand, the treatment effect is interpreted relative to its own prior trend (before the shock/discontinuous cutoff) only. That is, RDiT assumes the treatment group, if not for the shock, would have evolved over time following its pre-event trend, where the counterfactual is essentially its prior self. To the extent results from both sets of analysis point in the same direction, it provides additional evidence that our findings are not sensitive to our assumptions about the counterfactual.[39]

Directionally, the RDiT method produces a similar result. Specifically, Appendix Table A.2 shows that when we estimate a dual linear spline RDiT, we observe a significant drop in home prices after the capping event, consistent with both the raw data and the DiD methodology above. In the first column, we tabulate the robust coefficient as the RD parameter of interest, as described in Calonico et al. (2014) and the corresponding documentation for the 'RDrobust' statistical package. The more than doubling of the effect size in the RDiT result is due in part to the modeling assumptions and interpretation of the results (e.g., the dual linear spline allows the trend to vary before and after the event), but is also due to the short window size selected by the optimal bandwidth calculation as per Calonico et al. (2014), which in this circumstance forces a steeper post-trend change and thus inflating the discontinuity size. When we vary the bandwidth in the final two columns of the table, the large bandwidth coefficient estimate is substantially closer to the DiD coefficient and the drop we observe in the raw data.

Based on the recommendations of Imbens and Lemieux (2008),[40] we consider additional specification tests to address a couple potential issues. First, we test whether there are any structural breaks in the covariates that could call into question the identification of the cutoff (Imbens and Lemieux, 2008), or some compositional shift in the sample. In Appendix Table A.2 we estimate whether the types of homes in the treated area had a discrete compositional shift in terms of their observable characteristics, like square footage living area (column 2), lot size (column 3), and age of the structure (column 4). If there were systematic pattern of lower quality homes put on the market at a greater rate after the capping, for example, we would also expect to see a discontinuity in these characteristics (which should be correlated with quality or desirability of the home). In all three specifications, we find no evidence of a statistically significant compositional shift in the types of homes that were put on the market right after the announcement in the treated area nearest to Fresh Kills. To be clear, this does not rule out compositional shifts in unobservable characteristics, but our analysis in Panel B of Table 4 and other work (e.g., Wentland et al. (2023)) shows a high correlation between desirable home characteristics (i.e., the quantity and quality of bedrooms and bathrooms are correlated with square footage).

As an additional robustness test in Appendix Table A.2, we condition our original RD analysis on only homes that existed prior to 1997 in the fourth column of table (as determined by the "year built" variable). In this specification (and untabulated DiD specifications), there is no evidence that the results are driven by a glut of newly built homes onto the market in 1997 that might have coincided with the timing of the Fresh Kills' capping. The direct evidence is more consistent with an effect driven primarily by existing homes sales that might not have been as

[39]See also Cheng and Long (2022) for a very recent paper that employs both difference-in-differences and time RD in their main sets of analyses. They also note that the interpretation of the results using these methods is different which we discuss in greater detail in the context of our empirical setting below.

[40]Specifically, Imbens and Lemieux (2008) suggest that, for robustness, we should be "estimating jumps at points where there should be no jumps. As in the treatment effect literature (*e.g.,* Imbens (2004)), the approach used here consists of testing for zero effect in settings where it is known that the effect should be zero" (p. 632).

liquid in the period prior to the announcement.[41]

6 Conclusion

In this paper, we examine housing market dynamics stemming from the transitioning of a major environmental disamenity, the infamous Fresh Kills landfill on Staten Island. With its closure delayed for decades, the uncertain conditions effectively made homes closest to the landfill substantially less liquid. Initially, we find that the final announcement to close the landfill did not significantly move the housing market (i.e., no significant effect on prices or quantity of transactions), consistent with the historical account that local policymakers lacked sufficient credibility to dispel uncertainty Melosi (2020). It was only until a major phase of implementation closing Fresh Kills (i.e., capping the main mounds) was completed in the spring of 1997 when housing market activity picked up, catalyzing a sharp increase in the quantity of home sales near the landfill. On net, however, we observe that home prices fell near the landfill after its capping. This somewhat counterintuitive result is consistent with supply and demand principles, whereas once an illiquid asset becomes substantially more liquid, the supply response of existing homes can dominate a positive demand shock associated with being a more hospitable locale.

The Staten Island housing market would eventually rebound and follow national trends through the 2000s. Years later, when the housing market near Fresh Kills had more normal signs of liquidity (i.e., transaction activity more similar to the 'control' market further away), we observe an increase in home values (by about 2.2 percent) after Fresh Kills finally made major step toward transitioning to parkland with a new park in 2012 (Schmul Park). In other words, the completion and opening of a new amenity resembled a more typical demand shock when sales activity was more typical. While both of these key events were "positive" shocks for the housing market, the results underscore the idea that the context of what was happening with supply and liquidity is critical for understanding price dynamics.

More generally, the evidence lends support to a basic idea in microeconomics that prices alone do not provide the full picture for evaluating shocks to a given market. The results in this paper emphasize why evaluating both price and quantity are essential for measuring and understanding supply and demand shocks to any market, especially the housing market. From a policy standpoint, the results also highlight how uncertainty about (dis)amenities can disrupt housing liquidity, underlining the value of transparency and clarity of forward guidance by policymakers more generally. Further, the results provide empirical support for the critical role of credibility in the effect of policy announcements on market prices, as the Staten Island experience serves as a striking example of how only tangible actions move markets when policymaker credibility has been lost.

Finally, our findings may speak to a puzzle in a separate but related literature. There is sizable literature seeking to understand another disamenity reversal, Superfund site cleanup, and its impact the housing market.[42] A number of studies (e.g., Messer et al. (2006), Kiel and Williams (2007), Gamper-Rabindran and Timmins (2013), Mastromonaco and Maniloff (2018) and others) have found these sites to have heterogeneous effects on home prices. While the positive effect of cleanup is intuitive, a null effect or even a negative effect on home prices prove more puzzling, leading some like Messer et al. (2006) to conclude that this could be due (at least in part) to "stigma" or psychological factors affecting housing market participants. More recent research by Taylor et al. (2016) has examined this stigma hypothesis more closely using a diff-in-diff design, finding one explanation of this puzzle was that prior literature had not sufficiently considered other commercial/industrial properties as counterfactuals. Though we do not examine

[41] In prior drafts of this paper, we had tabulated additional specifications and tests for our DiD and RDiT analyses, which we removed for length considerations. Overall, our main results are directionally the same when we include alternative sets of controls and as we vary arbitrary specification choices. Prior drafts are available upon request of the authors.

[42] See, for example, Kohlhase (1991), Kiel (1995), Kiel and Zabel (2001), Gamper-Rabindran and Timmins (2013), Mastromonaco (2014), Greenstone and Gallagher (2008), Taylor et al. (2016), and numerous others.

Superfund sites, the results from our paper pose another possibility to complement Taylor et al. (2016) and others seeking to understand peculiarities in environmental externality capitalization. Our findings suggest the possibility that resolving long-term uncertainty associated with lengthy delays can fuel a 'rushing-to-the-exits' supply response, which other studies may overlook if transaction volume goes unexamined.

Both Messer et al. (2006) and, more recently, Mastromonaco and Maniloff (2018) found that the counterintuitive results from Superfund sites were associated with long delays in cleanup, which make this a plausible analogue. Mastromonaco and Maniloff (2018) speculate why the Philadelphia market may have a different response to Superfund sites, noting that: "While it would be nearly impossible to account for all the differences between Philadelphia and the other cities in a way to draw valid inferences, one possibility could be that given the sheer number of Superfund Sites in the Philadelphia MSA compared to the other cities, housing market participants in Philadelphia have become more skeptical of the program and await Deletion from the NPL before viewing the area as remediated" (p. 26). We leave examination of this possibility for Superfund site cleanup and other disamenity reversals for future research, offering evidence from Fresh Kills as motivation for others to determine whether a similar effect may still be relevant for similar settings with credibility issues and uncertainty shrouding market decisions.

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A Online Appendix

Figure A.1: Garbage received between 1948 and 2001

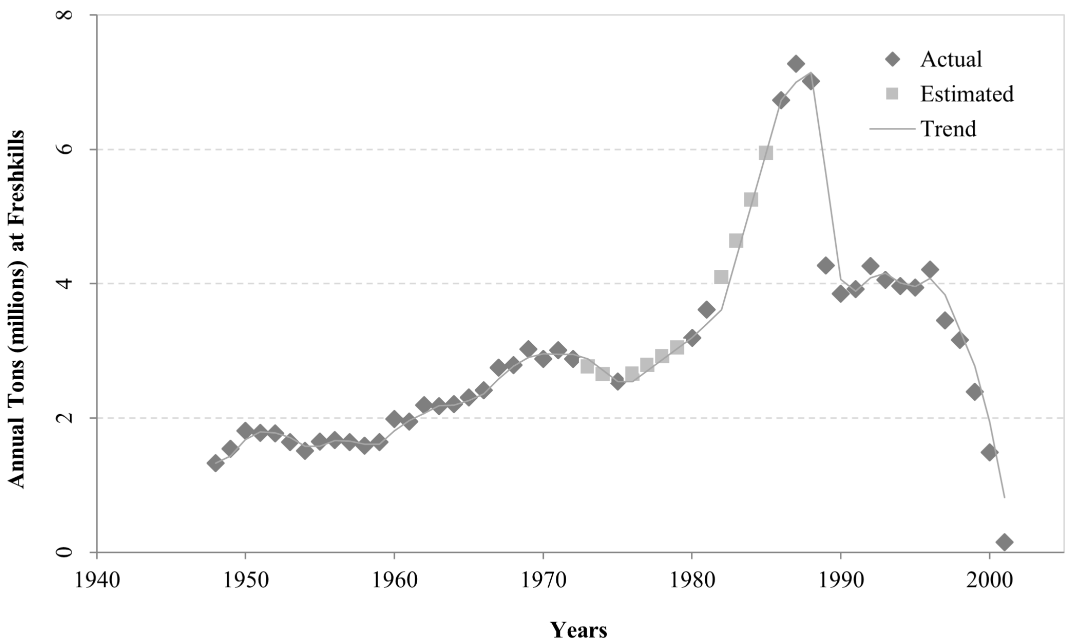
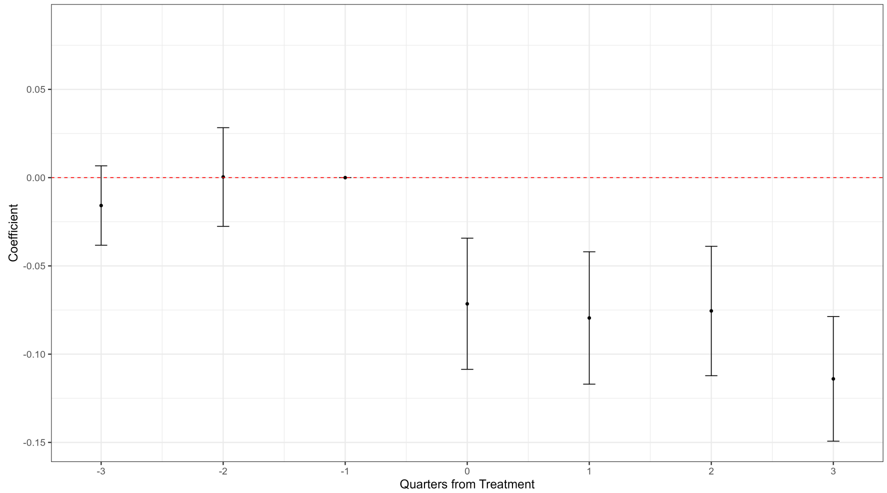


Table A.1: Summary Statistics: Placebo Landfill Markets

		25th Percentile	Median	Mean	75th Percentile	Std. Dev.
		Full Sample: 225,290 observations				
	Price	80,000	122,000	137,633	170,000	96018
	Lot Size (Acres)	0.11	0.17	6.66	0.29	459.15
	Square Footage	1248	1624	1779	2150	764
	Number of Stories	1.00	2.00	1.64	2.00	0.56
	Age	9.00	35.00	37.70	63.00	32.04
		Treated Homes (< 1 mile): 29,369 observations				
New Jersey	Price	74,000	114,900	124,242	162,000	77728
All Landfills > 35 Acres	Lot Size (Acres)	0.11	0.16	0.33	0.26	1.56
	Square Footage	1232	1554	1693	2016	660
	Number of Stories	1.00	2.00	1.65	2.00	0.6
	Age	9.00	36.00	38.43	66.00	32.12
		Control Homes (> 1 mile but < 3 mile): 195,921 observations				
	Price	80,000	123,450	139,640	170,550	98311
	Lot Size (Acres)	0.12	0.18	7.60	0.29	492.35
	Square Footage	1252	1636	1792	2169	778
	Number of Stories	1.00	2.00	1.64	2.00	0.55
	Age	9.00	34.00	37.59	62.00	32.03
		Full Sample: 8,410 observations				
	Price	122,000	149,500	159,766	190,000	62,511
	Lot Size (Acres)	0.12	0.19	0.20	0.22	0.56
	Square Footage	1290	1587	1695	2050	563
	Number of Stories	2.00	2.00	1.84	2.00	0.43
	Age	5.00	10.00	11.79	18.00	18.39
		Treated Homes (< 1 mile): 2,905 observations				
Illinois	Price	105,500	132,500	140,505	166,000	56296
Mallard Lake Landfill	Lot Size (Acres)	0.09	0.18	0.21	0.21	0.89
≈ 230 acres	Square Footage	1225	1464	1591	1828	536
	Number of Stories	2.00	2.00	1.80	2.00	0.46
	Age	5.00	14.00	13.00	21.00	8.74
		Control Homes (> 1 mile but < 2 mile): 5,505 observations				
	Price	131,000	158,500	169,930	201,000	63,236
	Lot Size (Acres)	0.13	0.19	0.20	0.23	0.26
	Square Footage	1328	1640	1751	2106	569
	Number of Stories	2.00	2.00	1.86	2.00	0.42
	Age	5.00	9.00	11.15	17.00	35.00
		Full Sample: 5,118 observations				
	Price	116,500	144,000	160,622	188,569	88,173
	Lot Size (Acres)	0.138	0.170	0.752	0.271	1.94
	Square Footage	1121	1456	1620	1930	735
	Age	19.00	35.00	32.14	43.00	16.41
		Treated Homes (< 1 mile): 1,860 observations				
California	Price	110,000	139,000	146,236	176,000	64,617
Puente Hills Landfill	Lot Size (Acres)	0.15	0.18	1.35	0.28	2.92
≈ 700 acres	Square Footage	1066	1375	1481	1737	563
	Age	24.00	34.00	33.18	42.00	13.20
		Control Homes (> 1 mile but < 2 mile): 3,258 observations				
	Price	120,000	147,000	168,835	200,000	98211
	Lot Size (Acres)	0.14	0.16	0.41	0.26	0.87
	Square Footage	1170	1532	1699	2012	807
	Age	16.00	37.00	31.55	43.00	18.00

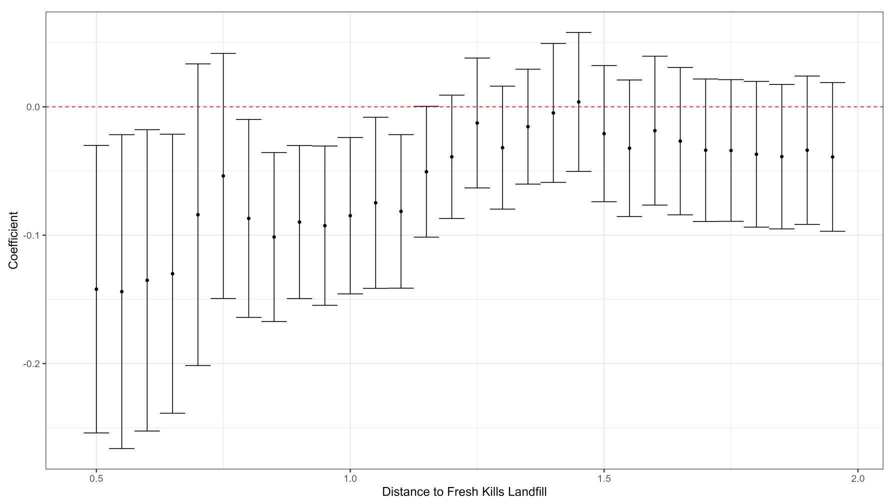
Note: For New Jersey, the sample is all houses within 3 miles of any landfill that is larger than thirty-five acres. While significantly smaller than Fresh Kills, these landfills (108 of them) are the largest in the state. The expanded area in the control group is to account for less densely populated areas. For both Illinois and California we have chosen a single, large landfill outside of major metropolitan areas (Chicago and Los Angeles respectively) and thus have kept the same treatment and control distances used in Staten Island.

Figure A.3: The Impact of Capping: An Event Study Graph



Note: Here we have plotted event study coefficients with time measured in quarters to treatment. The reference period is quarter one of 1997, the last complete untreated quarter, and all coefficients are relative to that period. Treatment occurs early in the second quarter of 1997 (labeled 0) and the price effect is immediately visible. In untabulated results this difference remains persistent, arguably growing slightly, through the fourth quarter of 2000.

Figure A.2: Sensitivity of Distance Threshold Choice



Note: To examine the sensitivity of our results to changes in the threshold we vary the treatment delineation value from 0.50 miles to 2 miles in increments of $1/20^{th}$ of a mile. In all cases the control group is a symmetric distance (*e.g.,* on-half mile treatment zone implies control is between one-half and one miles). These coefficients and 90% confidence intervals are produced using specification from Table 2 and Table 3 respectively. Note that for the price equation we keep the full set of controls in despite the changes in sample size.

Table A.2: RD Results: Staten Island Capping Effect on Price

	Dependent Variable: Log Price					
	Main Results	Square Footage	Acreage	Pre-1997	BW↑	BW ↓
Robust Estimate	-0.196***	0.040	-0.043	-0.224***	-0.111 **	-0.159
	(0.069)	(0.026)	(0.037)	(0.076)	(0.052)	(0.112)
Bandwidth in Weeks	11.714	19.367	23.374	11.542	18.820	4.705
Observations Left	1029	1750	2154	882	1629	401
Observations Right	1283	2264	2777	1069	2163	478
Home Characteristics	✓	✓	✓	✓	✓	✓
Zip Code Fixed Effects	✓	✓	✓	✓	✓	✓

Note: This table provides RD results using a week based running variable. Bandwidths (listed in weeks) are calculated using optimal bandwidth calculations as per Calonico et al. (2014). Columns 3 through 9 present similar regression results for various robustness criteria. Columns 4 through 6 use continuous hedonic element of the property as the dependent variable to test for discontinuity in the covariate. In column 7 we limit the sample to only homes built prior to 1997 to establish that new homes aren't driving the results. Columns 8 and 9 increase and decrease bandwidth respectively. Standard errors clustered by nearest neighbors in parentheses. In untabulated results we use a triangular kernel instead of uniform with little change in the coefficient of interest. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1