Contaminated Capital: Brownfield Cleanups, Entrepreneurship, and Place-Based Redistribution*

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Abstract

We study whether environmental cleanup incentives operate as effective place-based redistribution by transforming contaminated land into productive capital for local entrepreneurs. We link the universe of grant applicants to the Environmental Protection Agency's Brownfields Program with business credit records, firm outcomes, property tax parcels, mortgage activity, and rental market data. Exploiting discontinuities in cleanup grant scores via a stacked dynamic regression discontinuity design, as well as EPA funding expansions in 2018 and 2022, we find that cleanup grants increase local firm entry, survival, employment, and business credit access, with complementary effects on parcel liquidity and mortgage credit that suggest remediation improves the collateral value of formerly contaminated land. Real effects are concentrated within three miles of remediated sites and persist after construction activity ends, suggesting that remediation does more than generate temporary cleanup demand. Treatment effects are strongest in places with more severe pre-existing contamination, industrial decline, and credit frictions, and the gains accrue primarily to local owners and entrepreneurs. Environmental remediation incentives therefore increase aggregate local growth while redistributing investment towards historically capital-starved places through flypaper effects, mitigating the misallocation problem traditionally associated with place-based subsidy programs.

Keywords: brownfields, entrepreneurship, collateral, industrial pollution, place-based policies, stacked dynamic regression discontinuity, flypaper effect

JEL Classifications: G32, H23, J21, Q53, R58

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You cannot put a contaminant in the ground and just think that Mother Nature whips it up and runs it off somewhere else and we never see it again.

Contaminated water is not a problem limited to Flint. Think of New Jersey, where school fountains were found to contain unsafe levels of lead. Or the EPA's 33,000 superfund sites, which are highly-polluted areas that require long-term clean-up operations. The problem is so large that it feels insurmountable.

— *Erin Brockovich*

1 Introduction

Contaminated land is an unusual form of impaired capital. A parcel may be well located and potentially productive, yet remain vacant or underused because environmental liability, uncertain remediation costs, and limited access to financing make redevelopment prohibitively costly. These frictions can be especially important in places already affected by industrial decline and weak labor demand (Bromm, 1998; Burnham-Howard, 2004; Witkin, 2004). The U.S. Environmental Protection Agency (EPA) estimates that roughly 450,000 brownfields exist nationwide, spanning approximately 15 million acres.[1] This area is equivalent to approximately 5.5% of currently habitable U.S. land (U.S. Department of Agriculture, 2024). Returning even a fraction of this land to productive use could therefore affect the local supply of commercial and residential space, including in markets facing acute housing shortages (Patel et al., 2024).

We study whether public spending on environmental remediation can transform this impaired land into productive capital for local firms and entrepreneurs. Our setting is the EPA's Brownfields Program, which provides grants to local governments, nonprofit organizations, land banks, and other eligible entities to clean contaminated properties and prepare them for reuse. Rather than subsidizing a particular incumbent firm, the program alters an underlying local input: the stock of usable land. Cleanup can generate temporary demand for construction and environmental services, but its potentially more important effect is to remove a fixed barrier to investment that affects many prospective users of the site and surrounding neighborhood.

This distinction motivates a different set of questions from those traditionally emphasized in the environmental remediation literature. Existing work on brownfields and more severely contaminated Superfund sites has focused primarily on whether cleanup is capitalized into nearby house prices (Freeman III, 1974; Gamper-Rabindran and Timmins, 2011, 2013; Haninger et al., 2017). We instead ask what happens to the *productive economy* after contaminated land is rehabilitated. Does remediation induce firms to enter and expand? Does it improve access to credit by increasing the collateral value of nearby real estate? And can a relatively small federal grant coordinate substantially larger commitments of public and private capital that would not otherwise flow to the site? These questions connect environmental cleanup directly to entrepreneurship, corporate finance, and the allocation of capital across space.

Our headline findings show that remediation generates real effects beyond housing market capitalization. In our baseline stacked regression discontinuity design, grant authorization raises establishment employment within three miles of a brownfield site by approximately 5–6% over the

[1] This statistic comes from the EPA's "about" webpage: https://www.epa.gov/brownfields/about.

post-award period and increases the new firm entry rate by approximately 1.6 percentage points within the same radius, with entry responding later than incumbent employment. Employment remains elevated for as long as ten years after authorization, well after the temporary increase in construction activity dissipates. Property market effects are more geographically concentrated; house prices within approximately one-half mile rise by about 5% by year seven and approach 10% by year ten, while effects attenuate substantially as the radius expands towards one mile. We also find a striking financing response. Crossing the grant cutoff increases EPA cleanup funding by an implied approximately $292,000, but increases non-EPA leveraged financing by approximately $5.25 million and total recorded funding flowing to the site by approximately $6.57 million. Together, these results suggest that remediation both improves the value of the underlying local asset and helps mobilize a substantially larger pool of capital around it.

The Brownfields Program is particularly well suited to studying the mechanisms behind the real effects of remediation. Federal support expanded substantially following the Better Utilization of Investments Leading to Development (BUILD) Act of 2018 and the Infrastructure Investment and Jobs Act (IIJA), which increased both the number and size of available cleanup grants (Figure 1). The Inflation Reduction Act (IRA) of 2022 further increased incentives for certain investments on qualified brownfield properties.[2] These expansions occurred as the geographic incidence of the program shifted towards lower-income, less densely populated, lower-value housing markets and more rural communities (Table 1). The program therefore provides a useful setting for studying whether environmental remediation can redirect capital towards places that have historically attracted relatively little private investment, against a backdrop of widening regional divergence in income (Gaubert et al., 2021) and prime-age male employment (Austin et al., 2018).

A distinctive institutional feature of the program is that EPA grants generally represent only one layer of the financing required to remediate and redevelop a site. Applicants combine federal funds with state and local grants, land bank resources, nonprofit support, required cost shares, loans, and private developer capital. This capital stack creates the possibility of a "flypaper" effect: a marginal federal dollar may cause additional resources to become attached to the same project rather than merely replacing spending that would have occurred anyway. A longstanding public finance literature documents and seeks to explain this tendency for intergovernmental transfers to "stick where they hit" (Megdal, 1987; Hines and Thaler, 1995; Strumpf, 1998). Whether this pattern represents genuine crowd-in rather than endogenous grant allocation or omitted local spending preferences remains an important identification challenge (Knight, 2002). Sharp causal evidence of crowd-in is comparatively scarce, with the natural experiments created by federal highway funding providing an important exception (Leduc and Wilson, 2017).

We provide new evidence on the flypaper effect by exploiting the EPA's administrative scoring rule. Applications receive numerical scores based on community need, environmental conditions, redevelopment plans, organizational capacity, stakeholder involvement, and anticipated benefits. Within a funding cycle, crossing the relevant score margin sharply raises the probability of receiving a grant. We obtain FOIA-requested application records for the universe of scored cleanup grant applicants between

[2]The IRA introduced a 10 p.p. bonus to the federal Investment Tax Credit and Production Tax Credit for renewable-energy projects located on qualified brownfield sites, raising the maximum effective subsidy for some projects to 40%.

2006 and 2025 and combine them with EPA administrative records on cleanup progress and funding, finely geocoded brownfield locations, establishment data, commercial credit bureau records, real estate and mortgage data, and neighborhood characteristics. Our sample contains 1,656 distinct applicant sites. The resulting panel allows us to follow local firms, property markets, and financing outcomes for up to a decade after a funding decision.

Our main research design exploits time-varying funding cutoffs in a stacked dynamic regression discontinuity design (Biasi et al., 2025; Ruggieri, 2023). The design compares economic outcomes around sites whose applications score above and below the relevant funding margin while retaining the full distribution of scored applications. We explicitly account for the institutional changes in EPA scoring over time, repeat applications, and application histories. Following related work using administrative subsidy scores (Hasan et al., 2021; Hyman et al., 2025), we also subject the assignment rule to a range of robustness exercises. Results are similar when we define cutoffs using raw cohort-specific scores or standardized scores and funding request ranking pools, vary the polynomial in the running variable, alter weighting and treatment-history controls, restrict attention to first-time applicants, and use alternative conventions for establishments exposed to multiple treated and control sites. We additionally show that the results are robust to alternative tie-breaking rules for applications around the funding margin, to defining the running variable using standardized scores rather than the cohort-specific midpoint cutoff, and to conventional local RDD specifications that select MSE-optimal bandwidths around the cutoff. The score density is smooth through the funding threshold, while grant receipt exhibits a large first stage discontinuity.

The financing first stage provides direct evidence for one mechanism behind the employment and entry effects. The magnitude of the leveraged financing response suggests that the EPA award does more than pay for a portion of remediation. It can act as seed capital that helps a project assemble a larger capital stack by resolving a fixed environmental constraint, certifying project viability, or coordinating complementary investments by local governments, land banks, nonprofits, lenders, and developers. In this sense, the score discontinuity provides unusually direct evidence of a site-level flypaper effect.

A second mechanism operates through the value of the underlying real estate. The capitalization effects we estimate are similar in magnitude to those found for earlier Brownfields Program cleanups by Haninger et al. (2017) and somewhat smaller than estimates for more severely contaminated Superfund sites (Gamper-Rabindran and Timmins, 2011, 2013). Superfund remediation is itself much more intensive: historical cleanups average roughly $43 million and can cost billions of dollars (Greenstone and Gallagher, 2008).[3] Unlike this earlier literature, however, capitalization is an intermediate outcome in our analysis rather than the endpoint. By increasing the value and liquidity of previously impaired real estate, remediation can expand collateral capacity and make nearby investment easier to finance. This channel connects the environmental intervention to a large corporate finance literature showing that collateral values (Black and Strahan, 2002; Adelino et al., 2015; Schmalz et al., 2017) and credit access (Brown and Earle, 2017) affect entrepreneurship and firm investment.

These mechanisms also distinguish brownfield cleanup from conventional place-based subsidies. Tax

[3] For instance, the cleanups of Assarco LLC and Chemtura sites cost $3.6 billion and $2.1 billion, respectively. The preliminary assessment and planning phase averages 9.4 years, with the actual cleanup typically requiring an additional decade (GAO, 1997).

credits and hiring subsidies generally change the private return to locating a particular firm or activity in a targeted place. Remediation instead changes the place itself by rehabilitating a durable local asset that can subsequently be used by many firms and property owners. That distinction matters for both incidence and efficiency. Existing evidence from Opportunity Zones shows that geographically targeted incentives can generate substantial investment even when some of that investment may have occurred elsewhere in the absence of the subsidy (Kennedy and Wheeler, 2022; Corinth and Feldman, 2024), and a growing literature studies how geographically targeted tax incentives affect entrepreneurship and new business formation (Denes et al., 2023; Fairlie et al., 2025; Xu, 2026). More broadly, the literature remains divided over the circumstances in which place-based interventions increase aggregate welfare rather than merely reallocate activity across space (Glaeser and Gottlieb, 2008). Brownfield cleanup potentially mitigates this concern because the intervention removes a pre-existing distortion—contamination and its associated liability—rather than simply paying firms to choose one otherwise substitutable location over another. In this sense, remediation is better viewed as a *place-making* policy than as a subsidy to a particular firm.

Our cost per job calculations reinforce this distinction. Using the standardized-score version of the RDD, we estimate an all-in cost of approximately \$5,400 per additional job when we include the public and private capital mobilized around the marginal project. A deliberately conservative linear specification raises the estimate to approximately \$26,100 per job. These estimates are low relative to much of the causally identified place-based subsidy literature surveyed by LaPoint (2026) and are comparable to the lower end of firm subsidy estimates. The comparison is particularly notable because our numerator is an *all-in* measure that incorporates leveraged financing rather than only the direct EPA expenditure. The federal cost per job is mechanically lower. More broadly, the appropriate welfare calculation must recognize that much of the capital stack is not a fiscal cost to the federal government and that remediation creates benefits for workers, firms, property owners, and local tax bases. We therefore complement the CPJ calculation with a framework for measuring the marginal value of public funds, following Finkelstein and Hendren (2020) and Hendren and Sprung-Keyser (2020), and account for local fiscal incidence in the spirit of Busso et al. (2013).

Our findings contribute to three literatures. First, we move the environmental remediation literature beyond its traditional focus on capitalization and health effects. Prior studies establish that contaminated sites depress nearby property values and that cleanup can generate substantial local amenity benefits (Freeman III, 1974; Gamper-Rabindran and Timmins, 2011, 2013; Haninger et al., 2017; Klemick et al., 2020). Brownfields have received less attention than Superfund sites despite being much more numerous, partly because there has not been a centrally compiled, finely geocoded national database of applicant sites. We construct such a database, geocoding 1,656 cleanup grant applicant sites and linking them to parcel records, Microsoft's U.S. Building Footprints, and boundary shape files generated from Phase II and Phase III environmental maps. Brownfields also differ economically from Superfund sites; they are generally less severely contaminated and are rarely remediated directly by the federal government. Their redevelopment therefore provides a particularly useful setting for understanding how public remediation interacts with private capital.

Second, we contribute to research on environmental liability, entrepreneurial finance, and firm dynamics. Existing work shows that exposure to environmental liability changes firms' *ex ante* financing and investment decisions (Akey and Appel, 2021; Bellon, 2025; Chen, 2025). Related work shows that environmental conditions themselves can affect entrepreneurial activity (Rizzi, 2023). We study the other side of that friction: what happens when public policy removes environmental impairment from a local capital asset. Entrepreneurship is particularly relevant because young and privately held firms depend heavily on local credit conditions, collateral, and the availability of productive space. Although entrepreneurship plays an important role in local economic growth (Haltiwanger et al., 2013; Decker et al., 2020), relatively little is known about how environmental remediation affects business formation and financing. Our results complement evidence that geographically targeted policies can alter entrepreneurial activity (Denes et al., 2023; Fairlie et al., 2025; Xu, 2026) and can strengthen clusters by lowering fixed costs of entry and increasing local labor demand (Glaeser et al., 2010). By linking remediation to firm entry, employment, credit outcomes, real estate capitalization, and the financing of the cleanup itself, we provide a more direct view of how environmental liability and collateral interact with local business dynamism. This responds to a broader limitation of the place-based-policy literature, which has relatively little evidence on the responses of privately held firms (LaPoint, 2026).

Third, we contribute to the public finance literature on the incidence of place-based spending. Our results show that evaluating a cleanup grant based only on the face value of the federal award misses an important part of the policy response. Public, private, and nonprofit resources adjust endogenously when a marginal project receives federal support, making the combined capital stack central both to the mechanism and to welfare measurement. The resulting flypaper effect provides a link between the classic literature on intergovernmental transfers and a setting in which public spending may crowd in private investment (Inman, 2009). Transfers across jurisdictions can affect household location and the spatial allocation of economic activity (Boadway and Flatters, 1982; Albouy, 2012), while their benefits may be capitalized into local land and housing values and thereby accrue disproportionately to property owners rather than current residents (Barrow and Rouse, 2004). We examine which workers, firms, property owners, and communities capture the gains from remediation, allowing us to study whether environmental cleanup can redirect investment towards capital-starved locations rather than simply shift activity among already viable places. We complement our main analysis with a stakeholder survey designed to characterize project financing, redevelopment constraints, and the counterfactual role of EPA funding.

The remainder of the paper proceeds as follows. Section 2 offers institutional background on the legal liability and funding sources for site remediation. Section 3 outlines our data sources and how we geocode brownfields sites to link them to firm establishment and individual outcomes. Section 4 discusses our empirical strategies exploiting EPA funding expansions and administrative grant score cutoffs. Section 5 presents our main results on employment and firm entry. Section 6 studies the capitalization of site cleanup into house prices. Section 7 conducts cost-benefit analyses of brownfield grants and provides evidence of flypaper effects. Section 8 concludes.

2 Institutional Background

This section describes the institutional structure of the EPA Brownfields Program, with particular attention to the assessment and cleanup process, the application scoring rule, and the financing of funded projects. These features motivate both the timing and identification of the empirical designs that follow.

2.1 Federal Brownfields Program

Brownfields are real properties whose expansion, redevelopment, or reuse may be complicated by the presence or potential presence of hazardous substances, pollutants, or contaminants (EPA, 2025). Environmental liability, together with uncertainty regarding the extent and cost of contamination, can deter property transactions and redevelopment (Sigman, 2010). Consequently, brownfields can impose substantial costs on surrounding communities by increasing health risks (Litt et al., 2002), contributing to higher crime rates (Brown et al., 2004), and depressing nearby property values (Haninger et al., 2017). The EPA estimates that between 450,000 and 1 million brownfield sites exist nationwide (EPA, 2026a).

In the mid-1990s, the EPA began providing seed funding to local governments for hundreds of two-year pilot projects and developed guidance and technical tools to support the cleanup and redevelopment of brownfield sites. Congress placed the program on a statutory foundation with the Small Business Liability Relief and Brownfields Revitalization Act of 2002. The Act authorized grants for site assessment and cleanup, capitalization of revolving loan funds, and funding for training, research, and technical assistance (U.S. Congress, 2002). The focus of this study, cleanup grants, finances remediation activities at sites owned by the grant recipient. Under the original statute, these grants were capped at $200,000 per site and generally required recipients to provide a 20 percent cost share.

The BUILD Act, enacted in 2018, raised the cleanup grant limit to $500,000 per site and authorized EPA to waive that limit up to $650,000 based on the anticipated level of contamination, the size of the site, or its ownership status (U.S. Congress, 2018). BUILD also authorized multipurpose grants of up to $1 million. A multipurpose grant may support inventory, assessment, planning, and cleanup activities at one or more brownfields within a proposed area; selection depends in part on the recipient's revitalization plan, capacity, and site needs.

More recently, the Infrastructure Investment and Jobs Act (IIJA) substantially expanded federal support for brownfield remediation by appropriating $1.5 billion for fiscal years 2022–2026. IIJA raised the cap on cleanup grants to $5 million and exempted such grants from the statutory cost-sharing requirement. This temporary authority supplements, rather than replaces, the permanent cleanup grant limit of $500,000 and the $650,000 waiver authority.

Figure 1 illustrates the expansion of the program. Panel A shows that total annual cleanup grant funding increased from approximately $20 million to $130 million by 2024. Panel B shows that the average grant size increased from approximately $200,000 to $500,000 in 2019 and reached nearly $2 million by 2024. We exploit the increase in the cleanup grant ceiling introduced by the BUILD Act in a triple-difference design, which we describe in greater detail in Section 4.1.

An important local institution in the redevelopment process is the *land bank*. Land banks are public or nonprofit entities created under state-enabling legislation with powers to acquire, hold, assemble, and transfer vacant or abandoned property. Figure 3 maps cleanup grant applicant sites alongside counties containing active land banks, illustrating the geographic overlap between federal brownfield funding and these local redevelopment institutions. Land banks can play a particularly important role after remediation by taking title to difficult properties, assembling parcels, resolving ownership or disposition problems, and combining EPA support with local government resources and other sources of redevelopment capital. They therefore provide one potential institutional channel through which a relatively small federal cleanup grant can mobilize a much larger capital stack. Land banks are geographically common but are rarely the grantee themselves. Just under half of cleanup grant applicant sites (47.9 percent) lie in a county served by an active land bank, counting the seven statewide land banks as covering their entire state; restricting attention to county, municipal, and regional land banks gives 30.2 percent. Yet, a land bank is the named applicant for only 1.8 percent of applicant sites, and for 1.5 percent of applications in the full population of cleanup grant applications. Counting also the municipalities and counties that charter a land bank raises those shares to 5.4 and 4.8 percent respectively. Neither measure is discontinuous at the funding score cutoff (Table 2), so the institutional channel we describe operates through post-award redevelopment capacity rather than through selection into funding. This role motivates our later analysis of the flypaper effect, in which we ask whether EPA funding causes additional public and private resources to flow towards the marginal site.

2.2 Cleanup Grant Selection and Implementation

Each year, the EPA administers a national cleanup grant competition through a Notice of Funding Opportunity, which specifies the evaluation criteria, the estimated amount of available funding, and the anticipated number of awards. After applications are submitted, the EPA first screens them for threshold eligibility requirements. Eligible applicants include state, tribal, and local governments, as well as certain nonprofit entities; individuals and for-profit organizations are ineligible. Applicants must also own the site by the application deadline, establish an eligible liability status, and have completed a Phase II environmental site assessment or an equivalent site-investigation report demonstrating a basic understanding of the contamination at the site (EPA, 2023).

The Phase II requirement means that a cleanup grant generally occurs after a substantial amount of information about the property has already been assembled. A Phase I Environmental Site Assessment (ESA) reviews historical uses, regulatory records, and current site conditions to identify recognized environmental conditions or other areas of concern. EPA-funded Phase I assessments must satisfy the All Appropriate Inquiries requirements, for which the EPA recognizes the ASTM E1527-21 standard (EPA, 2026b). Where potential contamination is identified, a Phase II ESA collects physical samples of soil, groundwater, and, when relevant, soil vapor or building materials to determine whether releases have occurred and to characterize their location and severity (Bompoti, 2022).

These investigations also link the federal program to state environmental regulation. Brownfield investigation and remediation commonly proceed in coordination with state environmental agencies or voluntary cleanup programs, which can impose investigation, remediation, and closure requirements beyond the federal grant eligibility rules. From the Phase II ESAs, the EPA obtains information on the types of contamination brownfield projects most commonly address. As shown in Figure 4, the most prevalent affected media are subsurface soil or petroleum-related media (68%), soil (67%), and groundwater or petroleum (41%), while building materials are implicated at 25% of sites. On the contaminant dimension, lead is the most frequently recorded pollutant (47%), followed by petroleum (39%), other metals and PAHs (36% each), asbestos (33%), and VOCs (32%). The prevalence of soil, groundwater, petroleum, metals, and asbestos is consistent with the industrial and commercial legacy uses that characterize many brownfields and helps explain why remediation often entails a combination of excavation, capping, groundwater management, and building material abatement.

After Phase II, some projects undertake additional site characterization—sometimes described as a Phase III ESA—to delineate the three-dimensional extent of contamination, migration pathways, and affected media and to support remedial planning (Bompoti, 2022). Phase III terminology and requirements are not uniform nationally, and this additional investigation can occur before or after the cleanup grant application depending on the state and project. The Phase II and Phase III materials are particularly useful for our purposes because their sampling and contamination maps provide much finer information about the affected footprint than the site centroid retained in the EPA's administrative records. We describe how we use these documents to construct brownfield boundary shapefiles in Appendix D.

Applications that satisfy the threshold requirements are forwarded to an evaluation panel, which assigns points based on the narrative criteria specified in the solicitation. These criteria typically assess the quality of the redevelopment strategy, the characteristics and condition of the site, the anticipated environmental and community benefits, risks to sensitive or vulnerable populations, and the applicant's organizational capacity. Funding decisions depend not only on evaluation scores but also on the availability of funds and additional programmatic considerations. These considerations may include whether the site is located in a federally designated floodplain, whether the proposed redevelopment supports renewable-energy development, the distribution of funding across urban and non-urban areas and among the EPA's ten regions, and whether the project is located in an IRS-designated Opportunity Zone.

Both the scoring rubric and the relevant ranking structure vary across funding cycles. For example, the maximum narrative score increased from 100 points in fiscal year 2018 to 180 points in fiscal year 2024 (EPA, 2017, 2023). Following the large increases in maximum grant amounts under the BUILD Act and IIJA, later competitions also divide some applications into separate ranking pools according to the amount of funding requested. For example, the FY2024 competition separately ranks applications requesting up to $500,000, between $500,001 and $2 million, and between $2,000,001 and $5 million. The relevant funding margin can therefore differ not only across fiscal years but also across ranking pools within a fiscal year. Raw evaluation scores are consequently not directly comparable across all cohorts, motivating our alternative specifications that standardize scores and account explicitly for pool-specific cutoffs.

These institutional features motivate a fuzzy regression discontinuity design. Applications are ranked within the relevant funding competition, and crossing the funding margin sharply increases the probability of selection without determining it perfectly because EPA also considers funding availability and other programmatic factors. We exploit this discontinuity in funding probability to study the impact of cleanup grants on economic activity. Section 4.2 describes the stacked dynamic RDD, while Section 4.3 separately estimates the first stage effects of crossing the cutoff on award receipt, cleanup implementation, and other public plus private funding flows.

Selection for funding does not itself constitute a completed grant or remediation event. A selected applicant must negotiate an approved work plan and budget and satisfy all applicable terms and conditions before EPA issues a cooperative agreement. An EPA project officer then remains substantially involved throughout implementation (EPA, 2023). Cleanup can proceed through excavation and disposal, *in situ* treatment, groundwater remediation, or combinations of methods depending on the contaminants and affected media, as discussed in Appendix B. Regulatory closure can additionally depend on state-specific cleanup standards and the intended future use of the property.

The resulting implementation period is heterogeneous and often long relative to the initial grant decision. Among completed cleanups in our sample, the median duration is 1.0 year, the 75th percentile is 2.2 years, and the 90th percentile is 4.0 years. In the broader sample of sites with recorded cleanup start dates, including projects that remain ongoing, elapsed duration has a median of 1.7 years and a 75th percentile of 7.4 years. Approximately one-quarter of projects remain incomplete ten years after cleanup begins (Figure B.2). This distinction between grant authorization and realized remediation is important for interpreting our event studies. Event time in the stacked RDD is defined relative to the application cohort, while the cleanup start and completion dates allow us to assess when the underlying environmental intervention actually occurs. The gradual implementation process provides a natural reason for employment, firm entry, and capitalization effects to emerge with a lag.

2.3 Funding Sources and the Remediation Capital Stack

EPA cleanup grants generally finance only one component of a larger remediation and redevelopment capital stack. Projects can combine federal cleanup and assessment funding with state brownfield programs, local government appropriations, land bank resources, tax increment financing, infrastructure spending, revolving loan funds, philanthropic support, and private developer capital. The precise composition varies across sites and over the redevelopment lifecycle. Public funds may initially finance environmental investigation or remediation that private investors are unwilling to undertake while contamination and liability remain unresolved, while private capital becomes more important once cleanup reduces those risks.

For each site, we aggregate EPA funding across all recorded assessment and cleanup activities and similarly aggregate funding from leveraged, non-EPA sources. Figure 5 plots the resulting funding shares. EPA funding accounts for an average of 63.6% of recorded funding, while leveraged funding accounts for an average of 33.2%.[4] A large mass of sites rely almost entirely on EPA funding, while another sizable group obtains most of its recorded funding from non-EPA sources.

This layered financing structure is economically important because a federal cleanup award can affect more than the resources supplied directly by EPA. By financing a discrete remediation stage, certifying the project's viability, or reducing uncertainty about environmental liability, the award may facilitate complementary commitments by other public and private actors. We test this possibility directly in the first stage analysis in Section 4.3, which separates EPA cleanup funding from non-EPA leveraged financing and

[4]Due to missing values in the EPA database, the distributions are not simple mirror images of each other.

total funding flowing to the site. The distinction subsequently enters the policy analysis in Section 7, where we report both the resources mobilized around the marginal project and the government expenditures relevant for cost per job and MVPF calculations.

3 Linking Brownfield Sites to Neighborhoods and Firms

This section describes the administrative, firm-level, credit, household, and real estate data that we link to EPA brownfield applicants. A central feature of our data construction is the fine geocoding of brownfield sites, which allows us to measure economic outcomes at varying distances from contaminated and remediated land.

Real estate and EPA grant site data: We combine several real estate datasets with newly FOIA-requested data from the EPA on individual cleanup grant scores, site information, and award amounts. EPA data include all applicants from 2006 to 2025 – both those selected and not selected for funding – but desk-rejected applications do not receive a score. Brownfield sites that narrowly missed the score cutoff in a given year serve as our comparison group.[5]

Since most federal and state-enumerated EPA brownfields do not have coordinates or perimeters attached, we adopt several approaches in Appendix D to construct shapefiles. Recovering this information renders possible research designs relying on continuous measures of distance. For instance, it may be that cleanup only increases house prices at distances x miles away from the center of the site due to lingering stigma about its history of contamination. Such a threshold x is an important policy parameter for assessing broader spatial spillovers and multiplier effects of environmental cleanup initiatives.

We merge together a suite of datasets on nationwide real estate transactions and property assessments from Cotality (recently rebranded from CoreLogic). In particular, we use each property's unique parcel identifier to merge information on house prices and buyer and seller names (*Owner Transfers*), property characteristics such as the number of bedrooms and property taxes (*Property Basic*), and any first or second-lien mortgages tied to the deed (*Mortgage*). To identify new real estate development, we apply keyword parsing techniques from Bellon et al. (2024) to individual completed permit filings in Cotality *Building Permits*, separating single-family from multi-family projects.

Cotality *Involuntary Liens* records any environmental liens placed on properties by the government. Such liens can reduce liquidity, access to mortgage financing, and depress prices in property markets, because potential buyers and lenders may be reluctant to assume liability by purchasing the property or attempting to redevelop it without a cleanup effort performed at scale in the surrounding area. By testing whether cleanup helps expunge such liens, we estimate the relative importance of this legal friction to the revitalization of former brownfield neighborhoods.

Since Cotality only covers information that is in the public record, it does not include rental contracts. We therefore use data on rental listings from Altos Rental to isolate the effects of brownfield cleanup on multifamily housing supply and rental prices. We take the last observed listed price within each rental

[5]The EPA began awarding cleanup grants in 2002. However, we were unable to obtain application scores prior to 2006 via FOIA request. Still, we control for past awards received by sites from 2002 to 2005 by hand-collecting from the EPA's website information on these earlier grant cohorts.

listing spell to approximate the contracted monthly rental rate.

We then geocode the EPA grant data to compute distances of each property to the center of a funded cleanup site. Table 1 offers preliminary evidence that the demographic composition of neighborhoods receiving EPA funding towards brownfield cleanup shifted towards less affluent areas after the passage of the IRA. Post-IRA cleanup sites are more rural (i.e., nearly half as densely populated), with 19% lower average household incomes, feature lower assessed values for single-family homes, and cheaper rental units on a price per square foot basis.[6]

Credit bureau data: We use commercial credit bureau data from Equifax to observe business creation. The extract consists of firms with an open credit line located near an EPA brownfield site. Using this dataset has several advantages over the Census datasets traditionally used in the literature on entrepreneurship. First, Census Longitudinal Business Database (LBD) vintages are released with a three to four-year lag. Second, Equifax has information on the business owner, whereas the Census LBD does not. Third, the credit bureau collects data on unincorporated businesses. These businesses, especially if they have no employees, are not systematically collected by the LBD. Finally, the Equifax data record a detailed set of characteristics regarding the financial health of the businesses.

***Dun & Bradstreet* (D&B) establishment data:** We use the *D&B* data to obtain annual employment for establishments linked to their industry classification and parent firm. *D&B* obtains their data from surveys, public records, credit inquiries, and state business registries (see Barnatchez et al., 2017 for an overview). Coverage of the *D&B* data is on par with that of the Census LBD, except that *D&B* undercounts smaller firms, particularly in brownfield zip codes. The lack of coverage motivates our intent to purchase credit bureau data for measuring new business entry responses. However, one advantage to the *D&B* data is that we can finely geocode distance to the site using the establishment's mailing address.[7]

Data Axle Reference Solutions: We track quarterly household income and non-mortgage credit access before and after nearby remediation using data from consumer research firm Data Axle.[8] We link unique households in Data Axle to properties in Cotality using coordinates, 5-digit zip code identifiers and years. In cases where the address corresponds to multiple units, we additionally match on the unit number if this is populated. In the remaining cases, we restrict to owner-occupied addresses where Data Axle only has a single household on file as being in residence.

EPA Cleanups in My Community (CIMC) Database: We supplement the application records with property-level administrative data from the EPA's *Cleanups in My Community* (CIMC) database, which we scrape for the universe of Brownfields properties reported in the system (EPA, 2026c). The underlying Brownfields records originate in EPA's Assessment, Cleanup and Redevelopment Exchange System

[6]We compare zip code codes within a 3-mile radius of an awarded site in 2021 to the same definition of zip code codes nearby an awarded site in 2023. We adopt a 3-mile radius for this test, since effects tend to dissipate at longer distances. However, these general patterns are qualitatively the same if we shrink or expand the radius.

[7]Using the Census Bureau's geocoder API, we match 95% of *D&B* plants to coordinates, and for the remainder with an address we retrieve coordinates from Google Maps API or Data Axle Business.

[8]Data Axle imputes this information using proprietary models feeding in data based on the consumption patterns of households located at an address and those of surrounding addresses, along with income statistics from the U.S. Census Bureau and self-reported survey data. According to Data Axle, the model has also been validated against both IRS and Census income information.

(ACRES), through which grant recipients report property-level assessment, cleanup, and redevelopment activity. CIMC therefore provides information that is not contained in the cleanup grant application files, including the timing and type of environmental assessments, cleanup start and completion dates, acres remediated, state or tribal cleanup-program participation, institutional and engineering controls, and reported redevelopment activity. Of particular importance for our analysis, the records separately identify EPA and leveraged financing associated with the assessment and cleanup stages, including EPA assessment funding, leveraged assessment funding, EPA cleanup funding, leveraged cleanup funding, required cost-share contributions, and total reported project funding. We use these fields to construct the project's remediation capital stack and to distinguish the direct federal investment from complementary public and private resources mobilized around the site. These measures underlie the first stage effects on funding in Table 3 and the all-in cost calculations in Section 7.

Cleanup start/end dates: The EPA's CIMC database contains information on brownfield cleanup start and end dates for funded projects (see Figure B.1 for grant duration statistics by contamination type). However, as noted by Haninger et al. (2017), the true decontamination timeline may differ from that implied by grant start and end dates depending on jurisdiction-specific hurdles, such as more or less onerous permitting processes, labor market conditions, or difficulties in obtaining machines and materials used to decontaminate the land. As a check on how we assign the timing of cleanup events, we supplement CIMC dates with project start and end dates extracted from news article mentions using the LLM tools embedded in ProQuest TDM Studio. We present more statistics on cleanup duration in Appendix B.

4 Empirical Strategies

We introduce two main research designs for isolating the impact brownfield cleanup on local economic growth. One strategy isolates the intensive margin of investment using an increase in the budget allocated towards EPA's Brownfields Program, while the other strategy exploits application cohort-specific grant score cutoffs over all applicant years to isolate the effect of receiving decontamination funding.

4.1 BUILD Act Expansion of EPA Brownfields Program Funding

To study how cleanup funding influences employment, we exploit the passage of the 2018 BUILD Act, which more than doubled the maximum EPA cleanup grant from \$200,000 to \$500,000. We compare establishment-level employment near sites that received grants in 2019 with those near recipients in 2018, before the increase in the funding cap. Since the composition of applicants may vary across years, we introduce a third difference by comparing employment near sites that applied for but did not receive funding in either 2018 or 2019.

We implement the following triple-differences specification using establishments located within three miles of sites that applied for EPA cleanup grants in either 2018 or 2019, with our sample spanning 2014 to 2025 (a symmetric window around the enactment year):

$$\begin{aligned} \ln(\text{Emp}_{i,j,t}) = \beta \cdot \text{Funded}_i \times \text{Applicant2019}_i \times \text{Post}_t + \mu_i + \psi_{j,t} \\ + \text{Funded}_i \times \text{Year FE} + \text{Applicant2019}_i \times \text{Year FE} + \varepsilon_{i,j,t}, \end{aligned} \tag{1}$$

where $\text{Emp}_{i,j,t}$ denotes employment at establishment i in industry j in year t. Applicant2019_i is an indicator equal to one if establishment i is located within three miles of a site that applied for a cleanup grant in 2019, and zero if the nearby site applied in 2018. Funded_i equals one if the nearby site ultimately received cleanup funding. Post_t is a dummy equal to one in 2019 or later.

Our baseline specification includes establishment fixed effects μ_i to absorb time-invariant establishment characteristics and industry (2-digit SIC)-by-year fixed effects $\psi_{j,t}$ to account for time-varying industry-specific shocks. We also include $\text{Funded}_i \times$ Year FE and $\text{Applicant2019}_i \times$ Year FE. The former flexibly controls for time-varying shocks that differentially affect establishments near funded versus unfunded sites, addressing concerns that the EPA may disproportionately fund sites in economically distressed areas. The latter absorbs differential trends between the 2018 and 2019 applicant cohorts unrelated to the BUILD Act. In more stringent specifications, we add state-by-year fixed effects and population-quintile-by-year fixed effects. These controls account for state-wide changes in economic and environmental policies (e.g., differences in COVID-era restrictions or reopening policies) and allow for heterogeneous responses to macroeconomic shocks across areas with different population densities.

The coefficient of interest, β, captures the differential change in employment for establishments near funded relative to unfunded sites among the 2019 applicant cohort, compared with the corresponding funded-unfunded difference for the 2018 applicant cohort following the BUILD Act's increase in the cleanup funding cap. Because sites applying in 2019 became eligible for substantially larger cleanup grants, whereas 2018 applicants were evaluated under the pre-BUILD funding regime, the estimate captures the differential employment effects associated with increased cleanup funding availability.

4.2 Stacked Dynamic Regression Discontinuity Design

While the the triple-differences strategy raises the bar for confounders, an omitted shock could still satisfy all three criteria: correlated with 2019 applicant, funding, and post-period. Second, the design draws on only the 2018 and 2019 application cohorts, so it is unclear whether the estimated effects generalize to the broader brownfields program.[9] To address both concerns, we complement the triple-differences approach with a stacked dynamic regression discontinuity design (RDD) that exploits the EPA's scoring of cleanup grant applications across twenty application cohorts.

Each year, the EPA begins by screening all applications to determine whether they satisfy eligibility criteria. Applications that pass initial screening are then assigned evaluation scores based on a broad set of criteria.[10] These criteria typically include the quality of the redevelopment strategy, the description and condition of the site, the anticipated environmental and community benefits, threats to sensitive or vulnerable populations, and the organizational capacity of the applicant. Proposals are selected for funding based not only on their evaluation scores, but also on the availability of funds and additional programmatic

[9] Further, the immediate post-BUILD period overlaps with pandemic-era stimulus and infrastructure packages which may have played a role in accelerating growth in areas around previously contaminated sites. To the extent that these local outlays are correlated with prior EPA grant receipt, we cannot decompose how much of the job gains we observe after 2019 are directly caused by initial brownfield cleanup investments.

[10] The specific scoring criteria and programmatic considerations may vary across funding cycles.

considerations.[11]

These institutional features connote a fuzzy regression discontinuity design. Specifically, sites receiving scores above the yearly cutoff experience a sharp increase in the probability of receiving funding. Two features of the program complicate a textbook RDD implementation: rejected sites frequently reapply in later rounds (22%), and treatment effects unfold over many years, so comparison observations drawn from the same calendar window may already be partially treated by earlier applications. We address both issues with the cohort-stacked design of Biasi et al. (2025), which builds a separate event-time panel for each application cohort, defines clean comparisons within each cohort, and stacks the cohort-specific panels before estimating a single set of event-time coefficients. This isolates the contrast between barely funded and barely unfunded sites within a cohort and removes the bias that arises when comparison sites have already received treatment in the estimation window. The approach extends the dynamic RDD of Cellini et al. (2010) to allow for heterogeneous effects across cohorts. Such heterogeneous effects could arise due to, for instance, differences in the scope of contamination or effectiveness of available remediation techniques.

For each application cohort c, we build a site-by-event-time panel covering event time $k = t - c \in [-5, +10]$. A site i enters cohort c as treated if its application score exceeds the cohort-specific cutoff score, and as a control if its application score falls below the cutoff and it receives no above-cutoff score during the subsequent decade $[c, c + 10]$ (i.e., the site remains untreated throughout the post period).[12] Cohort panels are stacked, and we estimate:

$$y_{j,c,t} \ = \sum_{k=-5;k\neq 0}^{+10} \left[\beta_k \, D_{j,c,k} + g_p(d_{j,c}, \ \mathbb{1}(d_{j,c} \geq 0))\right] + \sum_{k>0} \mu_k \, M_{j,c,k} + \alpha_{j,c} + \lambda_{s(j),c,t} + \varepsilon_{j,c,t}, \tag{2}$$

where the outcome $y_{j,c,t}$, measures economic activity around site i in calendar year t in cohort c. We consider two outcomes: the log of total establishment employment within a three-mile radius of the site each year; and the new firm entry rate, defined as the number of new firms' plants near a site each year, scaled by the site's average number of establishments in the three years prior to cohort c. Some establishments lie within the radius of both treated and control sites. To address this issue, we consider three alternative assignment rules: (i) assigning establishments to the geographically closest site (our preferred specification), (ii) assigning establishments to all nearby sites when aggregating to the site level, and (iii) assigning establishments to treated sites whenever overlap occurs. We also estimate Poisson regressions to account for the bias related to taking logs of "count" variables highlighted by Cohn et al. (2022). We report robustness of our results to these conventions in Appendix E.

$D_{j,c,k}$ is an indicator equal to one if site j's application in cohort c scored above the funding cutoff and the observation is k years after the application year. $d_{j,c}$ is the running variable, defined as the site's score minus the cohort c's funding cutoff. The function $g_p(\cdot, \cdot)$ is a polynomial of order p in the running variable,

[11] These considerations may include whether the site is located in a federally designated floodplain, whether the redevelopment facilitates renewable energy use, the geographic distribution of funding across urban and non-urban areas and across the EPA's ten regions, and whether the project is located in an IRS-designated Opportunity Zone.

[12] We exclude site-years with both a selected and a non-selected application (7 site-cohorts out of 2,156 site-cohorts). When same-year applications fall on the same side of the cutoff, we average their scores and sum up their funding dollars (154 out of 2,156 site-cohorts).

fully interacted with the above-cutoff indicator, thus allowing the functional forms to vary on either side of the cutoff. Our baseline uses a quadratic polynomial ($p = 2$), with linear and cubic specifications as robustness checks. The terms $M_{j,c,k}$ are indicators for site i having filed *any* application k years earlier.[13]

The coefficients of interest, $\{\beta_k\}$, trace out the dynamic reduced-form effect of scoring above the funding cutoff. Conditional on a flexible polynomial in the running variable, identification comes from comparing sites narrowly above vs. below the cutoff. The inclusion of application history controls further accounts for persistent factors that may influence a site's propensity to seek cleanup funding. Stacking cohort-specific panels avoids contamination from staggered treatment timing and accommodates treatment effect heterogeneity across cohorts.[14]

The specification includes site-by-cohort fixed effects ($\alpha_{j,c}$) and state-by-cohort-by-year fixed effects ($\lambda_{s(j),c,t}$, where $s(j)$ denotes site j's state). The former absorb all time-invariant site characteristics within a cohort panel, such as past-use history and property size. The latter absorb statewide shocks that vary across cohorts—an important control because many states operate their own cleanup and voluntary remediation programs whose scope and generosity change over time.[15] We weight observations by the site's establishment base: the three-year mean establishment count within three miles over the years preceding the site's first application. Because this base predates every application the site files, it is unaffected by treatment, and weighting by it makes the estimated coefficients representative of the average establishment.

The validity of the design rests on two conditions: (1) applicants are unable to precisely manipulate their position relative to the funding cutoff, and (2) crossing the cutoff meaningfully changes the probability of receiving funding (i.e., a strong first stage). We assess these conditions in three ways. First, a density test of the running variable (McCrary, 2008), implemented with the local-polynomial estimator of Cattaneo et al. (2020), fails to reject continuity of the score density at the cutoff with a p-value of 0.89 (Figure 6). This finding is consistent with an absence of sorting around the funding threshold. Second, conditional on flexible controls for the running variable, treatment and control sites are balanced on a wide array of characteristics, including *ex ante* neighborhood poverty and median income, pre-period employment and establishment counts, property size, contamination media, prior land use indicators, Paycheck Protection Program (PPP) funding exposure, and both the presence of a land bank in the site's county and whether the applicant is itself a land bank (Table 2). Third, as discussed in more detail below, the first stage is strong: crossing the cutoff generates a sharp increase in the probability of receiving funding by 41 p.p. with the estimated discontinuity statistically significant at the 1% level (Figure 7).

[13]The corresponding future-application controls $\{M_{j,c,k}\}_{k<0}$ are omitted because future applications could themselves be a downstream consequence of treatment, raising a bad control concern.

[14]Power calculations indicate that the stacked RDD can detect effects of roughly 0.01–0.03 standard deviations in plant-level employment, with the minimum detectable effect rising to approximately 0.05 under the most restrictive sampling assumptions. Because treatment occurs at the site level, adding plant-level observations does not eliminate this constraint; a conventional RDD using a symmetric MSE-optimal bandwidth substantially reduces the number of treated and control sites and raises the minimum detectable effect by roughly 40%. Therefore, we adopt the stacked approach in our main analysis.

[15]Six states offer cleanup grants subject to competitive scoring procedures, such that we can replicate our main regression discontinuity designs using data from the state-level environmental protection agencies. These six states are: CA, CT, MN, OH, VA, and WI.

4.3 First Stage Effects on Grant Selection and Funding

We estimate the first stage effect of scoring above the cutoff on grant selection, cleanup implementation, and funding flowing to the site. This first stage is central to the interpretation of our subsequent estimates because crossing the cutoff changes the probability of receiving and implementing a cleanup grant rather than deterministically assigning treatment. The outcome estimates that follow therefore capture the intent-to-treat, effect of crossing the funding threshold.

To do so, we estimate the following regression discontinuity design using the full universe of scored cleanup applications, 2,157 site-fiscal-year records covering 1,656 distinct sites over the twenty application cohorts from 2006 to 2025.

$$Y_{j,c} \;=\; \tau \cdot \text{Above}_{j,c} \;+\; g_2(d_{j,c}, \text{Above}_{j,c}) \;+\; \mu_c \;+\; \varepsilon_{j,c}, \qquad |d_{j,c}| \leq h_Y, \tag{3}$$

The outcomes include indicators for whether an application is selected for funding and whether cleanup is subsequently recorded as initiated or completed, as well as EPA cleanup funding, non-EPA funding flowing to the site, and total funding flowing to the site. Each outcome is estimated within its own MSE-optimal bandwidth h_Y, selected following Calonico et al. (2014), using a quadratic local polynomial. We estimate linear probability models for the binary outcomes. For the funding outcomes, which contain a large mass at exactly zero and are highly right-skewed, we estimate Poisson regressions.

The running variable $d_{j,c}$ is application j's final review score in cohort c minus the cohort-specific funding cutoff. We define the cutoff as the midpoint between the lowest-scoring selected application and the highest-scoring non-selected application in the same fiscal year, and set Above$j, c = \mathbb{1}(d_{j,c} \geq 0)$. The function $g_2(\cdot,\cdot)$ is quadratic in the running variable and fully interacted with the above-cutoff indicator, allowing the score profile to differ flexibly on either side of the threshold. The cohort fixed effects, μ_c, absorb differences across application cohorts. Consequently, the effect of crossing the cutoff is identified from within-cohort variation, comparing applications with scores just above the cohort-specific threshold to those with scores just below it. Standard errors are clustered by site.

Figure 7 plots the discontinuity in grant selection, and Table 3 reports the full set of estimates. Panel A shows that crossing the cutoff raises the probability of being selected for funding by 41.1 percentage points (s.e. = 0.070), an effect equal to 64 percent of the mean selection rate inside the bandwidth and significant at the 1 percent level. The probability that cleanup is recorded as having started increases by 23.1 percentage points, while the probability that cleanup is recorded as completed increases by 19.4 percentage points. Both estimates are statistically significant at the 5 percent level. These results show that crossing the cutoff leads to economically large increases in both grant selection and subsequent cleanup activity.

Our design is effectively a fuzzy RD, since crossing the cutoff threshold increases the probability of an award (and cleanup) by less than 100%. There were several key changes made to the scoring criteria after the BUILD Act in 2019 and the IIJA in 2023, whereby with the maximum grant amount increases, the EPA sorts applicants into different bins based on the sought funding amount and then sets separate cutoffs for each bin. Our baseline RDD compares sites within the same cohort, so they largely share the same cutoff and score except for when the site is a repeat applicant. Under a standardized version of the running variable, the probability of a site receiving funding jumps by 49.2 p.p. with a t-stat of 7.15. We still satisfy the McCrary test of no statistically significant discontinuity (p-value = 0.55) in the density of scores around the cutoff, bolstering our identification strategy.

Panel B examines the monetary consequences of crossing the cutoff. Scoring above the threshold increases EPA cleanup funding by an amount corresponding to approximately $292,000 when evaluated at the sample mean. This magnitude is consistent with the scale of the program: the statutory per-site cleanup cap was $200,000 before 2018 and $500,000 thereafter (Section 2.1), so the estimated discontinuity corresponds roughly to one cleanup grant.

More importantly, the federal grant appears to be accompanied by substantially larger commitments of funding from other sources. Crossing the cutoff increases non-EPA funding flowing to the site by an implied $5.25 million and total recorded site funding by an implied $6.57 million, with both estimates statistically significant at the 5 percent level. These magnitudes suggest that the direct federal cleanup grant serves as a catalyst for additional investment rather than merely substituting for funding that the site would otherwise have received. In this sense, the relatively modest EPA award appears to generate a considerably larger increase in the resources ultimately directed towards remediation and redevelopment of the marginal site.

5 Main Results on Job Growth and Firm Creation

This section presents our main evidence on the effects of brownfield cleanup funding on local employment and firm creation. We first examine the BUILD Act funding expansion and then turn to the stacked RDD, before exploring heterogeneity across neighborhoods and the spatial incidence of job gains.

5.1 Results from BUILD Act Expansion

Table 4 presents results from estimating equation (1). We progressively add fixed effects across columns (1)–(4). Column (1) estimates the baseline specification with funded$\times$year, 2019 applicant$\times$year, and industry$\times$year fixed effects. Column (2) adds population-bin$\times$year fixed effects, controlling for time-varying factors that may differentially influence employment in more versus less populous areas. Columns (3) and (4) further add EPA region$\times$year and state$\times$year fixed effects, respectively. These fixed effects absorb regional and state-level shocks that could otherwise be correlated with both brownfield redevelopment and employment, including changes in local economic conditions or environmental policy.

Across specifications, the estimated effect of the BUILD Act expansion is positive and statistically significant. The point estimates range from 1.2% to 2.2%, with the most saturated specification implying a 1.4% increase in establishment employment near funded sites exposed to the larger post-BUILD cleanup grants. The stability of the coefficient as we progressively absorb population-, region-, and state-specific shocks is reassuring because the identifying variation comes from a narrow policy transition during which the composition of local labor markets and the macroeconomic environment could also have changed. Moreover, the estimand differs from a simple comparison of funded and unfunded sites. The triple difference asks whether the funded–unfunded employment gap changed more for the 2019 applicant cohort, which became eligible for substantially larger grants, than for the otherwise comparable 2018 cohort evaluated under the old funding cap. The results therefore provide evidence on the *intensive margin* of remediation funding. That is, conditional on participating in the grant program, greater cleanup resources translate into greater subsequent local employment.

Figure 8 examines the timing of this effect using a dynamic version of the most stringent specification in column (4) of Table 4. In Panel A, the coefficients before the BUILD Act are close to zero and statistically insignificant, indicating that employment near funded and unfunded 2019 sites did not exhibit differential trends relative to their 2018 counterparts before the policy change. Following the expansion, the effect emerges gradually, becomes positive by 2020, reaches roughly 2% around 2021, and remains positive through the end of the sample. The delayed response is economically intuitive given that grant authorization does not immediately convert contaminated land into productive space. Recipients must complete work plans, environmental remediation, and often additional site preparation before redevelopment can proceed. The timing is therefore more consistent with a gradual relaxation of a redevelopment constraint than with an immediate response to the announcement of additional federal funding.

Panel B of Figure 8 helps distinguish employment generated directly by remediation from longer-lived changes in economic activity. Construction employment responds during the period in which cleanup and redevelopment are underway, but the construction effect returns towards zero within several years, consistent with the distribution of cleanup durations in Figure B.2. Aggregate employment, by contrast, remains elevated after this temporary construction response dissipates. The figure also reports results separately for tradable and non-tradable industries, providing an additional view of how the local employment response extends beyond workers directly engaged in remediation. Taken together, these patterns suggest that the BUILD Act did more than temporarily increase demand for environmental and construction labor: greater cleanup funding appears to make previously impaired locations more productive for subsequent economic activity.

The BUILD design nevertheless has two important limitations, discussed in Section 4.1. It relies on only the 2018 and 2019 applicant cohorts, and the post-BUILD period overlaps with the pandemic and unusually large subsequent federal spending programs. The triple-difference structure absorbs a broad set of shocks, but a confounder that differentially affected funded 2019 sites could still bias the estimate. We therefore view the BUILD results as complementary evidence and next turn to the stacked RDD described in Section 4.2, which exploits the EPA's grant-scoring discontinuities over the full twenty-year sample.

5.2 Site-Level Stacked RDD Results

We present results from the site-level stacked dynamic RDD in equation (2). This design complements the BUILD analysis in two respects. First, rather than using a statutory increase in the size of available grants, it exploits the discrete change in funding probability at the EPA's application-score cutoff. Second, it uses application cohorts spanning 2006–2025 rather than a single policy transition. The resulting estimates therefore provide evidence on the extensive margin of grant authorization across a much broader set of sites, funding environments, and local economic conditions.

Figure 9 plots the dynamic effects of scoring above the yearly funding cutoff on employment and firm entry within three miles of the applicant site. Panel A shows little evidence of differential employment trends before the application year: the lead coefficients are small and generally statistically indistinguishable from zero. After authorization, employment rises gradually rather than jumping immediately. Averaging across the ten post-application coefficients, our baseline specification implies an employment increase of approximately 6.2%. The dynamic coefficients generally hover around 5% during much of the post-period and remain positive late in the event window. This persistence is important for interpretation. If the estimates reflected only the direct labor required to remove contaminated soil or perform environmental engineering, the effect should decline as cleanup is completed. Instead, the employment response persists over a horizon that extends beyond the typical remediation period shown in Figure B.2, consistent with cleanup facilitating subsequent use and redevelopment of the site.

Panel B of Figure 9 shows a related, but more delayed, response in firm creation. The pooled post-period estimate implies an increase in the new firm entry rate of approximately 1.6 percentage points, with the dynamic estimates reaching roughly 2 percentage points during parts of the post-award period. Entry responds later than employment, which provides a useful distinction between incumbent expansion and entrepreneurial activity. One interpretation is that remediation initially raises demand for incumbent businesses—including construction and other local non-tradable establishments—while the fixed costs associated with opening a new business require more time to adjust (Glaeser et al., 2010). As cleanup progresses, uncertainty about the site's environmental condition falls, surrounding economic conditions improve, and new commercial uses become feasible. Under this interpretation, the delayed entry response is part of the transition from remediation itself to a broader redevelopment equilibrium rather than a contemporaneous response to grant receipt.

Because crossing the score cutoff raises the probability of receiving a grant rather than deterministically assigning funding, the coefficients in Figure 9 are intent-to-treat effects of scoring above the funding threshold. This is important for gauging their magnitude: some above-cutoff sites do not ultimately receive or complete cleanup funding, while some outcomes can respond before a project is fully remediated. The estimates should therefore be interpreted as the reduced-form effect of the EPA's funding allocation rule, not as the treatment-on-the-treated effect of completed cleanup. The strong first stage documented in Section 4.3 nevertheless implies that the score cutoff produces economically meaningful variation in grant receipt, cleanup activity, and funding flowing to the site.

The employment results are robust to a broad set of alternative specifications reported in Appendix E. The pooled post-period employment estimate remains close to the baseline when we add application-history controls, remove the baseline weights, or replace state-by-cohort-by-year fixed effects with cohort-by-year fixed effects. A cubic running variable specification also produces a similar positive estimate. The more restrictive linear specification yields a smaller and statistically insignificant effect, which we retain as a deliberately conservative benchmark in the cost per job analysis of Section 7.1. We additionally obtain similar employment dynamics under alternative rules for establishments located within three miles of both treated and control sites, including, assigning an overlapping establishment to its nearest site, retaining both exposures ("keep both"), or treating the establishment as exposed whenever it is near a treated site ("treat as treated") produces similar post-award employment dynamics. In particular, the nearest-site and keep-both specifications produce nearly identical employment paths, while the treat-as-treated specification provides a useful upper-bound assignment rule. The entry estimates are less precisely estimated across specifications, but remain positive throughout the robustness exercises.

The relative imprecision of the entry results partly reflects limitations of the Dun & Bradstreet data rather than an absence of an economically meaningful response. As discussed in Section 3, D&B undercovers very small and newly created establishments, which are precisely the firms most relevant for measuring entrepreneurship (Neumark et al., 2011; Barnatchez et al., 2017). We therefore treat the D&B entry results as suggestive and use commercial credit bureau microdata elsewhere in the paper to measure business formation and financing more directly. Importantly, the employment and entry results already point to a common pattern: remediation first affects activity at existing firms and subsequently induces new firms to enter. Section 5.3 next examines where these responses are strongest and provides additional evidence on the local conditions under which contaminated land constitutes a binding constraint on business activity.

5.3 Heterogeneous Results by Neighborhood Type

To assess where cleanup grants generate the largest gains, we re-estimate equation (2) after splitting applicant sites by predetermined neighborhood characteristics measured before grant authorization. Figure 10 reports these heterogeneous effects for employment and firm entry. The strongest patterns emerge along baseline socioeconomic conditions. Panels A–D split sites by neighborhood poverty, income, unemployment, and population density, respectively. Cleanup grants have larger effects in higher-poverty, lower-income, and lower-density areas. These results suggest that brownfield remediation is especially productive in places where contaminated land is likely to be a binding constraint on redevelopment and local business expansion.

Panels E and F examine complementary local capacity. In Panel E, employment effects are similar across areas with high and low bank-branch presence, but firm entry effects are larger where local financial intermediation is stronger. This pattern is consistent with cleanup grants and private credit acting as complements: public remediation may make sites viable, while local lenders help finance subsequent business formation. Brown and Earle (2017) show that commercial bank branches play a crucial role in underwriting Small Business Administration (SBA) loans. Panel F shows somewhat larger employment and entry gains in counties with higher debt service burdens, suggesting that EPA grants may be particularly valuable where local governments have less fiscal capacity to fund remediation on their own.

Panel G distinguishes between labor market thickness. Employment gains are concentrated in thick labor markets, where a deeper pool of workers and firms may allow remediated land to translate into job growth through agglomeration and matching channels. By contrast, entry effects are more similar across labor market types. Finally, Panel H splits observations by past industrial use and shows that employment and entry effects materialize more quickly at sites with a history of industrial activity. This pattern is consistent with former industrial sites facing greater contamination and environmental liability concerns, resulting in a more labor-intensive cleanup process. Consequently, the estimated employment effects for industrial vs. non-industrial sites start to converge five years post-award.

6 Real Estate Capitalization Effects

We next examine whether brownfield cleanup grants are capitalized into local real estate values. One potential mechanism underlying the employment and firm entry effects is that environmental remediation raises the value of nearby land by reducing environmental risk, uncertainty about future liability, stigma, and other local disamenities. Higher property values may in turn improve collateral values and relax financing constraints for households and firms.

6.1 Housing Price Data and Empirical Design

Our primary housing outcome is the census tract-level repeat-sales house price index constructed by Contat and Larson (2024). We use this index rather than estimating the baseline capitalization specification directly from individual property transactions in Cotality. Property-level deeds data have the advantage of allowing extremely fine spatial measurement and are closer to the transaction-level approach used by Haninger et al. (2017). In our setting, however, this approach produces a severe thin market problem. Brownfield sites are disproportionately located in relatively rural, lower-value, or otherwise less liquid housing markets, and at the sub-mile distances at which environmental capitalization should be strongest there are frequently too few arms-length transactions to estimate stable event-time effects.

The Contat–Larson index substantially mitigates this problem. Its construction pools information from mortgage and refinancing records rather than relying solely on observed deed transactions, yielding considerably broader coverage of housing market activity in the neighborhoods surrounding brownfield sites. The repeat-sales construction also adjusts for persistent differences in housing quality, so unlike a transaction-level specification we do not need to rely on a rich set of observed housing characteristics to absorb compositional changes in the homes that sell over time.[16]

We adapt the stacked dynamic RDD in equation (2) to the census-tract level. Let j index census tracts and c application cohorts. A tract enters cohort c if its centroid lies within a cumulative radius R of at least one brownfield site with an application in cohort year c. Because the same tract can be exposed to sites applying in different years, a tract can appear in multiple cohort stacks. For tract j in stack c, we estimate

$$\begin{aligned}\log(HPI_{j,c,t}) = &\sum_{\substack{k=-5 \\ k\neq 0}}^{+10} \pi_k D_{j,c,k} + \sum_{k>0} \theta_k M_{j,c,k} + \sum_{\substack{k=-5 \\ k\neq 0}}^{+10} f_k(d_{j,c,k}) \\ &+ \alpha_{j\times c} + \alpha_{s(j)\times c\times t} + \varepsilon_{j,c,t},\end{aligned} \tag{4}$$

where $HPI_{j,c,t}$ is the house price index for tract j in calendar year t. The indicator $D_{j,c,k}$ equals one when a nearby application associated with event time k lies above its cohort-specific funding cutoff. The running

[16] Our baseline analysis uses the tract-level index associated with the working-paper version of Contat and Larson (2021), rather than the public FHFA tract-level Developmental House Price Index. The two series should not be treated as interchangeable in our application. Although the public FHFA series has a longer time span, it has substantially less complete coverage of tracts near brownfield sites. In our stacked sample covering cumulative radii from 0.5 to 3 miles, the public FHFA index is missing approximately 51–69% of tract-year observations, with missingness highest at the narrowest radii, whereas the Contat–Larson working-paper index has essentially complete coverage. Moreover, FHFA missingness is approximately 5–9 percentage points greater for treated than control tract-cohorts, depending on radius. The coverage difference therefore creates not only a loss of statistical power but also scope for differential sample selection.

variable $d_{j,c,k}$ is that application's score relative to its relevant funding cutoff, and $f_k(\cdot)$ is a quadratic function of the running variable that allows the score relationship to differ on either side of the cutoff. As in equation (2), the event window is $k = -5$ through $k = +10$ with $k = 0$ omitted, and the treatment indicators and running variable controls are estimated over this same window.

As in the employment design, repeated applications require us to distinguish the focal treatment from other nearby application activity. $M_{j,c,k}$ therefore indicates whether any nearby brownfield application, above or below the cutoff, occurs at the corresponding event time. We include these application-history controls only for realized prior applications and omit controls based on future application behavior, following the convention used in the employment and firm entry analysis. Conditioning on future applications could absorb behavior that is itself affected by treatment.

A second complication is that a tract can lie near several applicant sites in the same calendar year. Our tie-breaking rule is designed to preserve the economically relevant treatment rather than allow an unrelated nearby application to determine the tract's running variable. Within each relevant treatment category, we retain the qualifying application whose score lies closest to its own funding cutoff. For the pooled baseline treatment indicator, the tract is treated when either relevant within-type search identifies an above-cutoff application; the corresponding closest-to-cutoff score supplies the treatment-side running variable, while the closest qualifying below-cutoff application supplies the control-side running variable when no treated application is present. This convention prevents the treatment status of a tract from switching merely because an application of another type happens to lie marginally closer to its own cutoff.

The specification includes tract-by-cohort fixed effects, $\alpha_{j\times c}$, which absorb time-invariant differences across tract-cohort stacks, and state-by-cohort-by-year fixed effects, $\alpha_{s(j)\times c\times t}$, which absorb state-level housing market shocks specific to each application cohort. Standard errors are clustered by the brownfield site nearest to each tract. Tracts are linked to sites using centroid-to-centroid distance based on Census tract internal points. We estimate the design over a sequence of cumulative radii so that progressively more distant housing markets enter the exposure definition. Because the radii are cumulative, attenuation as R increases has a natural interpretation: if capitalization is concentrated very close to the remediated site, adding more distant and weakly exposed tracts mechanically dilutes the average treatment effect.

This cumulative-radius exercise also connects our RDD to spatial difference-in-differences methods. Characterizing how treatment effects evolve with distance is important for measuring the geographic reach of cleanup benefits, and a declining treatment gradient provides the spatial variation exploited by continuous or ring-based difference-in-differences estimators such as McCulloch (2026). Our score-cutoff RDD addresses the endogenous selection of sites into federal cleanup funding, while the distance gradient characterizes how the consequences of that quasi-exogenous funding shock propagate through space. The two sources of variation are therefore complementary.

6.2 Capitalization Dynamics and Spatial Decay

Figure 11 plots the dynamic effects of cleanup grant authorization on nearby house prices. Across the local radii, we find little evidence of differential price trends before the application year, suggesting that housing markets around sites barely above and below the funding cutoff follow similar trajectories before grant authorization. The post-authorization dynamics reveal both a pronounced delay and substantial spatial decay in capitalization. House prices change little during the first several years after authorization and begin to rise only approximately five to six years later. At the shortest distances, the estimated effect reaches approximately 5% by year 7 and can approach 10% by year 10. Estimates become smaller and less precisely estimated as increasingly distant tracts enter the cumulative treatment radius. The pattern is consistent with a steep underlying spatial gradient in the value of remediation rather than a uniform improvement extending throughout the surrounding housing market.

The magnitude of these effects is also closely aligned with prior estimates of the capitalization benefits of brownfield remediation. Most directly comparable is Haninger et al. (2017), who study an earlier national sample of EPA Brownfields Program cleanups using property-level transaction data and estimate average house price increases of approximately 5.0–11.5%, with alternative matching estimates as large as 15.2%. Our long-run estimate of roughly 10% within 0.5 miles therefore falls squarely within their main range, despite our use of a different identification strategy and a tract-level repeat-sales index. Estimates for more severely contaminated Superfund sites tend to be somewhat larger. Gamper-Rabindran and Timmins (2013), for example, estimate approximately 15% appreciation in median block-level housing values following cleanup, with larger effects for homes that are more likely to be located near the contaminated site. More recent evidence from RCRA hazardous waste cleanups similarly finds positive but highly localized capitalization effects (Cassidy et al., 2022). Taken together, our estimates are economically similar to the existing brownfields evidence and somewhat smaller than estimates for more severely contaminated hazardous waste sites, while reproducing the common finding that capitalization attenuates rapidly with distance.

The delayed response is also consistent with the institutional timing of brownfield remediation. Event time begins at grant authorization, whereas cleanup and redevelopment occur only after subsequent work-plan, permitting, environmental remediation, and construction stages. The first stage estimates in Table 3 show an immediate effect of crossing the cutoff on grant selection but smaller effects on cleanup initiation and completion. Capitalization therefore appears to occur as remediation is implemented, uncertainty about environmental conditions is resolved, and the land becomes available for productive reuse rather than immediately when federal funding is announced.

A key distinction between housing and employment outcomes is the spatial scale over which we expect treatment effects to operate. The direct amenity value of removing a contaminated site may decline relatively quickly with distance because households farther from the site experience smaller changes in perceived environmental risk, stigma, visual blight, and neighborhood quality. Our finding here is thus consistent with a broader urban economics literature finding highly localized capitalization of neighborhood shocks. For example, the house price effects of toxic plant openings and closings in Currie et al. (2015) and nearby foreclosures (e.g., Campbell et al., 2011; Gerardi et al., 2015) are concentrated at short distances from the underlying event. We therefore expect the house price gradient around a rehabilitated brownfield to be substantially steeper than the corresponding gradient in employment.

The same prediction need not hold for local economic activity. Jobs generated during remediation itself should be highly localized: construction, environmental engineering, soil removal, and related activities occur on or immediately adjacent to the brownfield. Indeed, Panel B of Figure 8 shows a short-run increase in construction employment close to treated sites that subsequently dissipates. Longer-run employment gains can operate over a broader geography. Redevelopment may attract firms, increase demand at incumbent businesses, improve collateral values, and generate agglomeration or input-output spillovers throughout the surrounding labor market. Workers need not live immediately adjacent to the site, and firms benefiting from the redevelopment need not occupy the remediated parcel itself. Thus, a sharply decaying house price response alongside employment effects extending one to three miles is not contradictory. Rather, it is consistent with cleanup first removing a highly localized disamenity and subsequently affecting economic activity through a broader set of local market linkages.

These results help distinguish the channels through which brownfield cleanup affects local economies. House price effects are concentrated close to the site, as expected if remediation primarily removes a geographically localized environmental and land use disamenity. Employment effects extend farther and persist after the temporary construction response dissipates, suggesting that the economic consequences subsequently propagate through firms, workers, and credit markets beyond the properties that directly capitalize the amenity improvement. This distinction is consequential for the aggregate cost-benefit calculations in Section 7.

Our estimates remain intent-to-treat effects of scoring above the funding cutoff, since crossing the threshold raises the probability of grant receipt and eventual cleanup rather than deterministically assigning remediation. The tract-level HPI also necessarily averages homes located immediately adjacent to the brownfield with homes farther away within the same census tract. This spatial aggregation can attenuate the magnitude of very local capitalization effects. We therefore view the tract-level index as our preferred baseline because it solves the more consequential thin market and differential coverage problems, while property-level Cotality transactions remain useful for complementary specifications that recover the spatial gradient at finer geographic resolution where transaction density is sufficient.

7 Cost-Benefit Analysis with Flypaper Effects

This section evaluates the policy implications of the estimated employment and capitalization effects. We begin with cost per job calculations that incorporate leveraged financing and then describe how the same estimates can be extended into a broader marginal value of public funds (MVPF) framework.

7.1 Cost Per Job from Brownfield Remediation Funds

A natural starting point for evaluating the efficiency of brownfield grants is the cost per job (CPJ), a widely used partial-equilibrium measure of place-based policy effectiveness. Following LaPoint (2026), the economically relevant denominator is the number of jobs causally attributable to the policy rather than employment growth among recipients that might have occurred in the absence of treatment. In our setting, the fuzzy nature of the grant assignment rule requires scaling the reduced-form employment effect by the first stage effect of crossing the score cutoff on grant receipt.

The numerator requires more care because EPA awards appear to crowd in substantially larger amounts of capital. Panel B of Table 3 shows that crossing the funding cutoff increases EPA cleanup funding by approximately \$292,000 at the sample mean, but raises non-EPA leveraged funding by approximately \$5.25 million and total recorded funding flowing to the site by approximately \$6.57 million. This pattern is consistent with a flypaper effect in which federal cleanup funding helps unlock complementary state, local, nonprofit, and private financing. The broader flypaper literature has long debated whether observed correlations between intergovernmental grants and local spending reflect genuine crowd-in or endogenous grant allocation (Megdal, 1987; Hines and Thaler, 1995; Strumpf, 1998; Knight, 2002). Clean causal evidence of crowd-in is comparatively scarce, with federal highway spending providing an important exception (Leduc and Wilson, 2017; Garin, 2019). The score discontinuity in our setting therefore provides unusually direct evidence that marginal EPA awards are accompanied by additional resources flowing to the same redevelopment sites.

For this reason, we define the numerator conservatively to include not only the EPA cleanup grant itself, but also the additional public and private financing directed towards the site. An EPA-only CPJ would attribute the full employment response to a relatively small federal expenditure even though the first stage estimates indicate that substantially more capital is mobilized alongside it. At the same time, the large ratio of leveraged funding to the initial EPA award helps explain how a modest federal intervention can generate economically meaningful local employment effects. In this sense, brownfield grants operate as seed capital that converts contaminated and underused land into a larger redevelopment investment, consistent with the flypaper mechanism emphasized in the abstract.

We obtain a range of CPJ estimates from alternative definitions of the grant score running variable and specifications of the stacked RDD. In an alternative implementation, we standardize application scores by the maximum attainable score in each funding cycle and account for separate ranking pools associated with funding request categories. Under this definition, crossing the cutoff raises award probability by 49.2 percentage points. The pooled post-award employment effect of 4.8 log points therefore implies an award LATE of $4.8/49.2 = 9.8\%$. Evaluated at mean pre-award employment within three miles of approximately 18,300 workers, this corresponds to roughly 1,787 additional jobs. Combining this estimate with \$9.69 million in total site funding associated with an award yields an all-in CPJ of \$5,422. The estimate is sensitive to the functional form of the running variable; using the more conservative linear specification raises the implied CPJ to \$26,144. Thus, the standardized-score design produces a range of approximately \$5,400–\$26,100 per job.

Our current baseline instead uses the raw application score centered at the cohort-specific cutoff. Table 3 shows that crossing this cutoff raises the probability of grant selection by 41.1 percentage points and increases total recorded funding flowing to the site by approximately \$6.57 million, including \$5.25 million in leveraged non-EPA financing. The baseline stacked RDD in Table E.1 implies a pooled post-period employment effect of 6.2 log points. Applying the same conversion as above gives an award LATE of $6.2/41.1 = 15.1\%$, or approximately 2,760 jobs, and an implied all-in CPJ of about \$2,400. The alternative quadratic and cubic specifications in Table E.1, which produce employment effects between 4.6 and 6.5 log points, imply CPJs of approximately \$2,300–\$3,200. The linear-running variable specification produces a much smaller and statistically insignificant employment estimate of 1.0 log point; treating this estimate as a conservative lower bound on job creation raises the CPJ to approximately \$14,700. Taken together with the standardized-score results, our estimates span approximately $2,300 to $26,100 per job across alternative specifications and definitions of the funding cutoff.[17]

These magnitudes place brownfield remediation towards the lower end of estimates for other place-based policies. LaPoint (2026) surveys causally identified CPJ estimates for place-based policies targeting firms and reports values ranging from roughly $4,200 to $115,000 in real 2023 dollars. Among U.S. programs, estimates include approximately $9,000–$12,000 for the California Competes Tax Credit using an RDD, $17,500 for Opportunity Zones, $25,600 for Empowerment Zones, and $28,000 for state bonus-depreciation programs. Our standardized-score baseline estimate is also close to the $6,829 estimate reported by Hyman et al. (2025) for the California Competes program. Even the upper end of our preferred sensitivity range lies well within the distribution of CPJ estimates for other place-based interventions.

There are important limitations to interpreting these comparisons as welfare rankings. First, our "all-in" numerator is deliberately broader than the fiscal-cost concept used in much of the CPJ literature because it includes leveraged private as well as public financing. Second, the calculations above convert an average proportional employment effect into a level of jobs rather than explicitly valuing the persistence of those jobs over the event window. An alternative accounting exercise constructs job-years by applying each event-time coefficient to the pre-treatment employment base and summing the resulting employment increments over time. Finally, CPJ captures only the employment margin. It does not value the property price capitalization documented in Section 6, increased business formation and credit access, environmental benefits, or fiscal externalities generated by an expanding local tax base. These omissions motivate a broader welfare calculation.

7.2 Marginal Value of Public Funds

The marginal value of public funds (MVPF) provides a natural framework for incorporating these additional margins (Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020). Conceptually, the calculation requires three accounting layers: the resources devoted to remediation and redevelopment, the willingness to pay of the parties benefiting from the policy, and the fiscal externalities generated by changes in economic activity. For site i, we separate the full capital stack into

$$C_i^{AllIn} = C_i^{EPA} + C_i^{NonEPA,Public} + C_i^{Private}, \tag{5}$$

while the government-cost component relevant for the MVPF denominator is

$$C_i^{AllPublic} = C_i^{EPA} + C_i^{NonEPA,Public}. \tag{6}$$

This distinction is important in the brownfields setting. Private capital that is crowded in by an EPA award is part of the economic resources used to redevelop the site and is therefore relevant for an all-in CPJ, but it is not itself a government expenditure. Constructing the MVPF therefore requires classifying the leveraged funding observed in CIMC by source—federal, state, local, quasi-public, philanthropic, or private—and

[17] For comparability across the two versions of the design, these calculations use the same mean pre-award employment within three miles that underlies the standardized-score calculation. The exact CPJ consequently changes slightly with the estimation sample and the employment base used to translate proportional effects into job counts.

identifying the timing of each funding flow.

The second layer measures willingness to pay. Brownfield remediation potentially creates surplus for several distinct groups:

$$WTP_i^{Total} = WTP_i^{Workers} + WTP_i^{Residents} + WTP_i^{Firms} + WTP_i^{PropertyOwners}. \tag{7}$$

Our empirical results provide components of this accounting exercise. Employment effects measure gains accruing through the labor market, while the resident vs. commuter decomposition identifies whether those gains accrue to workers already living near the brownfield or to workers entering from elsewhere. Firm entry, survival, and credit outcomes provide information about gains to entrepreneurs and incumbent businesses. Finally, the capitalization estimates in Figure 11 provide a revealed-preference measure of the value placed on remediation by the local property market. Additional environmental benefits can also enter the numerator when they are not already reflected in these capitalization effects.

The house price estimates provide a particularly transparent starting point for constructing a conservative MVPF benchmark. Let $\widehat{\tau}_b$ denote the estimated capitalization effect for properties in distance bin b, N_{ib} the number of residential properties exposed to site i in that bin, and $\overline{P}_{ib0}$ their average pre-treatment value. The aggregate change in residential property values attributable to remediation can be written as

$$\Delta V_i = \sum_b N_{ib} \overline{P}_{ib0} \left[\exp(\widehat{\tau}_b) - 1 \right]. \tag{8}$$

Using pre-treatment property values avoids mechanically scaling the welfare gain by post-treatment appreciation, while our spatial assignment procedures allow properties exposed to multiple brownfield sites to be counted only once. The strong attenuation of the estimates with distance in Figure 11 is useful here because it allows the capitalization calculation to reflect the geographically localized nature of the benefit rather than applying the treatment effect to all properties in a broad jurisdiction.

The third layer accounts for fiscal externalities. The appropriate denominator is not gross public spending, but net government cost:

$$NGC_i = C_i^{AllPublic} - PV(\Delta TaxRevenue_i) - PV(\Delta TransferSavings_i). \tag{9}$$

The house price results directly imply one such fiscal channel. If m_i denotes the effective property tax rate, adjusted for jurisdiction-specific assessment rules, then the increase in the property tax base generates additional revenues approximately equal to $m_i \Delta V_{it}$ in year t. Discounting these flows gives

$$PV(\Delta T_i^{Property}) = \sum_{t=0}^{T} \frac{m_i \Delta V_{it}}{(1+r)^t}. \tag{10}$$

The employment effects can generate additional income and payroll tax revenue and reduce means-tested transfer payments, while firm entry and growth can generate additional business and property tax receipts. Because cleanup, redevelopment, employment, and capitalization occur with substantial lags, both benefits and fiscal flows need to be placed on a common present-value basis relative to grant authorization.

Combining these components produces an all-government MVPF,

$$MVPF^{AllGov} = \frac{WTP^{Total}}{C^{AllPublic} - PV(\Delta TaxRevenue) - PV(\Delta TransferSavings)}. \tag{11}$$

We can also construct nested versions that make increasingly strong assumptions about which observed outcomes represent incremental social surplus. A conservative capitalization-based benchmark uses only the increase in residential property values,

$$MVPF^{Cap} = \frac{\Delta V^{Residential}}{NGC^{AllGov}}. \tag{12}$$

An intermediate calculation adds labor-market surplus accruing to workers who commute into the affected area, while a broader calculation additionally incorporates gains to workers, firms, and environmental quality.

An important caution in moving from the conservative to broader measures is double counting. House prices can capitalize not only improved environmental quality but also expected future employment opportunities, neighborhood amenities, and redevelopment prospects. The capitalization effect therefore cannot necessarily be added mechanically to separate estimates of each underlying benefit. We consequently view the capitalization-based MVPF as a transparent lower-information benchmark and broader MVPF calculations as sensitivity exercises that require explicit assumptions about which benefits are already reflected in property values. This decomposition also allows the aggregate welfare calculation to be paired with an incidence analysis showing whether the benefits of remediation accrue primarily to incumbent property owners, renters, nearby workers, commuters, new firms, or local governments.

8 Conclusion

We study whether environmental remediation can transform contaminated land from an impaired asset into productive local capital. Exploiting discontinuities in the EPA's Brownfields Program cleanup grant scores, we find that grant authorization raises establishment employment within three miles by roughly 5–6% and increases the new firm entry rate by approximately 1.6 percentage points, with effects persisting well beyond the period of active remediation. housing market effects are more spatially concentrated: house prices within one-half mile rise by approximately 5% by year seven and approach 10% by year ten. These findings move beyond the traditional focus of the brownfields and Superfund literatures on property value capitalization and show that remediation can generate sustained effects on entrepreneurship and local business activity.

The financing response helps explain why relatively modest cleanup grants can produce effects of this magnitude. Crossing the funding cutoff increases EPA cleanup funding by an implied approximately \$292,000, while increasing non-EPA leveraged financing by roughly \$5.25 million and total recorded funding by approximately \$6.57 million. Thus, federal cleanup funding appears to serve as seed capital around which governments, nonprofits, lenders, and private developers assemble a much larger capital stack. Together with the increase in nearby property values, this evidence is consistent with remediation operating through both a flypaper effect and a collateral channel. Cleanup removes environmental liability, increases the value of previously impaired real estate, and can relax financing constraints facing firms and entrepreneurs. These conclusions are robust to alternative definitions of the funding cutoff, tie-breaking rules, polynomial specifications, treatment-history controls, and conventional local RDD bandwidth choices.

These mechanisms distinguish brownfield remediation from a conventional place-based firm subsidy. Rather than paying a particular firm to locate in a targeted area, remediation changes the productive capacity of the place itself by restoring a durable asset that can support many subsequent users. Consistent with this distinction, our implied all-in cost per job ranges from approximately $5,400 in our preferred specification to $26,100 under a conservative linear specification, comparing favorably with many estimates for firm-specific subsidies. More broadly, the results suggest that environmental cleanup can simultaneously address a legacy environmental distortion and redirect private capital towards historically underinvested places. Understanding when public remediation can unlock complementary capital, expand collateral capacity, and foster entrepreneurship is therefore central to evaluating environmental policy as a form of place-making economic development.

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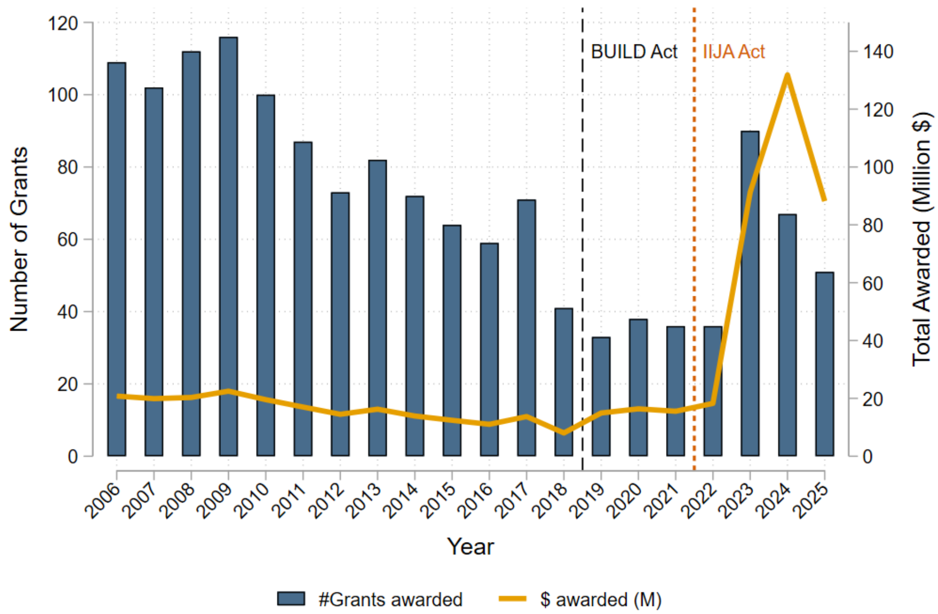
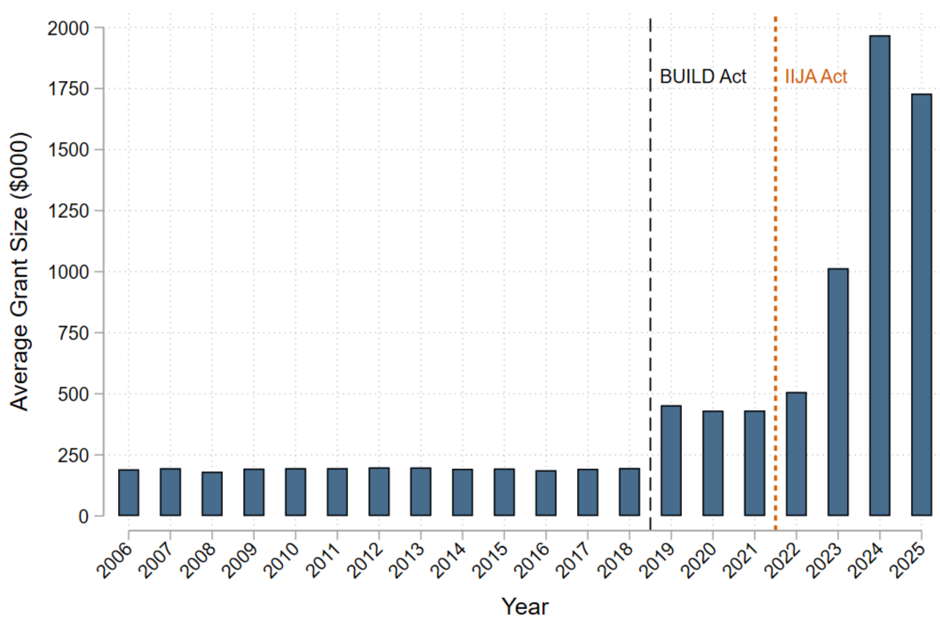
Figure 1: Grants and Dollars Awarded towards the EPA Brownfield Cleanup Program

A. Total Grants Awarded

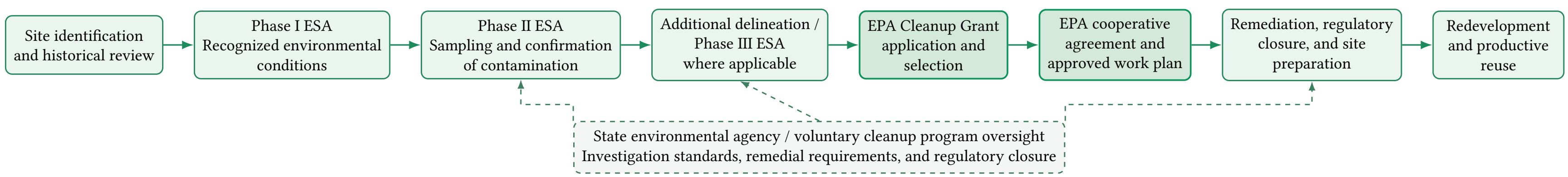


B. Average Brownfield Grant Size



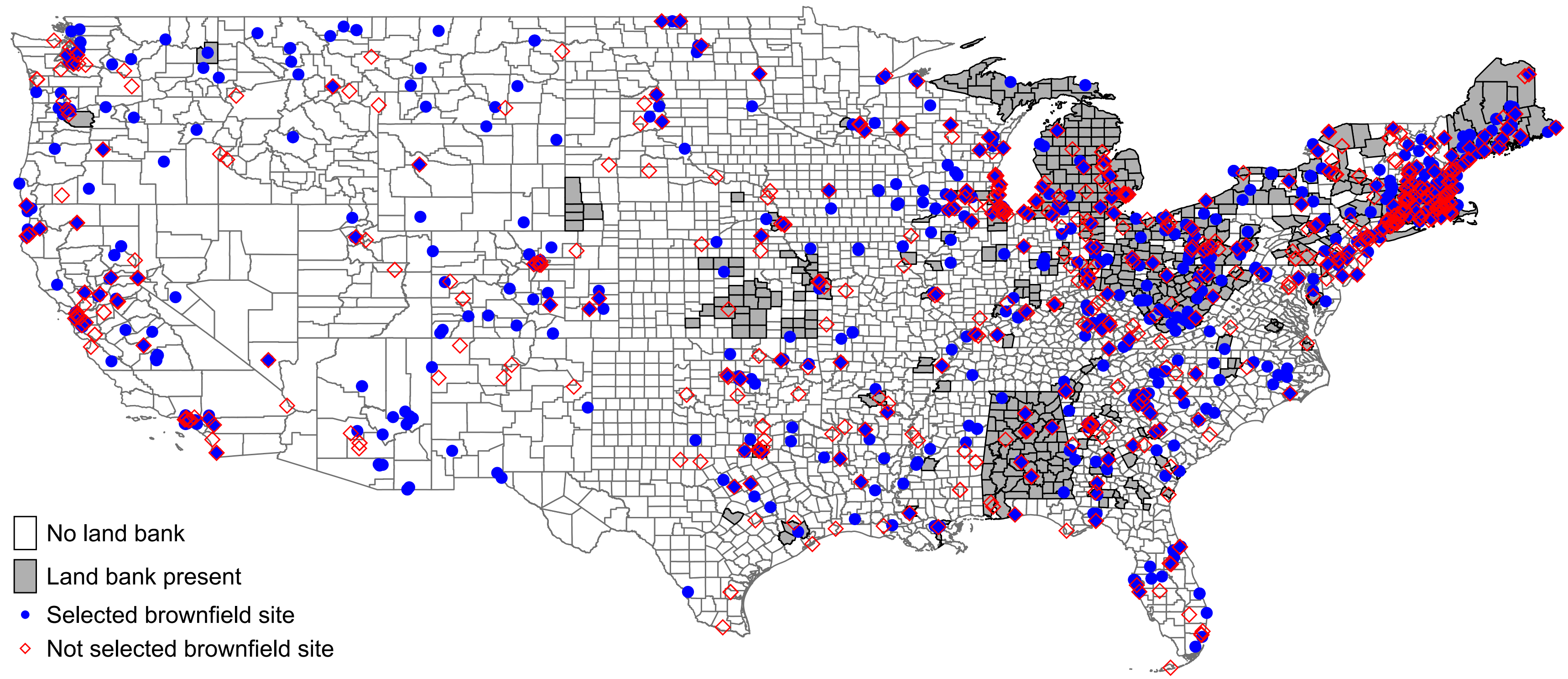
Notes: Panel A of this figure plots the time series of the number of EPA grants awarded (left-hand axis) and total funding in millions of nominal dollars allocated towards brownfield cleanup projects. Panel B plots the average grant size between 2006 and 2025. In both panels, the black dashed vertical line marks passage of the Brownfields Utilization, Investment, and Local Development Act (BUILD Act) and the red short-dashed vertical line marks passage of the Infrastructure Investment and Jobs Act (IIJA Act). We obtained the grants data from a series of FOIA requests submitted to the EPA.

Figure 2: Typical Brownfield Assessment, Cleanup, and Redevelopment Process



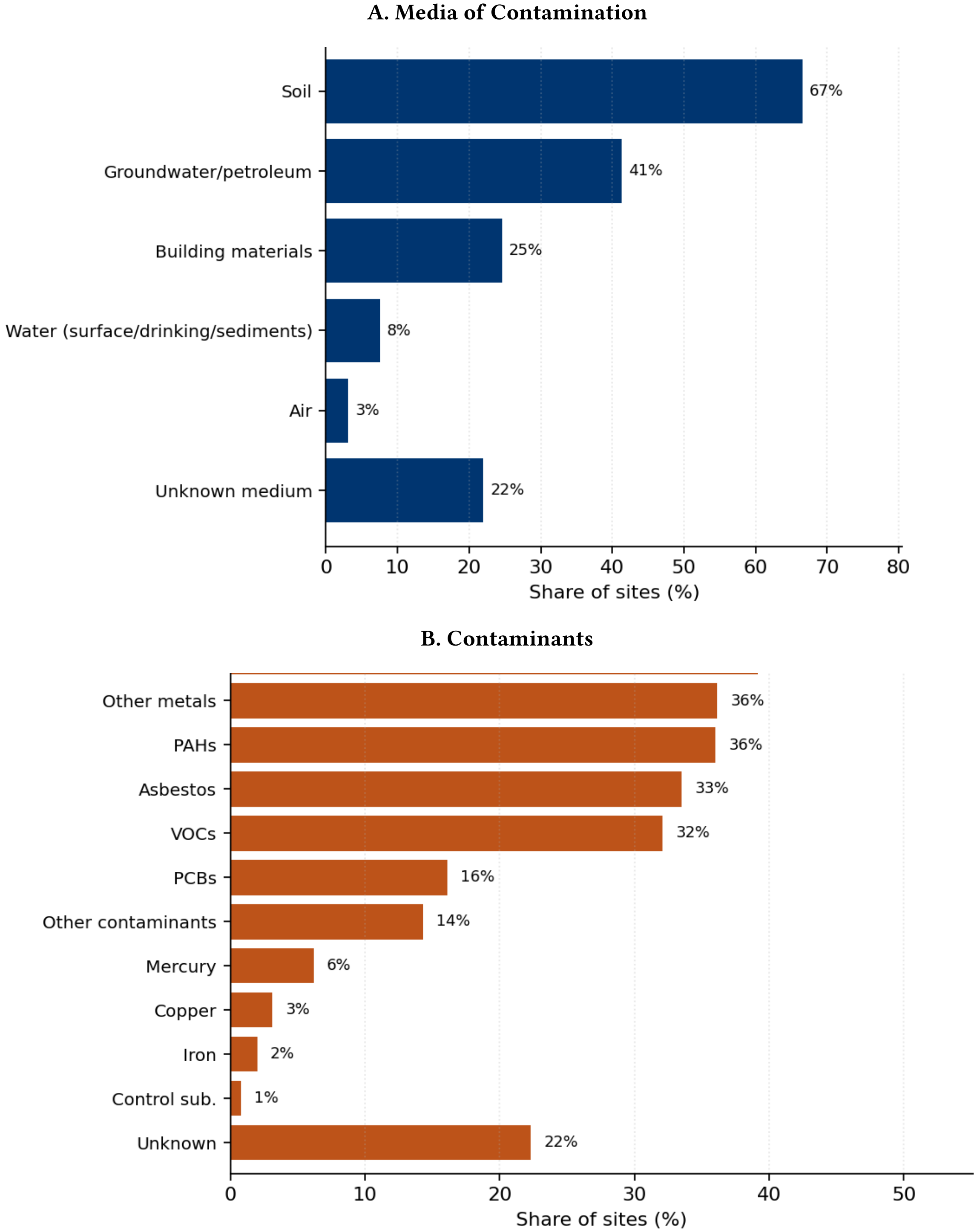
Notes: The figure summarizes a typical brownfield redevelopment sequence rather than a legally required sequence common to every state. A Phase II ESA or equivalent site investigation is generally required before an EPA cleanup grant application. Additional investigation, sometimes termed a Phase III ESA, can occur before or after the grant application and is governed in part by state-specific requirements. State environmental agencies or voluntary cleanup programs may remain involved from site investigation through remedial approval and closure. Appendix D describes how we use Phase II and Phase III maps to recover the spatial extent of contamination.

Figure 3: Locations of Land Banks and Cleanup Grant Applicant Sites



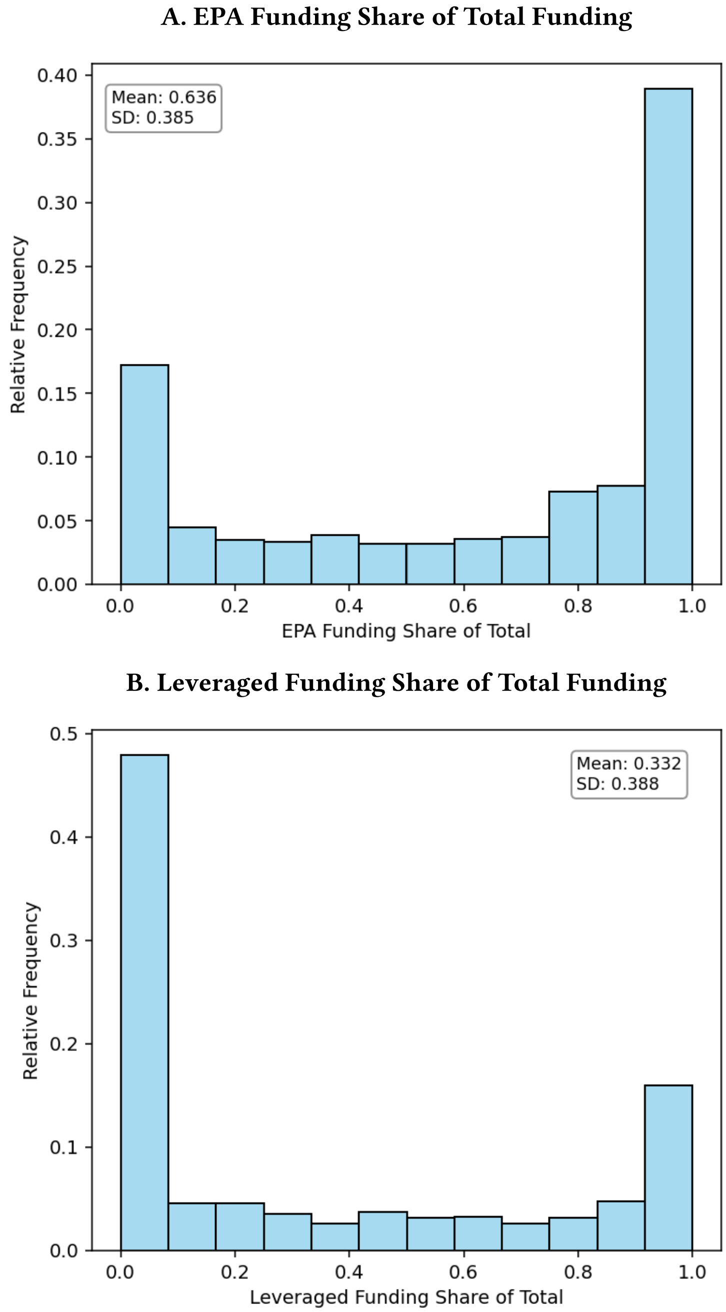
Notes: The map displays U.S. counties with active land banks, highlighted by gray-shaded areas, overlaid with the locations of brownfield sites that received EPA cleanup grants (blue dots) and those that did not (red diamonds). Land banks are public entities with unique governmental powers, created pursuant to state-enabling legislation, that are solely focused on returning problem properties to productive use in alignment with community goals. Land banking programs are run by nonprofit or government entities that focus specifically on the acquisition, holding, and sale of vacant and abandoned properties. The roster covers land banks in 31 states and Puerto Rico. Most land banks are chartered to operate within the county where they are headquartered, and are shaded in that county; municipal and regional land banks are shaded in the counties their jurisdictions fall in. Seven land banks operate statewide (Alabama, Connecticut, Maine, Massachusetts, Michigan, Rhode Island, and West Virginia) and are shaded across their entire state. We obtained land bank locations from the Center for Community Progress and map each to counties using the roster's own Census geographic identifiers. We geocode brownfield sites using the centroid of the site, as reported either from the EPA's *Cleanups in My Community* database or by obtaining coordinates for grant sites which received a cleanup grant and also received an assessment grant for which precise location information is available.

Figure 4: Prevalence of Contaminated Media and Contaminants for Brownfield Sites



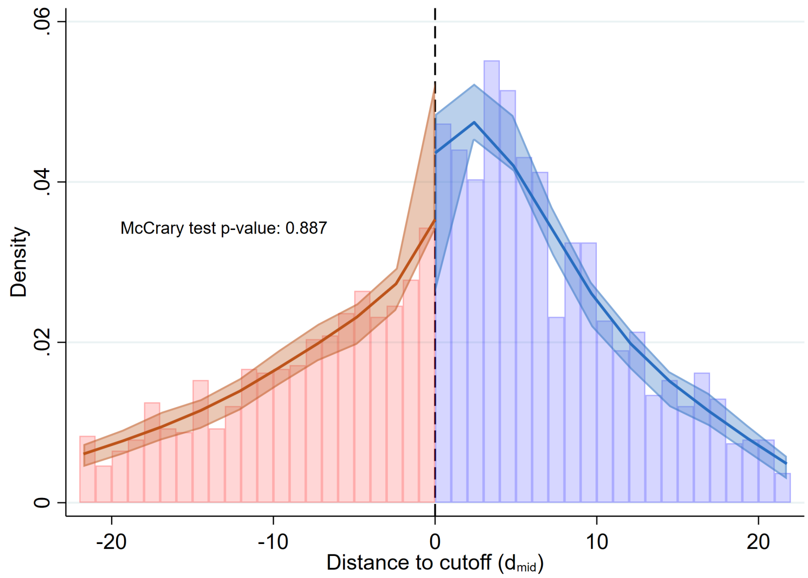
Notes: This figure reports the share of brownfield sites in the EPA's *Cleanups in My Community* (CIMC) database for which each contamination medium or contaminant is listed. These include sites which applied for EPA funding, whether from a cleanup grant, assessment grant, multi-purpose grant, or for revolving loan funds. Shares need not sum to 100 percent because a site may involve multiple affected media and multiple contaminants. "Unknown" denotes sites for which contamination is recorded but not further classified in the relevant field.

Figure 5: Composition of Recorded Brownfield Funding



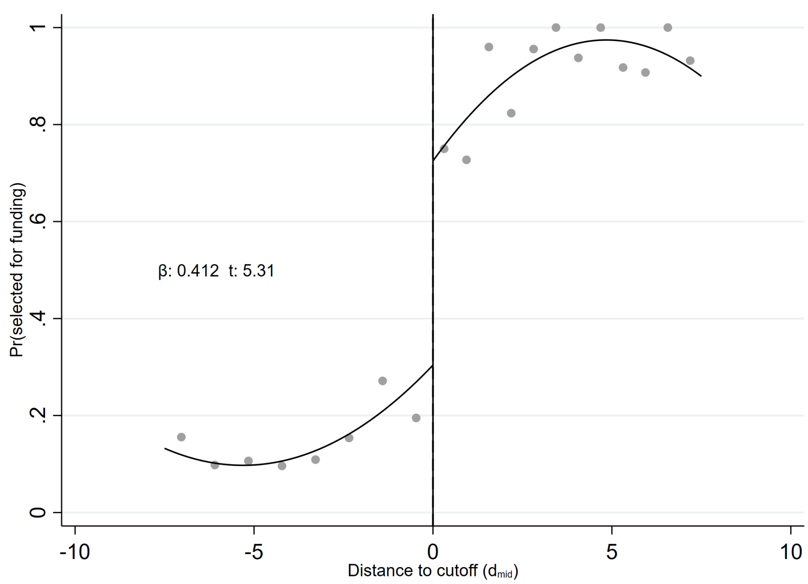
Notes: This figure plots the distribution of EPA and leveraged funding shares across brownfield sites in the EPA's *Cleanups in My Community* (CIMC) database. We aggregate EPA funding across all recorded assessment and cleanup activities associated with a site; sites may receive multiple assessment grants or record multiple assessment activities before receiving a cleanup grant. Leveraged funding consists of reported non-EPA funding associated with assessment and cleanup activities. Panel A plots the EPA share of total recorded funding, while Panel B plots the corresponding leveraged share. The distributions are not exact mirror images because missing funding information is not imputed when constructing stage-specific shares. In particular, when total assessment funding is missing, the corresponding assessment funding share is left missing and excluded from the histogram. When aggregating observed dollar amounts across stages within a site, however, a missing component contributes zero to the observed sum. Thus, a site with observed cleanup funding but missing assessment funding can have an observed cleanup-stage funding share equal to one even though the corresponding assessment-stage share is undefined. The reported means and standard deviations are calculated over the non-missing observations entering each panel.

Figure 6: McCrary Density Test of Continuity in Grant Score Distribution



Notes: McCrary density test on the running variable $d_{j,c} = \text{finalscore}_{j,c} - c_c$ on the full per-application sample ($N = 2,156$). Histogram bins are one score unit wide; the solid line is the local-polynomial density estimator with 95% confidence interval bands.

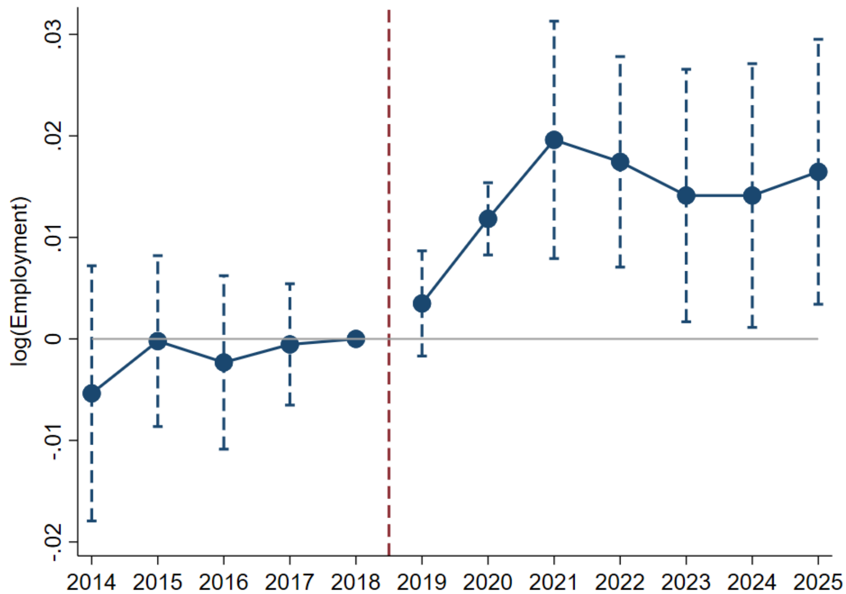
Figure 7: First stage Discontinuity in Cleanup Grant Award Probability



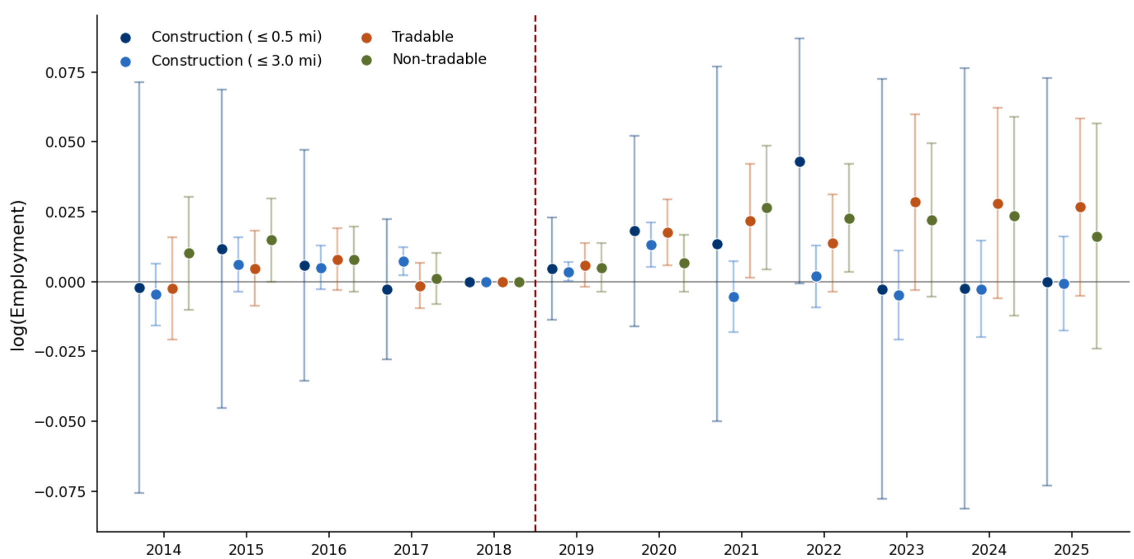
Notes: The figure plots the probability of selection for funding around the cutoff score under the EPA's Brownfield Grants Program. Dots represent binned averages, and solid lines show fitted local polynomial regressions estimated separately on either side of the cutoff. The vertical line denotes the cutoff, defined as the midpoint between the selected application with the lowest score and the non-selected application with the highest score. The point estimate is 0.412 with a standard error of 0.078 (t-stat = 5.31). The optimal bandwidth is selected using the mean squared error optimal bandwidth (MSERD) proposed by Calonico et al. (2014), yielding a bandwidth of ±8 points.

Figure 8: Dynamic Effects on Employment: Evidence From the BUILD Act

A. Baseline Employment Effects



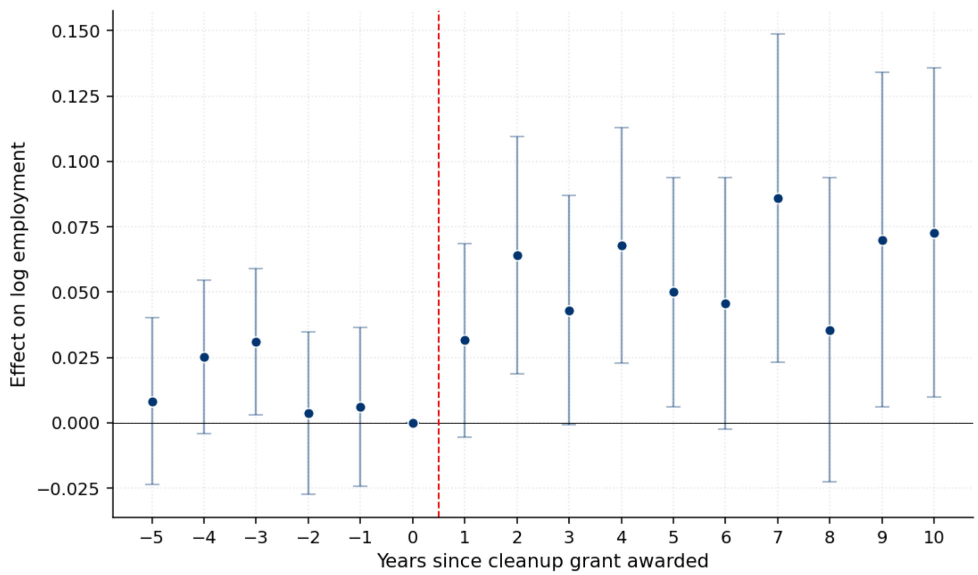
B. Heterogeneity by Construction and Tradability



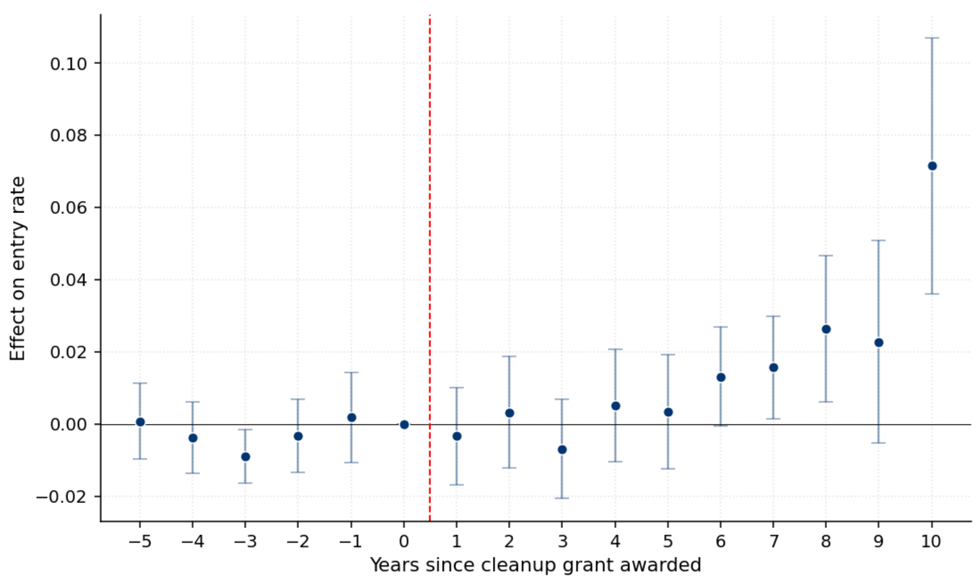
Notes: This figure plots dynamic effects of cleanup funding on nearby log employment by estimating the same specification as column (4) of Table 4 and equation (1), replacing *Post* with a series of event-time indicators and using 2018 as the omitted reference year. Panel A reports effects for total establishment employment. Panel B reports heterogeneous effects for construction employment within 0.5 and 3.0 miles of the applicant site, and for tradable and non-tradable industries. To classify industries, we crosswalk Dun & Bradstreet SIC codes to NAICS codes and apply the tradable and non-tradable sectoral definitions from Mian and Sufi (2014). Bars represent 95% confidence intervals based on site-clustered standard errors. The vertical dashed line denotes passage of the BUILD Act.

Figure 9: Effects on Employment and Firm Entry: Stacked RDD Evidence

A. Log *D&B* Plant-Level Employment



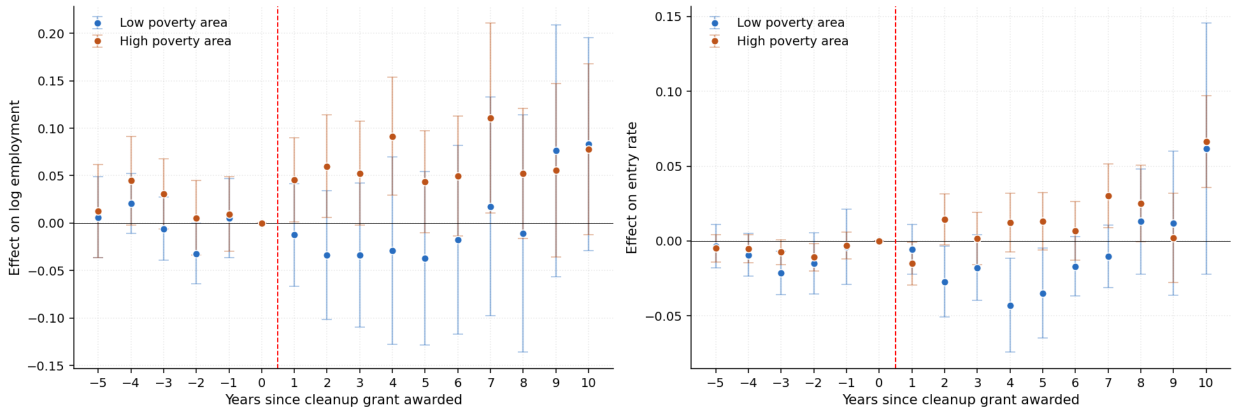
B. Firm Entry



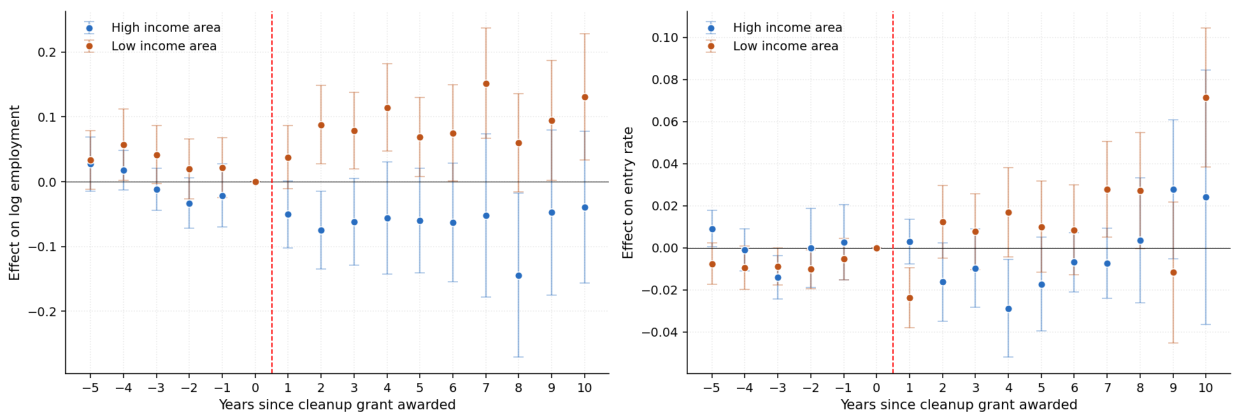
Notes: Panel A plots the dynamic effects of scoring above the cleanup grant funding cutoff on nearby employment, while Panel B plots the corresponding effects on nearby firm entry. Coefficients are estimated from the stacked dynamic regression discontinuity design described in equation 2, where event time is defined relative to the focal application year and 0 denotes the omitted reference year. Bars represent 90% confidence intervals based on standard errors clustered at the site level.

Figure 10: Heterogeneous Employment Effects by Neighborhood Characteristics

A. Neighborhood Poverty



B. Neighborhood Income



C. Neighborhood Unemployment Rate

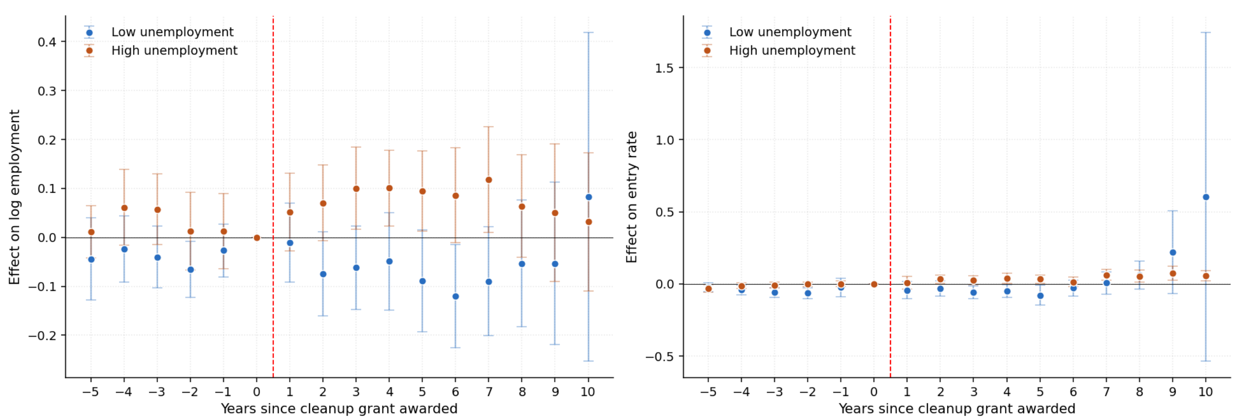
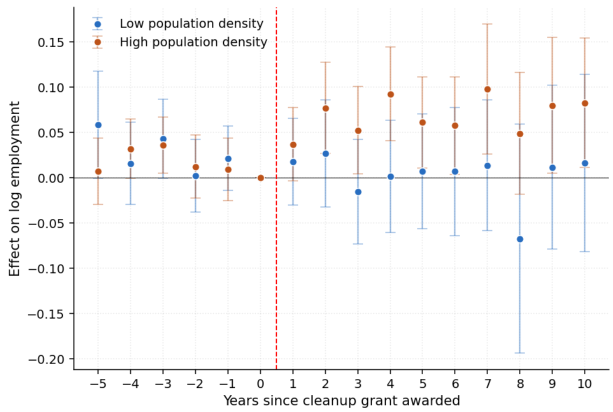
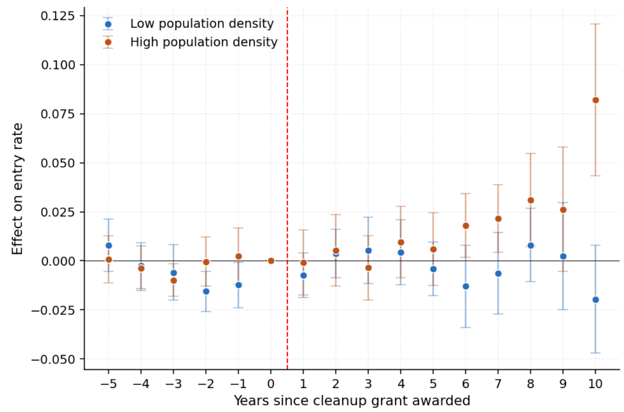


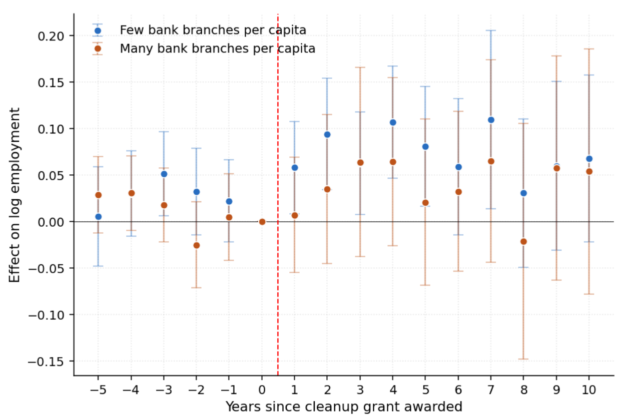
Figure 10: Heterogeneous Employment Effects by Neighborhood Characteristics (Continued)

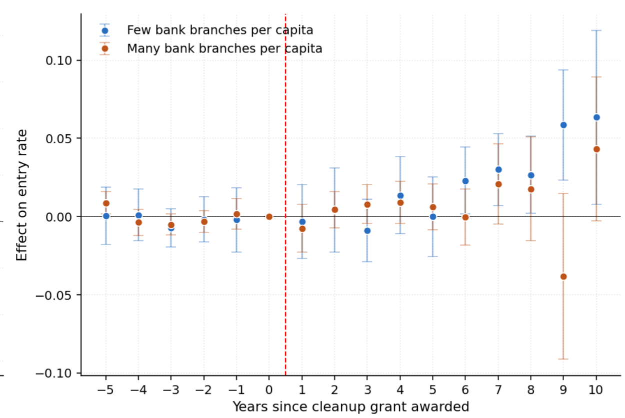
D. Population Density



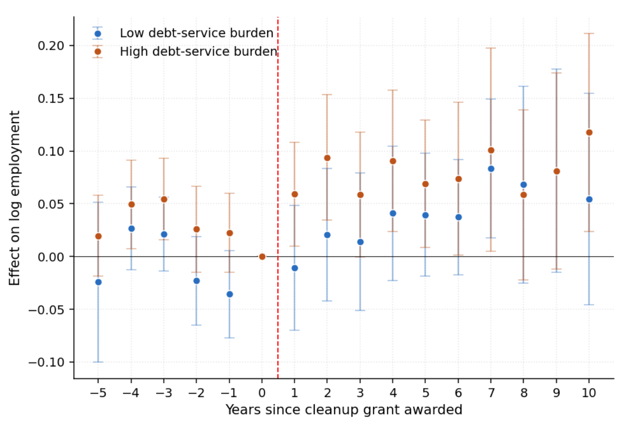


E. Bank Branches Per Capita





F. Interest Burden



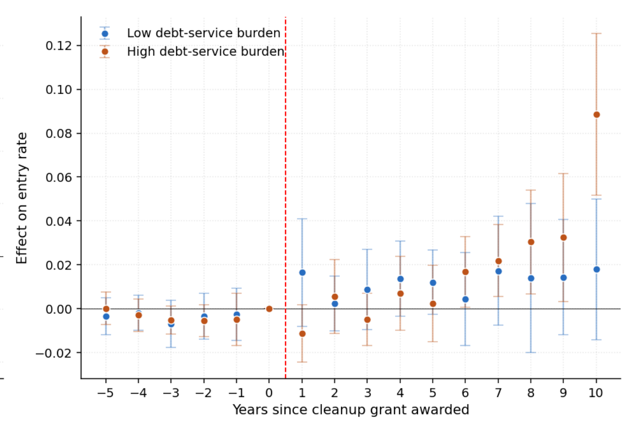
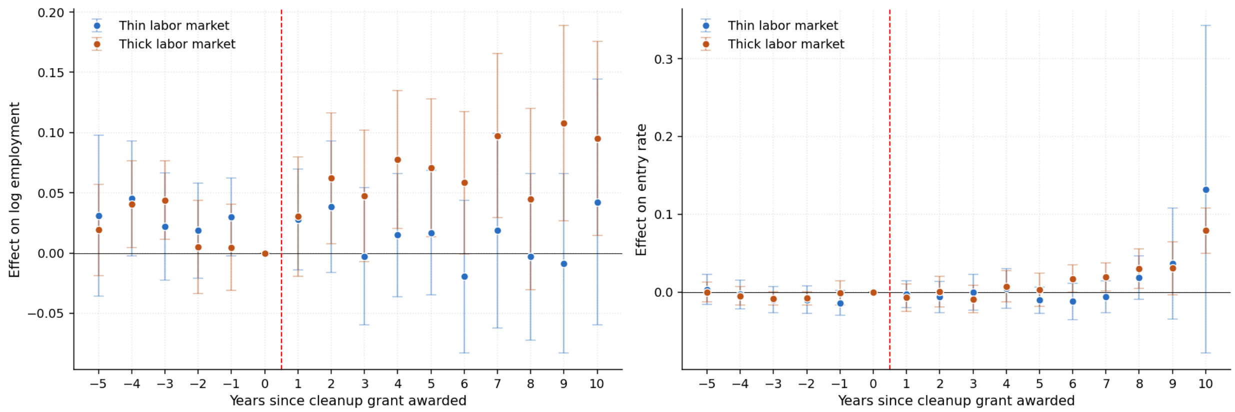
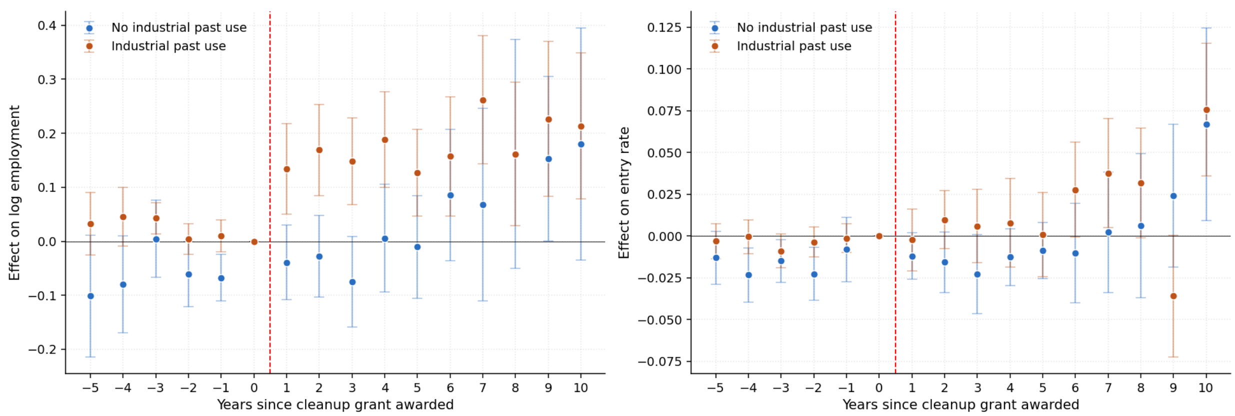


Figure 10: Heterogeneous Employment Effects by Neighborhood Characteristics (Continued)

G. Labor Market Thickness

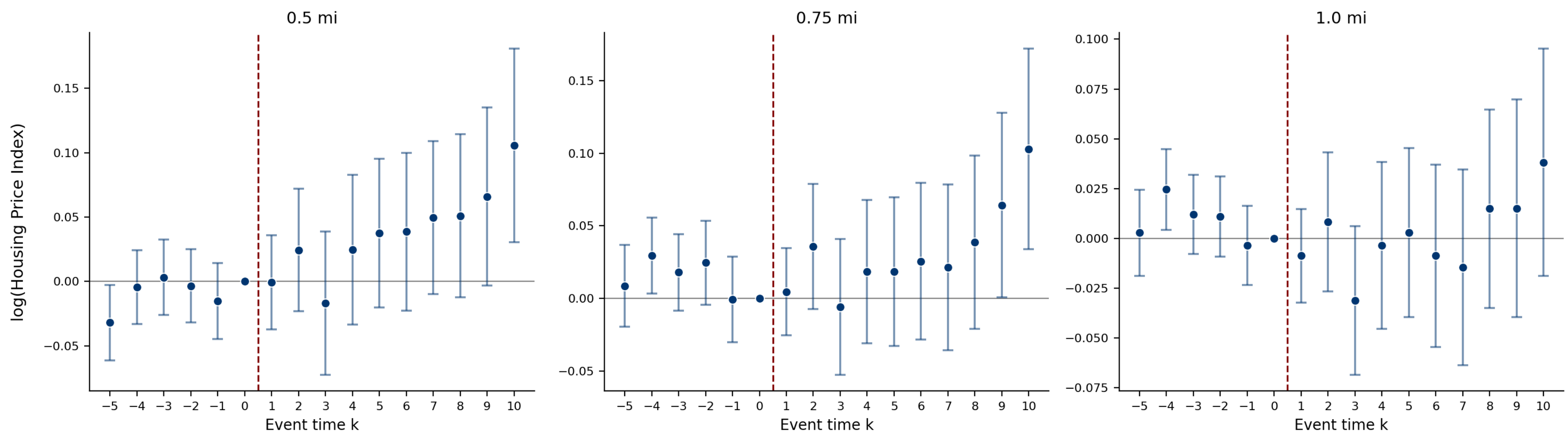


H. Past Industrial Use



Notes: Each subfigure overlays the LOW (light blue) and HIGH (orange) event-time paths for log employment (left panels) and the entry rate (right panels). We set the radius parameter to $r \leq 3$ miles, adopt a quadratic local polynomial, $p = 2$, and report 90% confidence intervals in bars. Panels A through D split neighborhoods according to 5-digit zip code-level ACS variables. Panel E splits sites by FDIC bank branches per capita at the zip code-level. We obtain bank branches by collapsing data from the FDIC Summary of Deposits API: https://api.fdic.gov/banks/docs. Panel F splits by the ratio of the county's interest payments on outstanding debt relative to general revenue (Census of Governments). Panel G splits by labor market thickness, measured as total county wage-and-salary employment in the year prior to grant authorization (LODES WAC). Panel H splits by past industry use, measured using data from the EPA CIMC database. In all panels, HIGH denotes the above-median group; LOW denotes the below-median group. ACS 5-year estimates at the ZCTA level are available beginning with the 2007–2011 vintage. For site-cohorts where $fy - 1 < 2011$ (cohorts 2006–2011, comprising approximately 38% of focal applications), we assign the 2007–2011 ACS vintage in place of the contemporaneous pre-grant value. Because the 5-year estimate is a rolling average over 2007–2011, the assigned value partly overlaps with the post-grant period for the earliest cohorts, introducing a potential measurement error in the group indicator G_i. Results are robust to restricting the sample to cohorts 2012 and later, for which the ACS year is strictly pre-treatment. LODES county employment and Census of Governments interest burden are available from 2002 and are not subject to this restriction.

Figure 11: Housing Price Capitalization Effects



Notes: This figure reports the effect of grant authorization on housing prices in census tracts located within 0.5, 0.75, and 1 mile of brownfield sites by estimating a stacked dynamic RDD design. The outcome is the log of the annual census tract-level house price indices (HPI) of Contat and Larson (2024) and produced by the Federal Housing Finance Agency (FHFA), available for download here: https://www.fhfa.gov/research/papers/wp2101. Points represent coefficient estimates, and vertical bars denote 90% confidence intervals.

Table 1: Summary Statistics Comparing Nearby Cleanup Zip Codes Pre vs. Post-IRA

	Before (2021)	**After (2023)**	**Difference**
Panel A: Census ACS			
Population	25,277	21,571	$-3{,}706^{*}$
Population Density	6,230	3,465	$-2{,}765^{***}$
Median Household Income	67,960	55,054	$-12{,}906^{***}$
Median Household Age	38.759	39.174	0.415
% Households Renting	0.450	0.473	0.026
% Non-Hispanic White	0.521	0.589	0.067^{***}
% Black	0.204	0.177	-0.027
% Native	0.014	0.009	-0.005
% Asian	0.019	0.035	0.018^{**}
% Hispanic	0.053	0.065	0.009
% Non-College	0.436	0.417	-0.019
% With Mortgage	0.603	0.577	-0.026
Panel B: Altos Rental Listings			
Rent	2,298.280	1,972.019	-326.261^{***}
Bedrooms	2.124	1.767	-0.357^{***}
Bathrooms	1.374	1.195	-0.179^{***}
Floor Size	1,799.421	1,116.753	-682.668^{***}
Rent Per Sq. Ft.	4.937	2.514	-2.424^{***}
Panel C: Cotality Single-Family Homes			
Assessed Value	243,068	198,528	$-44{,}540^{***}$
Transaction Price	426,146	385,193	$-40{,}953^{***}$
Year Built	1,941	1,957	16.179^{***}
Mortgage Amount	232,161	178,025	$-54{,}136^{***}$
Conventional Loan	0.571	0.700	0.130^{***}
FHA Loan	0.098	0.077	-0.021^{**}
Private Lender	0.045	0.047	0.001
VA Loan	0.032	0.021	-0.011

Notes: This table presents summary statistics comparing demographic, housing, and economic variables within a 3-mile radius of brownfield cleanup grant recipient sites before and after the enactment of the Inflation Reduction Act (IRA). We follow the literature in using a 3-mile radius for this test Gamper-Rabindran and Timmins (2013). However, the general patterns are qualitatively the same if we shrink or expand the radius. The table is organized into three panels, each drawing from a different data source. Panel A uses data from the 2021 American Community Survey (ACS), restricting to zip code codes where the population centroid is located within three miles of brownfield sites. Panel B draws on rental unit listings data from Altos Rental, covering properties within a 3-mile radius of brownfield sites in 2021. Panel C uses property and mortgage transaction data from Cotality, also from 2021, restricting the sample to single-family homes located within three miles of brownfield sites. Columns (1) and (2) report the mean values of the variables for the years right before (2021) and right after (2023) the IRA, respectively. Column (3) shows the difference between these means. Continuous variables are winsorized at 0.5% and 99.5% levels. Asterisks next to the difference indicate statistical significance at conventional levels from a two-sided t-test: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Balance Test around Brownfield Grant Score Cutoff

	Levels (pre-period $t=-1$)			Change ($Y_{-1}-Y_{-2}$)		
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Time-varying pre-period characteristics						
Poverty rate	0.008	0.007	0.004	0.001	0.001	0.004
	(0.006)	(0.011)	(0.013)	(0.002)	(0.003)	(0.004)
Median HH income ($)	-1788.661*	503.430	183.347	-109.371	397.278	254.375
	(920.983)	(1623.343)	(2032.784)	(206.777)	(495.300)	(468.636)
Log *D&B* emp ($\leq$ 3 mi)	0.186	0.013	-0.119	-0.003	0.007	0.010
	(0.123)	(0.214)	(0.236)	(0.006)	(0.012)	(0.016)
Log *D&B* plants ($\leq$ 3 mi)	0.172	0.088	-0.029	0.003	0.015*	-0.002
	(0.111)	(0.197)	(0.207)	(0.004)	(0.009)	(0.007)
PPP exposure	0.002	-0.023	0.007	—	—	—
	(0.007)	(0.014)	(0.012)			
Panel B: Time-invariant site characteristics						
Property size (acres)	-6.843*	-3.312	-2.071	—	—	—
	(3.812)	(5.901)	(9.031)			
Number of contaminants	0.105	-0.153	-0.197	—	—	—
	(0.102)	(0.201)	(0.241)			
Groundwater contam. (0/1)	0.080***	-0.042	-0.032	—	—	—
	(0.022)	(0.042)	(0.053)			
Soil contam. (0/1)	0.152***	0.050	0.014	—	—	—
	(0.022)	(0.041)	(0.048)			
Past commercial use (0/1)	-0.027	-0.008	0.012	—	—	—
	(0.029)	(0.056)	(0.069)			
Past industrial use (0/1)	0.007	-0.029	-0.048	—	—	—
	(0.029)	(0.058)	(0.075)			
Land bank in county (0/1)	0.005	-0.020	0.007	—	—	—
	(0.023)	(0.044)	(0.026)			
Land bank is applicant (0/1)	-0.001	-0.003	0.000	—	—	—
	(0.006)	(0.012)	(0.012)			
Cohort FE	Yes	Yes	No	Yes	Yes	No
2nd-order polynomial in d	No	Yes	Yes	No	Yes	Yes
State $\times$ Cohort FE	No	No	Yes	No	No	Yes

Notes: Each row reports a separate regression of site characteristics on an indicator for whether the site's score is above the cohort-specific cutoff, $\mathbb{1}(d_{j,c} \geq 0)$. Columns (1)–(3) use levels measured in the pre-period and are estimated on the per-application sample. Columns (4)–(6) use the pre-period change, $Y_{-1}-Y_{-2}$, as the dependent variable. ACS poverty rate and median household income use the 5-year ACS ending in cohort year minus one. Cohorts with first-grant year before 2012 use the 2011 ACS5 vintage, the earliest available. Change columns (4)–(6) exclude cohorts with first-grant year before 2013, for whom the lagged ACS vintage does not exist. Establishments are assigned to the nearest site when they are located within three miles of both treated and control sites in a given cohort. The PPP exposure measure comes from Granja et al. (2022). We use their realized zip code-level measure, defined as the share of local small businesses that received a PPP loan. *Land bank in county* equals one if the site's county contains an active land bank, using the Center for Community Progress national land bank roster; statewide land banks are treated as covering every county in their state. *Land bank is applicant* equals one if the grant applicant is itself a land bank, identified from the applicant name by a land bank keyword or a within-state match to the Center for Community Progress roster. The two measures capture different things and do not nest: land banks are geographically common (48% of applicants are in a land bank jurisdiction) but are rarely the grantee (only 2% of cases). Asterisks indicate statistical significance from a two-sided t-test: $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table 3: First Stage Effects on Grant Selection and Funding

Panel A: Award Receipt and Cleanup Progress			
Outcome:	$\mathbb{1}(Selected)$ (1)	$\mathbb{1}(Cleanup\ Started)$ (2)	$\mathbb{1}(Cleanup\ Completed)$ (3)
Above cutoff	0.411***	0.231**	0.194**
	(0.070)	(0.095)	(0.079)
Effect relative to mean outcome	0.640	0.475	0.512
MSE-optimal bandwidth	7.5519	5.8057	7.3809
Observations	1,153	924	1,114
Panel B: Funding Flows to the Site			
Outcome:	$Cleanup\ Grant$ \$ (4)	$Leveraged\ funding$ \$ (5)	$Total\ funding$ \$ (6)
Above cutoff	0.831**	1.493**	1.366**
	(0.384)	(0.689)	(0.621)
Implied effect at sample mean	$292K	$5.25M	$6.57M
MSE-optimal bandwidth	4.7206	8.6952	8.9180
Observations	780	1,260	1,267

Notes: This table reports regression discontinuity estimates of the effect of scoring above the EPA funding cutoff, using observations within each outcome-specific MSE-optimal bandwidth. The running variable is the application's final review score. Each specification includes a quadratic in the running variable interacted with the above-cutoff indicator. Standard errors, clustered by site, are reported in parentheses. Panel A reports linear probability models. "Effect relative to mean outcome" is the coefficient divided by the mean of the dependent variable. Panel B reports Poisson pseudo-maximum likelihood estimates for dollar-valued outcomes. EPA Cleanup Funding is the EPA grant awarded to the application; Leveraged Funding is non-EPA public and private funding plus the recipient's required cost share; and Total Site Funding is funding from all recorded sources. "Implied effect at sample mean" converts the estimated proportional effect into dollars evaluated at the sample mean. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Effect of Cleanup Grants on Nearby Employment: Evidence from the BUILD Act

	Log Employment			
	(1)	(2)	(3)	(4)
Applicant2019 × Funded × Post	0.022**	0.019**	0.012**	0.014**
	(0.009)	(0.008)	(0.006)	(0.006)
Establishment FE	Yes	Yes	Yes	Yes
Funded × Year FE	Yes	Yes	Yes	Yes
Applicant2019 × Year FE	Yes	Yes	Yes	Yes
SIC-2 × Year FE	Yes	Yes	Yes	Yes
Population Bin × Year FE	No	Yes	Yes	Yes
EPA Region × Year FE	No	No	Yes	Yes
State × Year FE	No	No	No	Yes
Observations	2,668,968	2,668,968	2,668,968	2,668,964
Adj. R^2	0.937	0.937	0.937	0.937

Notes: This table reports estimates of the effect of EPA brownfield cleanup grants on nearby plant employment using a triple-differences design around the 2018 BUILD Act expansion. The dependent variable is the log of establishment employment conditional on positive employment. *Post* is an indicator for the post-BUILD Act period ($t \geq 2019$). *Applicant2019* is an indicator for establishments located near sites that applied for a cleanup grant in the 2019 funding round. *Funded* is an indicator for whether an establishment is located near a site that received cleanup funding. Establishments are assigned to the nearest brownfield site when located within three miles of both treated and control sites. All specifications include establishment fixed effects, funded × year fixed effects, and 2019 applicant × year fixed effects. Columns progressively add SIC-2 × year, zip code population bin × year, EPA region × year, and state × year fixed effects. Standard errors are clustered at the site level. Asterisks indicate statistical significance at conventional levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Online Appendix to

Contaminated Capital: Brownfield Cleanups, Entrepreneurship, and Place-Based Redistribution

by Aymeric Bellon (UNC–Chapel Hill), Jason Chen (Auburn University), *and* Cameron LaPoint (Yale SOM)

Contents

A Examples of Brownfield Sites

This appendix provides three examples of the types of sites contained in our brownfield applicant database. These examples illustrate why brownfield redevelopment is difficult to measure using only parcel records or grant award amounts. Environmental Site Assessment (ESA) Phase II and Phase III environmental reports offer useful narrative evidence on redevelopment frictions and serve as inputs into our geocoding procedures, since maps of sample locations, soil exceedances, and groundwater plumes help distinguish the contaminated portion of a site from the broader parcel or redevelopment footprint.

85 Hawthorn Street, Hartford, Connecticut: The 85 Hawthorn Street site is an approximately 6.79-acre, City-owned parcel in Hartford. The property is currently vacant and overgrown, but historically contained a roughly 130,000 square foot building constructed circa 1912. The building was used for manufacturing until the late 1980s by Arrow Hart & Hegeman, Arrow-Hart, Inc., and Cooper Industries. Reported operations included manufacturing of electrical components such as light switches, industrial motor controls, and wiring devices. The production process involved metal stamping, screw machine work, drilling, tapping, degreasing, machining, burnishing, heat treating, acid baths, welding, metal plating, painting, and assembly. After the manufacturing period, the building housed commercial tenants, including a car detailing and tire repair center, before burning in 1999.

The Phase II report documents how this legacy industrial activity translated into multiple potential release mechanisms. Earlier sampling identified naphthalene in soil above regulatory criteria, along with elevated lead and chromium. The later Phase II work found exceedances for several PAHs and metals in selected soil samples, including lead and arsenic, while PCBs and total cyanide were not detected above laboratory reporting limits in the analyzed samples. Groundwater impacts were more limited but not absent: extractable total petroleum hydrocarbons were detected above the applicable surface-water protection criterion in one monitoring well, and low-level VOCs including PCE and TCE were detected in another well below applicable criteria. The site therefore illustrates a common manufacturing brownfield pattern: contamination is spatially heterogeneous, partly tied to urban fill and former building footprints, and relevant for redevelopment even when the most severe impacts are localized.

1082–1076 Tolland Turnpike and 15 Depot Street, Manchester, Connecticut: The Manchester example, also used in Appendix D to illustrate our geocoding procedure, consists of three parcels totaling approximately 1.2 acres. The site was home to a family business started in 1929, with historical uses including a gasoline service station, automotive repair garage, rental truck business, convenience store, used auto sales, auto detailing, post office, and cultivated fields. Gasoline sales reportedly ended in 2012, but the site still contained service-station features such as garage bays, former and existing underground storage tanks, fuel dispensers, fuel lines, floor drains, a septic system, stained surface areas, and petroleum-related infrastructure.

The Phase III ESA identified 18 areas of concern typical of a long-running gasoline and automotive repair facility. Constituents of concern included extractable total petroleum hydrocarbons, VOCs, PAHs, lead, arsenic, PCBs, and pesticides. The report is especially useful for our purposes because it delineates both soil remediation areas and a groundwater plume. Petroleum impacts were concentrated near the UST and former UST areas, fuel infrastructure, and building footprint, with evidence that shallow overburden groundwater was migrating off site to the southeast. Pesticides appear to be tied to agricultural uses that predated the service station and were generally limited to shallow soils in the southern undeveloped portion of the site; pesticides were not detected in groundwater during the Phase III investigation. Because soil impacts were generally shallow, the report recommended a remedial action plan involving removal of USTs and associated piping, hydraulic lifts, floor-drain piping, and the septic system, with excavation as a viable soil-remediation approach and monitored natural attenuation or *in situ* additives for groundwater.

Blackhawk Junction, Prairie du Chien, Wisconsin: The Blackhawk Junction site in Prairie du Chien, Wisconsin, illustrates an earlier stage of the brownfield redevelopment process, where contamination has been detected but not fully delineated. The property is approximately 9.13 acres and consists of a largely vacated shopping center site built over a 20-year period beginning in 1962. Historical uses included multiple commercial, service, and retail operations, including several dry cleaners as well as a car wash and gasoline service station. At the time of the Phase II sampling plan, the site contained one approximately 60,000 square foot vacant building and one approximately 20,000 square foot commercial building occupied by several tenants. The surrounding area included residential and municipal properties, which heightens the importance of characterizing potential exposure pathways.

The Wisconsin work plan reports that tetrachloroethene (PCE) was detected in soil and groundwater in 1991 after chlorinated VOC contamination was found in two nearby municipal wells. Although limited assessments were conducted in 2009–2010, the nature, degree, and extent of contamination remained unknown, creating a barrier to redevelopment. A 2014 fire destroyed a significant portion of the larger building, including the area where dry cleaners had operated, and Crawford County acquired the property through tax forfeiture in 2019. The Phase I assessment identified recognized environmental conditions and vapor encroachment concerns associated with PCE and other chlorinated VOCs in soil, groundwater, and soil vapor, as well as potential releases from former gasoline USTs and a pump island. The Phase II plan therefore proposed soil, groundwater, and soil-vapor sampling to evaluate both dry-cleaning and petroleum-related impacts. This case highlights a distinct brownfield friction: even before remediation begins, uncertainty about the horizontal and vertical extent of contamination can itself delay reuse, financing, and transfer of the property.

B Background on Decontamination Methods

Brownfield redevelopment requires matching remediation technology to the contaminant, affected medium, subsurface conditions, and intended future use of the site. Common contaminants include petroleum hydrocarbons, chlorinated solvents, polycyclic aromatic hydrocarbons (PAHs), wood-preserving chemicals, metals, perchlorate, and PFAS. These contaminants may affect soil, groundwater, or both. The engineering literature therefore emphasizes that cleanup cost-effectiveness is highly site-specific: the relevant question for the effectiveness targeting funds towards cleanup is which approach removes the binding redevelopment constraint at the lowest social cost.

The most direct remediation approach is excavation, sometimes followed by off-site disposal ("dig and haul") or on-site treatment. Excavation is transparent and can be attractive when contamination is shallow, spatially concentrated, or when rapid redevelopment is valuable. Its disadvantage is that costs rise with depth, transportation, disposal fees, and backfill requirements. Case evidence suggests that excavation with off-site disposal can be substantially more expensive than *in situ* alternatives, although excavation combined with on-site treatment can narrow the cost gap (Thornton et al., 1999; Bates et al., 2002).[1]

in situ chemical treatment attempts to degrade, reduce, or immobilize contaminants without removing affected soil or groundwater. These methods include *in situ* chemical oxidation, chemical reduction, zero-valent iron, and related approaches. Their main advantage is that they can treat deeper or less accessible contamination with less disruption to the site. Their effectiveness, however, depends on whether reagents can be delivered uniformly through the subsurface. Field evidence on *in situ* chemical oxidation shows that costs and performance vary sharply with treatment-zone size, contaminant distribution, delivery technology, and the presence of dense non-aqueous phase liquids (DNAPLs), which can make regulatory cleanup goals harder to achieve (Krembs et al., 2010).[2]

Bioremediation relies on naturally occurring or introduced microorganisms to degrade contaminants, often through biostimulation or bioaugmentation. It includes enhanced reductive dechlorination for

[1] Illustrative case study estimates include solidification/stabilization at approximately $64 per cubic yard at a wood-preserving site (Bates et al., 2002); *in situ* gaseous reduction at approximately $43 per cubic yard compared with excavation at approximately $214 per cubic yard for hexavalent chromium (Thornton et al., 1999); and *in situ* biological treatment at approximately $400,000 compared with $850,000 for excavation in an emergency-response setting (Flathman et al., 1996).

[2] Krembs et al. (2010) report an average ISCO project cost of roughly $220,000 and a median unit cost of approximately $94 per cubic yard across field applications. In a multi-site comparison of DNAPL source-depletion technologies, McDade et al. (2005) report median costs of approximately $125 per cubic yard for chemical oxidation, compared with approximately $29 per cubic yard for enhanced bioremediation.

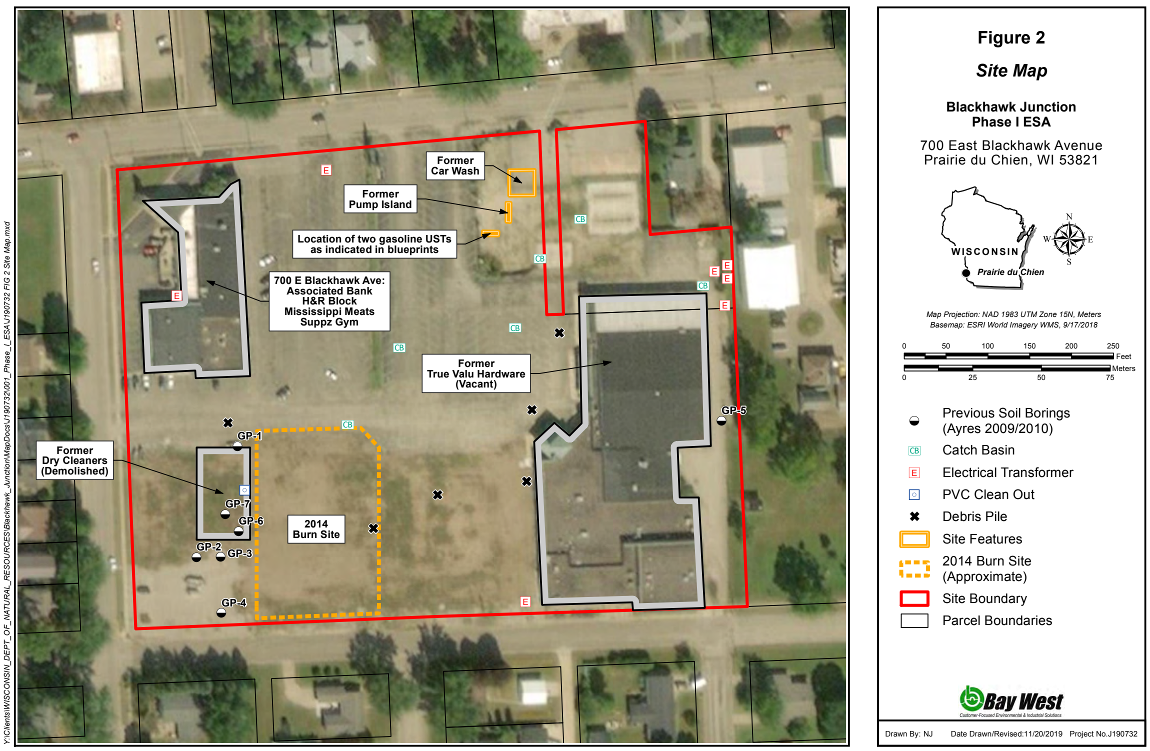
Figure A.1: Example Brownfield Sites in Connecticut

A. 85 Hawthorn Street, Hartford, CT (ESA Phase II)

B. 1082–1076 Tolland Turnpike, Manchester, CT (ESA Phase III)

Notes: Panel A shows the former Arrow Hart manufacturing site at 85 Hawthorn Street in Hartford, Connecticut, an approximately 6.79-acre vacant parcel formerly improved with a 130,000 square foot manufacturing building that burned in 1999 (Tighe & Bond, Inc., 2017a). Panel B shows the Gerich's Garage site at 1082–1076 Tolland Turnpike and 15 Depot Street in Manchester, Connecticut, a 1.2-acre former gasoline service station and automotive repair property with documented soil and groundwater contamination (Tighe & Bond, Inc., 2017b).

Figure A.2: Example Brownfield Site in Wisconsin: Blackhawk Junction (ESA Phase I)



Notes: This figure shows the Blackhawk Junction site at 700 East Blackhawk Avenue in Prairie du Chien, Wisconsin. The site map identifies the parcel boundary, former dry-cleaner area, former car wash, former pump island, suspected gasoline underground storage tank locations, prior soil borings, and the approximate 2014 burn site. The Phase II sampling plan describes the property as an approximately 9.13-acre former shopping-center site with multiple historical commercial, service, retail, dry-cleaning, and gasoline-service uses (Bay West LLC, 2020).

chlorinated solvents, *ex situ* treatment of excavated soils, landfarming, bioventing, and related biological processes. Cross-site evidence suggests that enhanced bioremediation can be relatively low cost when contaminants are biodegradable and site conditions allow treatment to proceed over time (McDade et al., 2005). Brownfield case studies document successful use of bioremediation for perchlorate-contaminated soil and chlorinated-solvent sites, although these examples are best interpreted as context-specific rather than general technology rankings (Evans et al., 2008; Fowler and Dockter, 2010).[3]

Some sites require combined or sequential technologies. Mixed remedies are common because contamination is rarely uniform across depth, medium, or chemical type. A site may use excavation for a concentrated source area, *in situ* treatment for groundwater, and monitored natural attenuation or institutional controls for residual risk. Case studies suggest that combined approaches can reduce costs when they avoid unnecessary disposal or target the most contaminated portions of the site first (Buehlman et al., 1998; Robinson and Angyal, 2008). PFAS contamination illustrates a related challenge: because many PFAS compounds are persistent, management may rely on containment or sorption technologies with long-run operation and maintenance costs (Birnstingl and Wilson, 2024).[4]

A related emerging approach is remineralization, which adapts enhanced rock weathering concepts to stabilize metals in shallow contaminated soils. Rather than removing contaminated material, this strategy uses carbonate–silicate blends to accelerate soil-aging processes that can reduce the mobility and bioavailability of cationic metals such as lead, zinc, copper, cadmium, and nickel (Zacharias, 2026). This approach is conceptually closer to *in situ* stabilization than to contaminant removal, and is most relevant for sites where reducing direct-contact, dust, or leaching risk can enable safe reuse without full excavation.

The regulatory context is central for interpreting remediation costs. Superfund sites tend to be larger, more complex, and subject to more stringent cleanup standards than typical brownfield sites. As a result, average Superfund remediation costs can be much higher than costs observed in brownfield case studies. Moreover, risk-based cost-effectiveness can appear low when large expenditures avert small expected health risks (Hamilton and Viscusi, 1999).[5] Brownfield programs often permit cleanup standards tailored to intended land use, which can make redevelopment feasible without requiring complete restoration to background conditions.

For our empirical analysis, this engineering background has three implications. First, EPA grant dollars may purchase different amounts of risk reduction depending on contaminant type, depth, hydrogeology, and regulatory requirements. Second, cleanup technology affects timing. excavation may enable faster redevelopment at higher upfront cost, while passive *in situ* or biological approaches may reduce direct costs but delay reuse. Third, cost per job and marginal value of public funds calculations should account for the fact that grants may either finance cleanup directly or unlock complementary private and local

[3]Reported bioremediation costs vary widely across applications. Examples include approximately \$35 per ton for perchlorate-contaminated soil at a large brownfield site (Evans et al., 2008), approximately \$18 per ton for a former dry-cleaner site using a mixed remediation strategy (Fowler and Dockter, 2010), and approximately \$190–\$200 per ton for slurry-phase biological treatment when total project costs are included (Jerger and Woodhull, 1995).

[4]Birnstingl and Wilson (2024) compare *in situ* colloidal activated carbon with pump-and-treat for PFAS plume management and estimate 30-year costs of approximately \$7.2 million for the passive *in situ* approach compared with \$19 million for pump-and-treat.

[5]Hamilton and Viscusi (1999) report average Superfund remediation costs of approximately \$17.3 million for soil and \$13.1 million for groundwater, with costs exceeding \$100 million per cancer case averted at many sites.

investment by resolving environmental liability and uncertainty. Thus, heterogeneity in redevelopment outcomes likely reflects not only local economic conditions, but also the interaction between remediation technology and the physical characteristics of contaminated land.

The EPA CIMC data also show that cleanup timelines vary substantially across contaminants and affected media. Figure B.1 indicates that projects involving volatile organic compounds (VOCs) have the longest median durations, at roughly 600 days, while many common contaminant categories – including petroleum, PCBs, PAHs, other metals, lead, and copper – cluster around 400–450 days. Cleanup times are also longer when contamination affects groundwater, surface water, soil, or sediments, consistent with the greater difficulty of treating subsurface or mobile contamination relative to more localized building-material or air pathways.

Figure B.2 further shows that cleanup duration is highly right-skewed. Among completed projects, the median cleanup lasts approximately one year, with the 75th percentile at about 2.2 years and the 90th percentile at about 4 years. When conditioning instead on sites with a recorded cleanup start date, including those not yet completed, the implied timeline is longer: the median project reaches completion after roughly 1.7 years, the 75th percentile is approximately 7.4 years, and about one-quarter of projects do not finish within 10 years. These patterns reinforce the importance of modeling cleanup as a dynamic process rather than a one-time treatment shock, and they motivate our event-time specifications that allow employment, firm entry, and property market responses to emerge gradually as remediation and reuse proceed.

C Survey of Brownfield Cleanup Grantees and Stakeholders

This appendix describes the implementation and sampling frame for our survey of entities involved in known brownfield remediation attempts. We report the text of our open-ended questions common to both survey waves, targeting an average completion time of 30 minutes across respondents.

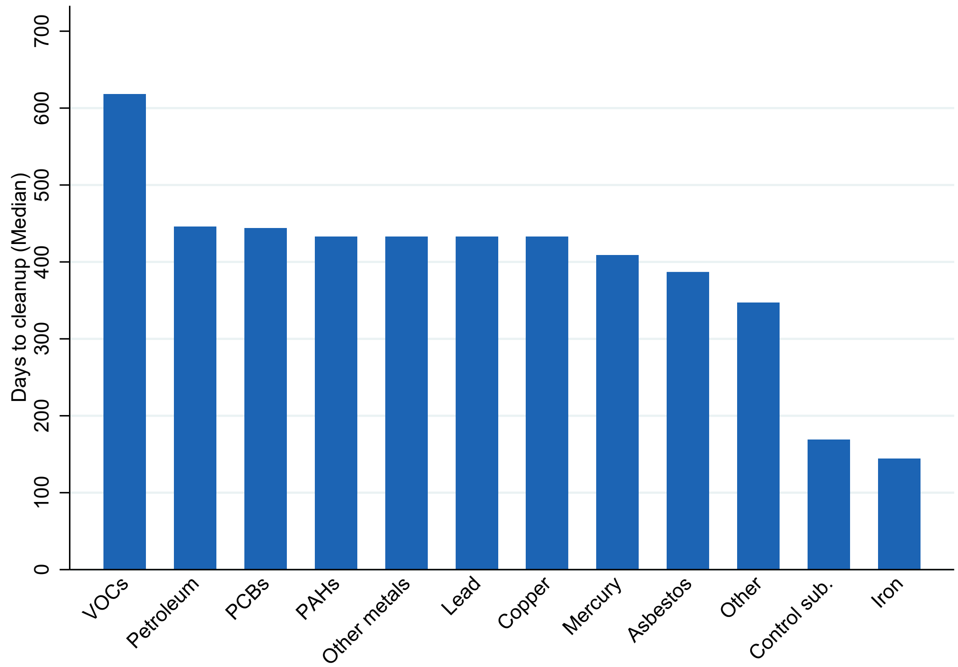
C.1 Survey Administration

The stakeholder survey is designed for online administration using SurveyMonkey and is subject to approval by the Yale Institutional Review Board. The sampling frame includes up to 10,000 brownfield stakeholders drawn from five groups: (i) federal and state EPA grantee organizations, (ii) land banks, (iii) state and federal EPA Region officials, (iv) local development boards, and (v) councils of governments. The survey uses two waves. Wave 1 focuses primarily on organizations receiving grants during the post-IRA period. Wave 2 adds a new sample of respondents from pre-IRA cleanup projects and recontacts first-wave respondents with follow-up questions about cleanup progress, redevelopment milestones, and evolving implementation constraints.

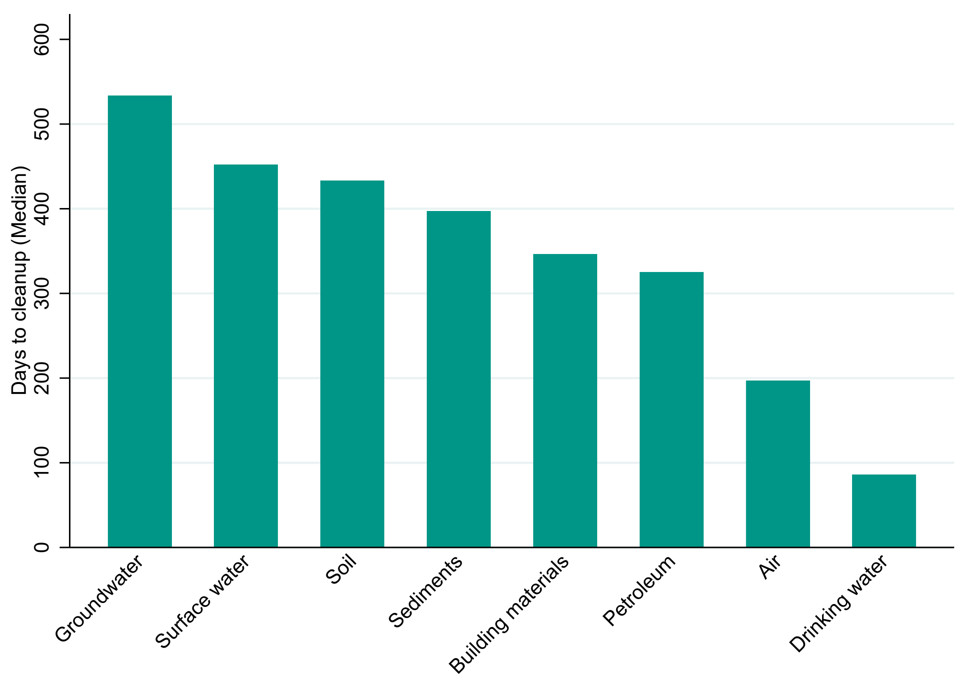
Survey invitations include a concise consent statement describing the purpose of the research, the voluntary nature of participation, expected completion time, confidentiality protections, and contact information for the research team. The consent language states that respondents may skip any question and may stop the survey at any time. To preserve participant anonymity, the protocol stores the contact list separately from survey responses and uses randomly generated respondent identifiers to link invitation

Figure B.1: Cleanup Grant Duration by Contaminant and Contaminated Media

A. Contaminant Type

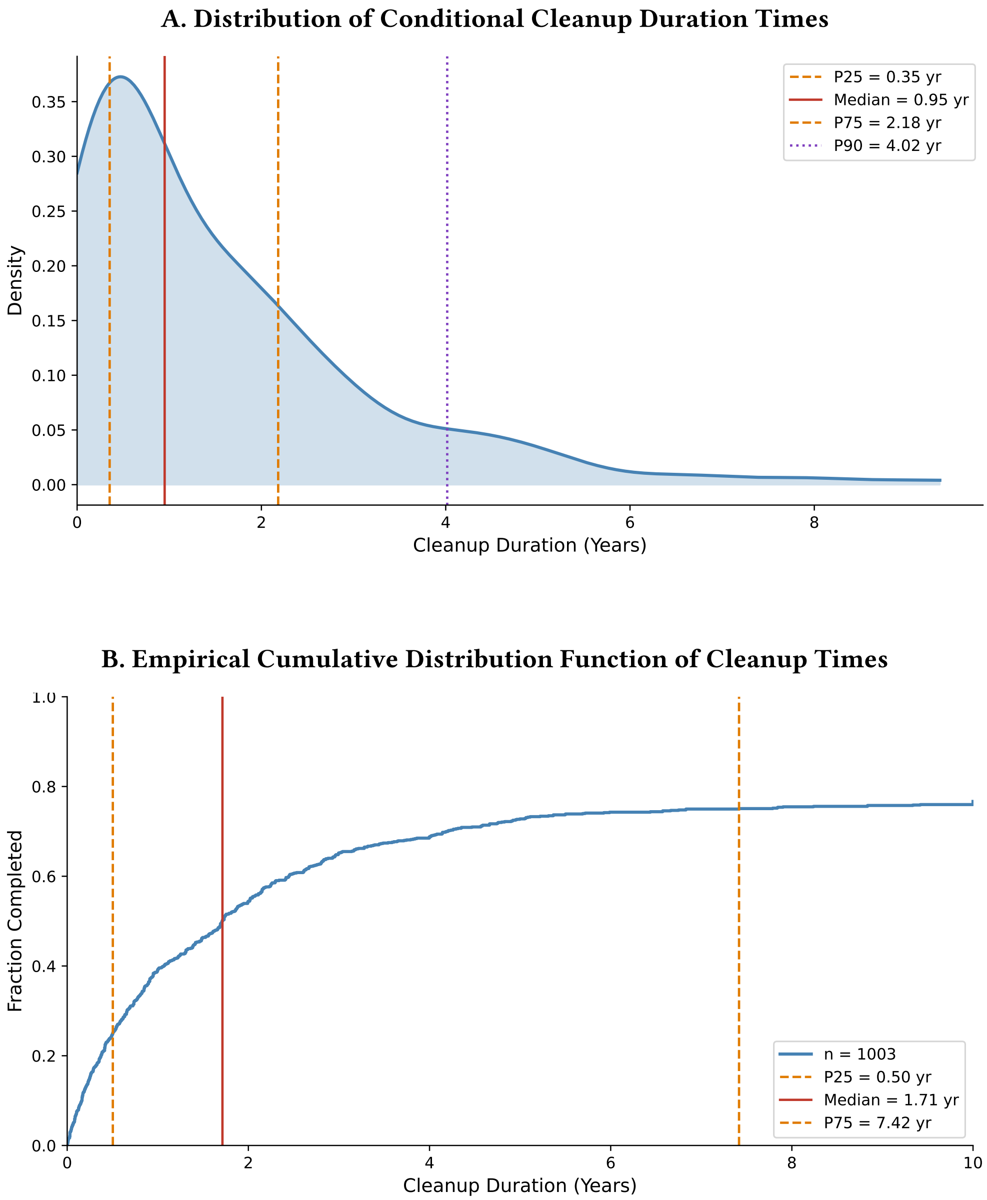


B. Contaminated Media



Notes: The figure plots median cleanup durations for sites that received EPA cleanup grants. Panel A groups sites by contaminant type, and Panel B groups sites by contaminated media. Cleanup duration is measured as the number of days between cleanup start and completion date, as recorded in the EPA's *Cleanups in My Community* database. We exclude observations without a recorded completion date. Individual sites may contain multiple contaminants and may impact multiple media, so the categories are not mutually exclusive.

Figure B.2: Distribution of Brownfield Cleanup Project Duration (EPA CIMC Database)



Notes: This figure summarizes the distribution of cleanup project durations for EPA brownfield cleanup grants reported in the Cleanups in My Community (CIMC) database. Cleanup duration is measured as the time between the EPA-reported cleanup start date and cleanup completion date, conditional on both dates being non-missing and implying a positive duration. Panel A plots the distribution of project durations among completed cleanups. Panel B plots the corresponding empirical cumulative distribution function (CDF), showing the share of completed cleanup projects with durations less than or equal to each value on the horizontal axis. To generate the CDF, we include only those sites which have a non-missing start date; of that subset, 25% of cleanup projects do not finish within 10 years.

records, stakeholder type, and survey wave. Survey responses are stored on Yale-approved secure storage systems with access limited to the research team. Any quotations used in publications or policy briefs are anonymized and, where necessary, lightly paraphrased to avoid identifying a specific respondent, organization, or redevelopment project. Publicly released survey outputs are aggregated so that individual respondents and organizations cannot be re-identified.

The protocol follows standard best practices for online qualitative research and includes a pilot with practitioners and academic colleagues to verify that the questions are understandable, neutrally worded, and feasible to complete in approximately 30 minutes. To reduce order effects and confirmation bias, question order is randomized within thematic modules where doing so does not disrupt logical flow; consent, eligibility, project background, and final reflection questions remain fixed. The survey includes at least one validation question asking respondents to select a specified response or briefly restate an instruction, allowing us to flag responses from participants who do not read the survey text. We also track response duration, item non-response, duplicate submissions, and unusually short or non-substantive answers as data quality checks. The qualitative analysis uses a structured coding protocol, double-codes a subset of responses across research team members, reconciles coding discrepancies, and distinguishes common themes from isolated examples.

C.2 Survey Questions

The survey contains 29 main questions about respondents' experience with brownfield remediation, together with conditional follow-up prompts when a topic is particularly relevant to the stakeholder's experience. To keep expected completion time to approximately 30 minutes, the additional questions concerning administrative-record validation and information available before cleanup are primarily multiple-choice. The survey is divided into ten parts spanning the mechanisms and outcomes explored in the paper.

I. Respondent and Project Background

Q1. Please briefly describe your organization's role in the brownfield cleanup and redevelopment process.

Q1a. *If your organization partnered with another public agency, nonprofit, land bank, or private developer:* Please describe the division of responsibilities.

Q2. Please describe the site or sites supported by the EPA cleanup grant, including prior use, contamination concerns, and redevelopment status before the grant was received.

Q2a. *If applicable:* What were the main community, economic, or environmental goals of the project at the time of application?

II. Administrative Record Validation

The following questions provide checks on respondents' recall by asking about project characteristics that can be independently verified using EPA grant records, the Cleanups in My Community (CIMC) database, state brownfield inventories, and public application materials. We use discrepancies as indicators of response reliability rather than interpreting them mechanically as intentional misreporting, since respondents may not have been directly involved in earlier stages of a project.

Q3. Our records identify the following brownfield project as the project associated with your organization: *[pre-filled site name and address].* Is this the correct project for which you are responding?

- Yes.
- No.
- Not sure.

Q3a. *If no:* Please provide the correct site name, address, or project identifier, if known.

Q4. Our records indicate the following grant characteristics: *[pre-filled grant type, fiscal year, and award amount].* To the best of your knowledge, are these records correct?

- Yes, all are correct.
- Some of the information is incorrect.
- Not sure.

Q4a. *If some information is incorrect:* Please identify which item or items should be corrected, if known.

Q5. Which of the following media were affected by contamination at the site, to the best of your knowledge? Please select all that apply.

- Soil.
- Groundwater.
- Surface water.
- Sediments.
- Building materials.
- Air or vapor intrusion.
- Other.
- Not sure.

Q6. Which contaminants were identified at the site, to the best of your knowledge? Please select all that apply.

- Petroleum or petroleum-related compounds.

- Volatile organic compounds or chlorinated solvents.
- PCBs.
- PAHs.
- Lead.
- Asbestos.
- Other metals.
- PFAS.
- Other.
- Not sure.

Q7. Which statement best describes the current status of the project, and did the project obtain funding or in-kind support beyond the EPA cleanup grant?

- Cleanup has not yet started.
- Cleanup is underway.
- Cleanup has been completed but redevelopment has not begun.
- Cleanup has been completed and redevelopment is underway.
- Cleanup and redevelopment have both been completed.
- Not sure.

Q7a. *Regarding additional funding:* Did the project receive or secure non-EPA grants, loans, private financing, in-kind support, or other leveraged funding? If yes, please briefly identify the source or type, if known.

III. Information Available Before Cleanup

The next questions distinguish sites where contamination constituted a well-known constraint on development from sites where the scope of contamination was learned only during assessment or remediation. These responses allow us to examine whether redevelopment effects reflect the removal of a previously understood barrier or the resolution of uncertainty about environmental conditions.

Q8. Before applying for EPA brownfield funding, how much did your organization know about the type, location, and severity of contamination at the site?

- Very little was known.

- Some information was available, but major uncertainty remained.
- The main contaminants and affected areas were mostly known.
- The extent of contamination was well understood.
- Not sure.

Q8a. Please briefly explain what the main sources of information were before the grant application, such as prior environmental assessments, Phase I or Phase II reports, owner disclosures, state records, local knowledge, or past industrial use.

Q9. During assessment or cleanup, did the project reveal contamination that was more extensive, more severe, or different in type than expected?

- Yes, substantially more than expected.
- Yes, somewhat more than expected.
- No, findings were broadly consistent with expectations.
- No, contamination was less severe than expected.
- Not sure.

Q9a. *If the findings differed from expectations:* Please describe what was surprising and how it changed the cleanup plan, budget, timeline, or redevelopment strategy.

Q10. Before cleanup began, how widely known was the site's contamination history among local residents, businesses, developers, lenders, or public officials?

- Widely known.
- Known mainly among public officials or specialized stakeholders.
- Known only to a small number of people or organizations.
- Largely unknown before assessment or cleanup.
- Not sure.

Q10a. *If contamination was widely known:* Did that knowledge appear to discourage nearby investment, lending, property transactions, or redevelopment before the EPA grant? Please describe.

Q10b. *If contamination was not widely known:* Did learning about the contamination create stigma, uncertainty, delays, or concerns among nearby residents, businesses, developers, or lenders? Please describe.

Q11. Before the EPA grant, had the local economy or land market already adjusted to the site's contamination history? For example, had developers avoided the site, had nearby properties remained vacant or underused, or had lenders treated the area as risky?

Q12. In your view, did cleanup have a larger impact because it removed a long-known barrier to redevelopment, or because it resolved uncertainty that had not previously been well understood?

- It mainly removed a long-known barrier to redevelopment.
- It mainly resolved uncertainty about contamination.
- Both channels were important.
- Neither channel was important.
- Not sure.

Q12a. Please explain how prior information about contamination shaped the site's redevelopment prospects before and after cleanup.

IV. Usefulness and Counterfactual Role of EPA Funds

Q13. How did the EPA cleanup grant change what your organization was able to do at the site?

Q14. What would likely have happened to the site in the absence of the EPA cleanup grant?

Q15. Were there parts of the cleanup or redevelopment process that the EPA grant was especially well-suited to support? Were there important costs or activities that the grant could not cover?

Q15a. *If the grant could not cover important costs:* How did your organization address those gaps?

Q15b. *If applicable:* Did the size, timing, or administrative structure of the grant affect the pace or scope of redevelopment?

V. Layered Funding, Land Banks, and Local Capacity

Q16. What other sources of funding or in-kind support were combined with the EPA grant? Examples might include state cleanup programs, local government funds, land bank resources, tax increment financing, infrastructure funds, philanthropic funds, or private capital.

Q17. Did the EPA grant help attract additional funding, partners, or political support that would not otherwise have been available? Please describe.

Q18. If a land bank, redevelopment authority, or similar entity was involved, what role did it play in acquiring, holding, clearing, assembling, marketing, or transferring the property?

Q18a. *If no land bank or redevelopment authority was involved:* Were there functions that such an entity could have helped provide?

Q18b. *If applicable:* What local institutional capacities were most important for moving from cleanup to reuse, such as staff expertise, legal authority, developer relationships, zoning flexibility, or community trust?

VI. Redevelopment Process and Bottlenecks

Q19. What were the largest obstacles encountered between grant receipt and completed cleanup?

Q20. What were the largest obstacles encountered between completed cleanup and actual site reuse?

Q21. How important were zoning, permitting, infrastructure, title issues, environmental liability, or site ownership problems in shaping the redevelopment timeline?

Q21a. *If one of these factors was especially important:* Please describe how it affected the timeline or project scope.

Q21b. *If applicable:* Were there delays or unexpected costs that substantially changed the project?

VII. Private Investment, Financing, and Business Formation

Q22. Did cleanup funding change the willingness of developers, lenders, businesses, or property owners to invest near the site? Please describe any examples.

Q23. Have you observed new businesses, business expansions, construction activity, or employment growth connected to the site? Please describe the timing and nature of these changes.

Q23a. *If yes:* Were the economic effects concentrated in particular types of activity, such as construction, housing, retail, manufacturing, logistics, clean energy, public facilities, or community services?

Q23b. *If financing was relevant:* Did local credit conditions or banking relationships affect whether the cleaned site could be redeveloped or reused?

VIII. Housing, Property Values, Fiscal Effects, and Distributional Impacts

Q24. Did the cleanup affect nearby property values, housing development, rents, neighborhood perceptions, local tax revenues, or the municipal tax base? Please describe.

Q24a. *If property values or tax revenues changed:* Did those changes affect local public finances or public service capacity?

Q24b. *If housing development occurred:* Did the project affect housing affordability or the mix of renters, homeowners, or businesses nearby?

Q25. Were there concerns about displacement, affordability, or unequal distribution of benefits from redevelopment? If so, how were those concerns addressed?

Q25a. *If applicable:* Which groups or stakeholders appear to have benefited most from the cleanup and redevelopment process so far?

IX. Targeting and Program Design

Q26. Based on your experience, what types of brownfield sites or communities are most likely to benefit from EPA cleanup grants?

Q27. Are there cases where cleanup grants may be less effective, even when environmental need is high? Please explain.

Q28. What changes to EPA grant scoring, eligible uses, award size, timing, reporting requirements, or technical assistance would make the program more effective?

Q28a. *Optional:* What information should policymakers consider when deciding whether a brownfield site is likely to generate large community, economic, or fiscal benefits after cleanup?

X. Final Reflection

Q29. What is the most important lesson from this project that other communities pursuing brownfield redevelopment should know?

Q29a. *Optional:* Is there anything else about your experience with EPA brownfield cleanup funding that was not captured above?

D Brownfield Geocoding Procedures

We recover a centroid coordinate for each brownfield applicant site and convert that point location into an approximate site boundary that can be linked to datasets described above. For the subset of cleanup grant sites that also received an EPA assessment grant, we use latitude and longitude coordinates reported in the CIMC database, since assessment records contain more precise geolocation information than many cleanup records. For the remaining applicant sites, we geocode the cleaned site address using the Google Maps API and the U.S. Census Geocoder.[6] When both services return valid coordinates, we retain the location after checking that the two points are geographically consistent; when they differ, we prioritize the coordinate that best matches the EPA site name, municipality, and street address.

We then construct site boundary shapefiles using two complementary methods. When Environmental Site Assessment (ESA) Phase II/III or related remediation maps are available, we render the PDF maps as images and use an AI-assisted image extraction pipeline to trace the site boundary and convert the resulting polygon into a georeferenced shapefile.[7] This procedure has the additional advantage of segmenting the

[6] The Census Geocoder uses the Topologically Integrated Geographic Encoding and Referencing (TIGER) database to map addresses to coordinates: https://www.census.gov/programs-surveys/geography/technical-documentation/complete-technical-documentation/census-geocoder.html.

[7] 60% of sites receiving cleanup grants also initially received site assessment grants, which would have resulted in the creation of ESA Phase II/III maps.

contaminated portion of the property, which may be smaller than the full parcel or redevelopment area. For sites without usable ESA maps, we use the centroid and the EPA-reported acreage to construct an area-equivalent search radius around the site. We then collect nearby Cotality tax parcels as of the most recent tax year prior to the first grant award and infer the most likely site boundary from the joint clustering of parcels and structures around the centroid. Building footprints from Microsoft Maps offers an additional non-tax parcel geo-referencing method to construct redevelopment intensity measures.[8]

For the subset of sites with both ESA maps and centroid coordinates, we validate the parcel-based method out of sample by temporarily withholding the map-derived polygon, constructing the parcel-imputed boundary, and comparing the two polygons using overlap, area, and centroid-distance metrics. This validation exercise allows us to quantify boundary error and assess whether measurement error in site geometry is likely to affect our estimates of exposure for nearby establishments, households, and properties.

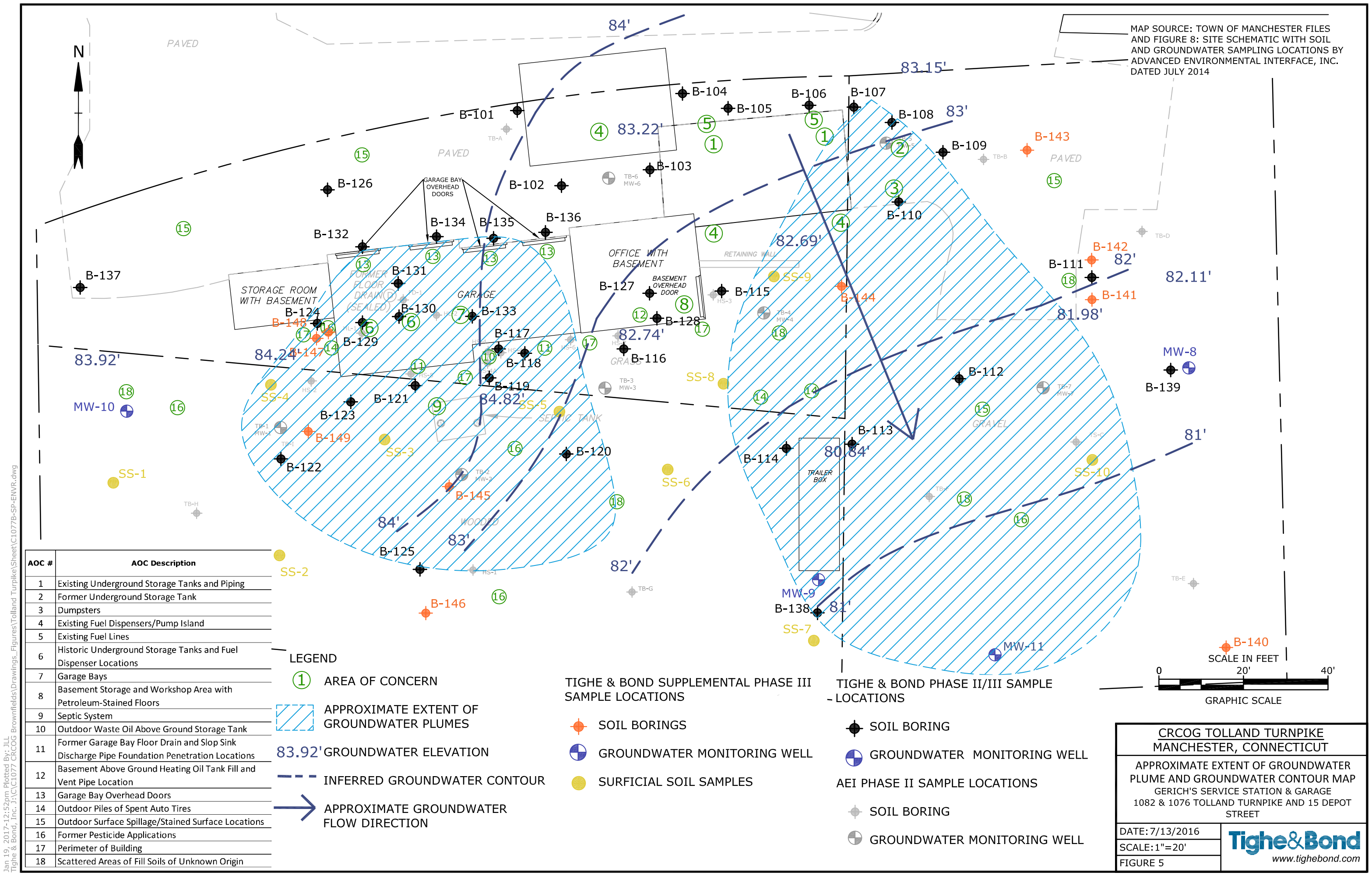
Environmental site assessments (ESAs) follow a staged due-diligence process that generates increasingly spatially precise information about contamination. A Phase I ESA reviews site history, ownership and regulatory records, interviews, site reconnaissance, and historical graphics to identify recognized environmental conditions (RECs) or areas of concern (AOCs). Phase I ESAs conducted with EPA Brownfields Assessment Grant funds must comply with the All Appropriate Inquiries rule at 40 CFR Part 312, for which EPA recognizes ASTM E1527-21 as a compliant standard (EPA, 2026b). A Phase II ESA then tests whether releases occurred by sampling soil, groundwater, and, when relevant, soil vapor at the RECs/AOCs identified in Phase I. A Phase III ESA performs additional investigation to characterize the nature, degree, and three-dimensional extent of contamination, evaluate migration pathways and receptors, and support remediation planning (Bompoti, 2022).

These Phase II and Phase III reports therefore contain the spatial inputs needed for our geocoding exercise. For example, University of Connecticut's Technical Assistance to Brownfields (TAB) Program training materials describe Phase II reports as including sampling maps for soil borings and monitoring wells, while Phase III reports define the distribution and extent of contamination across soil, groundwater, sediment, surface water, soil vapor, or indoor air (Bompoti, 2022). In a Phase III example from Manchester, Connecticut (Figure D.1 and Figure D.2), the report appendix includes a site location map, aerial photograph, sample location map, an approximate extent of areas requiring soil remediation, and an approximate extent of the groundwater plume with groundwater contours (Tighe & Bond, Inc., 2017b). We use these map exhibits as the source material for digitizing contaminated-area polygons, rather than relying only on parcel boundaries or site centroids.

Another advantage to the Phase II/III reports is that the maps demarcate the boundaries of contaminated areas of a property. In cases where the site is large and the contamination is limited to a smaller, contiguous area, the site may put to productive use while simultaneously undergoing decontamination, thereby generating income for the owner which can be used to fund the cleanup. These separate shapefiles allow us to distinguish the role of financing constraints by comparing outcomes for

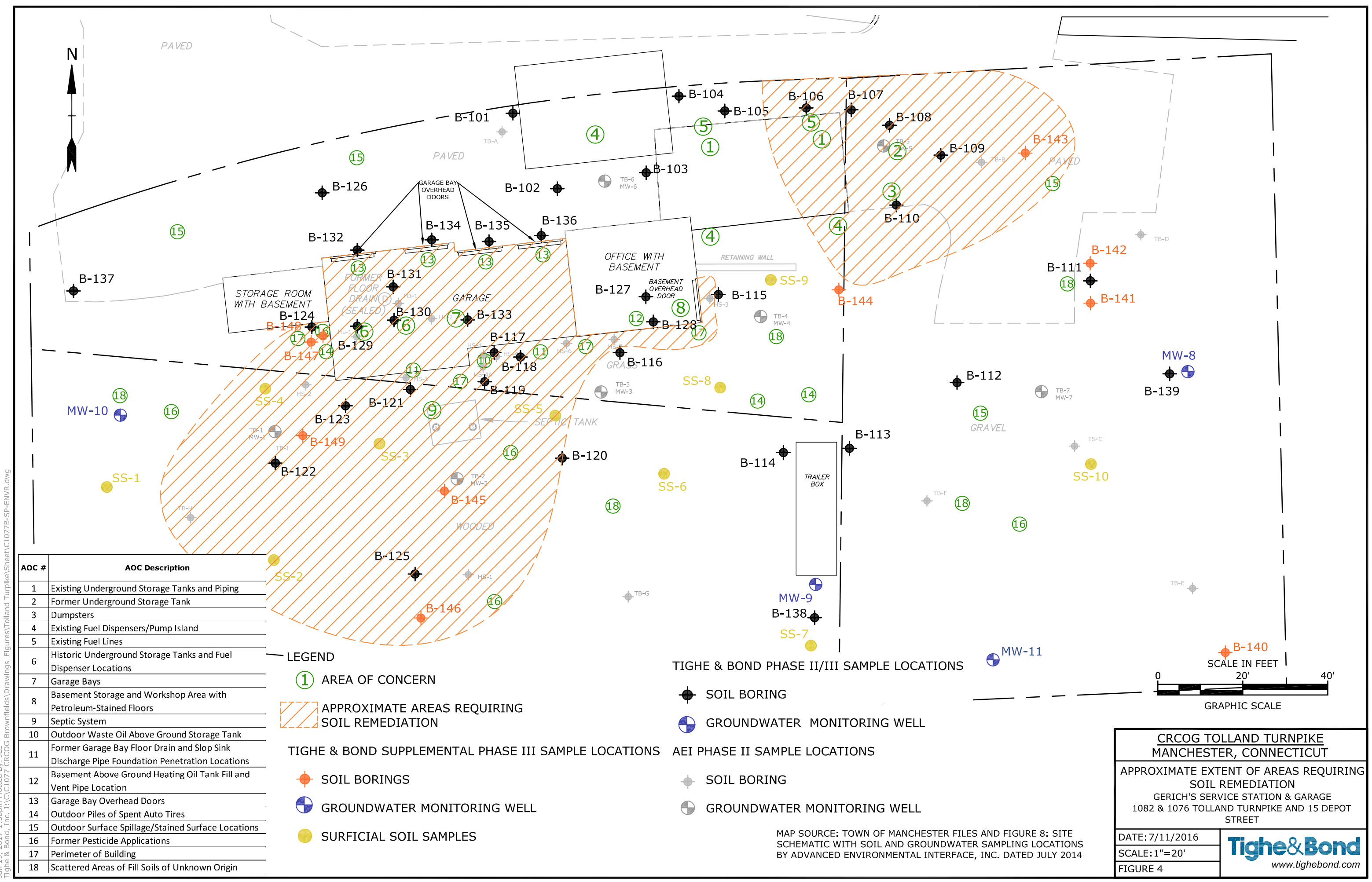
[8] See the Microsoft Building Footprints GitHub repo here: https://github.com/microsoft/USBuildingFootprints?tab=readme-ov-file.

Figure D.1: Phase III Groundwater Plume Map: 1082–1076 Tolland Turnpike, Manchester, CT



Notes: This figure reproduces the Phase III groundwater map for the 1082–1076 Tolland Turnpike brownfield site in Manchester, Connecticut. The map identifies areas of concern, monitoring wells, soil borings, groundwater elevation contours, inferred groundwater flow direction, and the approximate extent of groundwater plumes. In Appendix D, we use maps of this type to illustrate how Phase III environmental reports can be converted into georeferenced polygons for measuring contaminated-site boundaries.

Figure D.2: Phase III Soil Remediation Map: 1082–1076 Tolland Turnpike, Manchester, CT



Notes: This figure reproduces the Phase III soil remediation map for the 1082–1076 Tolland Turnpike brownfield site in Manchester, Connecticut. The hatched areas indicate approximate areas requiring soil remediation, alongside areas of concern and Phase II/III sampling locations. We use these maps as examples of the source documents used to distinguish the contaminated portion of a site from the broader parcel or redevelopment footprint when constructing brownfield shapefiles.

sites that are completely shut down due to contamination with those that are only partially underutilized.

E Robustness of Stacked Regression Discontinuity Results

Table E.1 reports robustness of the post-period pooled effect to alternative sets of fixed effects, weighting, and polynomial order. Each cell contains the arithmetic mean of the ten post-grant event-time coefficients $\widehat{\beta}_{+1}, \ldots, \widehat{\beta}_{+10}$. The baseline specification row (1) has a pooled employment effect of 6.4% (p-value $<$ 0.05) and an entry rate effect of 1.6 p.p. (p-value $<$ 0.05). Row (2) augments the baseline with future-application-history controls; the employment estimate decreases modestly to 4.9% remains significant, while the entry rate coefficient remains unchanged. Row (3) implements un-weighted regressions; the point estimate remains positive and statistically significant for employment, while the effect on entry rate becomes statistically insignificant. Row (4) replaces the state$\times$cohort$\times$year fixed effects with cohort$\times$year fixed effects only; estimates barely change for both employment and entry rate. Row (5) estimates unweighted regressions with cohort$\times$year fixed effects.

Rows (6) and (7) vary the polynomial degree. Switching to a linear running variable ($p = 1$) yields insignificant estimates for both employment and entry rate, but the treatment and control groups are not strongly balanced on *ex ante* observables (see Table 2). A cubic polynomial ($p = 3$) produces estimates of 4.7% and 1.9 p.p., closely mirroring the quadratic baseline. Finally in row (8), we consider Poisson regressions given that employment and entry are count variables (Cohn et al., 2022); this leads to slightly larger employment effects and substantially larger entry rates while accounting for the fact that some site-cohort-year cells experience no new firm entry. Taken together, the results show that across all specifications for both outcome variables the point estimates are positive. However, the statistical significance of the effect on entry rate is noisier due to the limited coverage of small firms in the *D&B* data.

Tables E.2 and E.3 examine sensitivity to how establishments located near both a treated and a control site are handled. The three columns correspond to our preferred nearest-site assignment (Column 1), a keep-both rule that counts each overlapping establishment for all sites within whose scope it falls (Column 2), and an upper-bound specification that assigns every overlapping establishment to the treated side, what we refer to as "treat-as-treated" (Column 3). Pre-period coefficients are small and lack a discernible trend across all three contamination treatments for both outcomes, supporting the identifying assumption of parallel pre-trends.

For log employment (Table E.2), the nearest-site and keep-both columns track one another closely throughout the event window. For instance, at $k = +6$ (95th percentile of cleanup duration times in Figure B.2), the nearest-site estimate is 4.5 log points and the keep-both estimate is 4.4 log points, with overlapping confidence intervals. The treat-as-treated column yields somewhat attenuated point estimates. At $k = +10$, nearest-site and keep-both estimates converge to roughly 8 log points, while the treat-as-treated estimate is 3.4 log points and statistically insignificant.

For the new firm entry rate (Table E.3), the nearest-site and keep-both estimates again move in lockstep through the post-grant window. The treat-as-treated column produces larger point estimates throughout the post period. This is by construction, because removing overlapping establishments from the control aggregate mechanically raises the treated-to-control contrast. Therefore, the treat-as-treated estimates should be interpreted as an upper bound. The close agreement between the two assignment-neutral columns (nearest and keep-both) for both outcomes provides reassurance that the different treatments of overlapping establishments does not materially distort our baseline estimates.

Table E.1: Stacked Dynamic RDD Specification Robustness

Specification	Log employment	Entry rate
(1) Baseline (nofuture, weighted, state-cohort-yr FE)	0.057^{**}	0.015^{**}
	(0.024)	(0.007)
(2) Add M_n controls	0.043^{**}	0.016^{**}
	(0.019)	(0.008)
(3) Unweighted	0.044^{*}	0.008
	(0.026)	(0.007)
(4) Cohort-year FE only (no state)	0.063^{***}	0.021^{**}
	(0.023)	(0.009)
(5) Unweighted cohort-year FE	0.061^{**}	0.008
	(0.025)	(0.007)
(6) Baseline, linear running variable ($p = 1$)	0.012	0.003
	(0.021)	(0.005)
(7) Baseline, cubic running variable ($p = 3$)	0.043	0.019^{**}
	(0.026)	(0.008)
(8) Poisson (PPMLE)	0.057^{***}	0.112^{**}
	(0.022)	(0.048)

Notes: Each cell reports the pooled post-period effect on employment or the entry rate, estimated as the arithmetic mean of the ten event-time coefficients $\hat{\beta}_{+1}, \ldots, \hat{\beta}_{+10}$, based on equation (2). Standard errors are Delta Method standard errors derived from the estimated variance–covariance matrix of the full set of event-time coefficients. In all rows, we adopt the nearest-site contamination correction to breaks ties in cases where an establishment is located near both a treated and control brownfield site. Each specification sets the radius to $r \leq 3$ miles, with a $p = 2$ local quadratic polynomial. In the final row, we estimate Poisson regressions using Poisson-Pseudo Maximum Likelihood (PPMLE). Modeling employment and entry as Poisson-distributed variables accounts for the fact that some DnB establishments report zero employment in some years, while also reducing possible bias from assuming log normality of count variables (Cohn et al., 2022). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table E.2: Overlapping Establishments: Log Employment

Event time	Nearest site	Keep both	Treat as treated
Pre-period			
$k = -5$	0.008	0.010	0.021
	(0.019)	(0.019)	(0.023)
$k = -4$	0.025	0.015	0.039*
	(0.018)	(0.015)	(0.021)
$k = -3$	0.031*	0.020	0.033**
	(0.017)	(0.015)	(0.016)
$k = -2$	0.004	-0.003	0.002
	(0.019)	(0.016)	(0.016)
$k = -1$	0.006	0.001	0.004
	(0.018)	(0.015)	(0.016)
Post-period			
$k = +1$	0.031	0.031	0.046
	(0.023)	(0.022)	(0.037)
$k = +2$	0.064**	0.054**	0.067*
	(0.028)	(0.024)	(0.040)
$k = +3$	0.043	0.043*	0.070*
	(0.027)	(0.025)	(0.042)
$k = +4$	0.068**	0.051**	0.075*
	(0.027)	(0.025)	(0.041)
$k = +5$	0.050*	0.045*	0.060
	(0.027)	(0.024)	(0.040)
$k = +6$	0.046	0.045*	0.037
	(0.029)	(0.026)	(0.037)
$k = +7$	0.086**	0.106***	0.101**
	(0.038)	(0.032)	(0.045)
$k = +8$	0.036	0.032	0.025
	(0.035)	(0.033)	(0.046)
$k = +9$	0.070*	0.079**	0.053
	(0.039)	(0.036)	(0.048)
$k = +10$	0.073*	0.070**	0.029
	(0.038)	(0.035)	(0.046)
Observations	21,303	21,307	21,294

Notes: This table examines whether the employment results are robust to alternative treatments of establishments located within the catchment area of both a treated and a control site in the same cohort. *Nearest-site* (Column 1, baseline): each overlapping establishment is assigned to the geographically closest brownfield site. *Keep-both* (Column 2): overlapping establishments are counted towards all sites within whose scope they fall, so the same establishment may contribute to both the treated and the control aggregate. *Treat-as-treated* (Column 3): overlapping establishments are removed from the control side and retained on the treated side, providing an upper bound on treatment effects. All specifications include site×cohort and state×cohort×year fixed effects, a quadratic polynomial in the running variable with separate slopes above and below the cutoff, and post-period reapplication controls. Observations are weighted by the pre-first-application mean establishment count within scope. Robust standard errors clustered by site in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table E.3: Overlapping Establishments: New Firm Entry Rate

Event time	Nearest site	Keep both	Treat as treated
Pre-period			
$k = -5$	0.001	0.001	-0.003
	(0.006)	(0.006)	(0.007)
$k = -4$	-0.004	-0.004	-0.005
	(0.006)	(0.006)	(0.006)
$k = -3$	-0.009**	-0.009**	-0.010**
	(0.005)	(0.005)	(0.005)
$k = -2$	-0.003	-0.002	-0.004
	(0.006)	(0.006)	(0.006)
$k = -1$	0.002	0.002	0.003
	(0.008)	(0.007)	(0.008)
Post-period			
$k = +1$	-0.003	-0.004	-0.006
	(0.008)	(0.008)	(0.009)
$k = +2$	0.003	0.002	0.006
	(0.009)	(0.009)	(0.010)
$k = +3$	-0.007	-0.012	-0.001
	(0.008)	(0.008)	(0.010)
$k = +4$	0.005	-0.001	0.014
	(0.010)	(0.009)	(0.014)
$k = +5$	0.003	-0.003	0.016
	(0.010)	(0.010)	(0.013)
$k = +6$	0.013	0.007	0.032**
	(0.008)	(0.008)	(0.013)
$k = +7$	0.016*	0.008	0.037***
	(0.009)	(0.009)	(0.012)
$k = +8$	0.026**	0.021*	0.057***
	(0.012)	(0.012)	(0.015)
$k = +9$	0.023	0.020	0.052***
	(0.017)	(0.017)	(0.019)
$k = +10$	0.071***	0.066***	0.109***
	(0.022)	(0.021)	(0.022)
Observations	22,831	22,831	22,831

Notes: This table examines whether the firm entry results are robust to alternative treatments of establishments located within the catchment area of both a treated and a control site in the same cohort. The entry rate equals the number of establishment entries within scope divided by the site's mean establishment count in the three years prior to first application (predetermined denominator). *Nearest-site* (Column 1, baseline): each overlapping establishment is assigned to the geographically closest brownfield site. *Keep-both* (Column 2): overlapping establishments are counted towards all sites within whose scope they fall. *Treat-as-treated* (Column 3): overlapping establishments are removed from the control side and retained on the treated side (upper bound). All specifications include site×cohort and state×cohort×year fixed effects, a quadratic polynomial in the running variable with separate slopes above and below the cutoff, and post-period reapplication controls. Observations are weighted by the pre-first-application mean establishment count within scope. Robust standard errors clustered by site in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.