Do City Climate Plans Reduce Emissions?

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Abstract

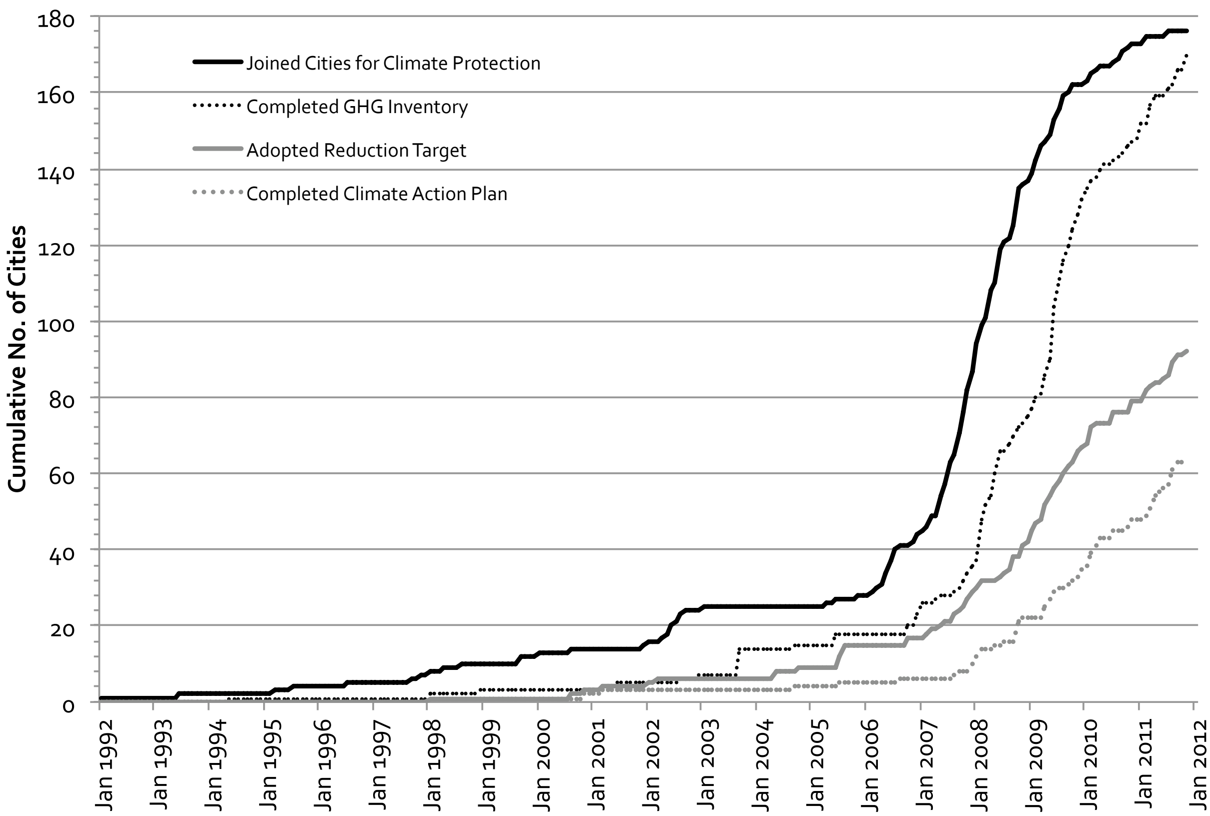
More than 600 local governments in the U.S. are developing climate action plans that lay out specific measures to reduce emissions from municipal operations, households and firms. To date, however, it is unclear whether these plans are being implemented or have any causal effects on emissions. Using data from California, I provide the first quantitative analysis of the impacts of climate plans. I find that cities with climate plans have had far greater success in implementing strategies to reduce greenhouse gas emissions than their counterparts without such plans. For example, they have more green buildings, spend more on pedestrian and bicycle infrastructure, and have implemented more programs to divert waste from methane-generating landfills. I find little evidence, however, that climate plans play any causal role in this success. Rather, citizens' environmental preferences appear to be a more important driver of both the adoption of climate plans and the pursuit of specific emission reduction measures. Thus, climate plans are largely codifying outcomes that would have been achieved in any case.

Keywords: climate action plan; climate change; local government; environmental preferences

1 Introduction

"Hyberbolic clouds of rhetorical gas" was how the *Seattle Times* (1988) dismissed efforts by local elected officials in the late 1980s to establish a climate policy for King County, Washington. Since then, local efforts to reduce greenhouse gas emissions have become mainstream. By December 2011, more than 600 local governments in the U.S. had joined the Cities for Climate Protection (CCP) Campaign,[1] and had committed themselves to complete a greenhouse gas inventory; adopt an emissions reduction target; and develop and implement a Climate Action Plan. In California, which provides the empirical setting for this paper, more than one-third of cities were involved in climate planning by the end of 2011 – a sixfold increase in five years (Figure 1).

Figure 1 **California Climate Planning Activity**



Sources: See Section 3.4. In 2011, there were 482 incorporated cities in California. Data current through November 2011.

[1] The Cities for Climate Protection (CCP) campaign was founded in 1993 by nonprofit ICLEI: Local Governments for Sustainability. Its forerunner between 1991 and 1993 was the Urban CO_2 Reduction Project (Lindseth 2004). In this paper, I treat joining the Urban CO_2 Reduction Project as equivalent to joining CCP.

Many cities have set ambitious targets as part of this process. Voters in Berkeley, California, for example, approved a goal of an 80% reduction by 2050. Lutsey and Sperling (2008) go so far as to conclude: "Governments have largely overcome the "commons" problem in dealing with climate change, with a broad range of effective state- and city-level policy mechanisms being put in place." Even the *Seattle Times* reversed its earlier stance. "They're not laughing at [former King County councilmember who initiated the 1988 effort] Ron Sims now," proclaimed its headline (Ervin 2006).

Local climate plans may certainly generate political pressure for action on the part of higher tiers of government (Knigge and Bausch 2006). In terms of their stated objectives, however, there has been little evidence to date to contradict the *Seattle Times'* original view that climate plans are largely rhetorical, and have minimal impact on decisions by local policymakers, firms or households. Indeed, most early work concluded that any impacts have been limited to small-scale energy efficiency programs with short paybacks, such as the installation of LED traffic signals (Bulkeley and Betsill 2003; Kousky and Schneider 2003; Wheeler 2008). But research of this type has either analyzed the plan documents rather than the impacts of those plans on specific projects and on emissions; or else been based on qualitative case studies, which can shed light on causal processes but may not generalize to the universe of cities undertaking climate action.

Local climate plans typically operate at an overarching policy or strategic level. They set out a menu of local government policy, regulatory and expenditure actions to achieve the emission reduction target that the city sets for itself. Implementation usually requires further City Council or staff action. For example, a plan may specify that the city will adopt a mandatory green building ordinance, which in turn is implemented through subsequent City Council legislative action. Some plan elements may rely more on a city's persuasive power or coordinating capacity, or be more aspirational. For example, the plan of the City of Albany, California, includes measures to encourage commercial property owners to install solar photovoltaic systems.

While the plans themselves lack regulatory force, they may have a causal impact through several pathways (discussed in detail in Millard-Ball under review). Climate planning may reduce information barriers through generating local knowledge about specific measures that can be taken to reduce emissions, and their relative costs. The plan may shape the preferences of residents, developers and elected officials in support of incurring costs to reduce emissions, or put climate change on the local agenda. A climate plan may also help prevent political backsliding, making it more difficult for elected officials to renege on the specific commitments in the plan. Thus, while specific emission reduction measures, such as green buildings or bicycling infrastructure, can be achieved through market outcomes or the regular local policy process, regardless of whether a city has a climate plan in place, the plan can increase the likelihood of implementation.

Understanding whether and how city climate plans reduce emissions, or have an effect on specific emission reduction projects and policies, is of both theoretical and policy interest. First, studying climate planning can help explain the wide variations in greenhouse gas emissions between cities (Glaeser and Kahn 2010). It can improve our understanding of the extent to which the heterogeneity observed by Glaeser and Kahn is due to physical factors such as prevailing temperatures, and to what extent cities are choosing of their own volition to contribute to the global public good of emission reductions. Second, climate planning provides a new window into long-standing debates in urban economics, discussed in more detail in Section 2, regarding the extent to which zoning and urban planning constrain market outcomes and affect welfare. Third, this paper builds on a nascent literature in urban economics about how political preferences affect local planning and regulatory decisions, as explored by Kahn (2011) in the context of new housing supply.

From the policy standpoint, considerable resources are being expended by cities and regional agencies on developing plans and completing related work such as emission inventories. Often, the plans are funded by state, federal or regional agencies, most recently through the federal Energy Efficiency and Conservation Block Grant Program. Some states are pressuring cities to complete climate plans – in California, through the threat of legal action.[2] In addition, states may see local government efforts as an important way to achieve the targets that states have set for themselves. While cap-and-trade or a carbon tax may be the most efficient way of reducing emissions, it is useful to understand the extent to which climate planning can achieve the same outcome, and whether the claim of Lutsey and Sperling (2008) – that cities are overcoming the commons problem without the need for a tax or cap-and-trade – is justified.

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[2] The California Attorney General has taken legal action under the California Environmental Quality Act against cities such as Stockton, arguing that the impacts of greenhouse gas emissions were not adequately considered during General Plan updates. As part of their settlement agreement, some cities committed to develop a Climate Action Plan. The Attorney General's Office has also encouraged cities to develop climate plans as a way to mitigate their greenhouse gas emissions (and, implicitly, avoid legal action). See http://ag.ca.gov/globalwarming/pdf/CEQA_GP_FAQs.pdf (last accessed December 9, 2011).

In this paper, I find that cities with climate plans have done more to reduce greenhouse gas emissions than their counterparts without such plans. For example, they have more green buildings, spend more on pedestrian and bicycle infrastructure, and have implemented more programs to divert waste from methane-generating landfills. These are unconditional results, however; they do not control for any of market and policy influences on local environmental decisions. After conditioning on environmental preferences and other control variables, I find little robust evidence that climate plans play any causal role in implementing greenhouse gas reduction strategies. Instead, citizens' environmental preferences appear to be a far more substantial driver behind the pursuit of specific emission reduction measures, and environmental preferences also help to explain why cities pursue climate plans in the first place. In the same way as zoning may largely "follow the market" and codify the market outcomes that would have happened anyway (Pogodzinski and Sass 1991; Pogodzinski and Sass 1994), climate planning appears to simply "follow the preferences" and codify outcomes in the same way. In a separate paper (Millard-Ball under review), I present results from qualitative case studies of select cities. The case studies confirm the validity of the quantitative findings reported here, and explore specific causal processes in greater detail.

While my findings call for caution when assessing the contribution that city climate planning efforts can make towards greenhouse gas reduction targets, they reinforce the view that cities are important players in climate change policy. Cities are taking action to reduce emissions, but the environmental preferences of their residents appear to be more important than the content of their climate plans. This conclusion suggests that rather than a focus on the creation of city-level climate plans, policymakers might be better served by redirecting those efforts towards the *implementation* of specific programs; funding emission-reduction projects in the progressive cities that already want to implement them; and (perhaps through marketing campaigns) shaping environmental preferences in a direction supportive of climate change mitigation, reinforcing the altruistic behavior that I suggest already exists.

The next section reviews the literature on climate planning, and relates it to the broader body of research on the impacts of zoning and land-use regulation. In Section 3, I outline my empirical approach to the quantitative analysis, with particular attention to controlling for selection bias and measuring environmental performance. Section 4 presents the quantitative results. Section 5 concludes with implications for both climate policy and understanding of the factors that encourage cities to contribute to environmental public goods.

2 The Impacts of Planning and Land-Use Regulation

Local climate planning has a number of characteristics that set it apart from other types of urban planning or land-use regulation. In particular, the explicit aim of climate planning is to marshal local contributions to a global public good, rather than (as with most zoning) address local externalities from adjoining land uses or the fiscal impacts of land-use decisions. Nonetheless, it can be instructive to first examine the wider body of literature on the impacts of planning, zoning and other land-use regulations, before turning to climate planning specifically.

A large volume of work in urban economics has sought to assess the extent to which land-use regulations are a binding constraint on land-use decisions or otherwise affect property values. Typically, researchers estimate a hedonic price model for land parcels or properties, and test for statistically significant coefficients attached to zoning or regulatory variables. Other approaches include regression discontinuity designs (Grout, Jaeger et al. 2011) and matching estimators (Zhou, McMillen et al. 2008).

Some papers find that land-use zoning tends to lead to the same outcome that would have been achieved under the unrestricted market (McMillen and McDonald 1993; Pogodzinski and Sass 1994). Zoning regulations may be adopted according to political pressure from landowners to allow the "highest and best use" at each location – helping to explain why they often seem to mirror market outcomes. Moreover, the competitive market often segregates land uses, even in the absence of zoning that prohibits mixed land-use patterns (McMillen and McDonald 1999). And in cities such as Houston that lack zoning, civic and private organizations find other ways to exert an influence on land use, for example through deed restrictions, homeowners associations and government action such as land assembly (Qian 2010).

In contrast, other studies, particularly those of growth control measures (Quigley and Raphael 2005; Wu and Cho 2007; Glaeser and Ward 2009; Grout, Jaeger et al. 2011), find strong evidence that regulations have an impact on land-use outcomes. There is also evidence that comprehensive planning can increase the desirability of a community, as evidenced through higher home values in cities that spend more on comprehensive planning (Ihlanfeldt 2009).

In short, evidence regarding the impacts of planning and zoning is mixed, perhaps partly because of the numerous methodological challenges highlighted by Fischel (1990). One review paper concludes that “despite the volume of research…it is difficult to reach many definite conclusions” (Pogodzinski and Sass 1991: 618). Even within a single region, land-use regulations may be binding in some areas but not in others, as Grout et al. (2011) find in Portland.

Research that specifically addresses local climate planning has likewise yet to provide a clear answer as to whether the plans have a causal impact on levels of greenhouse gas emissions. Moreover, researchers have focused less on identifying any causal impacts of planning, and instead explored the types of cities that pursue climate plans (Hanak, Bedsworth et al. 2008; Zahran, Grover et al. 2008); assessed the quality and contents of adopted climate action plans (Wheeler 2008; Bassett and Shandas 2010; Boswell, Greve et al. 2010; Tang, Brody et al. 2010); or used climate planning as a window to address theoretical debates in disciplines such as geography, law and political science (for a review, see Betsill and Bulkeley 2007).

Where researchers have sought to assess the impacts of climate plans on policy or environmental outcomes, the findings fall into two primary categories. In the first category, the climate plan is either not implemented, or else it merely repackages existing initiatives or measures that would have been implemented anyway (Bulkeley and Betsill 2003). In the second category, the implementation largely consists of cost-saving measures with short paybacks such as energy efficiency retrofits, or measures that address other municipal policy priorities such as local air pollution (Lambright and Changnon 1996; Collier 1997; Betsill 2001; Kousky and Schneider 2003; Slocum 2004; Burch 2010). In other words, plans appear to be either following the market or following pre-existing policy preferences. However, the research to date suffers from two related limitations. First, studies have been exclusively qualitative, and given the apparent absence of purposeful case selection, the extent to which findings generalize is unclear. Second, even where measures have been implemented or environmental outcomes achieved, it is difficult to isolate the causal role of the climate plan, as the decision to adopt a climate plan is unlikely to be exogenously determined. It is unclear whether the same outcomes would have been realized in the absence of a climate plan, through market and local policy decisions. In contrast to much of the zoning literature (such as Pogodzinski and Sass 1994), climate planning research has tended to ignore the endogeneity of climate plan adoption.

These limitations extend beyond climate planning to research on other types of environmental planning initiatives, and indeed more generally to planning scholarship, which has paid scant attention to the implementation of plans (Talen 1996). There are, however, some recent exceptions, including a self-reported survey of sustainability activities (Conroy and Iqbal 2009); assessment of the extent to which patterns of wetlands development conform with adopted plans (Brody and Highfield 2005); quantification of per capita CO_2 emissions at the metropolitan level (Glaeser and Kahn 2010); and explaining variations in indexes of city sustainability efforts (Portney 2003; Lubell, Feiock et al. 2009).

These latter research efforts mark a considerable step forward in quantifying environmental outcomes. However, they have not addressed the question of the causal contribution of planning towards achieving these outcomes. Is the considerable spatial variation in environmental outcomes among cities – whether wetlands development or CO_2 reduction – simply a function of differences in market opportunities, political ideology, demographics and physical constraints? For example, do some cities have more green buildings simply because of local preferences that increase demand for energy efficiency and other green features? In this case, these cities may incidentally choose to adopt a plan that reflects the decisions that elected officials, firms or households would have taken anyway. Or can one ascribe some role to planning – does the development of a local environmental plan increase the likelihood that specific policies and projects will be implemented? The following section outlines the methodological approach that I adopt to pursue these questions.

3 Empirical Approach

3.1 Addressing Selection Bias

Identifying any causal impacts of local climate plans (or indeed of any non-mandated planning effort) poses a potential problem of selection bias. If the factors that encourage cities to pursue climate plans are the same as those that encourage them to pursue specific strategies such as green building standards, or that provide green market opportunities for firms, there is a risk of inflating estimates of the impact of climate planning and arriving at a spurious result.

A randomized controlled trial would be the most convincing way to deal with selection bias, but is infeasible in this instance. It is also difficult to conceive of a valid instrument for climate plan adoption that satisfies the exclusion restriction, i.e., that the instrument only affects outcomes through its effect on climate plan adoption, and not through any other route. Instead, I present four models that offer different ways of handling selection bias, based on different assumptions about the impact of observed controls and the presence of unobserved heterogeneity between cities. While it is not possible to rule out any remaining selection effects, the use of multiple techniques makes it less likely that bias remains across all four approaches.

The first model employs a series of controls that explain the decision to adopt a climate plan. This assumes that one can observe these control variables and specify their correct functional form, in order to render outcomes independent of plan adoption conditional on observed controls. The second model uses a preprocessing matching step to reduce model dependence and reduce bias caused by selection on observable characteristics.[3] This second model takes the same form as the first, but is estimated via weighted least squares or another weighted estimator instead of OLS. In effect, the matching step weights observations to artificially create a control group that is similar on observable characteristics to the treatment group of plan adopters, prior to running a parametric regression (Ho, Imai et al. 2007). The third model uses fixed effects to control for unobserved city heterogeneity, making the assumption that these unobservables are constant over time. The fourth model combines fixed effects with the matching step, again using a weighted estimator. There is some evidence that difference-in-difference matching estimators perform better than cross-sectional estimators through eliminating time-invariant bias (Smith and Todd 2005), and one might expect the same advantages to accrue to matching methods that use fixed effects. Identification of the difference-in-difference matching estimator (and, by extension, the fixed effects matching estimator) rests on the assumption of bias stability, i.e. that even if propensity score matching does not eliminate selection bias completely, the residual bias is constant over time. However, this is weaker than the strong ignorability assumption required for cross-sectional matching estimators (Heckman, Ichimura et al. 1998; Slottje, Millimet et al. 2007).

While the fixed effects control for time-*invariant* unobservables, a remaining concern is that there exist time-*varying* unobservables that are correlated with both the dependent variable and the decision to adopt a climate plan. For example, a previously skeptical mayor may experience an epiphany after watching Al Gore's documentary film, *An Inconvenient Truth,* and immediately pursue both a climate plan and specific emission-reduction measures such as energy efficiency retrofits. Alternatively, a developer in a city may experience the same epiphany, and seek LEED certification for all of her buildings while

[3] The second approach (matching) is similar in intent to the first "battery of controls" approach. Both aim to control for variables that explain plan adoption. However, the matching step gives results that are more robust to misspecifications in a parametric model, such as using a linear model when the (unknown) true model is logarithmic or quadratic. For a full discussion and examples, see Ho et al. (2007).

persuading her City Council to adopt a climate plan. However, plausible unobservables such as these epiphanies are likely to be positively correlated with both the outcome variable and the probability of climate plan adoption, and would thus tend to inflate the coefficient on the treatment variable. Given that this paper provides little robust evidence that climate plans have any impact, the unobservables would need to operate in a particularly perverse manner in order to explain this null result by shrinking the coefficient on the treatment variable. To take the previous example, the mayor would have to pursue a climate plan after watching Gore's documentary, and at the same time *reduce* the city's efforts to implement specific emission-reduction measures.

3.2 Unit of Analysis

My sample encompasses the 478 incorporated cities in California as of December 2008. I exclude Census Designated Places and other unincorporated areas; other units of government such as counties and special districts; and four cities that have incorporated since 2008. Limiting the analysis to California increases the number of variables that it is possible to incorporate, as many are unique to California or may not be collected in a consistent way across states. On a theoretical level, limiting the analysis to California reduces unobserved heterogeneity between cities in the sample. California also accounts for more than one-quarter of Cities for Climate Protection (CCP) members in the United States, including some of the earliest adopters such as San Jose and Santa Monica (ICLEI 2011).

3.3 Measuring Environmental Performance: Dependent Variables

The lack of data has been one of the main limitations of attempts to analyze the performance of urban sustainability strategies, and perhaps explains why the literature to date is dominated by case studies. Kahn (2006), for example, proposes a conceptual Green City index, but accepts that the data are lacking to operationalize it. Even Portney (2003), one of the most ambitious attempts to analyze the degree to which cities are "taking sustainability seriously" focuses on the extent to which selective policy measures are being addressed, rather than whether environmental outcomes are achieved. Portney's resultant index is a somewhat eclectic mix of policies, some of which are hard to define.

Ideally, it would be possible to use greenhouse gas emissions as the dependent variable. However, even where emissions inventories have been prepared, they are not usually regularly updated, and data are limited to cities that are pursuing climate planning efforts. Therefore, I measure environmental performance using a series of eight policy output or intermediate outcome variables where data are available (Table 1): the number of green (LEED) building projects; the presence of mandatory green building standards; installations of residential solar photovoltaic systems; operating expenditure on street lighting; the number of waste diversion programs; capital expenditure on pedestrian and bicycle infrastructure; gasoline sales; and commute mode share by single-occupant vehicle.

Table 1 Dependent Variables Used

Name	Category	Type	Operationalization	Source
LEED* Projects	Energy – community-wide	Cross-sectional, all projects through April 16, 2010	Number of non-confidential LEED-registered or certified projects located within a city. Excludes state and federal projects (over which cities have no zoning jurisdiction)	US Green Building Council online database
Green Building Ordinance	Energy – community-wide	Longitudinal, annual 2004 to 2009	Presence of mandatory energy efficiency or green building standards for private development projects	Compiled from city websites; Cal. Attorney General; Cal. Energy Commission; Build it Green; Cal. Building Officials
Solar PV	Energy – community-wide	Longitudinal, annual 2007 through October 2009	Number of confirmed or completed applications for rebates under the California Solar Initiative	California Solar Initiative
Street Lighting	Energy – in-house	Longitudinal, annual 1998/99 to 2007/08. Note that some cities fail to report	Operating expenditure on street lighting per capita	Office of the Controller, *Cities Annual Reports*
Waste Diversion	Waste – in-house	Longitudinal, annual 1995 to 2008	Number of programs to reduce waste generation and increase diversion, such as curbside pickup and composting	California Integrated Waste Management Board, *Jurisdictional Profiles*
Ped/Bike Expenditure	Transportation – in-house	Longitudinal, annual 1999/00 to 2007/08. Note that some cities fail to report.	Log of per capita municipal expenditure on construction and right-of-way acquisition for pedestrian ways and bikepaths	Office of the Controller, *Cities Annual Reports*
Gasoline Sales	Transportation – community-wide	Longitudinal, annual 1993 to 2008. For 272 largest cities	Log (ad valorem sales tax revenue from service stations in a city divided by average annual gasoline price)	Revenue data from Board of Equalization, *Taxable Sales in California.* Price data from EIA
Auto Mode Share	Transportation – community-wide	Longitudinal – 1990, 2000 and 2005-07	Percentage of workers 16+ commuting by single-occupant vehicle, by city of residence	U.S. Census Bureau, Census 2000 and American Community Survey

* LEED stands for Leadership in Energy and Environmental Design. Perhaps the most popular of the green building rating systems, it is administered by the U.S. Green Building Council.

Climate plans usually include specific measures that address each of these eight variables (Hanak, Bedsworth et al. 2008; Bassett and Shandas 2010). Indeed, such measures – such as promoting residential solar photovoltaic systems and mandating green buildings – are recommended in climate planning resource guides for local governments (for example, ICLEI 2006), and typically included in the plans adopted by California cities.[4] Thus, if climate planning efforts are being successfully implemented, there should be change in at least one of the eight dependent variables.

I do not claim that these dependent variables address the most important elements of a climate plan in substantive terms. Nor do I claim that change on these dependent variables in the intended direction always increases economic welfare – the price premium for green buildings, for example, partly reflects energy savings (Eichholtz, Kok et al. 2010), but it may not be efficient to mandate a specific technology level for all new buildings. Rather, these dependent variables indicate the extent to which a plan is being implemented. Nor do I seek to evaluate the effectiveness of specific strategies such as green buildings or bicycle paths on emission reductions. This is the subject of a large and growing literature (for example, Dill and Carr 2003; Newsham, Mancini et al. 2009), and I assume that change in the dependent variables is a necessary (but perhaps not sufficient) condition for emission reductions to occur.

3.4 Treatment Variables

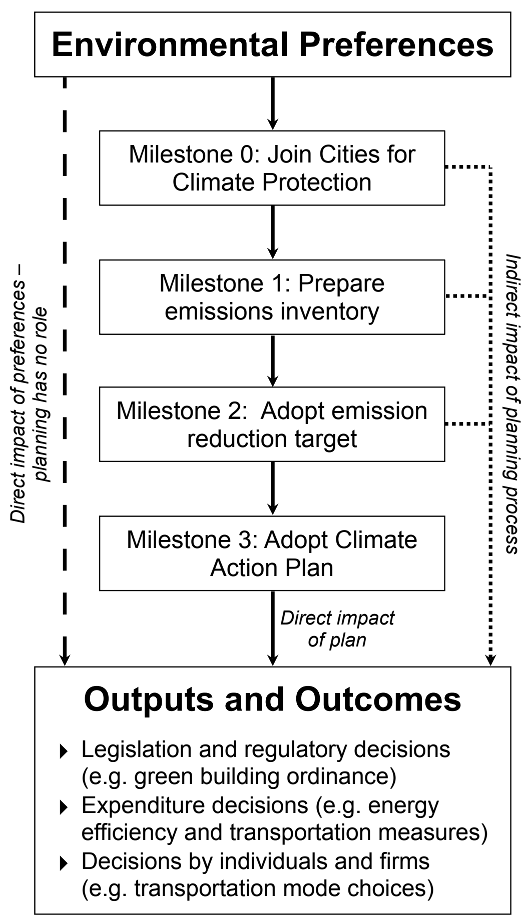
The independent variables fall into two categories: treatment variables and control variables. The empirical focus of this paper is on a set of treatment variables that represent cities' climate action planning efforts. I test the null hypothesis that the coefficients on all of these treatment variables are zero, i.e. that climate planning has no effect.

Members of the Cities for Climate Protection (CCP) campaign commit to follow a formal process of five milestones: (i) conduct a baseline inventory; (ii) adopt an emission reduction goal; (iii) develop a local action plan; (iv) implement emission reduction measures and policies; and (v) monitor and verify the results. Figure 2 shows how these steps may lead to changes in outputs and outcomes such as the number of green buildings. A technocratic hypothesis, steeped in a rational planning perspective, might be that climate planning only begins to have an effect once a plan is adopted – this pathway is

[4] With the exception of energy-efficient street lighting, each dependent variable was addressed by more than 80% of the adopted climate action plans in California. More than 90% of the plans addressed waste diversion and reducing fuel consumption and single-occupant vehicle commuting. Street lighting efficiency was addressed by two-thirds of the plans.

illustrated by the solid arrows in the center of Figure 2. A process-based hypothesis, rooted in a view that the planning process can be just as important as the actual plan, is that climate planning may affect decision-making even before a plan is adopted. For example, preparing an emissions inventory may promote policy learning or shape preferences among staff, and simply joining CCP may raise awareness among the public, provide political cover for officials to implement emission reduction strategies (Betsill and Bulkeley 2004), or increase the intangible value to firms of LEED and other green certifications. This pathway is illustrated by the dotted arrows on the right of Figure 2. By including treatment variables for earlier milestones, I am able to test both of these hypotheses: the technocratic pathway and the process-based pathway, and I am able to capture impacts of climate planning that may occur prior to plan adoption. In addition, preferences may lead directly to changes in outputs and outcomes without any role for planning, as illustrated by the dashed arrow on the left of Figure 2.

Figure 2 Potential Pathways for Climate Planning Impacts



The treatment variables I use represent the initial commitment to join CCP (Milestone 0), and the three subsequent milestones. The variables are defined polytomously in terms of the furthest milestone reached. For example, the Milestone 0 variable takes the value 1 if city has joined CCP but not completed any subsequent milestones, and 0 otherwise. If the city has adopted a climate plan, then Milestone 3 takes the value 1, and all other treatment variables are set to 0.

Milestone completion dates were primarily obtained from City Council minutes, planning documents and other official records. These searches were supplemented by lists of CCP members (ICLEI 2011 and earlier versions); websites of cities and regional climate planning collaboratives such as the Contra Costa County Climate Leaders Program; the UCLA database on climate planning activity in Southern California (UCLA 2011); and the California Planning Survey (OPR 2009 and earlier editions). Figure 1 shows the cumulative number of California cities that have reached each milestone.[5]

The primary results reported in this paper assume that each milestone has a constant effect, regardless of the length of time elapsed or the political preferences of cities. For the controls-only model with no lag in treatment effects, the basic form is shown in Eq. 1. If climate plans have an impact on emissions, but only through the technocratic pathway, then $\beta_0 = \beta_1 = \beta_2 = 0$, $\beta_3 > 0$. If climate plans work through the process-based pathway, then $\beta_3 > \beta_2 > \beta_1 > \beta_0 > 0$.[6] Recall that the treatment variables are polytomous, and thus the treatment coefficients refer to the combined effect of all milestones reached, not to the marginal effect of each additional milestone.

$$\begin{aligned} y_{it} = \alpha + MILESTONE0_{it}\beta_0 + MILESTONE1_{it}\beta_1 + MILESTONE2_{it}\beta_2 + \\ MILESTONE3_{it}\beta_3 + \mathbf{Z_{it}}\partial + \varepsilon_{it} \end{aligned} \tag{1}$$

Where i indexes cities; t indexes years; y_{it} is an outcome variable; α is the common intercept; $MILESTONE0_{it}$ through $MILESTONE3_{it}$ are the polytomous variables defined above; β_0 through β_3 are the coefficients of interest; $\mathbf{Z_{it}}$ is a vector of control variables; and ε_{it} is the error term (which may be serially correlated or spatially clustered).

[5] More than 1,000 cities have joined the US Mayors' Climate Protection Agreement and committed to "strive to meet or exceed Kyoto Protocol targets" (a 7% cut below 1990 levels by 2012). I do not count such a commitment as reaching Milestone 2, as joining the agreement is almost costless and is rarely backed up by analysis of feasible cuts in emissions.
[6] This assumes that an increase in the dependent variable is associated with a reduction in emissions, as for Solar PV. For variables such as Gasoline Sales where a decrease would reduce emissions, the signs must be reversed.

I also allow for the treatment effects to increase with the number of years since a milestone was achieved (Eq. 2), or to increase in cities with stronger environmental preferences (Eq. 3). These results are shown in the appendix, and except where noted in the main text, the results provide no additional evidence for any impact of climate planning.

$$y_{it} = \alpha + M0YRS_{it}\gamma_0 + M1YRS_{it}\gamma_1 + M2YRS_{it}\gamma_2 + M3YRS_{it}\gamma_3 + \mathbf{Z_{it}}\partial + \varepsilon_{it} \tag{2}$$

$$\begin{aligned} y_{it} = \alpha + M0_{it}ENVVOTING_{it}\lambda_0 + M1_{it}ENVVOTING_{it}\lambda_1 + M2_{it}ENVVOTING_{it}\lambda_2 + \\ M3_{it}ENVVOTING_{it}\lambda_3 + \mathbf{Z_{it}}\partial + \varepsilon_{it} \end{aligned} \tag{3}$$

Where: *M0YRS*, *M1YRS*, *M2YRS* and *M3YRS* are the number of years since reaching Milestones 0 through 3 respectively; *M0, M1, M2* and *M3* are the Milestone 0 through Milestone 0 variables; and *ENVVOTING* measures environmental preferences as defined in the subsequent section.

3.5 Control Variables

The control variables fall into three categories. *Demographic* variables include population, employment and education. *Political* variables attempt to account for differing political preferences across cities, which may influence the salience of climate change and the general willingness to undertake costly measures to reduce local emissions. Kahn (2011) shows how political ideology can affect urban development, with liberal cities in California limiting new housing supply, demonstrating the importance of including political preferences in any model. For ease of interpretation and to simplify the inclusion of interaction terms with treatment variables, I use principal components analysis to reduce the cross-sectional political variables to two dimensions – "Environmental Voting" and "Civic". *Other* variables include Latitude (which affects solar insolation) and Non-residential Construction. I also include fixed effects for year in order to control for changes in statewide economic conditions, advances in technology, and the availability of federal and state funding. The appendix provides a full list of control variables and their definitions and sources; summary statistics for both dependent and independent variables; and details of the principal components analysis.

4 Results

4.1 Why Cities Pursue Climate Plans

As noted in Section 3.1, matching techniques can be effective in reducing the dependence of results on a particular set of parametric assumptions or other forms of model dependence.[7] I follow the approach suggested by Ho et al. (2007) by first pre-processing the data to improve balance on covariates (i.e., match observations). Then, I run an ordinary least squares or another parametric regression on the weighted data. For theoretical and simulation evidence supporting a similar approach (linear regression adjustment of matched samples), see Rubin and Thomas (2000).

I use a probit model to estimate a propensity score as in Eq. 4:

$$\Pr\left[y_{it}=1 \mid y_{it-1}=0,\mathbf{X},\mathbf{y}_{t-1}\right]=f\left(\alpha+\mathbf{X}_{it}\beta+\rho\mathbf{W}\mathbf{y}_{t-1}+\varepsilon_{it}\right),\quad \varepsilon_{it}=\delta\varepsilon_{it-1}+\mu_{it} \tag{4}$$

The propensity score is the probability of beginning climate planning work i.e. joining the treatment group.[8] The function f is the probit link function. The vector **X** consists of the demographic and political controls listed in Table A-1, except for three variables (density, non-residential building and latitude) which seem unlikely to have a causal impact on the decision to undertake a climate plan. **X** also includes time trends[9]. I also include a spatially lagged dependent variable,[10] and an AR(1) serial autocorrelation term. The spatial lag accounts for diffusion effects, which may be present if cities tend to follow the lead of their neighbors in pursuing climate planning activity. The spatial lag is computed in GeoDa (Anselin, Syabri et al. 2003) with a 10-nearest neighbors spatial weights matrix **W**. The spatial lag is also temporally lagged one year, which simplifies the estimation through eliminating the endogeneity of the spatial lag, and is reasonable given that it should take some time for cities to react to

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[7] Intuitively, matching creates an artificial control group that is similar on observables to the treatment group. The matched control cities have a similar propensity to pursue a climate plan as the treatment cities that actually do so. By reducing the influence of observations that lie outside the region of common support, the matching step means that the results are less sensitive to assumptions such as a linear vs quadratic form.

[8] A city is considered as being "treated" if it has completed any milestone. Usually, this involves joining CCP, although in some cases a city undertakes subsequent milestones without first having joined CCP. I assume that once a city is treated it remains so, and discard subsequent observations. Thus, technically I am estimating transition probabilities from untreated to treated.

[9] Ideally, I would include dummy variables for each year to control for changes in the baseline probability of joining CCP (see Beck et al. 1998). However, in some years, no cities join CCP, meaning that the full set of dummies cannot be estimated as some would be perfect predictors. For this reason, I incorporate a quadratic time trend and also add dummy variables for 2007, 2008 and 2009. As shown in Figure 1, there was a dramatic spike in CCP membership in these years, meriting separate dummies in addition to the quadratic trend.

[10] See Wu and Cutter (2011) for evidence regarding the importance of explicitly modeling spatial dependence.

and adopt the policies of their neighbors.[11] In other words, the spatial lag variable takes the value 1 in a given year if all of a city's ten nearest neighbors had reached any of the milestones by the previous year; 0.9 if nine of the ten neighbors had done so; and so on.

The resulting coefficients and marginal effects for the propensity score estimates, reported in Table 2, provide substantively interesting results as well as being a prerequisite for the subsequent analysis. While none of the variables has a large marginal effect, this is primarily because the probability of beginning a climate planning process in a given year is very small in the first place (0.0225). The variables with the largest effects are the time trends (indicating that climate planning activity has accelerated over the period considered), the spatial lag (indicating the importance of policy diffusion or other spatial effects), and Environmental Voting (indicating the importance of political preferences). The latter result is in keeping with the findings of Zahran et al. (2008).

I then use the propensity scores to match cities, using a 5-nearest neighbors approach and the PSMATCH2 routine for Stata (Leuven and Sianesi 2003). I stratify the matching by year, so that a city that entered the treatment group in a given year is matched with the five control cities that have the most similar (smallest absolute difference) propensity scores in that same year. The same control city matches are used in all other years. Each treatment city receives a weight of one; each control city a weight of 0.2 multiplied by the number of times it is used as a match. A city that has been treated in any year through 2009 is ineligible to be used as a control city, even if it had not undergone treatment by the year of the match.

There is no generally accepted procedure for propensity score matching of panel data, as most of the methods have been developed for cross-sectional data. Thus, some caution should be attached to the results. The approach here is analogous to methods that match treated and control observations based on the propensity score in the year of treatment, but then apply a difference-in-difference rather than a fixed effects estimator (List, Millimet et al. 2003; Slottje, Millimet et al. 2007). My procedure is also very similar to the algorithm advocated by Nielson and Sheffield (2009), except that I perform exact

[11] More precisely, the temporal lag eliminates the endogeneity of the spatial lag if the spatial lag is predetermined, and there is no spatial interdependence within the one-year observational period. This strategy avoids computational complexities and is proposed by Rincke (2007) and, for continuous variables, by Beck et al. (2006). However, as Franzese and Hays (2009) note, the sensitivity of the time-lagging strategy to the lagged-interdependence-only and other assumptions has yet to be explored in Monte Carlo simulations.

matching by year, and the matching is based on propensity scores in the treatment year only, rather than (as in the Nielson and Sheffield case) all years prior to treatment.

Table A-4 shows the improvement in covariate balance that is achieved from the weighting. The t-tests reported in Table A-4 indicate there are still some significant differences between the treatment group and the artificially constructed control group on observed covariates, particularly education, median income and environmental voting. Most importantly, however, I cannot reject the null of equality of the mean propensity scores, as matching on propensity scores is equivalent to matching on a higher dimensional vector of covariates (Rosenbaum and Rubin 1983). Together with visual evidence that the common support (i.e., overlap) assumption holds for the propensity score (Figure A-1), this increases confidence in the ability of the matching procedure to reduce bias and model dependence. The overlap between treatment and control groups is also reasonable on the three covariates where there remain significant differences in means (Figure A-1).

Moreover, as I subsequently run parametric regression models on the weighted data rather than taking a simple difference in means, balance does not need to be perfect. The procedure is doubly robust in that subsequent regression can eliminate the remaining bias (Ho, Imai et al. 2007). Alternative matching algorithms such as kernel matching do not improve covariate balance any more and come at the cost of reducing the sample size further. And to the extent that bias remains, this should tend to inflate the coefficient on the treatment variable; I am therefore more confident in my finding of "no effect."

Table 2 Probit Results for Probability of Undertaking Climate Planning

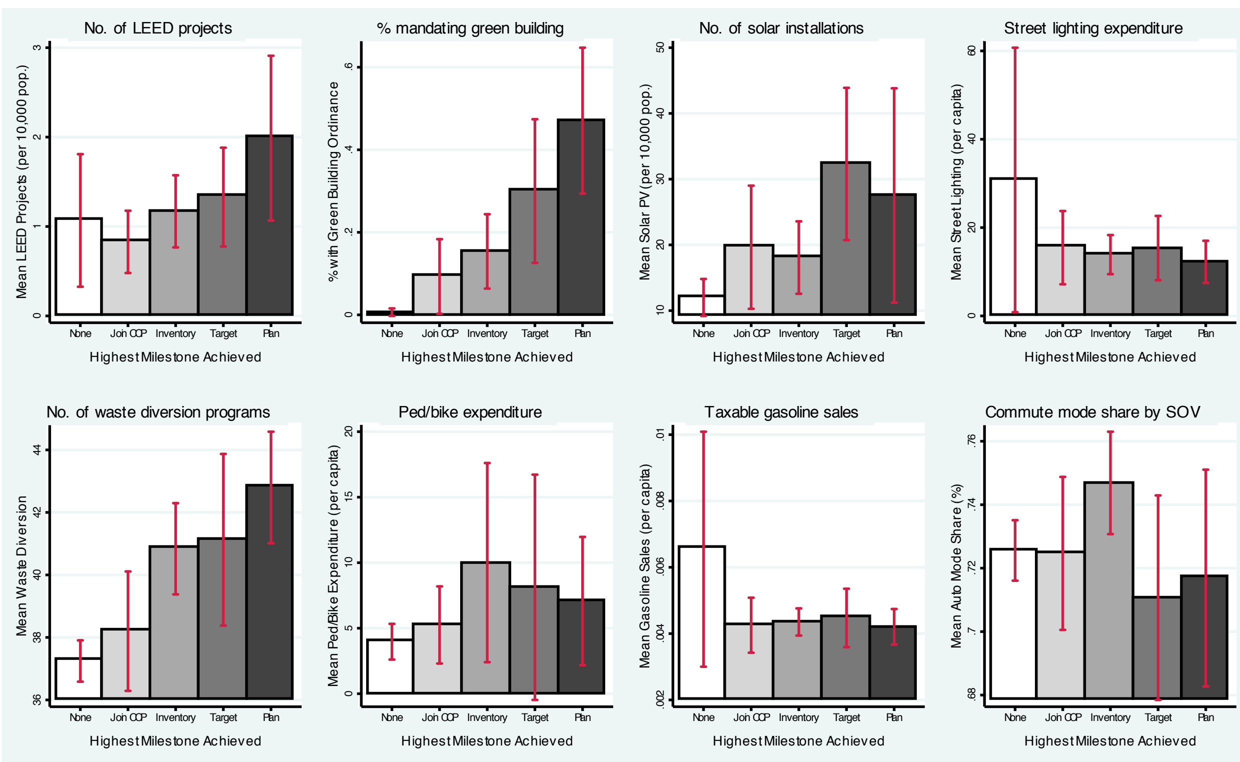
	Coefficient	Standard error	Marginal effect*
Log Population	0.127	(0.0987)	0.000661
Log Employment (2000)	0.0850	(0.0903)	0.000503
Median Income (1999)	-1.24e-07	(4.12e-06)	-0.0000151
Education (2000)	1.049	(0.760)	0.000922
Environmental Voting	0.259	(0.0645)	0.00120
Civic	0.0615	(0.115)	0.000281
Nonprofits	0.270	(0.0814)	0.000856
Spatial lag dependent variable	2.620	(0.220)	0.00156
Time	0.0598	(0.0904)	0.00145
$Time^2$	0.000683	(0.00481)	0.000319
Year=2007	0.617	(0.218)	0.0030
Year=2008	0.656	(0.281)	0.0031
Year=2009	0.578	(0.357)	0.0028
Constant	-6.269	(0.729)	
N	7383		

* For continuous variables, the marginal effect is the change in probability from a one standard deviation increase in the variable, with all variables held at their means. For the binary variables (Year=2007, 2008 and 2009), the marginal effect is the change in probability from an increase from 0 to 1, with all other variables held at their means.

4.2 Environmental Performance in Climate Planning Cities

Figure 3 shows the unconditional means of the eight dependent variables for cities at different milestones along the climate planning process. These results show that environmental performance tends to vary with the climate planning milestone reached. Cities that have climate plans perform better on all of the eight dependent variables compared to cities that have not joined Cities for Climate Protection. For example, they have a lower commute mode share for driving alone, more LEED buildings per capita, and are more likely to have adopted a green building ordinance. However, there is often high variability among cities that have achieved each milestone (as indicated by the error bars in Figure 3). Moreover, these results do not imply that climate plans have any causal role in these differences. Therefore, the subsequent sections present the results using the methods to control for selection bias discussed earlier.

Figure 3 Differences in Environmental Performance



Bars indicate 95% confidence interval

4.3 Impacts of the Climate Planning Process

Tables 3 to 5 present the estimates of coefficients and standard errors for each of the eight dependent variables. In each case, I perform Wald tests of two null hypotheses – first, that β_3 (the coefficient on Milestone 3) is equal to zero and climate plan adoption has no impact (the technocratic pathway); and second, that the marginal effect of the three earlier milestones sum to zero (the process-based pathway).[12]

In most cases, I lag the treatment variables by one year, assuming that there is a one-year delay between plan adoption and policy implementation. Most of the policies considered can take effect very quickly – for example, a mandatory green building ordinance requires only City Council action, while my measure of LEED buildings only requires registration (a costly signal of intent by a developer) rather than actual construction. For Gasoline Sales, it seems reasonable to expect a longer delay and accordingly I use a two-year lag. Unless stated, results are robust to longer two- or three-year lags (and four years in the case of Gasoline Sales). For five of the eight dependent variables, I present results based on all four specifications discussed in Section 3.1 (controls only, matching, and fixed effects with and without matching).[13] For the remaining three dependent variables (LEED Projects, Green Building Ordinance and Ped/Bike Expenditure), I present results for the first two specifications (controls only and matching).[14]

The parametric form varies with the dependent variable. For LEED Projects, the dependent variable is in count form with a large number of zero values (170 of the 478 cities have no LEED projects, and a further 90 have just a single one). I therefore use a negative binomial model. For similar reasons, I use a

[12] I use a one-sided Wald test. Note that for ease of computation, I redefine the coefficients for the process-based test, so that γ_a takes the value 1 if Milestone a has been reached and zero otherwise. Thus, $\beta_a = \sum_{i=0}^{a} \gamma_i$.
I then test the null hypothesis that $\gamma_0 + \gamma_1 + \gamma_2 = 0$. This is weaker than the hypothesis presented in Section 3.3, i.e. $\beta_3 > \beta_2 > \beta_1 > \beta_0 > 0$, but it is a necessary (albeit not sufficient) condition for the hypothesis to hold. Note that since Milestone 3 (plan adoption) does not enter into this test, the results should be considered in parallel with the test that $\beta_3 = 0$. It is difficult to conceive of a scenario in which Milestones 1, 2 and 3 have an impact that then disappears as soon as the plan is adopted, and thus evidence for the processual hypothesis requires rejection of both null hypotheses (that $\beta_3 = 0$ and $\gamma_0 + \gamma_1 + \gamma_2 = 0$).

[13] For Auto Commute Share, I use first differences instead of fixed effects as I have only three periods of data.

[14] For LEED, I treat the data as cross-sectional and am unable to estimate the fixed effects models as they require panel data for identification. For Green Building Ordinance, I use a probit model, and inclusion of fixed effects would restrict the sample to cities that have adopted a green building ordinance during the sampling timeframe. (For cities that have never adopted such an ordinance, the fixed effect would perfectly predict the decision not to adopt.) For Ped/Bike Expenditure, I use a tobit model that encountered computational problems when adding fixed effects.

negative binomial model for Solar PV. For Ped/Bike Expenditure, I use a tobit model due to left-censoring at zero (cities cannot make negative expenditure). For Green Building Ordinance, the dependent variable is binary and I use a probit model.[15] For the other dependent variables, I use a linear model. Where possible in the software routines, I account for spatial autocorrelation by allowing standard errors to cluster by county, and include a first-order serial autocorrelation term.

Unless otherwise stated, the results are robust to a series of robustness checks. These include using longer lags for the treatment variables as noted above; adding additional controls such as race and dummy variables for metropolitan area; eliminating controls where coefficients are small relative to their standard errors; and, in some cases, using different functional forms (such as a log-linear or zero-inflated negative binomial instead of a negative binomial model).

LEED Projects

The results for LEED Projects provide no evidence that climate planning has any role in promoting green buildings. Indeed, the coefficient on Milestone 3 is negative, indicating that cities with climate plans have fewer LEED buildings. In contrast, there are large effects from many other variables. As expected, cities with higher employment and non-residential growth have more LEED buildings. Environmental preferences (Environmental Voting and Civic) also have positive effects, possibly due to greater market demand for the intangible benefits of green building certification (Eichholtz, Kok et al. 2010; see also Deng, Li et al. 2012) or because LEED certification helps with obtaining entitlements in progressive cities. Density has a negative coefficient – perhaps because some LEED criteria are less suited to infill developments in established urban areas.

The coefficients in Table 3 are difficult to interpret directly, and the impact of environmental preferences can best be illustrated with examples. Using the results from Model 2 (matching), a one-standard deviation increase in the Environmental Voting score is associated with an additional 0.4 LEED projects; and a one-standard deviation increase in the Civic score with an additional 0.7 LEED projects. The effect size may seem small, but the median city only has a single LEED project. To take a more concrete example, Corona in Riverside County, southern California lies on about the 25% percentile for both Environmental Voting and Civic. Increasing Corona's Environmental Voting and

[15] Technically, I use a probit model to estimate transition probabilities of a city adopting a green building ordinance, conditional on having no such ordinance in the prior year. This is because no cities in the sample have repealed their green building ordinance. Cities drop out of the sample once they have adopted an ordinance.

Civic scores to the level of Berkeley, which has the highest Environmental Voting score and the second-highest Civic score, increases Corona's predicted number of LEED projects from 4.9 to 14.0 (the actual number of LEED projects in Corona is 4).

Green Building Ordinance

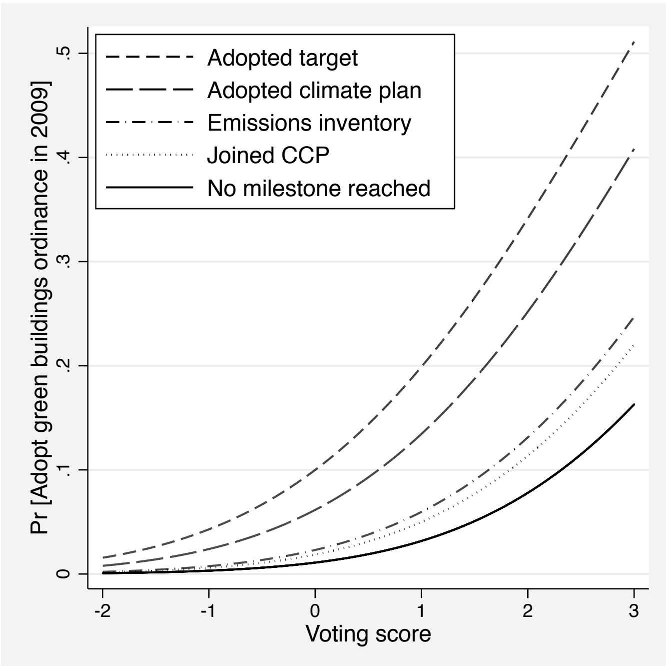
There are some indications that climate planning is associated with a higher probability of adopting a green building ordinance (Table 3). In Model 1 (controls only), I reject the null hypothesis that the coefficient on Milestone 3 is zero in both Model 1 ($p = 0.016$) and Model 2 ($p = 0.048$). There is also evidence that climate planning has an impact through the process-based pathway – I comfortably reject the null hypothesis in both models, and coefficients generally increase in magnitude as additional milestones are reached. The effect size is large, particularly for progressive cities with a high Environmental Voting score (Figure 4). For a city with a median Environmental Voting score, adopting a climate plan increases the probability of adopting a green building ordinance in the year 2009 from 0.01 to 0.06.

It is difficult to assert, however, from these results alone that the relationship between climate planning and adoption of a green building ordinance is a causal one. Partly, this difficulty stems from the inability to use fixed effects to control for unobserved heterogeneity, as discussed above. And partly, the difficulty in asserting causality is due to the timing of ordinance adoption. Of the 39 cities with a green building ordinance as of the end of 2009, nearly as many adopted the ordinance before the climate plan (five cities) as adopted the plan before the ordinance (eight cities). (Two adopted both in the same year; the remaining 24 had yet to adopt a climate plan by the end of 2009.) One possible explanation is that the same (unobserved) factors are driving the adoption of both climate plans and green building ordinances. In one case study city, for example, the same staff member has responsibility for green buildings and climate planning. In Silicon Valley, the local Sierra Club chapter runs a campaign that is advocating for both simultaneously.

Moreover, the results for green building ordinances are not robust to different lag structures, nor to the addition of further controls. While a one-year lag from plan adoption to ordinance adoption (as specified in Table 3) seems the most plausible, increasing the lag to two or three years means that I fail to reject the null hypothesis that the plan has no impact ($p = 0.29$), although earlier milestones still have a large effect. A similar result is obtained through adding a control for nonresidential building permits;

it is plausible, although not particularly likely, that cities may be more likely to adopt a green building ordinance in the face of rapid growth.

Figure 4 Impact of Climate Planning on Green Building Ordinance Adoption



Predicted probabilities from Model 2 (matching). All other variables held at means

Solar Energy

The results for Solar PV from Models 1 and 2, without fixed effects, suggest that climate planning does increase the number of residential solar photovoltaic installations. For these two pooled models, I comfortably reject the null hypothesis that the coefficient on Milestone 3 is equal to zero (Table 3). The effect size is also large. In Model 2, adopting a climate plan is associated with an additional 13 residential solar installations, with all other variables held at their means. For comparison, the median city has only 20 installations.

Education and population have large positive effects, but surprisingly, the coefficient on Environmental Voting is negative, indicating that environmentally progressive cities have fewer solar installations. Also surprisingly, Latitude has a positive effect indicating that the number of solar installations is decreasing in the amount of solar insolation.

The positive impacts of climate plan adoption disappear in the two fixed-effects models (3 and 4). Indeed, coefficients on all treatment variables become negative. Thus, the results from Models 1 and 2 are likely to be explained by unobserved heterogeneity, for example a pre-existing base of solar installers; favorable roof conditions in the housing stock; or high electricity bills that make solar installation more economic. One caveat with the fixed-effects models is that they entail some data loss; the 58 cities with no solar installation in any year are dropped from the analysis, as they would be perfectly predicted by the fixed effect. However, the mixed results from the different models suggest that caution is required in drawing conclusions.

Street Lighting

There is no evidence from any of the models (Table 4) that climate planning is associated with a reduction in street lighting expenditure, as would happen through the implementation of efficiency upgrades. Coefficients on the treatment variables are either positive, or are well within one standard error of zero. These findings are robust to alternative forms of the dependent variable, such as using a per-capita measure of street lighting expenditure, or restricting the sample to cities that operate street lighting services themselves rather than through a private contractor or other agency. Nor is there any detectable association between street lighting capital expenditure and climate planning, as might occur if a city undertakes a capital investment program to improve energy efficiency.

Waste Reduction

Models 1 and 2, without fixed effects, do not provide any evidence that climate planning is associated with a greater number of waste reduction programs (Table 4). They do suggest that environmental preferences (Environmental Voting and Civic) lead to the implementation of more programs.

The two fixed-effects models, in contrast, show the number of waste reduction programs increasing with each milestone achieved. However, the statistical significance of these results is borderline. At a 95% confidence level, I fail to reject the null that the coefficient on Milestone 3 is zero in Model 3 (p = 0.054), and I only just reject the null in model 4 (p = 0.049). Moreover, these results disappear in the face of slight adjustments to the model specification, such as dropping Nonprofits (which has a counterintuitive negative sign). Thus, it is possible that the association between climate planning and waste reduction programs in the fixed effects models represents a spurious result.

Pedestrian and Bicycle Expenditure

There is no evidence of any association between climate planning and pedestrian and bicycle expenditure. All coefficients on treatment variables are within one standard error of zero, or have a negative sign (Table 5). Indeed, the model as a whole lacks explanatory power. While expenditure increases as expected with population and employment, environmental preferences and demographic variables seem to have little impact.

Gasoline Sales Tax Revenue

The pooled models (1 and 2) do not reveal any impact of climate planning. In line with a large body of literature (e.g. Ewing and Cervero 2010), larger, denser cities consume less gasoline on a per-capita basis. But there is no evidence that environmental preferences have an independent impact (other than through choice of residential location).

Failure to reject the null hypothesis that climate planning has no impact could, however, be because the controls are failing to fully account for heterogeneity between cities. This view is supported by the results from the fixed effects models (3 and 4), where I reject the null hypotheses that the coefficient on Milestone 3 is zero. Moreover, the coefficients increase in magnitude as each subsequent milestone is reached. This suggests that unobservable factors are leading to higher gasoline sales for cities involved in climate planning.

A plausible explanation, however, is that the results from the fixed effects models are being driven by two cities – Berkeley and San Francisco – that adopted climate plans in 1998 and 2004 respectively. Of the cities in the sample, these had the two lowest drive-alone rates in 1990, indicating that there exist attractive transit and non-motorized transportation options for residents in these cities. In turn, these transportation options may have made it easier to switch to other modes following the run-up in gasoline prices in 2007 and 2008. Excluding Berkeley and San Francisco from the analysis means that I fail to reject the null hypothesis that climate plans have no impact ($p = 0.051$ for the fixed-effects model without matching; $p = 0.176$ for the fixed-effects model with matching).

Gasoline sales tax revenue is the one dependent variable for which the interaction between Environmental Voting and the treatment variable is large and statistically significant ($p < 0.05$ for all four models). Again, however, this result is being driven by Berkeley and San Francisco. Omitting from the sample leads to a non-significant result with a one-sided *p* value between 0.06 and 0.49 depending on the model.

Commute Mode Share

The results from the cross-sectional models 1 and 2 suggest that climate planning does have an impact on reducing commute mode share by single-occupant auto. However, the differenced models (3 and 4) show no such impact – again, indicating that the explanation lies with unobserved factors such as workplace locations, urban structure and preferences for public transportation use, rather than climate planning.

The cross-sectional results do point to the role of environmental preferences. A one-standard deviation increase in the Environmental Voting score is associated with a 4.6% decrease in the drive-alone rate. In Kahn and Morris' (2009) words, greens appear to be are "walking the walk" in matching their travel behavior to their ideological beliefs.

Table 3 Results for LEED Projects, Green Building Ordinances and Solar PV

Dependent Variable	**LEED Projects**		**Green Building Ordinance**		**Solar PV**			
Model Type	Negative Binomial		Probit		Negative Binomial			
Model	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects
Lag Milestone 0*	0.0857	0.128	0.330	0.212	-0.0994	0.0185	-0.244	-0.150
	(0.213)	(0.193)	(0.243)	(0.291)	(0.0832)	(0.0920)	(0.0572)	(0.0626)
Lag Milestone 1*	0.156	0.317	0.481	0.298	-0.153	0.0372	-0.461	-0.328
	(0.120)	(0.147)	(0.395)	(0.453)	(0.125)	(0.140)	(0.0838)	(0.0923)
Lag Milestone 2*	0.535	0.626	1.063	1.011	0.131	0.367	-0.452	-0.298
	(0.144)	(0.156)	(0.267)	(0.283)	(0.174)	(0.185)	(0.110)	(0.118)
Lag Milestone 3*	-0.495	-0.388	0.900	0.751	0.340	0.707	-0.310	-0.123
	(0.258)	(0.251)	(0.420)	(0.450)	(0.184)	(0.227)	(0.151)	(0.162)
Log Population*	0.0540	0.0193	-0.0740	0.146	0.759	0.623	0.0639	0.133
	(0.0998)	(0.0885)	(0.161)	(0.233)	(0.0471)	(0.0793)	(0.0914)	(0.0901)
Log Employment (2000)	0.815	0.848	0.211	0.0157				
	(0.0834)	(0.127)	(0.140)	(0.185)				
Density (2000)	-0.118	-0.0949	-0.00698	0.0193	-0.131	-0.154		
	(0.0327)	(0.0327)	(0.0444)	(0.0392)	(0.0242)	(0.0477)		
Education (2000)	0.994	2.220	-1.164	-1.133	2.738	4.106		
	(0.880)	(0.895)	(1.049)	(1.143)	(0.632)	(0.963)		
Median Income (1999)	-1.64e-06	-5.98e-06	1.30e-05	1.40e-05	-4.88e-06	-1.01e-05		
	(5.23e-06)	(5.23e-06)	(4.67e-06)	(5.22e-06)	(2.69e-06)	(3.86e-06)		
Non-Residential Building (2000-08)	0.000220	0.000210						
	(8.02e-05)	(0.000109)						
Environmental Voting	0.251	0.128	0.373	0.437	-0.218	-0.325		
	(0.0724)	(0.0824)	(0.0986)	(0.151)	(0.0592)	(0.0991)		
Civic	0.339	0.243	0.278	0.226	0.510	0.232		
	(0.125)	(0.119)	(0.138)	(0.164)	(0.0931)	(0.188)		
Lag Nonprofits							-0.00775	-0.00953
							(0.0107)	(0.0106)
Latitude					0.203	0.145		
					(0.0223)	(0.0362)		
Time			0.581	0.262				
			(0.265)	(0.333)				
Time^2			-0.0392	-0.00532				
			(0.0226)	(0.0349)				
Constant	-7.569	-7.755	-6.200	-6.161	-13.40	-9.809	1.696	0.754
	(0.897)	(1.117)	(1.337)	(1.343)	(1.111)	(1.719)	(0.983)	(0.962)
Fixed Effects for City?	No	No	No	No	No	No	Yes	Yes
Fixed Effects for Year?	No	No	No	No	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes
ln α (dispersion factor)	-0.918	-1.429						
	(0.210)	(0.507)						
Observations	423	284	3006	2019	439	298	1260	828
Wald test ($\beta_3 = 0$)	0.972	0.939	0.0162	0.0477	0.0325	0.000924	0.980	0.777
Wald test ($\gamma_0 + \gamma_1 + \gamma_2 = 0$)	0.00001	0.00000	0.0000619	0.000213	0.226	0.0199	1.000	0.998
Milestone 3 marginal effect** (95% conf. interval)	[-1.17, -0.16]	[-1.89, 0.06]	[-0.03, 0.07]	[-0.04, 0.10]	[-0.92, 8.59]	[2.24, 24.00]	[-0.61, -0.02]	[-0.44, 0.20]

Standard errors in parentheses, clustered by city (GREENBLDG and Solar PV models 1 and 2) or county (LEED Projects).

* For LEED Projects, the treatment variables and Log Population are measured in 2006, as the majority of LEED projects were registered in 2007-09. For Green Building Ordinance and Solar PV, the treatment variables are lagged one year and Log Population is not lagged.

* Marginal effects of completing Milestone 3, with Milestones 0 through 2 set to zero. All other covariates are held at means.

Table 4 Results for Street Lighting and Waste Reduction

Dependent Variable	Street Lighting				Waste Diversion Programs			
Model Type	Linear regression				Linear regression			
Model	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects
Lag Milestone 0	1.523	1.485	-0.591	0.247	0.193	0.191	0.199	0.372
	(0.993)	(1.085)	(2.158)	(1.641)	(0.133)	(0.149)	(0.415)	(0.395)
Lag Milestone 1	1.206	1.065	-0.506	0.178	0.677	0.661	0.903	1.276
	(0.857)	(1.028)	(2.168)	(1.742)	(0.318)	(0.333)	(0.867)	(0.910)
Lag Milestone 2	0.770	0.754	0.755	1.155	0.192	0.221	0.710	1.050
	(0.939)	(1.116)	(2.569)	(2.177)	(0.293)	(0.324)	(0.999)	(1.032)
Lag Milestone 3	0.645	0.622	-2.924	-1.370	-0.493	-0.435	1.272	1.646
	(0.880)	(1.249)	(3.010)	(2.284)	(0.397)	(0.447)	(0.776)	(0.970)
Log Population	-6.623	-6.952	2.283	11.48	1.354	1.821	0.975	4.763
	(2.615)	(3.761)	(6.662)	(7.502)	(0.383)	(0.564)	(1.261)	(2.149)
Log Employment (2000)	5.214	3.741			0.0905	-0.688		
	(2.016)	(3.023)			(0.347)	(0.432)		
Density (2000)	0.516	1.292			-0.407	-0.448		
	(0.379)	(0.720)			(0.148)	(0.179)		
Education (2000)	-10.61	7.784			2.545	9.036		
	(8.699)	(17.02)			(2.866)	(3.881)		
Median Income (1999)	9.81e-05	-4.25e-05			-2.09e-05	-4.88e-05		
	(5.80e-05)	(0.0001027			(1.27e-05)	(1.78e-05)		
Environmental Voting	0.975	1.034			0.414	0.544		
	(0.840)	(1.609)			(0.266)	(0.361)		
Civic	-0.199	-4.947			0.736	-0.116		
	(0.856)	(2.135)			(0.347)	(0.661)		
Lag Nonprofits			-0.134	-0.302			-0.00348	-0.00532
			(0.0860)	(0.170)			(0.0671)	(0.0685)
Latitude	-0.426	-0.409						
	(0.266)	(0.398)						
Constant	39.97	59.06	-4.052	-111.0	13.43	16.43	16.74	-22.09
	(16.9)	(21.93)	(67.89)	(79.34)	(2.093)	(3.495)	(12.77)	(22.44)
Fixed Effects for City?	No	No	Yes	Yes	No	No	Yes	Yes
Fixed Effects for Year?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2616	1829	3138	2012	5217	3579	5585	3592
Wald test (β_3 = 0)	0.766	0.691	0.168	0.276	0.893	0.835	0.0535	0.0488
Wald test ($\gamma_0 + \gamma_1 + \gamma_2 = 0$)	0.790	0.738	0.626	0.700	0.163	0.165	0.185	0.118
Milestone 3 marginal effect* (95% conf. interval)	[-1.10, 2.39]	[-1.83, 3.07]	[-8.82, 2.98]	[-5.85, 3.11]	[-1.27, 0.29]	[-1.31, 0.44]	[-0.25, 2.79]	[-0.26, 3.55]

Standard errors in parentheses, clustered by city (models 1 and 2) or county (fixed-effect models 3 and 4).

* Marginal effect of completing Milestone 3, with Milestones 0 through 2 set to zero. All other covariates are held at means.

Table 5 Results for Pedestrian/Bicycle Expenditure, Gasoline Sales and Commute Mode

Dependent Variable	Ped/Bike Expenditure		Gasoline Sales				Auto Commute Share			
Model Type	Tobit		Linear regression				Linear regression			
Model	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects	(1) Controls only	(2) Matching	(3) First differences	(4) Matching + first diffs
Lag Milestone 0*	0.134	-0.137	-0.0283	-0.0248	-0.0440	-0.0289	-0.00680	-0.00179	0.00848	0.00485
	(0.996)	(1.239)	(0.0166)	(0.0168)	(0.0269)	(0.0265)	(0.00768)	(0.00732)	(0.00651)	(0.00695)
Lag Milestone 1*	-1.124	-1.184	-0.0386	-0.0286	-0.0529	-0.0306	-0.0196	-0.0147	0.00130	-0.00439
	(1.377)	(1.690)	(0.0218)	(0.0215)	(0.0334)	(0.0385)	(0.00902)	(0.00897)	(0.0158)	(0.0147)
Lag Milestone 2*	-0.867	-0.900	-0.0382	-0.0275	-0.107	-0.0803	-0.0149	-0.00415	0.0168	0.0113
	(1.766)	(1.882)	(0.0245)	(0.0254)	(0.0456)	(0.0478)	(0.00708)	(0.00918)	(0.00980)	(0.0113)
Lag Milestone 3*	-2.895	-1.728	-0.0452	-0.0313	-0.161	-0.134	-0.0538	-0.0367	-0.00596	-0.00996
	(2.328)	(2.315)	(0.0302)	(0.0332)	(0.0656)	(0.0741)	(0.0199)	(0.0189)	(0.00731)	(0.00838)
Log Population	1.280	1.487	0.475	0.498	0.940	1.032	0.00437	-0.0214	0.00591	-0.00738
	(0.641)	(1.101)	(0.0484)	(0.0611)	(0.104)	(0.0975)	(0.00547)	(0.00828)	(0.00857)	(0.0113)
Log Employment (2000)	1.756	0.689	0.384	0.347			0.00889	0.0249		
	(0.580)	(0.941)	(0.0437)	(0.0560)			(0.00474)	(0.00768)		
Density (2000)	0.0122	0.218	-0.0340	-0.0243			0.00252	0.000955		
	(0.150)	(0.291)	(0.0103)	(0.0119)			(0.00254)	(0.00317)		
Education (2000)	5.743	12.02	-1.233	-1.213			0.00799	0.0820	-0.0390	-0.0192
	(4.497)	(6.743)	(0.389)	(0.391)			(0.0470)	(0.0483)	(0.0492)	(0.0549)
Median Income (1999)	-3.71e-05	-6.53e-05	4.02e-06	6.32e-06			7.48e-07	2.61e-07	9.66e-08	-7.92e-08
	(2.90e-05)	(3.60e-05)	(2.14e-06)	(2.52e-06)			(2.16e-07)	(2.36e-07)	(1.93e-07)	(2.49e-07)
Environmental Voting	-0.161	-0.0952	0.00714	-0.00963			-0.0457	-0.0575		
	(0.397)	(0.777)	(0.0257)	(0.0320)			(0.00346)	(0.00457)		
Civic	0.718	-0.889	0.109	0.105			0.0109	-0.0105		
	(0.553)	(0.943)	(0.0569)	(0.0659)			(0.00735)	(0.00858)		
Nonprofits**					-0.000530	-0.000142			-0.00999	-0.00758
					(0.00343)	(0.00398)			(0.00299)	(0.00251)
Ped/Bike Mode Share	4.080	4.677								
	(9.928)	(10.94)								
Constant	-37.96	-29.03	-3.264	-3.248	-4.641	-5.610	0.566	0.698	0.000477	0.00682
	(3.541)	(6.450)	(0.273)	(0.358)	(1.126)	(1.061)	(0.0352)	(0.0461)	(0.00411)	(0.00557)
Fixed Effects for City?	No	No	No	No	Yes	Yes	No	No	Differenced	Differenced
Fixed Effects for Year?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3881	2630	3867	2944	3979	3010	1051	731	660	440
Wald test ($\beta_3 = 0$)	0.893	0.772	0.0673	0.173	0.00913	0.0398	0.00339	0.0262	0.209	0.121
Wald test ($\gamma_0 + \gamma_1 + \gamma_2 = 0$)	0.725	0.716	0.0733	0.162	0.00565	0.0443	0.000974	0.00233	0.0149	0.0206
Milestone 3 marginal effect*** (95% conf. interval)	[-7.46, 1.67]	[-6.27, 2.81]	[-0.10, 0.01]	[-0.10, 0.04]	[-0.29, -0.03]	[-0.28, 0.01]	[-0.09, -0.01]	[-0.07, 0.00]	[-0.02, 0.01]	[-0.03, 0.01]

Standard errors in parentheses, clustered by city (models 1 and 2) or county (fixed-effect models 3 and 4).

* Lagged one year for Ped/Bike Expenditure; lagged two years for Gasoline Sales. ** Lagged two years for Gasoline Sales; differenced for Auto Commute Share

*** Marginal effect of completing Milestone 3, with Milestones 0 through 2 set to zero. All other covariates are held at means.

5 Conclusions

To conclude with confidence that city climate plans have an impact on greenhouse gas emissions, one would want to reject the null hypothesis of "no impact" for at least one of the eight dependent variables analyzed in this paper. This rejection of the null should also be robust across different models that make different assumptions about the nature of selection bias, and to slight changes in model specification. As summarized in Table 6, none of the eight dependent variables meet this threshold for either the technocratic pathway (i.e., that any impact follows formal plan adoption) or the process-based pathway (i.e., that an impact can also arise from earlier stages of the planning process). There is some scattered evidence of climate plan effects, particularly for green buildings and waste reduction, but these findings are not robust.

Instead, climate plans may largely be codifying outcomes that would have been achieved in any case. Cities with climate plans perform better on all eight dependent variables, and so it is likely that many measures in climate plans are in fact implemented. But it is difficult to attribute this success to a climate plan itself – plans have little causal role. Rather, it appears that environmental preferences are the driver behind a city's decision to pursue both a climate plan, and specific emission reduction measures pursued by city government, private firms and individual households. In the same way that some research finds that zoning is endogenous to land value and "follows the market," city climate plans appear to follow existing policy and market preferences.

As shown in Table 6, environmental preferences are strongly associated with several emission reduction measures, such as LEED buildings, adoption of green building ordinances, reduced car commuting and, to a lesser extent, more waste reduction measures.[16] This confirms findings reported elsewhere (Kahn and Vaughn 2009; Kahn and Morris 2009). But environmental preferences are also one of the strongest determinants of embarking on a climate planning process in the first place (see also Zahran, Grover et al. 2008), highlighting the importance of considering initial preferences in the evaluation of planning impacts.

[16] One concern is the potential endogeneity of environmental preferences. For example, the construction of LEED-certified buildings, pedestrian and bicycle expenditure and other environmental programs may change the preferences of existing residents, or attract new environmentally minded residents to a city. Using measures of environmental preferences from an earlier date avoid this issue, and do not change the magnitude of the results (although standard errors are generally somewhat larger). In place of Environmental Voting, I use the vote for Proposition 7, the 2000 parks and air quality bond measure. In place of Civic, I use the % of workers in CO_2 intensive industries.

Table 6 Summary of Results

Dependent Variable	Evidence for Impact of Climate Plans?	Evidence for Impact of Environmental Preferences?
LEED Projects	**None.**	**Yes**. Large and statistically significant coefficients for Environmental Voting and Civic.
Green Building Ordinance	**Unclear.** Cross-sectional models indicate an impact through both the technocratic and process-based pathways, but the results are not robust to minor specification changes. Also, ordinances often pre-date plans, suggesting that the statistical association may not be causal.	**Yes.** Large and statistically significant coefficients for Environmental Voting.
Solar Photovoltaics (PV)	**Limited.** Cross-sectional models indicate an impact through the technocratic pathway (and in one model, through the process-based pathway), but this effect disappears when controlling for city-specific unobservables.	**Limited.** Large and statistically significant coefficient for Civic, but only in one model.
Street Lighting	**None.**	**None.**
Waste Diversion	**Limited.** Fixed-effects models indicate an impact through the technocratic pathway, but in one case only at the 10% significance level. Results are not robust to additional controls.	**Limited.** Statistically significant coefficients for Environmental Voting, but only at the 10% level, and for Civic in one model.
Ped/Bike Expenditure	**None.**	**None.**
Gasoline Sales	**Unclear.** Fixed-effects models indicate an impact through both the technocratic and process-based pathways. However, results are sensitive to individual data points; the effect disappears when Berkeley and San Francisco are excluded.	**None.**
Auto Mode Share	**Limited.** Cross-sectional models indicate an impact through both the technocratic and process-based pathways, but this effect disappears when controlling for city-specific unobservables.	**Yes**. Large and statistically significant coefficients for both Environmental Voting and Environmental Nonprofits.

Note: References to statistical significance use the 5% level and a one-tailed test, unless otherwise stated.
No corrections have been made for multiple comparisons.

If environmental preferences lie behind both climate plan adoption and implementation of emission reduction policies, they would help to resolve the puzzlement among some scholars as to why cities would "irrationally" seek to adopt climate plans to mitigate a global commons problem.[17] Cities are simply acting in according with pre-existing preferences, and climate plans might best be interpreted as a signal of these altruistic preferences rather than an independent causal mechanism. The results here do not rule out any effect for climate planning, but they do suggest that environmental preferences are substantively more important for cities' actions. In light of other work that similarly identifies an impact

[17] See, for example, the critical discussion in Trisolini 2010.

of political ideology on urban outcomes (Kahn 2011), it may be instructive for future research to explore the pathways through which political preferences take effect.

What, then, might explain the apparent lack of an independent impact for climate planning? One possibility is a long implementation timeframe. While lags were used in the models in this paper, it seems reasonable to argue that some dependent variables – particularly those related to transportation behavior – might take five to ten years or more to be measurably influenced through a climate plan. This explanation is less convincing for other variables such as LEED buildings and pedestrian and bicycle expenditure, where a climate plan could be implemented very quickly,[18] but there still may be delays in implementing the plan.

A second possibility is model misspecification. Some of the models lack explanatory power, particularly for pedestrian and bicycle expenditure, and more generally there is a large amount of unexplained heterogeneity in the dependent variables. Further work on the determinants of cities' environmental performance may reveal some residual role for climate planning.

A third possibility is that the impact of climate plans is very small, or that the plans are only effective in some cities. The results certainly do not rule out the existence of a small effect.[19] Here, evidence from the case studies (discussed in detail in Millard-Ball, under review) also provides some support. Interviewees often talked of the benefits of climate planning in terms of exposure to new ideas and best practices, providing opportunities for cross-departmental communication, or helping to prioritize actions. Such impacts are certainly real, but seem unlikely to lead to fundamental changes in city actions or be large in magnitude. In progressive cities, staff I interviewed felt that they rarely needed to invoke a climate plan in order to advance a particular policy, as their colleagues and elected officials wanted to implement "green" measures regardless of the contents of a plan.

If policy makers wish to promote emission reductions at the city level, then as at present there is little penalty for not acting on the plan, investing in planning alone may not be the most effective course.

[18] Recall that the dependent variable for LEED does not require a project to be completed, but simply to have gone through the registration process with the US Green Building Council.

[19] An approximate indication of the possible size of a treatment effect can be obtained through examining the marginal effect of completing Milestone 3, which is reported in Tables 3-5. Even at the extreme of the 95% confidence interval, with one exception there is no substantively interesting effect that is robust across models, given the caveats about causality discussed in the text. For example, for Solar PV, model (4) suggests that even at the upper end of the confidence interval, a climate plan leads to an additional 0.19 residential solar PV systems. The exception is street lighting, where the variance of the estimates is very high.

Instead, increasing the costs – whether reputational or fiscal – of failing to achieve specified emission reduction targets might enable climate plans to provide an independent constraint on cities' behavior. And acknowledging the central role of environmental preferences might lead to efforts to target resources to the progressive cities that want to mitigate their impact on the global climate, or to reshape those preferences in favor of climate action.

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Appendix Variable Definitions and Supplementary Results

Control Variables

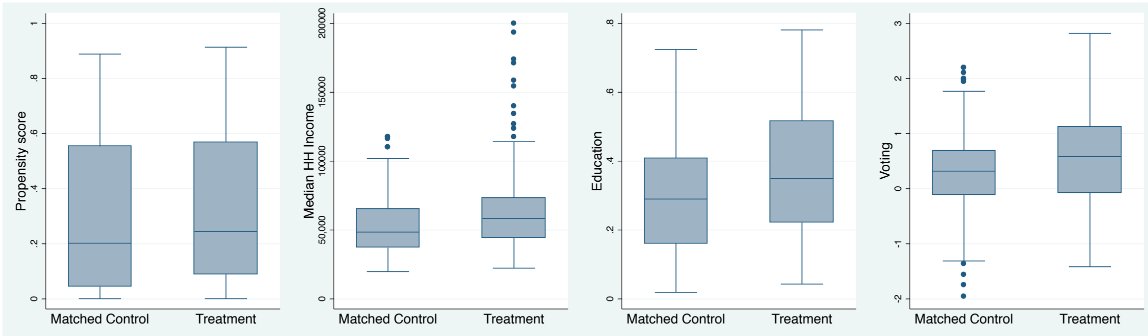
Demographic variables include log population by place of residence (Log Population) and work (Log Employment); residential density; education levels, measured by the percentage of residents with a Bachelor's degree or higher; and median household income. One might expect larger cities with more educated, affluent populations to have greater capacity to implement emission reduction programs.

Political variables attempt to account for differing political preferences across cities, which may influence the salience of climate change and the general willingness to undertake costly measures to reduce local emissions. I include the log of Sierra Club members per capita; the log of the number of environmental nonprofits; and the percentages of residents and workers employed in CO_2-intensive industries (after Zahran et. al., 2008). I also include several direct measures of political preferences expressed through voting in U.S. presidential and Barbara Boxer's 1998 and 2004 Senate races, and on California propositions with environmental significance; here, I broadly follow the approach taken by Kahn and Vaughn (2009). Note that Boxer has a more visible profile on the environment than California's other senator, hence the choice of races to include.

Given the collinearity between these political variables, I use principal components analysis to reduce these variables to a smaller number of dimensions (Table A-2). This is primarily for ease of interpretation, and to simplify the inclusion of interaction terms with treatment variables. I identify two principal factors, Environmental Voting and Civic, with eigenvalues greater than 2. All Environmental Voting variables, including environmental initiatives as well as presidential and U.S. Senate races, load strongly onto voting in the expected direction. The strongest loadings for Civic are residents and workers in CO_2-intensive industries in one direction, and Sierra Club membership and, to a lesser extent, environmental nonprofits in the other direction. I use Environmental Voting and Civic to measure environmental preferences where possible. However, they are time-invariant and so in the fixed effects models I use the one political control variable (Nonprofits) for which I have longitudinal data. While environmental nonprofits may not work specifically on climate issues or even local issues, their number can be seen as an indicator of broader environmental preferences.

Other variables are specific to particular dependent variables. For the LEED models, I include controls for the value of non-residential building permits issued; residential permits are not included as almost all LEED projects are commercial. For the Solar PV and Street Lighting models, I include latitude to account for differing day lengths and level of solar insolation. For Ped/Bike Expenditure, I include the commute mode share for bicycling and walking in 1990, to account for pre-existing travel behavior.

Figure A-1 Balance After Matching on Propensity Score and Selected Covariates



Note: Data only include the initial year of treatment (treatment group) and the same year for the corresponding matched control observations, which are weighted.

Table A-1 Control Variables Used

Name	Years Available	Operationalization	Source
Demographic			
Log Population	Longitudinal, annual 1991 to 2010	Official State estimates for number of residents on January 1 (natural log)	California Department of Finance
Log Employment	1990, 2000	Natural log of number of workers (population tabulation by place of work)	Census Transportation Planning Package
Density	1990, 2000	Housing units per acre	U.S. Census
Education	1990, 2000, 2005-07*	Percentage of individuals 25+ with a bachelors, masters, professional or doctoral degree	U.S. Census and American Community Survey
Median Income	1989, 1999, 2005-07*	Median household income	U.S. Census and American Community Survey
Political			
SIERRA	April 2008	Natural log of number of Sierra Club members per capita, first adding 1 to the total members to avoid missing values in cities with zero members. Aggregated from ZIP code level using ArcGIS and ESRI boundary files**	Sierra Club
Nonprofits	Longitudinal, annual 1989 to 2008	Natural log of the number of environmental nonprofits, first adding 1 to avoid missing values in cities with a zero value. Only includes nonprofits with more than $25,000 in gross receipts.	National Center for Charitable Statistics (NCCS)
CO2RESID	1990, 2000, 2005-07*	Percentage of employed civilians 16+ in CO_2-intensive industries (agriculture, forestry, mining, construction, manufacturing, transportation, warehousing and utilities), by place of residence	U.S. Census and American Community Survey
CO2EMPLOY	2000	Same as above, by place of work	Census Transportation Planning Package

Name	Years Available	Operationalization	Source
PRESIDENT	2000, 2004	Vote for the Democratic candidate for U.S. president, as a percentage of the vote for the Democratic and Republican candidates. Aggregated from precinct or census block level to city using ArcGIS	U.C. Berkeley Statewide Database
BOXER	1998, 2004	Vote for Barbara Boxer for U.S. Senate, as a percentage of the vote for the Democratic and Republican candidates. Aggregated from precinct or census block level to city using ArcGIS	U.C. Berkeley Statewide Database
PROP87	2006	Percentage "Yes" vote for Proposition 87, which would have levied a tax on oil producers to fund energy efficiency and alternative energy measures. Aggregated from precinct level to city using ArcGIS	U.C. Berkeley Statewide Database
PROP84	2006	Percentage "Yes" vote for Proposition 84, a bond measure for water quality, flood control, natural resource protection and park improvements. Aggregated from precinct level to city using ArcGIS	U.C. Berkeley Statewide Database
PROP12	2000	Percentage "Yes" vote for Proposition 12, a bond measure for parks, water, air quality and coastal protection. Aggregated from block level to city using ArcGIS	U.C. Berkeley Statewide Database
PROP7	1998	Percentage "Yes" vote for Proposition 7, which would have provided tax credits for air quality improvement. Aggregated from block level to city using ArcGIS	U.C. Berkeley Statewide Database
Other			
Non-Residential Building	Longitudinal, annual 1990 to 2008	Total nonresidential building permit valuations ($ million)	Construction Industry Research Board/RAND California
Latitude	N/A	Latitude of city centroid, measured in degrees	Calculated in GeoDa
Ped/Bike Mode Share	1990	Commute mode share by bicycling and walking	U.S. Census
Time	N/A	Rescaled year, where Time = 1 in 1992	N/A

* Note that American Community Survey three-year averages (2005-07) are only produced for larger cities (20,000+ population). Therefore, in much of the following analysis, the 2000 figures are used.

** Note that many ZIP codes span multiple cities. I allocated the members to each city based on their share of land area within each ZIP code, computed in ArcGIS. For ZIP codes that refer to PO boxes and do not have a geographic representation, I used the USPS online lookup facility.

Table A-2 Principal Components Analysis

Variable	Environmental Voting	Civic	Factor3	Factor4	Factor5	Uniqueness
Nonprofits (2006)	0.167	0.442	0.039	0.137	0.112	0.744
SIERRA	0.107	0.800	0.141	0.259	-0.035	0.261
CO2RESID (2000)	0.012	-0.865	0.166	0.090	-0.000	0.216
CO2EMPLOY	0.094	-0.637	0.300	0.196	-0.010	0.457
PROP7	0.726	0.249	0.320	-0.183	0.027	0.275
PROP12	0.809	0.071	0.134	-0.110	0.068	0.306
PROP84	0.934	-0.042	0.048	-0.197	-0.057	0.081
PROP87	0.939	0.283	0.065	0.059	-0.035	0.029
PRESIDENT (2000)	0.951	-0.219	-0.138	0.047	0.053	0.024
PRESIDENT (2004)	0.980	0.028	-0.064	0.115	-0.064	0.018
BOXER (1998)	0.909	-0.276	-0.185	0.080	0.121	0.042
BOXER (2004)	0.915	-0.133	-0.149	0.056	-0.110	0.108
Eigenvalue	*6.512*	*2.280*	*0.345*	*0.245*	*0.057*	

Note: only Environmental Voting and Civic factors are retained.
In order to aid interpretation, I reverse the sign of Civic so that a higher value is associated with higher Sierra Club membership and lower employment in CO2-intensive industries.

Table A-3 Summary Statistics

	Units (N)	Time Periods (T)	Mean	Standard Deviation	Minimum	Maximum
Cross sectional variables						
LEED Projects	478	1	5.312	23.33	0	345
Density (2000)	478	1	2.253	1.864	0.01000	20.03
EMP (2000)	444	1	28,233	93,693	295	1.625e+06
Log Employment (2000)	444	1	9.261	1.306	5.687	14.30
Education (2000)	478	1	0.259	0.187	0	0.781
Median Income (1999)	478	1	52,559	27,208	19,863	200,000
Environmental Voting	439	1	-1.48e-09	0.995	-1.961	2.818
Civic	439	1	-1.45e-10	0.953	-3.679	1.845
SIERRA	478	1	-5.754	1.456	-9.718	-1.779
CO2RESID (2000)	478	1	0.270	0.0951	0.0920	0.716
CO2EMPLOY (2000)	444	1	0.241	0.107	0.0504	0.680
PRESIDENT (2000)	478	1	0.542	0.151	0.184	0.963
PRESIDENT (2004)	475	1	0.525	0.161	0.183	0.931
BOXER (1998)	477	1	0.550	0.141	0.228	0.952
BOXER (2004)	475	1	0.589	0.160	0.0430	0.985
PROP7	477	1	0.424	0.0839	0.167	0.645
PROP12	470	1	0.631	0.124	0.129	1
PROP84	475	1	0.524	0.109	0.204	0.799
PROP87	475	1	0.434	0.119	0.148	0.832
Ped/Bike Mode Share (1990)	472	1	0.050	0.048	0.000	0.572
Latitude	478	1	36.113	2.297	32.569	41.964
Longitudinal variables						
Auto Commute Share	195-478	3	0.730	0.0832	0.173	0.867
Ped/Bike Expenditure	472-476	9	-4.435	5.761	-10.414	7.256
Gasoline Sales	261-268	14	5.594	0.824	3.254	9.683
Green Building Ordinance	all*	6	0.0366	0.188	0	1
Solar PV	478	3	16.209	37.793	0	594
Street Lighting	303-328	10	20.34	144.8	0.00003	2,929
Waste Diversion Programs	381-421	14	31.88	6.188	6	55
Milestone 0	all*	18	0.0317	0.175	0	1
Milestone 1	all*	18	0.0175	0.131	0	1
Milestone 2	all*	18	0.0120	0.109	0	1
Milestone 3	all*	18	0.0109	0.104	0	1
Log Population	all*	20	10.02	1.419	4.382	15.23
Non-Residential Building	441-468	19	26.58	84.70	0	1,848
Nonprofits	all*	20	0.360	0.630	0	4.220

*The number of incorporated cities increased from 458 in 1991 to 478 by 2004. In 2009 it increased further to 480, but I exclude these two most recently incorporated cities

Table A-4 Covariate Balance After Matching

	Matched (Weighted) Data			Raw (Unweighted) Data	
	Mean (Treatment Group)	Mean (Control Group)	P > |t|*	Mean (Treated Cities)	Mean (Untreated Cities)
Propensity Score	0.328	0.282	0.129	0.107	0.013
Log Population	10.6	10.5	0.349	10.4	9.79
Log Employment (2000)	9.72	9.57	0.280	9.72	8.99
Median Income (1999)	64,586	53,275	0.000	65,724	45,159
Education (2000)	0.374	0.300	0.000	0.378	0.192
Environmental Voting	0.540	0.356	0.044	0.540	-0.331
Civic	0.424	0.412	0.890	0.424	-0.260
Nonprofits	0.838	0.746	0.298	0.818	0.350
Spatial lag dep. variable	0.434	0.413	0.536	0.146	0.030
Year**	2006.4	2006.4	1.000	2000.5	2000.5

* T-test of difference in means between treatment and control group

** Exact matching performed on this variable

Note: Means for matched data only include the initial year of treatment (treatment group) and the same year for the corresponding matched observations. Means for unmatched data include all years in the sample. This accounts for the difference between the Mean (Treatment Group) and Mean (Treated Cities) columns.

Table A-5 Interactions with Adoption Years: LEED Projects, Green Building Ordinance, Solar PV

Dependent Variable	LEED Projects		Green Building Ordinance		Solar PV			
Model Type	Negative Binomial		Probit		Negative Binomial			
Model	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects
M0_YRS*	-0.219	-0.278	0.0246	0.0181	0.0249	0.0985	-0.0608	0.0169
	(0.155)	(0.146)	(0.0407)	(0.0442)	(0.0245)	(0.0303)	(0.0385)	(0.0414)
M1_YRS *	0.136	0.222	0.0588	0.0471	-0.0211	-0.0190	-0.114	-0.0977
	(0.122)	(0.133)	(0.0848)	(0.0855)	(0.0496)	(0.0623)	(0.0522)	(0.0549)
M2_YRS *	-0.0394	-0.0342	0.145	0.183	0.165	0.128	-0.00200	-0.0133
	(0.104)	(0.121)	(0.120)	(0.119)	(0.0799)	(0.0846)	(0.0675)	(0.0691)
M3_YRS *	0.122	0.0965	-0.199	-0.226	-0.147	-0.105	0.0759	0.0799
	(0.154)	(0.171)	(0.116)	(0.117)	(0.0784)	(0.0793)	(0.0758)	(0.0740)
Log Population*	0.0337	-0.0158	-0.0880	0.120	0.747	0.542	0.0693	0.141
	(0.0952)	(0.0877)	(0.145)	(0.224)	(0.0441)	(0.0761)	(0.0955)	(0.0895)
Log Employment (2000)	0.836	0.891	0.256	0.0702				
	(0.0843)	(0.135)	(0.128)	(0.178)				
Density (2000)	-0.115	-0.0909	-0.000828	0.0249	-0.131	-0.153		
	(0.0340)	(0.0304)	(0.0402)	(0.0386)	(0.0230)	(0.0369)		
Education (2000)	0.817	1.834	-0.956	-0.824	2.794	4.215		
	(0.806)	(0.933)	(1.008)	(1.103)	(0.641)	(0.924)		
Median Income (1999)	-1.24e-06	-5.00e-06	1.34e-05	1.42e-05	-4.93e-06	-1.03e-05		
	(5.01e-06)	(4.92e-06)	(4.45e-06)	(5.07e-06)	(2.78e-06)	(3.71e-06)		
Non-Residential Building	0.000153	0.000130						
(2000-08)	(9.11e-05)	(9.98e-05)						
Environmental Voting	0.248	0.132	0.401	0.448	-0.274	-0.438		
	(0.0721)	(0.0859)	(0.100)	(0.153)	(0.0573)	(0.0934)		
Civic	0.347	0.296	0.259	0.166	0.469	0.110		
	(0.123)	(0.131)	(0.152)	(0.174)	(0.0936)	(0.183)		
Lag Nonprofits							0.00342	-0.0102
							(0.0115)	(0.0117)
Latitude					0.184	0.116		
					(0.0226)	(0.0334)		
Time			0.462	0.201				
			(0.260)	(0.300)				
Time^2			-0.0262	0.00115				
			(0.0224)	(0.0306)				
Constant	-7.535	-7.802	-6.318	-6.377	-12.62	-7.920	1.651	0.612
	(0.914)	(1.195)	(1.337)	(1.384)	-12.62	-7.920	(1.027)	(0.952)
Fixed Effects for City?	No	No	No	No	No	No	Yes	Yes
Fixed Effects for Year?	No	No	No	No	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes
ln α (dispersion factor)	-0.891	-1.397						
	(0.211)	(0.490)						
Observations	423	284	3006	2019	439	298	1260	828
Wald test (β_3 = 0)	0.213	0.286	0.957	0.973	0.969	0.907	0.158	0.140

Standard errors in parentheses, clustered by city (Green Building Ordinance and Solar PV models 1 and 2) or county (LEED Projects).
Treatment variables M0_YRS – M3_YRS are number of years since milestone was attained.
* For LEED Projects, the treatment variables and Log Population are measured in 2006, as the majority of LEED projects were registered in 2007-09.

Table A-6 Interactions with Adoption Years: Street Lighting, Waste Diversion

Dependent Variable	Street Lighting				Waste Diversion Programs			
Model Type	Linear regression				Linear regression			
Model	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects
M0_YRS	-0.128	-0.0624	-0.451	-0.621	1.108	0.133	-0.125	-0.0752
	(0.197)	(0.305)	(0.393)	(0.572)	(0.239)	(0.107)	(0.108)	(0.111)
M1_YRS	0.760	0.700	0.566	1.055	0.236	0.334	0.525	0.523
	(0.425)	(0.448)	(0.697)	(0.357)	(0.286)	(0.113)	(0.142)	(0.147)
M2_YRS	-0.134	-0.240	0.623	0.175	-0.0852	-0.231	-0.134	-0.105
	(0.455)	(0.482)	(0.795)	(0.529)	(0.381)	(0.176)	(0.288)	(0.266)
M3_YRS	-0.974	-0.832	-1.430	-1.169	-0.443	0.0345	0.0137	-0.0400
	(0.721)	(0.749)	(0.606)	(0.472)	(0.437)	(0.269)	(0.268)	(0.258)
Log Population	-6.548	-7.139	2.367	11.76	4.169	1.730	0.910	4.645
	(2.609)	(3.801)	(6.663)	(7.512)	(0.464)	(0.556)	(1.250)	(2.092)
Log Employment (2000)	5.270	4.224			-2.312	-0.684		
	(1.976)	(3.011)			(0.420)	(0.430)		
Density (2000)	0.538	1.403			-0.667	-0.434		
	(0.371)	(0.742)			(0.158)	(0.173)		
Education (2000)	-9.812	5.741			6.279	8.807		
	(8.604)	(16.97)			(3.268)	(3.886)		
Median Income (1999)	9.49e-05	-1.51e-05			-3.85e-05	-4.77e-05		
	(5.74e-05)	(0.000107)			(1.49e-05)	(1.78e-05)		
Environmental Voting	1.007	1.248			0.282	0.435		
	(0.839)	(1.717)			(0.292)	(0.356)		
Civic	-0.226	-4.658			0.859	-0.173		
	(0.848)	(2.103)			(0.362)	(0.649)		
Lag Nonprofits			-0.0691	-0.235			-0.0221	-0.0289
			(0.0817)	(0.161)			(0.0705)	(0.0738)
Latitude	-0.395	-0.453						
	(0.257)	(0.394)						
Constant	37.47	56.39	-4.978	-114.0	12.29	17.37	17.41	-20.81
	(16.57)	(22.14)	(67.89)	(79.47)	(2.393)	(3.422)	(12.66)	(21.84)
Fixed Effects for City?	No	No	Yes	Yes	No	No	Yes	Yes
Fixed Effects for Year?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2619	1831	3138	2012	5217	3579	5585	3592
Wald test ($\beta_3 = 0$)	0.088	0.134	0.011	0.009	0.844	0.449	0.480	0.561

Standard errors in parentheses, clustered by city (models 1 and 2) or county (fixed-effect models 3 and 4).
Treatment variables M0_YRS – M3_YRS are number of years since milestone was attained.

Table A-7 Interactions with Adoption Years: Ped/Bike Expenditure, Gasoline Sales, Auto Commute Share

Dependent Variable	**Ped/Bike Expenditure**		**Gasoline Sales**				**Auto Commute Share**			
Model Type	Tobit		Linear regression				First differenced			
Model	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects	(1) Controls only	(2) Matching	(3) First differences	(4) Matching + first diffs
M0_YRS*	-0.165	-0.126	-0.000808	0.00221	-0.00869	-0.00516	-0.00268	-0.00189	0.000119	-0.000434
	(0.175)	(0.220)	(0.00536)	(0.00601)	(0.00726)	(0.00749)	(0.00150)	(0.00149)	(0.00107)	(0.00117)
M1_YRS *	0.170	0.142	0.00408	0.00495	0.0193	0.0197	0.00181	0.00126	-0.000213	-0.000225
	(0.340)	(0.343)	(0.0103)	(0.0109)	(0.0148)	(0.0154)	(0.00284)	(0.00257)	(0.00260)	(0.00259)
M2_YRS *	-0.149	-0.144	-0.0219	-0.0229	-0.0383	-0.0367	-0.00547	-0.00657	-0.00828	-0.00790
	(0.799)	(0.762)	(0.0136)	(0.0139)	(0.0271)	(0.0289)	(0.00440)	(0.00430)	(0.00376)	(0.00386)
M3_YRS *	-0.197	-0.0579	0.0127	0.0142	0.00664	0.00367	0.00102	0.00351	0.00695	0.00708
	(0.824)	(0.763)	(0.0130)	(0.0133)	(0.0182)	(0.0199)	(0.00472)	(0.00454)	(0.00507)	(0.00512)
Log Population	1.282	1.484	0.471	0.508	0.922	1.014	0.00417	-0.0215	0.00549	-0.00760
	(0.649)	(1.109)	(0.0469)	(0.0592)	(0.109)	(0.0992)	(0.00545)	(0.00828)	(0.00857)	(0.0113)
Log Employment (2000)	1.763	0.700	0.385	0.336			0.00923	0.0252		
	(0.583)	(0.941)	(0.0424)	(0.0533)			(0.00470)	(0.00763)		
Density (2000)	0.0254	0.231	-0.0334	-0.0261			0.00238	0.000807		
	(0.154)	(0.293)	(0.00988)	(0.0113)			(0.00254)	(0.00319)		
Education (2000)	5.940	12.10	-1.254	-1.151			0.0139	0.0853	-0.0330	-0.0140
	(4.524)	(6.788)	(0.382)	(0.370)			(0.0470)	(0.0481)	(0.0520)	(0.0570)
Median Income (1999)	-3.76e-05	-6.51e-05	4.06e-06	6.23e-06			7.24e-07	2.51e-07	8.05e-08	-8.92e-08
	(2.90e-05)	(3.61e-05)	(2.10e-06)	(2.35e-06)			(2.15e-07)	(2.34e-07)	(1.99e-07)	(2.54e-07)
Environmental Voting	-0.155	-0.106	0.00331	-0.0129			-0.0453	-0.0568		
	(0.396)	(0.769)	(0.0252)	(0.0309)			(0.00346)	(0.00458)		
Civic	0.709	-0.904	0.104	0.0965			0.0106	-0.0106		
	(0.555)	(0.943)	(0.0558)	(0.0638)			(0.00735)	(0.00852)		
Nonprofits**					0.000287	-7.62e-05			-0.00947	-0.00697
					(0.00306)	(0.00388)			(0.00287)	(0.00249)
Ped/Bike Mode Share	4.455	5.063								
	(9.963)	(11.06)								
Constant	-38.10	-29.19	-3.241	-3.275	-4.450	-5.433	0.564	0.694	0.000613	0.00675
	(3.631)	(6.573)	(0.267)	(0.361)	(1.181)	(1.081)	(0.0351)	(0.0462)	(0.00408)	(0.00553)
Fixed Effects for City?	No	No	No	No	Yes	Yes	No	No	Differenced	Differenced
Fixed Effects for Year?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3881	2630	4127	3141	4247	3211	1051	731	660	440
Wald test ($\beta_3 = 0$)	0.594	0.530	0.836	0.857	0.642	0.573	0.586	0.780	0.088	0.087

Standard errors in parentheses, clustered by city (models 1 and 2) or county (fixed-effect models 3 and 4).
* Lagged one year for Ped/Bike Expenditure; lagged two years for Gasoline Sales ** Lagged two years for Gasoline Sales; differenced for Auto Commute Share
Treatment variables M0_YRS – M3_YRS are number of years since milestone was attained.

Table A-8 Interactions with Environmental Voting: LEED Projects, Green Building Ordinance, Solar PV

Dependent Variable	**LEED**		**Green Building Ordinance**		**Solar PV**			
Model Type	Negative Binomial		Probit		Negative Binomial			
Model	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects
M0_VOTING*	-0.00162	0.00744	0.125	0.136	-0.109	0.00545	-0.205	-0.157
	(0.124)	(0.121)	(0.179)	(0.217)	(0.117)	(0.130)	(0.0625)	(0.0666)
M1_VOTING *	0.0864	0.150	0.258	0.222	-0.0389	0.172	-0.416	-0.321
	(0.0836)	(0.0989)	(0.239)	(0.275)	(0.132)	(0.164)	(0.0931)	(0.0984)
M2_VOTING *	0.313	0.354	0.496	0.502	0.140	0.383	-0.442	-0.333
	(0.157)	(0.170)	(0.212)	(0.272)	(0.171)	(0.202)	(0.105)	(0.110)
M3_VOTING *	-0.250	-0.227	0.150	0.174	0.304	0.679	-0.275	-0.120
	(0.118)	(0.112)	(0.221)	(0.285)	(0.153)	(0.195)	(0.152)	(0.161)
Log Population*	0.0457	0.0140	-0.0672	0.133	0.758	0.609	0.0759	0.123
	(0.0970)	(0.0877)	(0.143)	(0.209)	(0.0454)	(0.0772)	(0.0929)	(0.0879)
Log Employment (2000)	0.818	0.848	0.283	0.0794				
	(0.0851)	(0.132)	(0.125)	(0.170)				
Density (2000)	-0.117	-0.0948	-0.0151	0.0170	-0.137	-0.172		
	(0.0335)	(0.0374)	(0.0415)	(0.0374)	(0.0234)	(0.0401)		
Education (2000)	0.985	2.208	-1.459	-1.220	2.754	4.178		
	(0.872)	(0.889)	(0.982)	(1.044)	(0.669)	(0.972)		
Median Income (1999)	-1.49e-06	-5.89e-06	1.45e-05	1.48e-05	-5.12e-06	-1.01e-05		
	(5.22e-06)	(5.25e-06)	(4.27e-06)	(4.61e-06)	(2.90e-06)	(3.90e-06)		
Non-Residential Building (2000-08)	0.000212	0.000209						
	(7.94e-05)	(0.000114)						
Environmental Voting	0.262	0.147	0.370	0.380	-0.223	-0.373		
	(0.0721)	(0.0810)	(0.102)	(0.203)	(0.0616)	(0.102)		
Civic	0.340	0.243	0.351	0.226	0.500	0.167		
	(0.122)	(0.117)	(0.141)	(0.166)	(0.0918)	(0.182)		
Lag Nonprofits							-0.00311	-0.00745
							(0.0114)	(0.0113)
Latitude					0.198	0.136		
					(0.0226)	(0.0349)		
Time			0.461	0.209				
			(0.261)	(0.296)				
$Time^2$			-0.0245	0.00121				
			(0.0228)	(0.0303)				
Constant	-7.501	-7.691	-6.701	-6.473	-13.18	-9.265	1.520	0.864
	(0.902)	(1.162)	(1.268)	(1.339)	(1.088)	(1.572)	(1.004)	(0.941)
Fixed Effects for City?	No	No	No	No	No	No	Yes	Yes
Fixed Effects for Year?	No	No	No	No	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes
ln α (dispersion factor)	-0.913	-1.420						
	(0.213)	(0.523)						
Observations	423	284	3006	2019	1317	894	1191	828
Wald test ($\beta_3 = 0$)	0.983	0.978	0.248	0.271	0.0237	0.000251	0.964	0.772

Standard errors in parentheses, clustered by city (Green Building Ordinance and Solar PV models 1 and 2) or county (LEED Projects).
* For LEED, the treatment variables and Log Population are measured in 2006, as the majority of LEED projects were registered in 2007-09. For Green Building Ordinance and Solar PV, the treatment variables are lagged one year and Log Population is not lagged.
Treatment variables M0_VOTING – M3_VOTING are interactions between binary treatment variables and Environmental Voting.

Table A-9 Interactions with Environmental Voting: Street Lighting, Waste

Dependent Variable	Street Lighting				Waste Diversion Programs			
Model Type	Linear regression				Linear regression			
Model	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects
M0_VOTING	2.035	1.990	1.148	0.503	0.123	0.111	0.233	0.398
	(1.824)	(1.876)	(0.847)	(1.162)	(0.176)	(0.183)	(0.241)	(0.283)
M1_VOTING	1.994	1.879	0.0127	-1.134	0.497	0.484	0.859	1.136
	(1.263)	(1.353)	(0.828)	(1.410)	(0.185)	(0.191)	(0.341)	(0.356)
M2_VOTING	0.884	0.788	0.477	-0.449	0.462	0.436	0.855	1.093
	(1.110)	(1.181)	(0.784)	(1.221)	(0.220)	(0.236)	(0.641)	(0.715)
M3_VOTING	0.720	0.598	0.0367	-0.717	0.426	0.396	0.491	0.772
	(1.283)	(1.472)	(0.691)	(1.167)	(0.333)	(0.360)	(0.358)	(0.524)
Log Population	-6.665	-7.008	6.676	11.43	1.347	1.807	1.182	4.899
	(2.626)	(3.783)	(5.254)	(7.220)	(0.381)	(0.560)	(1.319)	(2.176)
Log Employment (2000)	5.257	3.768			0.0885	-0.690		
	(1.994)	(3.048)			(0.346)	(0.431)		
Density (2000)	0.512	1.307			-0.409	-0.447		
	(0.387)	(0.726)			(0.147)	(0.177)		
Education (2000)	-10.87	7.624			2.296	8.820		
	(8.867)	(17.24)			(2.848)	(3.895)		
Median Income (1999)	0.000102	-3.95e-05			-1.90e-05	-4.74e-05		
	(5.90e-05)	(0.000108)			(1.26e-05)	(1.79e-05)		
Environmental Voting	0.863	0.841			0.382	0.490		
	(0.859)	(1.643)			(0.265)	(0.358)		
Civic	-0.258	-5.051			0.729	-0.124		
	(0.855)	(2.121)			(0.346)	(0.657)		
Lag Nonprofits			-0.0747	-0.296			0.00205	0.00757
			(0.0806)	(0.171)			(0.0758)	(0.0785)
Latitude	-0.441	-0.416						
	(0.272)	(0.405)						
Constant	40.37	59.57	-60.62	-110.5	13.48	16.61	14.67	-23.53
	(17.06)	(22.15)	(54.60)	(76.34)	(2.074)	(3.445)	(13.59)	(22.73)
Fixed Effects for City?	No	No	Yes	Yes	No	No	Yes	Yes
Fixed Effects for Year?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2616	1829	2936	2012	5217	3579	5267	3592
Wald test ($\beta_3 = 0$)	0.713	0.658	0.521	0.271	0.101	0.136	0.088	0.074

Standard errors in parentheses, clustered by city (models 1 and 2) or county (fixed-effect models 3 and 4).
Treatment variables M0_VOTING – M3_VOTING are interactions between binary treatment variables and Environmental Voting.

Table A-10 Interactions with Environmental Voting: Ped/Bike Expenditure, Gasoline Sales, Auto Commute Share

Dependent Variable	**Ped/Bike Expenditure**		**Gasoline Sales**				**Auto Commute Share**			
Model Type	Tobit		Linear regression				First differenced			
Model	(1) Controls only	(2) Matching	(1) Controls only	(2) Matching	(3) Fixed effects	(4) Matching + fixed effects	(1) Controls only	(2) Matching	(3) First differences	(4) Matching + first diffs
M0_VOTING*	0.315	-0.222	-0.0293	-0.0274	-0.0316	-0.0231	-0.00732	-0.00437	0.00450	0.00155
	(0.893)	(1.099)	(0.0111)	(0.0114)	(0.0228)	(0.0220)	(0.00594)	(0.00479)	(0.00353)	(0.00357)
M1_VOTING *	-0.478	-0.553	-0.0178	-0.0113	-0.00688	0.00409	-0.0161	-0.0124	-0.00634	-0.00922
	(0.791)	(1.044)	(0.0166)	(0.0163)	(0.0251)	(0.0266)	(0.00207)	(0.00286)	(0.00548)	(0.00535)
M2_VOTING *	0.517	0.339	-0.0364	-0.0312	-0.116	-0.0966	-0.0327	-0.0111	0.0262	0.0191
	(1.119)	(1.267)	(0.0162)	(0.0164)	(0.0543)	(0.0549)	(0.0110)	(0.0166)	(0.0151)	(0.0183)
M3_VOTING *	-1.251	-1.004	-0.0499	-0.0445	-0.0943	-0.0813	-0.0340	-0.0257	-0.00318	-0.00629
	(1.358)	(1.267)	(0.0142)	(0.0163)	(0.0431)	(0.0446)	(0.00762)	(0.00601)	(0.00254)	(0.00238)
Log Population	1.249	1.474	0.474	0.497	0.937	1.020	0.00389	-0.0220	0.0105	-0.00777
	(0.642)	(1.091)	(0.0485)	(0.0611)	(0.105)	(0.0982)	(0.00550)	(0.00831)	(0.00691)	(0.0115)
Log Employment (2000)	1.764	0.690	0.384	0.348			0.00924	0.0254		
	(0.580)	(0.939)	(0.0438)	(0.0559)			(0.00474)	(0.00768)		
Density (2000)	0.0183	0.237	-0.0333	-0.0237			0.00272	0.00111		
	(0.155)	(0.295)	(0.0103)	(0.0120)			(0.00248)	(0.00314)		
Education (2000)	5.638	12.16	-1.221	-1.198			0.0102	0.0846	-0.0336	-0.0195
	(4.522)	(6.757)	(0.388)	(0.389)			(0.0473)	(0.0485)	(0.0429)	(0.0548)
Median Income (1999)	-3.54e-05	-6.52e-05	3.92e-06	6.23e-06			7.48e-07	2.59e-07	3.41e-08	-8.05e-08
	(2.92e-05)	(3.60e-05)	(2.13e-06)	(2.51e-06)			(2.17e-07)	(2.36e-07)	(2.39e-07)	(2.49e-07)
Environmental Voting	-0.212	-0.120	0.00747	-0.00865			-0.0456	-0.0572		
	(0.399)	(0.791)	(0.0256)	(0.0319)			(0.00344)	(0.00457)		
Civic	0.673	-0.923	0.109	0.105			0.0107	-0.0108		
	(0.553)	(0.942)	(0.0569)	(0.0659)			(0.00737)	(0.00858)		
Nonprofits**					-0.00123	-0.000690			-0.00860	-0.00735
					(0.00298)	(0.00352)			(0.00275)	(0.00244)
Ped/Bike Mode Share	4.092	4.990								
	(10.11)	(11.13)								
Constant	-37.80	-29.00	-3.264	-3.252	-4.619	-5.485	0.566	0.697	-0.000703	0.00695
	(3.557)	(6.345)	(0.273)	(0.358)	(1.135)	(1.067)	(0.0353)	(0.0461)	(0.00452)	(0.00565)
Fixed Effects for City?	No	No	No	No	Yes	Yes	No	No	Differenced	Differenced
Fixed Effects for Year?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preprocessing Matching?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3881	2630	3867	2944	3949	3010	1051	731	620	440
Wald test ($\beta_3 = 0$)	0.822	0.786	0.00023	0.00318	0.0170	0.0382	4.13e-06	9.20e-06	0.108	0.00579

Standard errors in parentheses, clustered by city (models 1 and 2) or county (fixed-effect models 3 and 4).
* Lagged one year for Ped/Bike Expenditure; lagged two years for Gasoline Sales ** Lagged two years for Gasoline Sales; differenced for Auto Commute Share
Treatment variables M0_VOTING – M3_VOTING are interactions between binary treatment variables and Environmental Voting.