Peer effects in voluntary environmental policies: An application to urban water quality*

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Abstract

Stormwater runoff is a growing source of urban water pollution, costing cities billions of dollars. We investigate peer effects in a voluntary residential green stormwater infrastructure program that mitigates stormwater runoff. Our identification strategy exploits households' relative position in eligible sewersheds that generates plausibly exogenous variation in eligible peers. Peer adoption causes a 0.2% increase in the annual adoption probability, a 66% increase relative to the mean. According to our calculations, the policy reduced compliance costs by $85–235 million for the Seattle metropolitan area, of which roughly 40% is due to peer effects.

Key Words: peer effects, water quality, green stormwater infrastructure, voluntary adoption
JEL Codes: Q25, Q52, Q53, L95

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1 Introduction

The majority of the financial costs of environmental regulation in the United States and other high-income countries is spent on managing air and water pollution. Research shows that investments to reduce air pollution improve human health and generate benefits that greatly exceed the compliance costs (Currie and Walker, 2019). Despite massive spending of roughly $35 billion per year, regulation to improve surface water quality has not consistently produced positive net benefits (Keiser and Shapiro, 2019a; Keiser et al., 2019; Keiser and Shapiro, 2019b). One explanation for the poor performance of water quality regulation is that water quality benefits are either unmeasured, or incorrectly estimated. A second, however is that existing policies and investments miss the most cost-effective opportunities to improve water quality.

Most existing water quality regulation focuses on "point source" pollution coming directly from industrial facilities and wastewater treatment plants. Since point-sources are directly regulated, and the regulations have become more stringent over time, it is likely that cost-effective mechanisms to reduce point source pollution have been exhausted. Cost-effective improvements in water quality likely require using "best management practices (BMPs)" in agricultural and urban areas on "non-point" sources. Research shows that non-point source solutions such as restoring wetlands and installing forest buffers on agricultural land are much more cost-effective than controlling point-source pollution for nitrogen (Kauffman, 2018). Economists argue for water quality trading programs based on large differences in cost-effectiveness across policies, where the low-cost options typically require voluntary adoption of BMPs (Jones et al., 2010; Olmstead, 2010; Shortle, 2013; Fisher-Vanden and Olmstead, 2013). The difficulty for regulators is that most types of non-point sources are unregulated under the Clean Water Act and therefore require incentivizing voluntary adoption. The challenge across a host of settings, however, is that voluntary adoption of water quality policies has been quite low despite generous financial incentives (Shortle et al., 2012; Stephenson et al., 2022; Read and Wainger, 2023). Understanding how to promote voluntary adoption of BMPs is therefore key to improving water quality in a cost-effective way.

In this paper, we investigate the role of peer effects in the adoption of a voluntary rebate policy for green stormwater infrastructure (GSI) designed to reduce stormwater runoff. In cities with a sewer system that combines flows from sanitary sewers and stormwater from street drains, large storms can overwhelm the collection system and the wastewater treatment plant. To avoid this, these combined systems are designed to prevent damage by allowing an automatic discharge straight to waterbodies. Because these "combined sewer overflow" (CSO) events discharge untreated sewage into water bodies, they are a violation of the Clean Water Act. Over 700 cities and municipalities across the U.S. have combined sewer systems (GAO, 2023)[1] and the National Governors' Association estimated that the costs of outstanding investments required to achieve compliance with the Clean Water Act was $67 billion in 2016.[2] The program we evaluate, called

[1]Stormwater runoff is the only source of water pollution that is currently growing in the U.S.

[2]See report here: https://www.nga.org/publications/balancing-stormwater-infrastructure-costs/.

RainWise, subsidizes two common forms of GSI - rain gardens and cisterns - on residential properties to reduce stormwater flows to the sewer system. A rain garden is a depressed area in the landscape that collects rain water from impervious surfaces and is planted with flood-tolerant plants. A cistern is a water storage device attached to the gutter system of a roof's home, sometimes called a "rainwater tank" or "rain barrel". (Examples of these are visible in the pictures in Appendix Figure A.1). Our research estimates how peer adoption affects participation in the program. Peer effects are important because they provide insights into adoption decisions, amplify voluntary adoptions, and increase the overall cost-effectiveness of voluntary programs.

Causal estimation of peer effects is notoriously challenging given the presence of contextual effects and correlated unobservables (Brock and Durlauf, 2001; Manski, 1993; Moffitt et al., 2001; Soetevent, 2006). To address these issues, our identification strategy relies on variation in a household's relative position within sewersheds that determine eligibility for the RainWise program. Sewersheds, which are selected based on hydrologic concerns, determine the ultimate location of excess stormwater flows. Thus, their boundaries are driven by topography and engineering models that are typically not correlated with economically meaningful demographic characteristics that influence participation in the program. This creates exogenous variation in the number of eligible peers, which, in turn, strongly affects the number of peer adoptions. Specifically, households in the middle of an eligible sewershed have more eligible peers than households located at the sewershed boundary. We use this exogenous variation in the number of eligible peers as an instrument for peer adoption to correct biases that are likely present in standard models.

Results from our instrumental variables (IV) model show large and significant peer effects. Specifically, we find that an additional peer adoption increases the annual probability that a household adopts RainWise GSI by 0.2%. This increase is large relative to the baseline annual adoption probability of 0.3%, representing a 66% increase in adoptions due to peer effects. We also find that without controlling for endogeneity, OLS significantly underestimates peer effects. This is likely due to the fact that areas with more eligible peers are *negatively* correlated with factors that could increase adoption, such as poor drainage.

Our results are robust to different definitions of the peer group and modeling specifications. In fact, peer adoptions appear to be one of the only predictors of voluntary adoption. The patterns of heterogeneity indicate that peer effects are driven by visibility. Rain gardens are more likely to be visible from the street, and they are the only form of GSI that generates peer effects. Peers who adopt cisterns – which are typically installed at the side or back of a house – have no impact on neighbors' adoption. We also incorporate Google Street View data to estimate whether visibility is a primary mechanism. While the identifying assumptions are more restrictive using the visibility data, the evidence suggests that visibility is an important mechanism. This is corroborated using proxies for visibility including corner lots, properties near public transit, and walkability. As expected, we find that peer effects are stronger when an adopter is part of a smaller peer group and peer adoption is more recent. This indicates that peer effects dissipate across both time and space.

We use the results to assess the cost effectiveness of RainWise compared to alternative methods to reduce stormwater pollution. RainWise is 5-13 times more cost effective than conventional infrastructure strategies such as CSO treatment facilities and underground storage facilities. By the end of 2020 only 2,000 of the roughly 65,000 eligible households have signed up, despite subsidies covering roughly 90% of the costs, so encouraging adoption has large economic consequences. We estimate that RainWise saved Seattle $85–$235 million by avoiding capital expenditure on conventional stormwater infrastructure, representing 6-15% of total compliance costs of almost $1.5 billion. These costs represent cumulative investment spending over the course of the consent decree as opposed to annual costs. which was Since peer effects account for 37% of all adoptions, this implies that $30–$80 million dollars in savings can be attributed to peer adoptions. Since peer adoptions have a multiplier effect spurring subsequent adoptions there are large benefits from encouraging earlier adoptions. The net present benefits of an adoption compounds at a rate of 21% due to peer effects. Our cost-effectiveness analysis is certainly not a welfare analysis: we do not consider the benefits of improving water quality but focus instead on how GSI affects the cost of meeting regulatory requirements. We also do not consider private benefits of GSI such as amenity values or reduced nuisance flooding. Stated preference research shows that consumers are willing to pay for GSI (Brent et al., 2017; Ando et al., 2020; Wang et al., 2022), although revealed preference research using property values finds mixed results (Zhang et al., 2015; Irwin et al., 2017).

This paper contributes to several research areas. First, we document how peer effects operate in a voluntary policy to improve water quality. Peers influence consumer behavior in a wide range of settings, including school and work (Duflo and Saez, 2003; Sacerdote, 2001; Sorensen, 2006; Graham, 2008), health (Munshi and Myaux, 2006), and housing markets (Harding et al., 2009; McCartney and Shah, 2021; Towe and Lawley, 2013). Understanding peer effects is especially important where peer behavior can amplify positive or negative externalities, such as in many environmental settings. Peer effects have been shown to affect the adoption of solar panels (Bollinger and Gillingham, 2012; Graziano and Gillingham, 2015; Bollinger et al., 2021; La Nauze, 2021), purchases of hybrid vehicles (Narayanan and Nair, 2013; Heutel and Muehlegger, 2015), water consumption (Bollinger et al., 2020), and energy use (Wolske et al., 2020). Estimating peer effects in water quality is an important contribution to the aforementioned literature. Solar panels and water efficient landscaping have a clear private benefit by reducing utility bills. In our setting, the private financial benefit is less obvious and the decision may rely on pro-social motivations. Additionally, we employ a novel identification strategy exploiting the number of eligible peers that is applicable in other settings with sufficient spatial variation in eligibility.

The research also contributes to the economic analysis of GSI for stormwater mitigation. Most municipalities have addressed these violations by increasing investments in traditional "gray" methods of stormwater management such underground storage facilities or investing to separate the sanitary and stormwater sewer systems . However, cities are increasingly expanding their investments in decentralized methods of stormwater control as a means of expanding their portfolio and controlling costs (Hopkins et al., 2018). Ando and Freitas (2011) estimate consumer demand for residential GSI using aggregate data and a survey. Other studies use stated preference methods to estimate the nonmarket values of GSI (Londoño Cadavid and Ando, 2013; Newburn and Alberini, 2016; Brent et al., 2017; Ando et al., 2020). Brent et al. (2022) investigate the distributional effects of GSI policy, which is particularly relevant in the presence of peer effects. Our results show that in the presence of strong peer effects the initial set of adopters will have a larger impact on the spatial distribution of adoption. Peer effects offer strategies to combat the challenge of engaging underrepresented minorities in programs (Brent et al., 2022), which affects progress on environmental justice. Policies that amplify peer effects, such as social comparisons highlighting peer adoption, may help spur further adoption. Emphasizing adoption in high priority areas early on will pay dividends through further peer adoptions.

2 Background & Setting

Stormwater is precipitation that runs off streets, buildings, and other impervious surface into a sanitary sewer system or directly into water bodies . This is primarily a problem in urban areas, where water running over impervious surfaces can pick up pollutants such as fertilizer and pesticides, oils and grease, and other chemicals before it is discharged directly into waterways. Stormwater creates an additional challenge in cities with a combined sewer system where CSO events violate the Clean Water Act (CWA), and municipalities enter into consent decrees with the EPA to reduce untreated stormwater from entering water bodies.[3]

As of 2017, 41 municipalities had consent decrees about CSO violations. The total estimated cost of compliance was over $31 billion (2017 dollars); ten municipalities have estimated compliance costs of over $1 billion dollars. Achieving compliance requires a combination of large conventional ("gray") infrastructure and decentralized GSI approaches. Storage tunnels store untreated water during precipitation events and then discharge it to wastewater treatment facilities after the rain event ends. Stormwater treatment plants are similar to wastewater treatment facilities but perform less stringent treatment and only operate on rainy days /footnoteStormwater treatment facilities, or "CSO treatment facilities" provide primary filtration of solid waste and disinfection using chlorine and the dechlorination of filtered combined stormwater. They are located at or very near the CSO outfall so that stormwater flows can be directed to the plant rather than discharged. More information is available at https://kingcounty.gov/depts/dnrp/wtd/system/cso-facilities.aspx. GSI works on a decentralized scale by using nature-based solutions to retain water in the environment, thereby preventing or slowing water from entering the combined sewer system. Many cities with consent decrees are using a combination of both conventional infrastructure and GSI to meet their statutory obligations to mitigate stormwater runoff (Hopkins et al., 2018).

[3]The acronym CSO can also be used to define the actual location of the event, the Combined Sewer Outfall (CSO). We later use the term "CSO basin" to refer to the hydrologic region of the city that feeds each outfall location.

We study this problem in the context of the Seattle metropolitan area, where from 2006 to 2010 Seattle and King County collectively released 1.1 billion gallons of untreated stormwater and sewage annually into water bodies from outfall locations, leading to each entering into a consent decree with the EPA in 2013 (EPA, 2013). Seattle Public Utilities (SPU) and King County Wastewater Treatment Division (WTD) own and operate separate parts of the drainage network, and are therefore legally responsible for overflow events at outfalls under their control.[4] At its inception, the Seattle consent decree mandated reducing annual stormwater runoff by 200 million gallons, or 99% of the total annual untreated stormwater runoff. King County's consent decree mandated an annual reduction of 666 million gallons or 95-99% of total untreated runoff. The cost estimates were $600 million dollars for Seattle and $711 million for King County EPA (2017). Revisions to the cost estimates place mitigation costs for King County at $860 million and the total stormwater mitigation at 855-900 million gallons .[5]

SPU and WTD cooperate closely in developing and implementing stormwater mitigation plans to bring the region into compliance. Their jointly-devised stormwater management plan proposed using GSI to reduce 700 million out of the 1.1 billion gallons of stormwater mitigation. One pillar of the GSI portion of the t plan is the RainWise program, which started in 2008. It provides rebates for private residents in single-family homes to install cisterns, rain gardens, or both (KCWTD, 2021). In addition to RainWise, SPU and WTD install and manage GSI projects on public land and mandate GSI for new buildings or renovations that increase impervious surface through the building code. Retention ponds, green streets, and bioswales are all examples of public GSI (SPU, 2021).

RainWise rebate eligibility is restricted to 20 distinct CSO basins, each of which drain to a specific outfall location (Figure A.2).[6] A sewershed is an area that drains to a specific location through the sewer network, and when that location is a combined sewer outfall location, then the sewershed is also a CSO basin. All sewersheds in our study location drain to a combined sewer outfall. SPU and WTD refer to the sewersheds as CSO basins, so for the remainder of the paper we will use the term CSO basin. Although King County WTD and SPU jointly fund RainWise, all eligible areas are within the Seattle city limits. Most households within a basin are eligible for a RainWise cistern. However, additional requirements for rain gardens include land stability (i.e., non-landslide prone area), adequate drainage, and sufficient distance from contaminated sites, landfills, and underground storage tanks. Rain garden-eligible homes are therefore a subset of cistern-eligible homes.

Participation in RainWise has been low. As of December 2020, only 2,005 of the 65,000 eligible properties had participated in the program's first 12 years. After cleaning the data and removing households that had missing installation years or incorrect spatial data, we use 1915 adoptions

[4]The region's wastewater treatment plants are owned and operated by King County.

[5]This is based on documentation from the EPA at https://www.epa.gov/enforcement/seattle-washington-and-king-county-washington-settlement, accessed 12/2/2024

[6]We requested information on how the CSO basins were selected as part of the public records request and did not receive any formal documentation on how these basins were selected. Conversations with staff indicate that the sewersheds chosen drained to outfalls that had numerous overflow events.

in our sample. Of these, 36% installed a rain garden, 49% installed a cistern, and 15% installed both a rain garden and a cistern. The low participation rate may partly be due to difficulties in financing the substantial installation costs. Although the program covers 90% of the cost (approximately $4,355 per household) (SPU, 2020), the rebate is paid only after installation and verification. This prompted the creation of the RainWise grant program in 2019, which offers additional financial assistance to low-income households (SPU, 2020). At the time of writing RainWise is considering increasing the size of the rebate.[7]

3 Data

We create a panel dataset on residential single-family households within 20 distinct CSO basins that were eligible for the RainWise program from 2010 through 2020.[8] Parcels comprise the cross sectional unit and we aggregate all adoptions to the calendar year.[9]

3.1 Data Sources

The primary RainWise program data was provided by SPU through a City of Seattle Public Disclosure Request. We obtained parcel characteristics from the King County Assessor's office. The data package provided by the City of Seattle contains spatial parcel-level eligibility data. This includes shapefiles for the eligible CSO basin boundaries and parcel-level data on cistern and rain garden eligibility within eligible CSO basins.[10] Figure A.2 shows RainWise eligibility variation by CSO boundary and the year that each CSO basin opened and, if applicable, closed. The RainWise participation data include a parcel identification number (PIN), date of installation, type of GSI (rain garden, cistern, or both), total cost, rebate amount, and the gallons of stormwater retained. To assess the validity of our instrument (described more below), we use parcel data from King County (organized by PIN) that includes property characteristics such as the address, property type, lot size, bedroom, and date of last renovation that required a building permit. We use Google Street View data to identify visible features of RainWise adoptions.

3.2 Data Preparation

Our primary dataset is a panel dataset on RainWise adoption and peer adoptions. We first restrict our sample to single-family residential households that were ever eligible for RainWise. Next, we

[7] More information on the rebate change, which comes after all of the data in this paper, is available at: `https://700milliongallons.org/rainwise/rebate-process/compare/`.

[8] There were very few adoptions from 2008 to 2010, and SPU did not collect detailed information including installation date, so we focus on the program data beginning in 2010.

[9] We have data on the date of adoption that allows us to choose the temporal aggregation. We choose an annual aggregation since we would have very few adoptions per unit for finer temporal units such as months or quarters.

[10] Although all eligible CSO basins are within the Seattle city limits, RainWise is jointly managed by SPU and King County WTD with each organization responsible for specific CSO basins. SPU provided parcel level eligibility for the 18 active CSO basins and King County provided parcel data for the two CSO basins they managed that are currently closed to eligibility .

define a peer group for each eligible household to construct our instrument (discussed in more detail in Section 4). We assume that a household's proximate neighbors are more likely to be in their peer group. Using spatial proximity is a common way to define peer groups and assumes that closer neighbors are more visible and/or more likely to be in a household's social network (Bollinger et al., 2020; Topa, 2001; Manchanda et al., 2008; McShane et al., 2012; Narayanan and Nair, 2013).

Our primary peer group is defined as the closest 100 homes within a 0.5-mile radius of each eligible RainWise household. Peers are limited to the nearest 100 households to capture common neighborhood interactions (e.g., traveling behavior, shared schools or recreation areas) (Kim et al., 2018). We use a variety of alternative peer group definitions to test the sensitivity of the results to our peer group definition including varying the spatial radius and changing the number of nearest neighbors (e.g. closest 20 neighbors). We also vary the the temporal dimension of the peer group since peer effects may wane over time. Figure 1 shows two examples of households' peer groups and how we leverage eligibility for our identification strategy.[11] While we restrict the sample to eligible households, an eligible household's peer group can contain ineligible households. The presence of these ineligible peers is central to our identification strategy described below.

We use the set of peer households to create two key variables for each household: the number of RainWise-eligible peers and the number of peer adoptions. Adoptions and eligibility change over time so we create an annual panel dataset from the sample of eligible homes. The eligible peers variables are the number of eligible peers (conditional on a peer group definition) in any given year. We create variables for the number of eligible peers based on two definitions of eligibility. As described above there are more stringent eligibility requirements for rain gardens. The broadest eligibility definition includes households who are eligible for any type of RainWise. Since almost all households in eligible CSO basins can install cisterns, we call these "cistern-eligible" households. A subset of cistern-eligible households are also eligible to install rain gardens because their property had appropriate drainage, slope, etc. The second definition of "rain garden-eligible" households are eligible to install both rain gardens and cisterns. We create variables for the number of cistern-eligible peers and rain garden-eligible peers. The peer adoption variable is the cumulative number of households that adopted RainWise within a peer group at the start of the year. For example, if a household had a peer that adopted RainWise in February 2012 this would register as a peer adoption starting in 2013. This ensures that we do not assign a peer adoption prior to the adoption decision. We create variables for GSI-specific peer adoption: any GSI, rain garden, cistern, and both. As shown in Figure A.2 eligibility changes over time and across space. We restrict the sample to eligible homes since these are the households that face an adoption decision, which results in an unbalanced panel.

Since we have the address of each RainWise adoption we use publicly available Google Street View (GSV) data to measure the visibility of RainWise adoption. For each RainWise adoption,

[11] Figure 4 shows an example of the peer group for a single representative household.

three different raters accessed GSV images taken after the installation date, code whether the property's GSI installation was visible, and whether it had a RainWise sign (see Figure A.1 in the appendix for examples of images with a RainWise sign and visible GSI). The raters did not know the type of GSI the address adopted, only the year. Multiple years of street view data allowed the raters to collect data on how long the RainWise sign was present. The details of the GSV data processing are available in the Appendix D.[12]

3.3 Summary Statistics

The summary statistics for the adoption data are shown in Table 1. We show the summary statistics for cistern-eligible households in panel (a) and rain garden-eligible households in panel (b). The annual adoption probability for any RainWise system for eligible households was 0.3% among the cistern-eligible sample and 0.4% among the rain garden-eligible sample. For our base peer group specification (100 peers within 0.5 miles), the average cistern-eligible home had 90 cistern-eligible peers and 53 rain garden-eligible peers. In the rain garden-eligible sample there were 90 cistern-eligible peers and 76 rain garden-eligible peers. Using our smallest definition of a peer group (closest 20 homes) there are slightly less than 19 cistern-eligible peers in both samples, 11 rain-garden eligible peers in the cistern-eligible sample, and 17 rain-garden eligible peers in the rain garden-eligible sample. On average, 2.3 of the 100 closest peers participated in RainWise among the cistern-eligible sample. Rain garden-eligible households had an average of 2.9 peer adoptions among their 100 closest neighbors. In total, 65,402 households are eligible for some type of RainWise installation: 27,652 are eligible for cisterns only, and 37,750 are also eligible for a rain garden and/or a cistern.[13]

Visibility varied by GSI type: 68% of rain gardens, 41% of cisterns, and 78% of homes with both rain gardens and cisterns were visible in GSV images.[14] Roughly one-third of households adopting RainWise placed a RainWise sign on their property long enough to be captured by GSV. Sign placement does not vary substantially by GSI type. A RainWise sign is likely the most salient form of visibility because it not only informs neighbors about the GSI, but also about the RainWise program. When household did place a RainWise sign it remained visible to coders for an additional 1.2 years on average.[15]

[12]The data present interesting opportunities, but also several challenges. First, there were some discrepancies in the raters coding of street view data. These were resolved by hand checking any discrepancies, but there is still some subjectivity to the coding which may introduce measurement error. Additionally, the data are not available every year so it is possible that a household put up a sign and then took it down before GSV could capture it. Conditional on the data availability households kept a sign up for a little over one year after the adoption year, and there were images roughly every two years. Lastly, sometimes the image was obstructed by cars or other objects that prevented accurate coding.

[13]Additionally, two eligible basins became ineligible due to progress in reducing CSO events. A property is ineligible for a RainWise rebate if it already has a stormwater detention facility on-site, which was mandated for certain properties when the stormwater code was updated in 1980 (Farmer, 2023).

[14]Table A.2 in the Appendix displays summary statistics for sign placement and visibility.

[15]Given that GSV images are available roughly every two years, it is possible that a sign was removed in a year without a GSV image. If we only observe a sign in one image we assume that it is removed that same year.

Table 1: Summary Statistics

(a) Cistern Eligible Sample
Eligible for Cisterns and/or Rain Gardens

	Mean	SD	N
Adoption			
Any	0.029	0.169	65402
Rain Garden	0.011	0.102	65402
Cistern	0.015	0.120	65402
Both	0.004	0.066	65402
Peer Adoption			
100 Peers	2.311	2.619	65402
20 Peers	0.481	0.868	65402
Peer Eligibility			
100 Eligible (CS)	89.706	15.575	65402
100 Eligible (RG)	52.678	35.144	65402
20 Eligible (CS)	18.584	2.926	65402
20 Eligible (RG)	10.793	8.180	65402

(b) Rain Garden Eligible Sample
Eligible for Cisterns and Rain Gardens

	Mean	SD	N
Adoption			
Any	0.038	0.190	37750
Rain Garden	0.017	0.129	37750
Cistern	0.014	0.117	37750
Both	0.007	0.082	37750
Peer Adoption			
100 Peers	2.928	2.991	37750
20 Peers	0.615	0.988	37750
Peer Eligibility			
100 Eligible (CS)	90.425	14.972	37750
100 Eligible (RG)	76.092	21.416	37750
20 Eligible (CS)	18.637	2.916	37750
20 Eligible (RG)	16.888	4.125	37750

Note: Any is the annual probability of RainWise adoption. Rain Garden, Cistern, and Both are the annual probabilities of adopting each specific form of GSI. "100 Peers" is the number of the 100 closest neighbors (within 0.5 miles)that adopted. "20 Peers" is the number of adopters among the 20 closest neighbors. Similarly, "100 Eligible (CS)" is the number of 100 closest neighbors within 0.5 miles that are eligible for cisterns and/or rain gardens. "100 Eligible (RG)" is the number of 100 closest neighbors that are eligible for both cisterns and rain gardens. Panel (a) describes the data for parcels eligible for at least a cistern and panel (b) describes the data for parcels which are also eligible for rain gardens.

4 Methodology

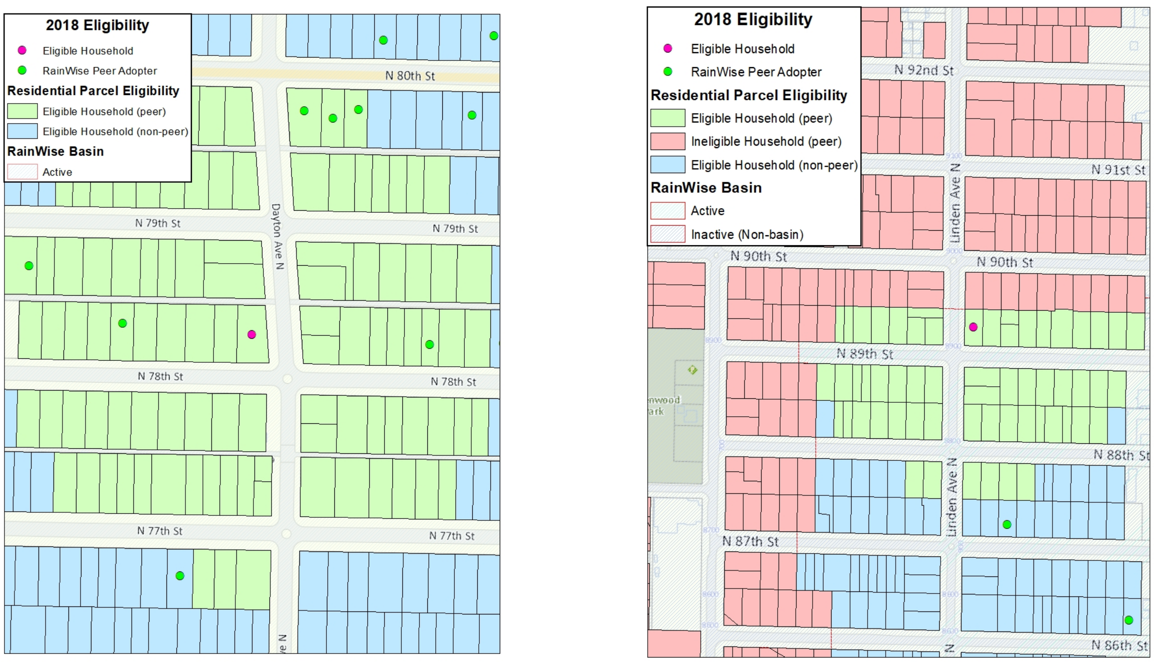
4.1 Identification Strategy

Our identification strategy makes use of the RainWise program's spatial eligibility boundaries. We assume that a household's *relative location* within an eligible CSO basin results in plausibly exogenous variation in the number of eligible and ineligible potential peers. The logic behind our identification strategy is that homes with more eligible peers will ultimately have more peers that sign up. For instance, a household located in the center of an eligible CSO basin will have more eligible peers than a household located near the eligibility boundary. Given that the City of Seattle selects eligible CSO basins for hydrologic reasons, we assume that the CSO basin boundaries are uncorrelated to household demographics or traditional neighborhood boundaries that may be correlated with RainWise participation. Additionally, the CSO basin boundaries themselves are based on the sewer network, which is unlikely to be known or correlated with unobservables affecting adoption. The exogenous number of eligible peers serves as an instrument for peer adoptions that allows us to estimate the causal effect of peer adoptions on a household's adoption decision.

Figure 1 illustrates our identification strategy by comparing two eligible households. Household 1 is in the center of an eligible CSO basin, and therefore neighbors in all directions are eligible. In contrast, Household 2 is located at the corner of an eligible CSO basin, and many of its closest neighbors are not eligible for RainWise. Not surprisingly, Household 1 has more peer adoptions than Household 2. We also show an example of how the temporal variation due to basins opening and closing also affect the number of eligible peers. Figure A.4 illustrates how changing eligibility over time affects the temporal variation in the number of eligible peers. Household 3 loses eligible peers over time since a neighboring basin became ineligible. However, there is more spatial variation in the number of peers than temporal variation. To further mo-

Figure 1: Examples of Eligible Peers

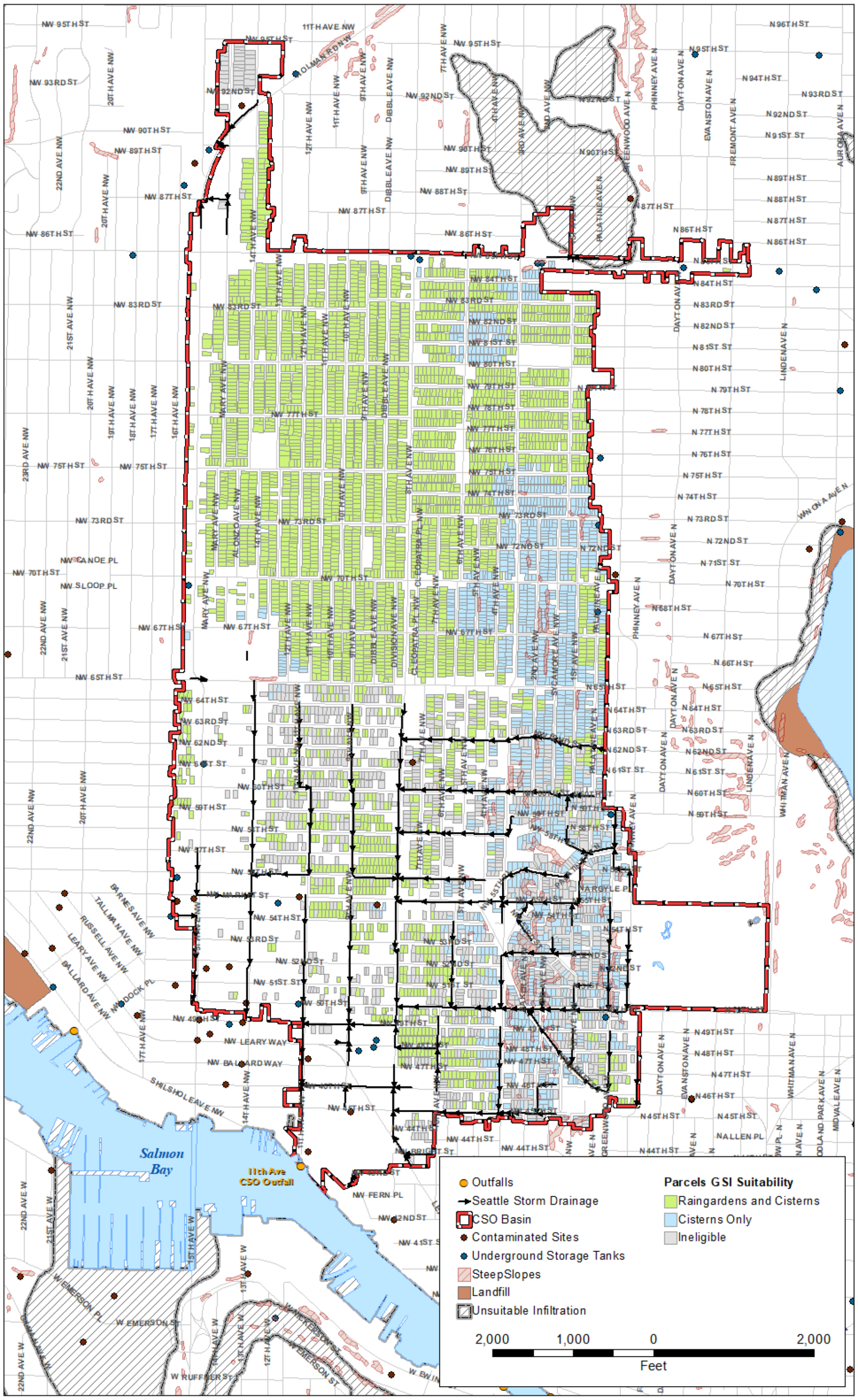
Household 1: 2018 **Household 2: 2018**



Note: The red dot represents the household of interest. The red and green parcels represent the peer group of the 100 nearest households within 0.5 miles. Eligible parcels are colored in green and ineligible parcels are red. Blue parcels are households outside of the peer group. The green dots represent households that signed up for RainWise.

tivate the exogeneity of our instrument we display a representative CSO basin in Figure 2. The first key point is that the CSO basin cuts directly through neighborhoods, and even through city blocks. The boundaries are determined by a combination of topography and the sewer network. The entire area within this CSO basin drains to a single CSO location, designated in yellow in Salmon Bay. This figure also displays the distinction between cistern and rain garden eligibility. The green parcels are eligible for both cisterns and rain gardens while the blue parcels are only eligible for cisterns. Cistern-eligible parcels may not be eligible for rain gardens due to poor draining soil, steep slopes, or proximity to contaminated sites. Lastly, the gray parcels are residential parcels within the eligible CSO basin, but are not eligible for other reasons such as being built after the more stringent stormwater code was in place.

Figure 2: CSO basin and eligibility



Note: The map show parcel eligibility within one CSO basin. The arrow lines at the bottom show the direction of the sewer network, which in part determines the boundaries. The CSO outfall location is in the lower left hand corner in yellow in Salmon Bay.

Our primary identifying assumption is that peer eligibility is not correlated with other factors that affect RainWise adoption. We empirically assess how peer eligibility correlates with economically-relevant characteristics by predicting peer eligibility using both property characteristics and census neighborhood variables. We attempt to mimic our primary empirical model with block group fixed effects, but since census variables are measured at the block group level we use census tract fixed effects when analyzing census variables. Since there is no time series variation in the property characteristics and minimal time series variation in the census variables we collapse the data to a single cross section.[16] Panel (a) of Table 2 predicts peer eligibility using property characteristics. The dependent variable is the number of peers eligible for cisterns (CS), rain gardens (RG), and an average of the two forms of eligibility (Avg). Using average eligibility has attractive inference properties that we discuss in more detail in section 4.2. We run the regressions predicting different forms of peer eligibility in both the cistern-eligible and rain garden-eligible samples. For ease of interpretation all independent variables are standardized, so a coefficient represents the increase in the number of eligible peers for a one-standard deviation change in the independent variable.

The first three columns of Table 2 show results for the cistern-eligible sample. The square footage of the home and the number of bathrooms are significant at the 5% level for rain-garden-eligible peers and at the 10% level for the average. In the rain garden-eligible sample the number of cistern-eligible peers has significant coefficients for the number of bedrooms and year built at the 10% level. However, the magnitudes are very small for all variables including the statistically significant variables. A one standard deviation increase in the property characteristics changes the number of eligible peers by one or less. The mean of the dependent variable is listed at the bottom of the table, and shows that a one standard deviation change in property characteristics at most changes the instrument by 2% of the mean.

Panel (b) focuses on census characteristics measured at the block group level. There are several variables that show consistent correlations that are sometimes statistically significant. Areas with a higher percentage of non-white residents and areas with more advanced degrees have fewer eligible peers. The largest effect is for education with a one standard degree increase in advanced degrees reducing the number of eligible peers by five to six. This is still relatively inelastic - the standard deviation of eligible peers is 15 for cistern-eligible peers and 21 for rain garden-eligible peers.

Additionally, in order for these correlations to bias our results the variables correlated with our instrument also need to be correlated with our outcome. Brent et al. (2022) show that within eligible areas income and demographics are not correlated with adoption, and we perform a similar analysis using our primary regression specification. Table A.1 in the Appendix regresses adoption on both property characteristics and census variables. None of the census variables are statistically significant, and the only variable significant at the 5% level is the year built - newer

[16] Since eligibility does change over time we focus on the maximum number of eligible peers a household had during the course of the sample.

homes are less likely to adopt. This could be because newer homes are built to comply with more recent building codes that require stormwater management. What is relevant for the validity of the identification assumption, is that the home vintage is not correlated with the number of eligible peers. Taken together, there is a small degree of correlation between our instrument and property and neighborhood characteristics, and the correlated variables do not affect our outcome variable. Additionally, the models with the census variables only use census tract fixed effects compared to our primary model using block group fixed effects. Therefore, our primary model absorbs more unobserved spatial variation than panel (b) of Table 2. This helps defend the exclusion restriction that the number of eligible peers is not correlated with drivers of RainWise adoption.

A remaining threat to identification is the possibility of that some unobserved variables are correlated with the number of eligible peers as well as with adoption. While one cannot rule these out, in section 5.4 we explore three confounders for which we have data to highlight how threats to identification operate in our setting. First, the relative location within a CSO basin may affect local nuisance flooding, which may increase participation in RainWise. Second, some CSO basins are bordered by water bodies. Homes near the water (where by definition there are no peer households) may have different numbers of eligible peers and also may be more or less likely to sign up for RainWise.[17] Third, the CSO basin borders may overlap with arterial roads for structural reasons such as topography, and factors that affect purchasing a home near an arterial road may also impact adoption. We test the sensitivity of our results to each of these threats to identification.

[17]Since we use the closest peers and control for peers water bodies like increase the total number of eligible peers because there are no ineligible households across the border of CSO basin when the border is water.

Table 2: Correlation of instrument with explanatory variables

(a) Property characteristics

Peer Eligiblity	(1) CS	(2) RG	(3) Avg.	(4) CS	(5) RG	(6) Avg.
Sq. Ft.	0.12	-1.04**	-0.46*	0.27	0.07	0.17
	(0.20)	(0.45)	(0.25)	(0.24)	(0.33)	(0.25)
Lot	-0.11	-0.46	-0.29	0.02	0.15	0.08
	(0.13)	(0.38)	(0.22)	(0.23)	(0.41)	(0.28)
Bedrooms	-0.11	0.20	0.05	-0.21*	-0.10	-0.16
	(0.11)	(0.20)	(0.14)	(0.12)	(0.16)	(0.13)
Bathrooms	-0.05	-0.46**	-0.25*	-0.09	-0.17	-0.13
	(0.12)	(0.23)	(0.15)	(0.16)	(0.22)	(0.18)
Year Built	-0.21	0.28	0.04	-0.29*	-0.02	-0.16
	(0.14)	(0.39)	(0.23)	(0.16)	(0.24)	(0.19)
Assessed Value	-0.33	-0.76	-0.55	0.06	-0.65	-0.29
	(0.38)	(0.91)	(0.37)	(0.28)	(0.56)	(0.36)
Fixed-effects						
Block Group	Yes	Yes	Yes	Yes	Yes	Yes
Eligibility	Cistern	Cistern	Cistern	Rain Garden	Rain Garden	Rain Garden
Mean Dep. Variable	80.21	45.93	63.07	78.71	65.42	72.06
# Block Group	238	238	238	222	222	222
Observations	65,402	65,402	65,402	37,750	37,750	37,750

(b) Census characteristics

Peer Eligiblity	(1) CS	(2) RG	(3) Avg	(4) CS	(5) RG	(6) Avg
Median Income	2.99	1.64	2.32	4.01	1.54	2.77
	(1.86)	(2.93)	(2.10)	(3.64)	(3.62)	(3.52)
Non-white	-3.24*	-3.12	-3.18	-4.24*	-3.72	-3.98*
	(1.81)	(2.64)	(2.01)	(2.33)	(2.48)	(2.30)
Active Trans.	3.57*	2.53	3.05	3.50	3.92	3.71
	(2.04)	(2.40)	(1.90)	(2.99)	(3.11)	(2.99)
Degree	-2.50	-6.69***	-4.60**	-3.35	-4.98**	-4.17*
	(2.27)	(2.24)	(1.83)	(2.35)	(2.27)	(2.20)
Owner Occupied	-1.77	-3.39	-2.58	-3.56	-3.95	-3.76
	(2.32)	(2.68)	(2.35)	(3.36)	(3.35)	(3.31)
Gov. Assistance	-1.58	-1.37	-1.47	-1.25	-1.36	-1.31
	(1.46)	(1.84)	(1.39)	(1.82)	(1.90)	(1.76)
Fixed-effects						
Tract	Yes	Yes	Yes	Yes	Yes	Yes
Eligibility	Cistern	Cistern	Cistern	Rain Garden	Rain Garden	Rain Garden
Mean Dep. Variable	80.21	45.93	63.07	78.71	65.42	72.06
# Tract	80	80	80	78	78	78
Observations	65,402	65,402	65,402	37,750	37,750	37,750

Note: The column headers display the dependent variable of each regression, which is either the number of peers eligible for cisterns (CS) or both cisterns and rain gardens (RG) using the 100 closest peers specification. The sample is designated as either eligible for cisterns or both cisterns and rain gardens. In both cases any peer or household eligible for a rain garden is also eligible for cisterns. All explanatory variables are standardized so the interpretation is marginal effect of a one standard deviation change. In panel (b) all census variables are measured at the block group level. Panel (a) uses block group fixed effects and panel (b) uses census tract fixed effects. All standard errors are clustered at the level of the fixed effect. *p<0.1; **p<0.05; ***p<0.01

4.2 Peer Effects Adoption Model

Our primary empirical approach models the annual RainWise adoption probability as a function of peer adoptions described in equation 1,

$$RW_{it} = \alpha + \delta PeerAdoptions_{it} + \boldsymbol{\beta X_{it}} + \tau_t + \phi_b + \epsilon_{it} \tag{1}$$

where RW_{it} is an indicator for whether household i signed up for RainWise in year t, Peer Adoptions$_{it}$ is the count of household i's peers that adopted RainWise by year t, $\boldsymbol{X_{it}}$ is a vector of parcel characteristics, and τ_t and ϕ_b year and census block group fixed effects. Our preferred specification only uses the number of total peers (eligible and ineligible) as a control variable with additional controls added in our robustness checks. We restrict the sample to **eligible households** and drop households after they adopt RainWise since we want to focus on homes that are able to sign up.

We estimate equation 1 using a linear probability specification since our main interest is in recovering the marginal effect of peer adoptions on household adoption rates.[18] Since peer adoptions are likely correlated and endogenous with individual adoption decisions, we instrument for peer adoption counts with each household's count of eligible peers. Thus, our instrumental variable model (IV) replaces $PeerAdoptions_{it}$ with $\widehat{PeerAdoptions}_{it}$, where the excluded instruments are either: (1) the number of cistern-eligible and rain-garden-eligible peers, entered separately or (2) the average of the two peer-count instruments with the average formed by summing cistern-eligible and rain-garden-eligible peers and dividing by two.[19] The advantage of the specification using a single average-value instrument is that it allows for the usage of a wider variety of robust inference measures that are only applicable in cases with a single endogenous variable and single instrument (Andrews et al., 2019; Lee et al., 2022).

We present our main results using both types of instruments; in models where we have multiple endogenous variables, as is the case when we identify the impact of rain gardens and cisterns individually, we use both instruments separately. We run multiple variations on our IV model to investigate the mechanisms behind adoption and to test the robustness of our main results.

5 Results

5.1 Peer Effects Results

Results from our main peer effects models are shown in Table 3. Column (1) shows output from the OLS model. Here, we find that an additional peer adoption decreases individual household adoption rates by 0.01 percentage points, on average, although the result is insignificant. This

[18] In later sections, we also estimate a series of nonlinear models to: (1) test the sensitivity of our main IV results to the linear probability assumption and (2) for use in forming predictions in our policy analysis section.

[19] We divide by two so that maximum of the variable combining both forms of eligibility is equal to the total number of peers similar to the single peer eligibility variables .

suggests that peers have a limited impact on RainWise adoption, but, as with most OLS peer-effects models, this estimate is likely biased. Specifically, we expect that there are correlated unobservables that impact both individual and peer adoptions leading to simultaneity bias in the coefficient estimates.

Columns (2) and (3) present results from our IV model where we address these endogeneity concerns using instruments based on the plausibly random location of peers within CSO basins. Column (2) presents results from the first stage of the IV model. Here, we find that both an additional cistern-eligible peer and a rain-garden-eligible peer increase the number of peer adoptions by 0.01.[20] Translating these into effect sizes, we find that a one standard deviation increase in the number of cistern- and rain-garden peers generates 0.17 and 0.33 additional peer adoptions, respectively, representing a 7% and 14% increase relative to the mean. The coefficients on both instruments are highly significant.

Results from the second stage of the IV model are shown in column (3). These results suggest that an additional peer adoption increases a household's annual adoption probability by 0.2 percentage points. Given that the mean annual probability of adoption in our sample is only 0.3%, these results imply that one additional peer leads to a 66% increase in the rate of adoption, a large relative impact. In addition, comparing the OLS results in column (1) with the IV results in column (3) we find that there is a significant downward bias in the OLS estimates highlighting the importance of accounting for unobservables in peer effects models.

While our IV results demonstrates the need to control for endogeneity bias, the fact that we find a downward bias in the OLS model is atypical in the peer effects literature. Specifically, most peer-effects research finds that unobservables are positively correlated with both peer and individual decisions, which works to overestimate the true peer effect. In our setting, if spatial unobservables cause a household and their peers to both adopt RainWise then we expect the OLS estimator to overestimate the impact of peer adoptions. However, in our data spatial clusters of adoptions and/or initial adoptions take place in areas where, absent peer effects, the adoption probability is actually lower. This implies that if there were no peer effects a typical household that observes more neighbors signing up for RainWise would be *no less* likely to sign up compared to a household with fewer peer adoptions. This implies that spatial clustering of adoptions is primarily due to peer effects. While we cannot provide a definitive reason for this behavior, one possible explanation relates to elevation and flooding in the King County area. Specifically, we find that homes with more eligible peers are located at higher elevations and thus are farther away from areas with drainage complaints. Thus, these locations are less prone to nuisance flooding, which is likely to decrease adoption probabilities conditional on any peer effects.

The primary IV results in Table 3 (columns 2 and 3) provide evidence of peer effects in the adoption of GSI. However, we need to address two empirical concerns related to instrumental variables. First, recent work highlights the importance of conducting inference that is robust to

[20] Note that these are not in percentage points, but are the number of peer adoptions, which are not bounded between 0 and 1 since a household can have multiple peers sign up for RainWise.

weak instruments (Andrews et al., 2019; Lee et al., 2022). The second issue concerns the proper interpretation of the coefficient estimate in our IV model.

We address the weak-instrument issue by employing several diagnostic tests from the literature. All results are shown in the bottom part of Table 3. We first report an effective F-statistic, robust to clustering from Olea and Pflueger (2013). The value for this statistic, 30.74, is well above the critical values derived in Stock and Yogo (2005). We also estimate Anderson-Rubin (AR) confidence intervals (Anderson and Rubin, 1949), which are produced by inverting the AR test statistic and are robust to weak instruments. The AR confidence interval for the IV model in column (3), $[0.18, 0.33]$, is similar to the 95% confidence interval calculated from the asymptotic standard errors, $[0.15, 0.32]$. Finally, we present results using valid t-Ratio inference (tF) based on methods in Lee et al. (2022). This paper shows that commonly used t-ratio-based methods of inference have severe large-sample distortions that can be fixed by making adjustments to the second-stage critical values and standard errors based on the first-stage F-statistic. These methods, however, apply only to single-instrument, single-endogenous-variable settings, so to apply them we use our average-value instrument (described in the previous section).

The results from the single-instrument model - shown in column (4) - are very similar to the baseline results in column (3). The effective F-statistic is even larger than in the model using two instruments, and the AR confidence interval is similar. Most critically, the second-stage critical value for the t-statistic, for a correct 5% rejection probability using tF inference, is 2.18 compared to a typical t-statistic of 1.96, and the confidence intervals are similar to AR and asymptotic confidence intervals in the baseline IV model.[21] While all of the weak-instrument diagnostic tests above indicate that our instruments are strong and asymptotic inference does not suffer from the poor coverage, we will continue to use the model specification with one instrument from column (4) in regressions that have only one endogenous regressor given the importance of robust inference in IV models; we will also report AR confidence intervals in these models.

[21]The tF is also well below the t-ratio of 5.2 for our model with two instruments and 5.3 for our model using a single instrument.

Table 3: Peer effects on adoption of any RainWise component (cistern, rain garden or both)

	(1)	(2)	(3)	(4)
	OLS	First Stage	IV	
Peer Adoptions	-0.012			
	(0.015)			
# Eligible Peers (CS)		0.011^{***}		
		(0.003)		
# Eligible Peers (RG)		0.010^{***}		
		(0.002)		
Peer $\widehat{\text{Adoptions}}$			0.235^{***}	0.237^{***}
			(0.045)	(0.045)
Fixed-effects				
Year (10)	Yes	Yes	Yes	Yes
Block Group (238)	Yes	Yes	Yes	Yes
Observations	416,477	416,477	416,477	416,477
F			30.74	45.73
AR CI			[0.18, 0.33]	[0.15, 0.33]
tF cF				2.18
tF CI				[0.14, 0.34]

Notes: The dependent variable is an indicator for whether a household signed up for RainWise in a given year. The IV specification in column (3) uses the number of peers eligible for cisterns (CS) and rain gardens and cisterns (RG) as instruments for the number of peer adopters. In column (4) a single instrument is used constructed as the average of cistern-eligible and rain garden-eligible peers. The regressions control for block group and year fixed effects and the total number of peers (eligible and ineligible). Standard errors are clustered at the block group level. The IV models report the effective-F statistic (Olea and Pflueger, 2013) and the Anderson-Rubin confidence intervals $Peer\widehat{Adoptions}$. Column (4) that uses a single instrument also present the adjusted 5% critical value for the second-stage t-statistic and the tF critical values from Lee et al. (2022). *p<0.1; **p<0.05; ***p<0.01

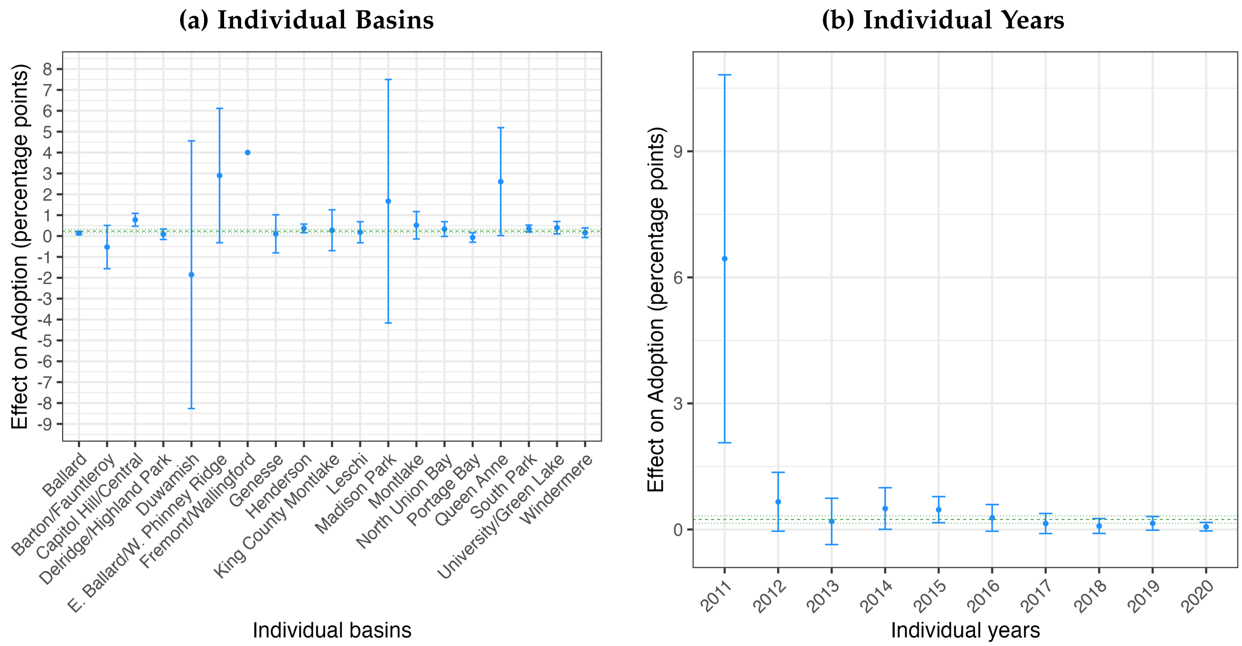
Our second empirical issue relates to treatment heterogeneity and how to interpret the coefficient in our IV model. Imbens and Angrist (1994) demonstrate that in a linear IV model with a binary endogenous variable and instrument without covariates the coefficient can be interpreted as a Local Average Treatment Effect (LATE), which is the treatment effect for compilers induced into treatment due to the instrument. In the presence of treatment heterogeneity this interpretation changes slightly to a positively weighted average of LATEs in the sub-population whose treatment status changes due to a change in the instrument. In our model, however, this interpretation does not hold as neither our peer effects variable nor our instrument are binary. Thus, in our model the interpretation of our IV coefficient must be based on the underlying assumption of linearity (functional form) and the homogeneity of the treatment effect in the population.[22]

[22]Recent work by Blandhol et al. (2022) has also called into question the LATE interpretation of coefficient estimates in models with binary treatment and instrumental variables when the model includes additional covariates as controls. They show that in such models (i.e., linear instrumental variable models) for the coefficients to have a LATE interpretation either the treatment effects must be homogeneous and/or the assumption of a linear first-stage functional form must hold. Otherwise, to estimate and interpret the results as LATE the model must be saturated and control for covariates nonparametrically.

While we cannot provide direct support for the homogeneity assumption, we can provide evidence of how treatment effects vary in specific subsamples of the data to help support the assumption. To do this, we first estimate the IV model separately by CSO basin and by year. The point estimates and 95% confidence intervals for the models estimated separately for each CSO basins are shown in the panel (a) of Figure 3, and the results for IV models estimated separately for each year are shown in panel (b). While the results for the individual basins and years display some heterogeneity, most of estimates are centered around the average effect in the main IV models (the dashed green line in the figure). One potential pattern is a slight decline in the peer effects over time some in panel (b) of Figure 3. This is consistent with results presented later on the timing of peer effects in Figure **??**. More recent peer adoptions generate larger peer effects and earlier years in the sample mechanically have a higher proportion of recent peer adoptions. Later in the sample the peer effects represent a weighted average of recent and distant temporal adoptions, which will bring down the average peer effect.

To provide further support for our homogeneity assumption, in Table 4 we present IV results for a group of models estimated using various subsets of the data that exclude basins that display significant treatment heterogeneity to test if these heterogenous basins are driving the aggregate results. We focus on basins where the effects are either particularly large or have a negative sign. Column (1) replicates our main IV results from column (4) of Table 4. In column (2), we estimate our model after dropping four basins with particularly large positive effects, which are those in Figure 3 with point estimates above the 1% line (E. Ballard, Fremont, Madison Park, and Queen Anne). The results from this model, while smaller, are still very close to those in the IV model using the full data. In column (3), we estimate the IV model after dropping basins with point estimates which are negative though not statistically different from zero (Barton, Duwamish, and Portage Bay). The results, as expected, are larger than in the baseline IV model but still very similar in size. Finally, in column (4) we present results for a model where we drop all seven basins used in the subsamples in columns (2) and (3). In this case, the results are extremely close to the baseline IV model. We also note, based on results in the bottom part of the table, that each of the sub-sample models pass the standard weak-instrument tests discussed previously. Based on the results in Figure 3 and Table 4, we feel confident in relying on the homogeneity assumption in interpreting our main IV results. We also show in our robustness section that our results are very similar to a fully saturated model using block group-by-year fixed effects as advocated by Blandhol et al. (2022).

Figure 3: Spatial and temporal heterogeneity



Note: This figure shows results from a series of IV models (point estimates and 95% confidence intervals) estimated separately for each CSO basin (panel (a)) and by year (panel (b)). The treatment effects are listed on the y-axis. The horizontal green dashed line shows the treatment effect (0.237) from the full IV model (column (4) of Table 3) along with a 95% confidence interval in dotted lines. All models include controls for the total number of peers. The individual basin models include block group and year fixed effects, and the individual year models only include block group fixed effects. In panel (a) the confidence interval for Fremont/Wallingford is missing because they are very large (≈ 100) and are dropped to make the graph legible.

Table 4: Robustness to removing heterogeneous basins

	(1)	(2)	(3)	(4)
$\widehat{\text{Peer Adoptions}}$	0.237***	0.222***	0.261***	0.245***
	(0.045)	(0.037)	(0.046)	(0.037)
Fixed-effects				
Year (10)	Yes	Yes	Yes	Yes
Block Group	Yes	Yes	Yes	Yes
# Block Group	238	203	215	180
Observations	416,477	369,834	401,937	355,294
Sample	Full	No Large	No Negative	No Large or Negative
F	45.7	48.2	43.5	45.8
AR CI	[0.15, 0.33]	[0.15, 0.3]	[0.17, 0.36]	[0.17, 0.32]

Notes: The dependent variable is an indicator for whether a household signed up for RainWise in a given year. Column (1) replicates the IV results (column 4) of Table 3. Column (2) estimates the IV model after removing four basins with large treatment effects - those with point estimates above 1%; column (3) estimates an IV model after removing three basins with negative treatment effects; and column (4) estimates a model after removing the seven basins with both large treatment effects and negative treatment effects. The regressions control for block group and year fixed effects as well as the total number of peers (eligible and ineligible). Standard errors are clustered at the block group level. *p<0.1; **p<0.05; ***p<0.01

5.2 Mechanisms for Adoption

In this section, we explore the mechanisms behind the adoption of RainWise technology. We begin by exploring peer effects based on both household, and peer, adoption of specific types of GSI technology. Next, we use Google Street View images and proxies for visibility to determine if RainWise visibility played a role in the peer effects we estimate in our IV model. Finally, we examine how peer group size and timing impacts the results.

5.2.1 Technology Type

Understanding the mechanisms behind RainWise adoption can help inform policymakers interested in leveraging our findings to increase participation in RainWise and similar voluntary environmental programs. We begin, in Table 5, by examining whether peer adoptions have a differential effect on the installation of rain gardens versus cisterns. Column (1) replicates the IV results from column (4) of Table 3. In columns (2) - (5) we replace the dependent variable with an indicator equal to one when a household adopts a rain garden or a rain garden and cistern (column (2)), only adopts a cistern (column (3)), only adopts a rain garden (column (4)), and adopts both a rain garden and a cistern (column (5)). The results of the technology-specific regressions show that peer adoptions only affect uptake of rain gardens. The coefficient is essentially zero for households that only adopt cisterns. All results have large first-stage effective *F*-statistics and the inference is robust to weak instruments.

Next, we conduct an analysis where we distinguish between the type of technology that peer households adopt. In these models, we include instruments separately for the number of peer adoptions of rain gardens and cisterns. Since we only have two instruments, we can only include a maximum of two endogenous variables and cannot include each possible peer adoption separately (only rain garden, only cistern, and both). We choose to model peer rain garden adoptions as any adoption that includes a rain garden, i.e., our rain-garden definition includes households that install both rain gardens and rain gardens and cisterns together. Thus, in all subsequent regressions, when we designate technology-specific peer adoptions of "Rain Garden", it includes both categories.

Table 6 shows the results of regressions with technology-specific peer adoption for each category of RainWise adoption. In all cases, peer effects only exist when peers adopt rain gardens. Peers adopting only cisterns have either no effect or actually decrease the probability of adoption, although the coefficients have large standard errors. We note that this inference is not robust to weak instruments and even so the confidence intervals on peer cistern adoptions are very large. This confirms the results in Table 5 that peer effects primarily operate through rain gardens, but that the specific magnitudes and inferences for the separate peer adoptions should be taken with some caution.

Table 5: Peer effects by type of technology adopted

	(1) Any	(2) Rain Garden	(3) Only Cistern	(4) Only Rain Garden	(5) Both
Peer $\widehat{\text{Adoptions}}$	0.237***	0.224***	0.013	0.147***	0.077***
	(0.045)	(0.035)	(0.036)	(0.030)	(0.015)
Fixed-effects					
Year (10)	Yes	Yes	Yes	Yes	Yes
Block Group (238)	Yes	Yes	Yes	Yes	Yes
Observations	416,477	416,477	416,477	416,477	416,477
F	45.7	45.7	45.7	45.7	45.7
AR CI	[0.15, 0.33]	[0.16, 0.31]	[-0.07, 0.08]	[0.09, 0.21]	[0.16, 0.31]

Notes: The dependent variable is an indicator for whether a household signed up for a specific type of RainWise GSI in a given year. Columns (1) replicates the IV model from column (3) of Table 3. The dependent variables in Columns (2) - (5) are dummies for adopting either a rain garden or a rain garden and a cistern (2), only a cistern (3), only a rain garden (4) or both a rain garden and a cistern (5). The IV specification uses the average number of eligible peers as an instrument for the number of peer adopters. The IV models report the effective-F statistic (Olea and Pflueger, 2013) and the Anderson-Rubin confidence intervals for $Peer\widehat{Adoptions}$. The regressions control for block group and year fixed effects and the total number of peers (eligible and ineligible). Standard errors are clustered at the block group level. *p<0.1; **p<0.05; ***p<0.01

Table 6: Peer effects by type of technology adopted and peer technology

	Any (1)	Rain Garden (2)	Only Cistern (3)	Only Rain Garden (4)	Both (5)
Peer Rain $\widehat{\text{Garden}}$ Adoptions	0.325***	0.335***	-0.010	0.237***	0.098***
	(0.085)	(0.052)	(0.052)	(0.043)	(0.022)
Peer $\widehat{\text{Cistern}}$ Adoptions	-0.205	-0.331	0.126	-0.301	-0.030
	(0.318)	(0.222)	(0.172)	(0.185)	(0.098)
Observations	416,477	416,477	416,477	416,477	416,477

Notes: The dependent variable is an indicator for whether a household signed up for a specific type of RainWise GSI in a given year. Columns (1) replicates the IV model from column (3) of Table 3. The dependent variables in Columns (2) - (5) are dummies for adopting either a rain garden or a rain garden and a cistern (2), only a cistern (3), only a rain garden (4) or both a rain garden and a cistern (5). The IV specification uses the number of eligible peers as an instrument for the number of peer adopters. The regressions control for block group and year fixed effects and the total number of peers (eligible and ineligible). Standard errors are clustered at the block group level. *p<0.1; **p<0.05; ***p<0.01

5.2.2 Visibility

We next explore how visibility may impact why a type of technology is more prone to peer effect compared to another. Specifically, given that cisterns are more likely to be installed in the back of a house it is likely that this will limit their visibility from the street. Alternatively, rain gardens are commonly installed in both the back and/or front yard and are therefore more likely to be seen from the street. Our Google Street View (Table A.2) data show that 68% of rain gardens are visible from Google Street View compared to 41% of cisterns. In addition to simply seeing a neighbor's rain garden or cistern, all RainWise participants are encouraged to display a sign stating that "I'm RainWise" in their front yard.[23] We explicitly explore the role of visibility by incorporating the GSV data into the empirical peer effect regressions. The details of the GSV data processing are available in the Appendix D.

We examine two forms of visibility: whether a household puts up a RainWise sign and whether the GSI is visible from the street. Putting up a sign is a more direct form of RainWise visibility because it specifically advertises the program. Households may not be able to recognize a rain garden or know that it is part of a program for which they could sign up. Each form of visibility also has implications for the identification and interpretation of the econometric results. The decision to put up a RainWise sign is not random. It is possible that households who are very enthusiastic about RainWise will be more likely to both talk to their neighbors about the program and put up a sign. Ideally, we would have a separate instrument or identification strategy to generate quasi-random variation in who puts up a sign. However, since all signs must come from adoptions our eligibility instruments do affect the decision to put up a sign. Since we have two instruments we can include both adoptions and adoptions with signs as endogenous variables. However, since we do not have an instrument that specifically generates random variation in signs compared to general adoptions we caution a strong causal interpretation of the sign coefficients. The same limitations on inference with multiple instruments and multiple endogenous variables applies to the visibility results. The visibility data may also suffer from endogeneity bias, although whether GSI is visible from the street is in part due to the technical feasibility of where GSI can be installed. There is still some choice from the household to install rain gardens or cisterns in the front or back yard.

Table 7 shows the results incorporating visibility. When including signs in column (1) the base peer effect becomes negative and the effect of signs is very large. Both effects are, however, imprecisely estimated. All adoptions with signs and that are visible are also peer adoptions so those variables have the interpretation of an interaction effect when peer adoptions is included in the regression. A similar pattern emerges when augmenting the peer effects regression with visible peer adoptions; the base peer effect is negative and visible adoptions have a large effect although none are statistically significant. In columns (3) - (5) we assume that peer effects only

[23]Examples RainWise signs are shown in Figure A.1. Additional examples of RainWise installations with pictures and testimonials from participants are available at https://700milliongallons.org/projects/ as of Monday 21st July, 2025.

operate through visibility so we drop any peer adoptions that do not have a sign or are not visible from the street. Column (3) assumes that peer effects only operate through adoptions with RainWise signs. The estimated peer effect for adoptions with signs is almost three times the size of the base peer effect. Column (4) includes only visible peer adoptions and does not account for signs. The effect is roughly one and half times the base peer effect. Lastly, column (5) includes visible peer effects and signs; signs have a positive while visible adoptions decrease adoption although the estimates are imprecise. While the GSV data have some limitations, combined with the technology-specific pattern of heterogeneity they suggest that visibility plays a role in peer effects. It is possible that homes that put up signs or make GSI visible from the front of the house are also more engaged socially with their peers, so these results cannot rule out a social diffusion mechanism.

Table 7: Peer effects heterogeneity by visibility and sign placement

	(1)	(2)	(3)	(4)	(5)
Peer $\widehat{\text{Adoptions}}$	-0.406	-0.775			
	(0.678)	(0.807)			
# $\widehat{\text{Signs}}$	1.84		0.688***		3.85
	(1.98)		(0.139)		(9.74)
# $\widehat{\text{Visible}}$		1.36		0.321***	-1.49
		(1.08)		(0.063)	(4.51)
Fixed-effects					
Block Group (238)	Yes	Yes	Yes	Yes	Yes
Year (10)	Yes	Yes	Yes	Yes	Yes
Observations	416,477	416,477	416,477	416,477	416,477

Notes: The dependent variable is an indicator for whether a household signed up for a specific type of RainWise GSI in a given year. Columns (1) includes both peer adoptions and adoptions with signs. Column (2) includes both adoptions and visible adoptions. Column (3) only includes adoptions with signs and column (4) only includes visible adoptions. Column (4) only includes visible adoptions and adoptions with signs. The regressions control for block group and year fixed effects and the total number of peers (eligible and ineligible). Standard errors are clustered at the block group level. *p<0.1; **p<0.05; ***p<0.01

We also explore visibility proxies that measure visibility less directly but with more consistent data and a cleaner identification strategy. We offer three proxies for visibility: corner lots, homes near bus stops, and neighborhoods that use active forms of transportation. Corner lots are more likely to be on walking paths and therefore more likely to be seen by peers. Homes near bus stops will be seen by commuters on their walk to and from the bus stop and when they are waiting for the bus.[24] Peer adoptions may be more visible in locations where people walk, bike, and take public transit more often. We create variables for the number of peers on corner lots and within 0.1 miles from a bus stop, and then calculate the number of peer adoptions that take place on corner lots and near bus stops. This approach allows us to create new instruments based

[24]Bus is the most utilized form of public transit in Seattle.

on the number of eligible peers on corner lots and near bus stops. These instruments are close to interactions of the original instruments with the corner and bus stop variables (Wooldridge, 2010), however we actually measure the number of eligible peers on corners and close to bus stops.[25] We incorporate active transportation using American Community Survey data at the block group level. We create an indicator variable equal to one if the parcel is in a census block group above the median for active transportation, where active transportation is defined as the proportion of commuters who walk, bike, or use public transit. We also control for peer effect heterogeneity based on income and home ownership that may be correlated with active transportation at the neighborhood level. The results are shown in Table A.3. Peer adoptions that take place on corner lots and near bus stops do have an positive incremental effect on adoptions, but neither effect is statistically significant. Neighborhoods with more active commuters do have larger peer effects. The peer effect is not moderated by high-income neighborhoods or neighborhoods that have a high home-ownership rate. The results on these visibility proxies provide further suggestive evidence that peer effects operate through visibility.

5.2.3 Peer Group Type

In this section, we assess the sensitivity of our results to our definition of peer groups; since we do not directly observe each household's peer group, we test how peer effects change using different peer group definitions.

We begin by varying the count of peers that comprise each household's peer group. We define the peer group as the closest k neighbors, where k ranges from 20 to 100 in increments of 20. The logic of this exercise is that closer neighbors are more likely to exert influence over the decision to adopt RainWise, i.e., a household's closest peers are more visible and more likely to be part of the household's social network. The results from this series of IV models are shown in panel (a) of Figure 4. The x-axis shows the number of peers used in defining the peer groups in each model (with the baseline 100-peer specification shown on the far right), and blue points and lines show the treatment effects estimates and the 95% Anderson-Rubin confidence intervals. As expected, the effect of an additional peer adoption increases as the number of neighbors in the peer group decreases. In the smallest peer group (20 peers), one additional peer increases the adoption probability by over 1%, which is almost five times larger than the effect of a peer adoption among the 100 closest peers. The suggests that location matters, a result that is generally consistent with our finding in the previous section on the visibility of RainWise signs.

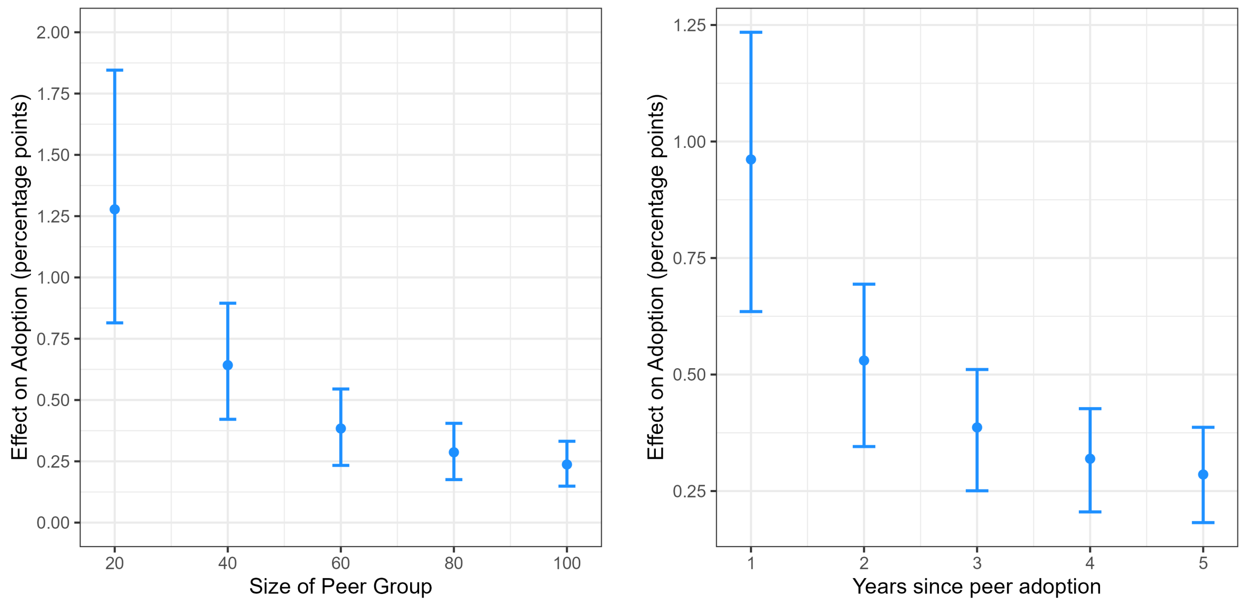
We also estimated a series of models where we vary the spatial boundary defining the peer group from 0.1 miles to 0.5 miles. Here, we find very little effect of the spatial boundary on the marginal effects of peer adoptions. One reason for the stability of these results is that Seattle is a relatively dense city and there are still many peers even within a 0.1 mile radius, and we restrict

[25] The distinction between our variable and simply interaction eligible peers with the number of corner lots is that a house could be near many homes with corner lots and many eligible peers, but the eligible peers do not in fact reside on corner lots. Therefore, explicitly calculating the number of eligible peers on corner lots and near bus stops is a more precise instrument.

the peer group to a maximum of the 100 closest households. Specifically, the average number of peers for the 0.5 mile radius is 100, but only drops to 83 for the 0.1 mile radius. The results from this exercise are shown in Figure A.5 in the Appendix.

Figure 4: Peer effects varying the composition of the peer group

(a) Number of peers **(b) Timing of peer adoption**



Notes: The dependent variable is an indicator for whether a household signed up for RainWise in a given year. The dots in panel (a) represent point estimates for different numbers of peers in the peer group. The 100 estimate in panel (a) replicates column (3) of Table 3. The x-axis in panel (b) represents the years since peer adoption, so 5 is the number of peer adoptions in the five previous years. The dots in panel (b) represent point estimates for different timing on peer adoption, each representing a separate regression. In both panels peer adoption is instrumented with the average of cistern-eligible and rain garden-eligible peers. The regressions control for block group and year fixed effects and the total number of peers (eligible and ineligible). The error bars are 95% Anderson-Rubin confidence intervals based on inverting the AR test statistic.

We also investigate how the heterogeneity in peer effects across GSI varies with the definition of peer group size. Specifically, in Figure A.6 we replicate the results in panel (a) of Figure 4, but here we allow for technology-specific peer effects for different types of GSI adoption in a manner similar to the regressions shown in Table 5; coefficients are estimated in separate regressions for each peer group definition ($k \in \{20, 40, ..., 100\}$) and type of GSI adopted. Decreasing the size of the peer group only amplifies the magnitude of a peer adoption for adoptions that include rain gardens. The effect for cisterns is consistently zero with larger confidence intervals for smaller peer groups.

Finally, we investigate how the timing of peer adoptions impacts the decision of households to adoption RainWise. Our main results in Table 3, should be interpreted as a combination of short-run and long-run peer effects from a peer adoption. However, it is plausible that more recent peer adoptions will be more impactful compared to those that happened in the more distant past for several reasons. First, it is likely that more recent adoptions may be more visible since adopters with a RainWise sign in their yard only keep it up for a limited period of time.

Second, neighbors may discuss recent changes to their home with their peers in the short period after those changes, or neighbors may notice their peers actively renovating their landscaping. Finally, there may be dis-adoption if homes are sold and new owners do not keep the GSI.

To tease out long- versus short-run effects, we create the cumulative number of peer adoptions within specific time intervals from adoption. We use the adoptions relative to year t as $t - h$, where h varies from 1 to 5. For example, if t is 2016 and h equals 4, then we include all peer adoptions that took place from 2012 to 2015. The results varying the temporal interval of peer adoption are shown in panel (b) of Figure 4, where each timing interval is a separate regression denoted on the x-axis. More recent peer adoptions have a stronger effect on adoption. If the main effect of 0.24 represents a combination of short-run and long-run effects , then the shortest-run effect (peer adoptions in the past calendar year) is roughly four times higher: a one percentage point increase in adoption. Once again, we also show the effects of timing by type of adoption in Figure A.7. Similar to other results in the paper, the peer effects in different time intervals are primarily driven by the adoption of rain gardens.

5.3 Duration and nonlinear models

We also address issues related to censoring and duration dependence. As discussed, the data used in our models is an unbalanced panel with balance determined by household entry and exit. Entry is based on whether a household becomes eligible for RainWise – when the household's CSO basin is opened. Exit is determined by whether a household's basin closes or whether a household adopts a rain garden and/or cistern, whichever comes first. One concern is whether this entry and exit of households impacts our results. Specifically, if households' entry and exit is non-random, then it could bias our parameters estimates.

The entry and exit resulting from a sewer basin's opening and closing is not an issue. The process of becoming RainWise eligible is based on aggregate decisions made by SPU and KCWTD about when and where to target GIS adoption and is not something individual households can influence. The imbalance resulting from a household's exit following RainWise adoption, however, may impact our estimates if it produces duration dependence and/or non-random censoring. While our linear probably model captures the marginal effect of an additional peer on a household's probability of RainWise adoption, the static nature of the model is not well suited for handling issues of duration dependence and censoring. This type of process – of a household exiting due to RainWise adoption and the resulting unbalanced and censored nature of the data – is best captured within a duration framework. At the time a household enters the data (when their basin opens) their clock starts and the data captures either their exit, because of RainWise adoption, or their censoring, because of a basin's closure or the end of the study period. To test the robustness of our results to these issues, we estimate a series of instrumental variable duration models that handle censoring, duration dependence, and endogeneity. Because time in our data is measured at an annual step, our primary specification takes a discrete-time form. Beck et al. (1998) show that in temporally coarse duration data with large numbers of ties (multiple exits within a given period), continuous-time models are well approximated by binary choice models. Specifically, they show that a complementary log-log binary model with year fixed effects provides a perfect approximation to a continuous-time, piece-wise exponential model; they also show that binary probit models are only marginally less precise. For both ease of interpretation and because of the need for joint normality in dealing with endogeneity issues related to peer adoption, we estimate and present results for IV binary probit models with year fixed effects.

The application of a binary choice model with year fixed effects to our data addresses both the issue of censoring and any biases introduced by duration dependence. However, we still have the original issue of endogeneity related to peer adoption. Because of the nonlinear nature of the duration model, it is not possible apply standard IV methods. To overcome this issue, we follow Wrenn et al. (2016) and apply a control method (Wooldridge, 2015).

The control function model is estimated in two stages. In the first stage, we regress peer adoptions on total peers, block group and year fixed effects, and our instrumental variables. We then recover the residuals. In the second stage, we estimate a set of binary panel data models with probit link functions including the residuals from the first stage as an additional covariate to control for endogeneity in our *Peer Adoptions* variable. Because the model is estimated in stages, we implement a block bootstrap procedure with 1000 replications at the household level to calculate the standard errors. All models include time fixed effects, to control for duration dependence, as well as block group fixed effects.

Results from the nonlinear control function models are shown in Table 8. Column (1) includes residuals in linear form for the probit model and column (2) includes an additional squared residual term. Panel (a) shows the coefficient estimates and panel (b) reports the marginal effects, averaged over all observations in the data, for the *Peer Adoptions* variable. The marginal effects from the control function duration models are very similar to our main results in Table 4. Thus, it does not appear that duration dependence or other biases related to censoring are impacting our results. Note that we do not re-scale the variable in the nonlinear models, so the marginal effects are in raw numbers not percentage points. The estimated effect is almost identical; an additional peer increases the adoption rate by 0.24 percentage points in the 2SLS models compared to 0.25 or 0.26 percentage points for the marginal effects in the nonlinear models using the control function approach.

Table 8: Nonlinear models using control function
(a) Coefficient estimates

	(1)	(2)
Peer Adoptions	0.195***	0.192***
	(0.022)	(0.022)
$\widehat{\text{Residual}}$	-0.201***	-0.202***
	(0.046)	(0.045)
$\widehat{\text{Residual}^2}$		0.002
		(0.001)
Observations	395,114	395,114

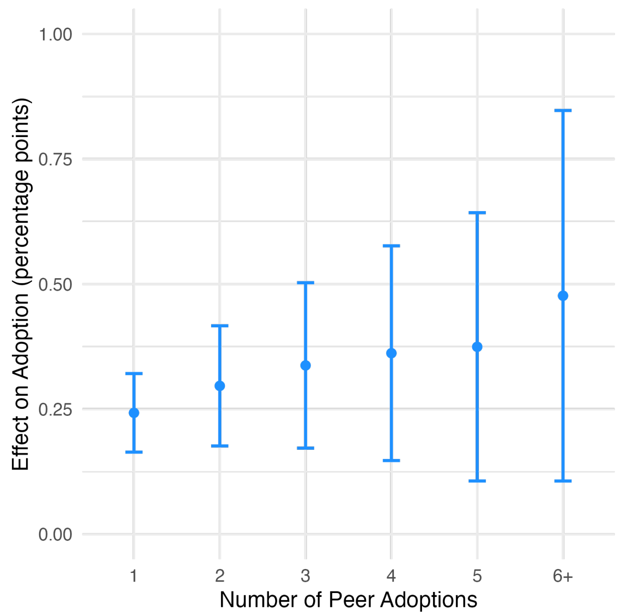
(b) Marginal effects

	(1)	(2)
Peer Adoptions	0.0026***	0.0025***
	(0.0003)	(0.0003)
N	395114	395114

Notes: The dependent variable is an indicator for whether a household signed up for RainWise in a given year. Columns (1) and (2) show the results for a probit model with a control function approach where the residuals from a regression of peer adoptions on eligibility, the number of peers, and basin and year fixed effects. Column (1) uses the residuals from the control function and column (2) adds a squared residual term. The standard errors are bootstrapped using 1000 draws resampling at the household level. Regressions include block group and year fixed effects and the total number of peers (eligible and ineligible). *p<0.1; **p<0.05; ***p<0.01

We also estimate a model to test whether there is a constant marginal effect of peer adoptions or whether there are threshold effects where multiple neighbors need to participate before convincing others to adopt. The results from this model are shown in Figure 5. Here, the regression includes dummies for the distinct number of peer adoptions from one up to six or more (there are few households with more than 6 peer adoptions). Since this technical transformation of the endogenous variable generates more endogenous variables than instruments we use the control function approach and bootstrap the standard errors. We find that the marginal effect of an additional peer adoption is close to linear, although the first peer adoption has a slightly higher magnitude.

Figure 5: Number of peer adoptions



Notes: The dependent variable is an indicator for whether a household signed up for RainWise in a given year. The x-axis shows coefficients for distinct numbers of peer adoptions from one regression. Since there are more coefficients than instruments a control function approach is used with the excluded variable is the average of cistern-eligible and rain garden-eligible peers. The regressions control for block group and year fixed effects and the total number of peers (eligible and ineligible). The standard errors are bootstrapped using 1000 draws resampling at the parcel level.

5.4 Robustness Checks

5.4.1 Rain garden-eligible sample

We perform several sets of robustness checks to test the identifying assumptions and assess the sensitivity of the results to modeling specifications. First, we restrict the sample to households in the rain garden-eligible sample since most of the peer effects are exist for rain garden adoptions. Table A.4 replicates the main results from Table 3 The results are essentially the same, with an additional peer adoption increasing the adoption rates by 0.2 percentage points. We also replicate the heterogeneous results from Table 5 in Table A.5 and the results are also consistent. In particular, the lack of a peer effect for cistern adoption shows up in the rain garden-eligible sample as well, which is further evidence that peer effects operate primarily in rain gardens.

5.4.2 Controls and sample selection

In section 4 we discuss threats to our identification strategy due to unobservable factors correlated with the number of eligible peers. While we cannot rule out all unobservables that may bias our results we do address three specific potential confounders: nuisance flooding, proximity to the water bodies, CSO basin boundaries that overlap with arterial roads. We first assess whether homes with more eligible peers are more or less likely to be exposed to nuisance flooding. We define nuisance flooding as when water pools on the street or in the basement due to poor drainage. In some settings water can back up from the sewer lines into the basement when the sewer network is overloaded. While we do not directly have data on whether a home experienced flooding, we use two proxies: the property's elevation and distance to capacity constrained drainage systems.[26] The first step is to assess whether areas that might have drainage problems also have more eligible peers. We regress the number of eligible peers on elevation from digital elevation maps and the distance to the closest capacity constrained drainage system. The results, available in the Table A.6, show that homes with more eligible peers are located at higher elevations and are further from capacity constrained drainage systems. This is true on average, and also in the most extreme cases: the lowest quintiles of elevation and the closest areas to drainage complaints have the fewest eligible peers. This alleviates concerns that our instrument is positively correlated with unobservables that increase adoption. If drainage problems increase the RainWise adoption probability, then our instrumental variable approach may be downward biased. Additionally, this may explain why the IV estimates are greater than the OLS estimates of peer effects. Areas with more eligible peers, and therefore more peer adoptions, may be less likely to adopt due to fewer drainage problems.

Next, we assess other potential confounders including CSO basin boundaries that coincide with water bodies and arterial roads. In Table 9 we first replicate our base effect in column (1). Column (2) includes parcel controls such as the assessed value, lot size, bedrooms, bathrooms, and year built. We retain the parcel controls in all subsequent regressions in Table 9. In columns (3) and (4) we include variables that measure the distance from a property to the nearest water body and arterial road that overlaps with a CSO basin boundary. Column (5) adds a variable for the number of peers that installed mandatory GSI. Column (7) includes elevation as a control. Column (8) includes all controls and excludes properties below the 20th percentile of distance to the nearest water body, and column (9) removes properties below the 20th percentile of distance to the nearest arterial road that shares a border with a CSO basin. The results are almost exactly the same across all specifications.

Our last set of robustness checks varies the spatial unit of the fixed effect. We start with CSO basins since there may be specific features of the basin that are correlated with adoption. Next, we use zip codes, sub-areas (a spatial unit used by the King County Assessor's office), and census tracts. Lastly, we use basin-by-year fixed effects in column (6) which accounts for the time each household is eligible and block group-by-year fixed effects in column (7). The results, shown in Table A.7, are consistent across all specification, but there is more variation in the coefficient estimates than in Table 9.

[26]Elevation is a proxy for drainage problems because water flows down to the lowest point. Properties at higher elevations are less likely to have drainage problems compared to properties at lower elevations due to the water flowing from higher to lower elevations.Capacity constrained data systems are different areas where, "a drainage system that the Director of SPU has determined to have inadequate capacity to carry drainage water". The data and description are available at https://data-seattlecitygis.opendata.arcgis.com/datasets/SeattleCityGIS::capacity-constrained-drainage-systems/about.

Table 9: **Robustness of peer effects to controls and sample criteria**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Peer $\widehat{\text{Adoptions}}$	0.24***	0.24***	0.23***	0.23***	0.24***	0.24***	0.24***	0.28***	0.22***
	(0.04)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.06)	(0.10)	(0.08)
Fixed effects, controls, and sample									
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parcel Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Water Control	No	No	Yes	No	No	No	Yes	Yes	Yes
Road Control	No	No	No	Yes	No	No	Yes	Yes	Yes
Mand. GSI Control	No	No	No	No	Yes	No	Yes	Yes	Yes
Elevation Control	No	No	No	No	No	Yes	Yes	Yes	Yes
Exlude near Water	No	No	No	No	No	No	No	Yes	No
Exlude near Road	No	No	No	No	No	No	No	No	Yes
Observations	416,477	416,477	416,477	416,477	416,477	416,201	416,201	330,483	332,935
F	45.7	45.2	37.3	45.5	44.9	45.4	37.0	26.8	25.0
AR CI	[0.15, 0.33]	[0.15, 0.34]	[0.1, 0.36]	[0.15, 0.33]	[0.15, 0.34]	[0.15, 0.34]	[0.11, 0.37]	[0.08, 0.49]	[-0.01, 0.47]

Notes: The dependent variable is an indicator for whether a household signed up for RainWise in a given year. Parcel controls are lot size, sq. ft. of the home, bedrooms, bathrooms, dummies for time ranges from last renovation, and a dummy for water problems. Water control is the distance to the nearest water body. Road control is the distance to the nearest arterial road that overlaps with a CSO basin boundary. Mand. GSI control is the number of peers that installed mandatory GSI by year t and is constructed in the same way as RainWise peer adoptions are constructed. Exclude near Water/Road drops observations below the 20th percentile of distance to the nearest water body or arterial road overlapping a CSO basin boundary. Standard errors are clustered at the block group level. *p<0.1; **p<0.05; ***p<0.01

6 Policy analysis

In this section, we explore the policy implications of the RainWise program with a focus on the role of peer effects. Our goal is not a welfare analysis, but a basic cost-effectiveness comparison between RainWise and other approaches for the city and County to achieve compliance with their consent decree and the Clean Water Act. We do not attempt to monetize the value of reducing CSO events in improved water quality in a benefit-cost context. Research shows, however, that not all investments in water quality generate benefits that exceed the costs (Keiser and Shapiro, 2019a; Keiser et al., 2019).

As discussed in Section 2, Seattle and King County each operate under federal consent decrees to control the number of times that the combined sewer system fills and discharges raw sewage at 124 outfall locations.[27] Both jurisdictions are pursuing a wide variety of strategies to meet these goals and it is important to note that compliance is evaluated spatially (by outfall locations) rather than as a general reduction in stormwater flows averaged over the entire region. As such, some strategies and investments are more likely to guarantee compliance, like storage or treatment infrastructure targeted at specific outfalls. We cannot assess the likelihood that any individual project will increase compliance with the consent decree without a spatially-explicit hydraulic model, which is beyond our scope. Instead, we build on SPU and King County's aggregate goal to "mitigate" 1.1 billion gallons of stormwater, 700 million of which is to come

[27]'Control' is defined by Washington State regulation as no more than one untreated discharge per outfall per year based on a 20-year rolling average.

from GSI. We assume that each gallon mitigated has the same effectiveness in meeting pollution targets, regardless of its location in the city. We also ignore the length of each project's useful life and its annual operating costs, focusing only on the initial capital costs. Therefore, the costs discussed should be considered one-time capital costs as opposed to annual costs. We use estimates of gallons mitigated from data provided by SPU and King County, not our own calculations.

For each RainWise project, we observe: a) the total installation cost, b) the amount rebated to the homeowner, and c) SPU's estimate of the number of gallons mitigated. In the absence of data, we make several crude estimates on SPU's staffing and marketing costs.[28] We sum these staff and marketing costs over each year and attribute them equally to each RainWise project installed that year. Adding these costs to the rebated amount, we find that the average RainWise installation costs public agencies \$0.67 per gallon mitigated. Cistern projects were more expensive (\$0.91 per gallon) than rain gardens (\$0.46 per gallon).

One comparison is with other types of GSI projects that are located on public land or in the public right-of-way. Data from 18 public GSI projects provided by SPU and King County as part of our public records request, suggest cost-effectiveness ratios ranging from \$0.57 per gallon to \$1.65 per gallon, with an average of \$1.12 per gallon. These are more expensive than RainWise, though they have provided the bulk of GSI mitigated volumes so far.[29] We consider RainWise to be the marginal form of GSI for three reasons. First and most importantly, only 2,000 out of over 60,000 eligible households have signed up so there is spare capacity in the RainWise program. Second, public GSI is constrained by the availability of suitable public places that offer cost-effective mitigation, a problem that many cities face (Montalto et al., 2007). Third, mitigation achieved through the revised stormwater building code is strongly tied to the pace of new development and redevelopment, which is outside the control of stormwater managers.

We therefore argue that the opportunity cost of not mitigating a gallon using RainWise is the cost of mitigating stormwater from gray infrastructure. In other words, if Rain Wise did not exist, gray infrastructure projects would need to be scaled up to achieve compliance with the consent decree. We do not have detailed costs on all gray infrastructure projects, but we were able to collect estimates based on publicly available information. As discussed in Section 2, two major forms of gray infrastructure are dedicated stormwater treatment facilities and underground storage tunnels. Both specifically address CSO events and operate during heavy precipitation events. WTD operates four stormwater treatment plants and is constructing a fifth - the Georgetown Wet Weather Station - which we use for our cost estimates. The cost of the Georgetown Wet Weather Station is estimated at \$250–\$275 million. It is expected to mitigate 70 million gallons of

[28]We assume the project employed a full-time program manager throughout - one full-time equivalent (FTE) in the first four years of the project and a second FTE from 2015 onwards. We took average salaries from Glassdoor (program manager: \$170k and staff: \$135k) and added 30% for overhead. We also added \$75k for marketing in each of the first three years of the project, and \$150k from 2015 forward.

[29]As of 2020, SPU and WTD achieved 419 million out of the 700 million gallon mitigation goal through GSI. Over 50% of mitigation volumes from GSI are agency-led retrofits owned and managed in public spaces. The remainder are from stormwater code (22%), developer incentives (13%), RainWise (7%), and voluntary adoption (5%). By 2025, RainWise's share is predicted to increase to 10% or 45 million gallons out of 450 million gallons mitigated from GSI.

stormwater annually.[30] The Ship Canal Tunnel is storage project, due to be completed in 2024. It is expected to cost \$615–650 million and mitigate 75 million gallons. Using data from these two projects, we assume that gray infrastructure costs between \$3.6 – \$8.6 per gallon mitigated.[31] We use this range of gray infrastructure costs to generate bounds the cost savings that could be attributed to RainWise.

The 1,692 RainWise projects with complete data on gallons managed and costs mitigate 24.5 million gallons annually. The savings from using RainWise to avoid additional gray infrastructure costs is therefore roughly \$70–196 million. If we assume that RainWise projects with missing data have similar costs and gallons mitigated, the savings increase to \$85–235 million. Using the probit model to generate predicted adoptions with and without peer effects, we find that 37% of all adoptions can be attributed to peer effects.[32] This implies that roughly \$30–\$80 million dollars of the RainWise savings can be attributed to adoptions driven by peer effects.

Early adoptions generate a compounding effect similar to investing. An adoption in year t creates future adoptions due to the peer effects, which in turn generate more peer adoptions. The peer effect scaled by the number of eligible peers is the rate of return on an initial adoption. This rate of return is the peer effect (0.24%) times the mean eligible peers for an adopting household (89), which equals 21%. One adoption in year t generates 2.6 additional peers in $t+5$. The present value of net benefits of an adoption at year $t-5$ is calculated by scaling the adoptions by the gallons saved and the difference in dollars per dollars for RainWise compared to gray infrastructure and subtracting the present value of the costs.[33] The net present value of an adoption at year t ranges between \$38,000 –\$110,000, and increases to between \$99,000–\$285,000 at $t-5$. A key take-away for policymakers is that encouraging early adoptions leverages compounding adoption from peer effects. Our results also suggest that peer effects might be enhanced by a) strongly encouraging adopting households to keep their signs up and visible, b) encouraging adoption of rain gardens which are visible from the street, and c) focusing outreach efforts at eligible properties which are on street corners or near bus stops.

7 Conclusion

We estimate causal peer effects in RainWise, a policy that subsidizes residential green stormwater infrastructure in Seattle. Our identification strategy relies on variation in a household's relative

[30] Sources for costs and mitigation are from the firm constructing the facility (https://www.jacobs.com/projects/Georgetown-WWTS) and the EPA (https://www.epa.gov/wifia/king-county-georgetown-wet-weather-treatment-station).

[31] We note again the limitations of this comparison. The Ship Canal Tunnel will be a very long-lived asset, and is more certain to help the City and County meets its water obligations since it stores water closer to the outfalls.

[32] We use the probit model because the policy analysis focuses on prediction compared to the rest of the paper that focuses on estimating causal marginal effects. The probit model ensures that the predicted probabilities are bounded between zero and one.

[33] The average difference in costs per gallons is \$3–\$5 and the average RainWise installation mitigates 14,443 gallons. These values equate to an average savings of between \$38,000 –\$110,000 for one RainWise installation. The average rebate amount is \$4,234.

position within economically arbitrary CSO basin boundaries that determine eligibility. Peer effects have a positive and economically meaningful effect on adoption. The effect of one additional peer is almost equal to the annual unconditional adoption probability. In fact, the number of peer adoptions is the only meaningful predictor of adoption when estimating RainWise adoption with data-driven regression trees.[34] This is important because despite generous subsidies and extensive marketing the program has relatively low enrollment - roughly 2,000 out of more than 65,000 eligible households have signed up. Voluntary adoption of residential green infrastructure is a critical component of many cities' stormwater mitigation strategies. In Seattle we estimate that the RainWise program – even with low take-up – may have reduced the costs of achieving stormwater mitigation goals by between \$80-\$235 million, or roughly 6-15% of estimated compliance costs. Therefore, understanding the mechanisms underlying adoption decisions is critical for these programs to be successful and meaningfully reduce stormwater pollution. Encouraging early adoptions pays dividends over time through impacts on subsequent peer adoptions. In addition to encouraging adopting households to continue displaying the RainWise sign in their yards, other tools that amplify the signal of peer adoptions, such as social comparisons, may also be effective in increasing adoption.

Our results have implications beyond how peer effects impact cost effectiveness. Advocates of GSI argue that, unlike conventional stormwater infrastructure, GSI provides a suite of co-benefits such as aesthetic values, urban cooling, reduced flash flooding, among others. Therefore, the spatial distribution has implications for the distribution of benefits from GSI subsidy programs. Brent et al. (2022) show that both eligibility and participation decisions affect the spatial distribution of GSI benefits. Peer effects would amplify any initial conditions. This is an important avenue for future research since the benefit cost ratios of GSI compared to conventional stormwater infrastructure often depend on the existence of local co-benefits (Liu et al., 2016).

[34] We predict cross sectional RainWise adoption at the end of the sample with a host of parcel characteristics and the number of peer adoptions. Households with more than 5 peer adoptions have a 7.1% adoption probability, compared to 2.2% for homes with less than 5 peer adoptions. This approach does not attempt to address the endogeneity of peer adoptions and is simply focused on predicting adoptions.

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