

Web Appendix to “What Do Emissions Markets Deliver and to Whom? Evidence from Southern California’s NO_x Trading Program”

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Appendix A: Ex post evaluation of the RECLAIM program

Evaluations of the RECLAIM program have been carried out by SCAQMD staff (SCAQMD, various years), the United States Environmental Protection Agency (US EPA 2002; US EPA 2006), and academic researchers (Gangadharan, 2000; Schubert and Zerlauth, 1999).

Although these studies and reports arrive at different conclusions, there is consensus that a RECLAIM program evaluation is an important exercise:

How have actual emissions reductions [in RECLAIM] compared to those that would have occurred under the subsumed CAC system? While there can be no definitive answer, this question is so central to the affected public in any area contemplating converting from CAC to a trading based program that we are obligated to try to answer it. (US EPA, 2002)

In the periodic program evaluations carried out by SCAQMD, the aggregate RTC permit allocation serves as a proxy for counterfactual emissions. The authors maintain that this is

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a reasonable, and potentially conservative estimate of counterfactual emissions because the aggregate permit allocation was designed to track *ex ante* expected endpoint mass emissions under the subsumed suite of CAC rules that were being fiercely opposed by industry. These periodic evaluations routinely conclude that RECLAIM is achieving emissions reductions equivalent to, and possibly greater than, what would have been achieved under the subsumed CAC measures.

A comprehensive EPA study (US EPA, 2002) argues that assumptions made during initial projections for the RECLAIM program were “not valid predictors of real world behavior,” nor were they substantiated with actual data (US EPA, 2002). Consequently, initial RTC allocations are dismissed as invalid measures of counterfactual emissions. The authors allege that RECLAIM has “produced far less emissions reductions than could have been expected from the subsumed CAC system” (US EPA, 2002).¹

Unresolved disagreements about what constitutes an appropriate measure of counterfactual emissions have resulted in a plurality of opinions regarding RECLAIM’s overall performance. Whereas the Deputy Executive Officer for the California Air Resources Board has stated publicly that RECLAIM “hasn’t done as well as the regulations it replaced” (US EPA, 2006), a Pew Center report concludes that “the [RECLAIM] program’s ten-year phase-in design and trading provided the flexibility that led to the achievement of environmental goals that had been previously elusive.” (Ellerman *et al.*, 2003).

¹SCAQMD was quick to respond to allegations that their counterfactual emissions significantly exceeded that which could realistically have been expected under the subsumed CAC rules. This dispute was never resolved. A more recent, retrospective overview of the RECLAIM program published by the US EPA concludes: “RECLAIM shows the critical nature of baseline credibility in a program’s perceived success or failure” (US EPA, 2006).

Appendix B : Additional robustness tests

This appendix provides additional evidence on the robustness of the main results to alternative specifications and matching estimators.

Number of neighbors

We use a leave-one-out validation approach to choose among the nearest neighbor estimators (Black and Smith, 2004). Our objective is to estimate the counterfactual emissions for RECLAIM facilities, $Y_{it'}(0)$. Although we do not observe this for any RECLAIM facility, we do observe this at the control facilities. Leave-one-out validation uses these control observations to determine which of the competing models best fit the data. The basic approach is as follows. We drop observation j in the control group and use the remaining control observations to estimate $\hat{Y}_{jt'}(0)$. The associated forecast error is given by $e_j = \hat{Y}_{jt'}(0) - Y_{jt'}(0)$. This process is repeated for all facilities in the control group. We select the estimator that minimizes the mean squared error of the forecasts.

Table A2 reports the robustness of the main nearest neighbors matching results to the number of neighbors. For the overall effect (period 1 to 4), the results are significant and qualitatively similar for 1, 2, or 3 neighbors. With more neighbors, the estimates are only weakly significant. For the trading effects (period 2 to 3), the results are quite similar for 2, 3, 4, or 5 neighbors.

Bias adjustments

We also examine the robustness of our nearest neighbor results to the bias adjustments we make. When matches between treatment facilities and the closest controls are inexact, our

semi-parametric matching estimator adjusts the difference in the predicted counterfactual outcome. This adjustment is based on an estimated regression of the emissions differences in the control group on historic emissions and industry fixed effects. This regression function, estimated using least squares, is best approximated with a linear function when the data are in levels. A more flexible specification is warranted when we use log-transformed data. We use a quadratic bias adjustment.

Table A3 reports results using no correction, a linear correction, and a quadratic correction. Patterns of coefficient significance are not significantly affected by these bias adjustments. Results are also robust to using additional covariates in the bias adjustment function (not shown).

Matching covariates

The baseline specification, which we emphasize in the paper, matches on pre-period attainment status, 4 digit standardized industry classification (sic), and historic NO_x emissions. Our approach assumes that, conditional on these variables, NO_x emissions at facilities operating under a CAC regime would follow parallel paths over the study period. This is a strong assumption, and we would ideally control for additional factors that could affect emissions (such as production technologies, firm size, or characteristics of the markets served by these facilities). Unfortunately, for many of the facilities in our analysis, there is a paucity of data available.

In an effort to augment our matching with additional facility level information, we gained access to an extract of the National Establishment Time-Series (NETS) Database that in-

cludes all business establishments in California over the sample period 1992–2004. These data are derived from Dun & Bradstreet data, and include detailed information about the location of establishments, establishment-level standard industrial classification, ownership structure, employment, and credit rating information. Merging these data with our database was not straightforward due to differences in how facilities report their facility names, locations, and primary industry classifications. We used a triple matching algorithm that searches for common three letter combinations in the names and locations of facilities appearing in our database and the NETS data, respectively. This allowed us to successfully merge approximately 40 percent of the facilities in our data with the NETS data.

Table A4 reports results from matching exercises not summarized in the paper. Note that the larger the number of variables we use, the less accurately we match on those variables for which we do not require exact matching. When we include additional matching covariates, we add an industry-specific emissions quartile indicator to the list of exact match variables.

Alternative matching estimator

Asymptotically, all matching estimators produce the same estimate. However, in finite samples, different matching estimators can yield very different treatment effect estimates, particularly if one or more of the identifying assumptions is violated. Since the seminal work of Rosenbaum and Rubin (1983), there has been considerable interest in methods that avoid adjusting directly for observable covariates and instead adjust for differences in the propensity score (*i.e.*, the conditional probability of treatment). An important result in the literature is that, if unconfoundedness holds, conditioning only on the propensity score

assures independence of D_i and $Y_i(0)$. Recent work has demonstrated that, when there is good overlap in the distribution of propensity scores for treated and control facilities, reweighting estimators outperform nearest neighbor or kernel matching in finite samples (Busso *et al.*, 2009). We implement a propensity score matching estimator. All treated observations receive a weight of one, whereas control observations receive a weight $\frac{\hat{p}}{1-\hat{p}}$ (where \hat{p} is the estimated propensity score).

The propensity score equation describes the process by which the data are filtered or selected to produce the observed sample. We estimate the propensity scores using a reduced form probit model. Explanatory variables include industry affiliation, historic emissions, and squared historic emissions. We enforce a common support. Balance is achieved and there is significant overlap in the propensity scores of the treatment and comparison groups.

Although matching on propensity scores balances treatment and controls across the set of covariates, facilities with very similar propensity scores may have different combinations of observable characteristics. In our case, we find that matching on p-scores does not always imply a close match on observables (even after adding higher order terms to the selection equation). This poor match quality can introduce bias. Consequently, we use a propensity score based refinement of weighted regression: the so-called “double robust” (DR) estimator (Robins *et al.*, 1995; Robins and Ritov, 1997). By combining propensity score matching with regression, we can reduce bias introduced by poor match quality.² Table A5 summarizes the main results. These SATT estimates are larger in absolute value as compared to the NN

²This double robust estimator will not always constitute an improvement upon the more standard parametric regression approach. Reweighting of observations will only add noise if the parametric regression model is correctly specified (Freedman and Berk, 2008).

estimates and somewhat noisier.

Heterogeneous treatment effects

Table A6 reports the results from estimating equation (4) in Fowlie, Holland, and Mansur using the log transformed data. From period 1 to 4, we find that larger historic polluters reduced emissions by a greater percentage. In specifications (3) and (6), we find weak evidence (*i.e.*, the coefficients are significant at the 10 percent level) that neighborhoods with greater percent minority experienced more emissions, all else equal. However, these results are no longer significant when one controls for the heterogeneous treatment effect of historic emissions, as in specifications (5) and (7). Panel B does not find evidence of heterogeneous treatment effects.

We test the robustness of these results as well as the results of Table 7 to using 2000 demographic data. We also examine these models using the restricted sample for the change in emissions from period 1 to 4, and for the full sample from period 2 to 3. In testing the robustness of Table 7, none of the 28 estimates of income and only one of the 28 estimates of percent minority is significant at the 5 percent level (percent minority is significant using the 2000 demographics data when looking at the period 2 to 3 trading for non-electricity facilities. Table A6 results are slightly more sensitive. None of the 28 estimates of income is significant at the 5 percent level. However, of the 28 estimates of percent minority, nine are significant. In particular, using 2000 demographics data, the coefficient on percent minority is significant in the regressions of changes in log emissions from period 1 to 4 for all firms as well as for non-electricity firms. Note that we use 1990 demographics in our main

specification because 2000 demographics are potentially endogenous. Finally, even with 1990 demographics, percent minority is significant at the 5 percent level with an implied elasticity of 0.63 when evaluating the change in log emissions from period 1 to 4 for the non-electricity facilities but not controlling for historic emissions (the equivalent of Column 3 of Table A6, Panel A).

Selection

Since the analysis matches on emissions levels before RECLAIM began, facilities which entering during the time frame of the study cannot be included in the analysis. Also, facilities exiting prior to the post-treatment period are excluded. Non-random entry and exit might introduce selection bias into our results. We first discuss the Heckman test for sample selection bias, then analyze patterns of entry and exit and explore whether our measures of entry and exit might simply arise from misreporting. We then test the robustness of our main result to imputing missing emissions.

To credibly identify a Heckman selection model, we need a variable that significantly determines selection into our sample, but can be credibly excluded from the outcome equation. We went to great lengths to find such a variable, but we were ultimately unsuccessful. Absent a credible exclusion restriction, the standard Heckman selection correction is technically possible to implement, but not very informative.

These identification issues notwithstanding, we do conduct a Heckman test for selection bias. When we include the inverse Mills ratio as an additional explanatory variable in our parametric regressions, it is not statistically significant. For reasons we have articulated, this

result is not very meaningful.

To develop a better sense of the patterns of entry and exit in the data, we define variables to indicate whether a facility entered or exited across two periods. For example, the variable `Exit14` takes a value of one if the facility reported positive emissions in period 1, but no emissions in period 4. For the panel of 535 RECLAIM facilities and 10,447 non-RECLAIM (*i.e.*, control) facilities, 30% of the RECLAIM facilities exited and 40% of the non-RECLAIM facilities exited between periods 1 and 4. When we regress the entry and exit indicators on a RECLAIM dummy, emissions in the observed period (*e.g.*, period 1 emissions when `Exit14` is the dependent variable), an interaction of these two variables and SIC fixed effects, we find that entry and exit are less likely in RECLAIM than in the rest of California. One possible explanation for the differential entry and exit rates might be missing data reports. We construct a missing indicator to show facilities which did not report emissions data without entering or exiting. For example, “`Missing14`” is one if emissions are reported in periods 1 and 4, but are missing in either period 2 or 3. Only about 1-2% of the facilities have missing emissions reports by this measure.

While differential entry and exit rates are not necessarily indicative of selection bias, they do warrant concern insofar as these differences could be indicative of non-random. In order to gain any traction on the selection issue, we need to make additional assumptions about the nature of the selection process. One approach to investigating the potential for selection bias involves imputing emissions for the facilities that drop out of our sample and examining whether the results change when we re-estimate our model using this “completed”

data set. If we assume that the missing emissions observations can be reasonably imputed using the emissions at similar facilities that remain in the data set, equivalent to assuming that selection is random conditional on observables, this approach can shed light upon how our results are affected by non-random exit. For those facilities that drop out of the data, we construct an imputed estimate of the missing emissions observations using data from similar facilities in the same emissions regulation regime. More precisely, we match the attrited facilities with similar facilities in the same industry and with similar period 1 emissions. We perform this matching separately for the treatment and control groups, respectively. Results are reported in Table A7. In all cases, the estimates using the completed sample are somewhat smaller in absolute value, but highly statistically significant.

Appendix C: Further evidence on the environmental justice implications of emissions trading

Concerns about the environmental justice implications of emissions trading have strongly influenced the debate surrounding California's greenhouse gas regulations (Hanemann, 2008; Sze et al., 2009). Lejano and Hirose (2005) show that, in the very early years of RECLAIM, the purchase of RTCs by sources in one low income community in particular (Wilmington California) led to NO_x concentrations that exceeded what would have been observed under autarky. These findings have since been interpreted as evidence that traditionally disadvantaged communities were more harmed under RECLAIM than they would have been under CAC (see, for example, Drury 2009).

Although these findings certainly warrant concern, there are two potential problems with

interpreting this as conclusive evidence that emissions trading disproportionately harmed low income communities in Southern California. First, using the initial RTC allocation as a proxy for the emissions that would have occurred under CAC is problematic. Emissions limits imposed by RECLAIM are allegedly much more stringent than what would have been politically feasible under CAC. Consequently, the initial RTC allocations likely provide a biased estimate of emissions absent RECLAIM. Second, in order to conclude that RECLAIM disproportionately harms traditionally disadvantaged communities, it is important to investigate the source of the permits that flowed into Wilmington, to consider emissions outcomes beyond 1996, and to look at the overall pattern of emissions trading under RECLAIM. The authors are careful to emphasize that a more comprehensive analysis is required in order to conclude that emissions trading in RECLAIM disproportionately harmed poor and minority communities.

In this appendix, we extend our analysis in order to revisit this relationship between permit allocations and emissions. More precisely, we investigate whether the relationship between facility-specific permit allocation trajectories and facility-level emissions trends over the study period vary systematically with the demographic characteristics of the neighborhood in which the facility is located. On average, we would expect trends in allocations and emissions to be strongly positively correlated. If emissions exactly equal allocations, this coefficient is exactly one. When facility-level changes in emissions over the study period are regressed on the corresponding change in facility-specific permit allocations the coefficient is 0.65 and precisely estimated (standard error 0.02). Because permits were initially

overallocated, permit allocations fell more precipitously than emissions on average.

If, as alleged, permits flowed disproportionately into low income communities, we would expect this positive correlation to be decreasing with income. If permits flowed disproportionately into minority communities, we would expect the positive correlation to be increasing with percent minority with income. When we interact the facility-specific allocation changes with our measures of neighborhood demographic variables, the coefficient on the income interaction is positive and statistically significant at the five percent level; the coefficient on the percent minority interaction is negative and statistically significant at the five percent level. These results suggest that, relative to the number of permits allocated, emissions fell relatively more sharply in low income and minority neighborhoods. These findings are not consistent with the claim that emissions permits flowed disproportionately into traditionally disadvantaged neighborhoods. Of course, additional research is warranted in order to definitively resolve this issue.

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Figures and Tables

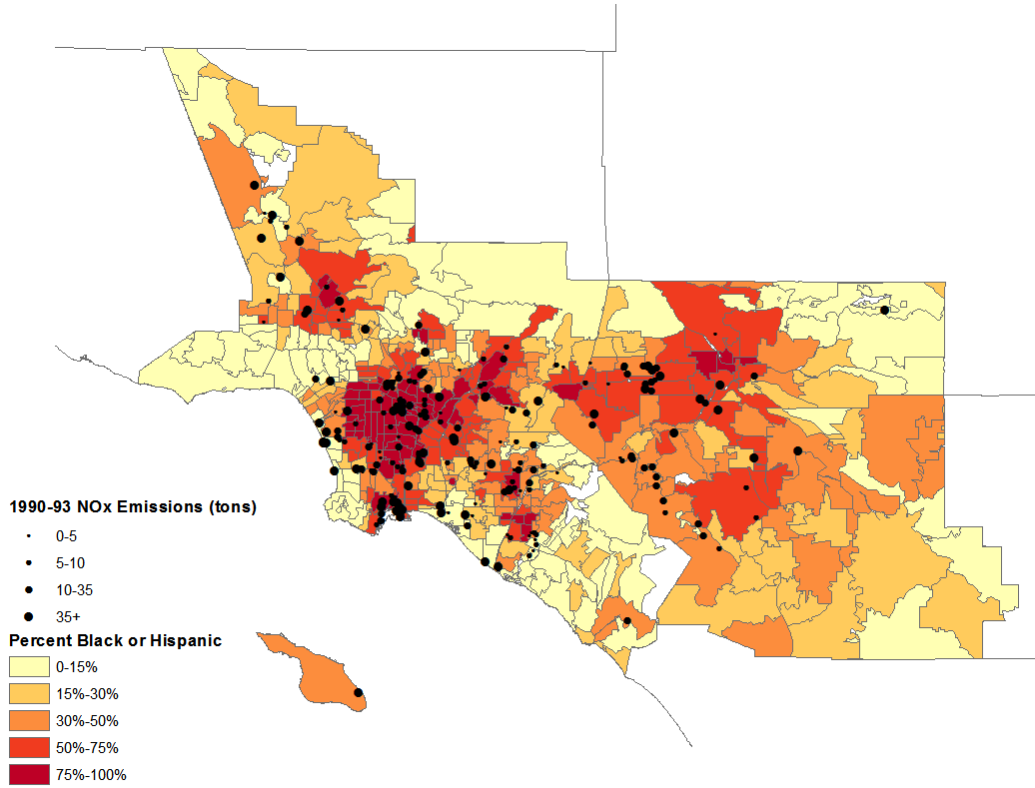


Figure A1: The South Coast Air Quality Management District.

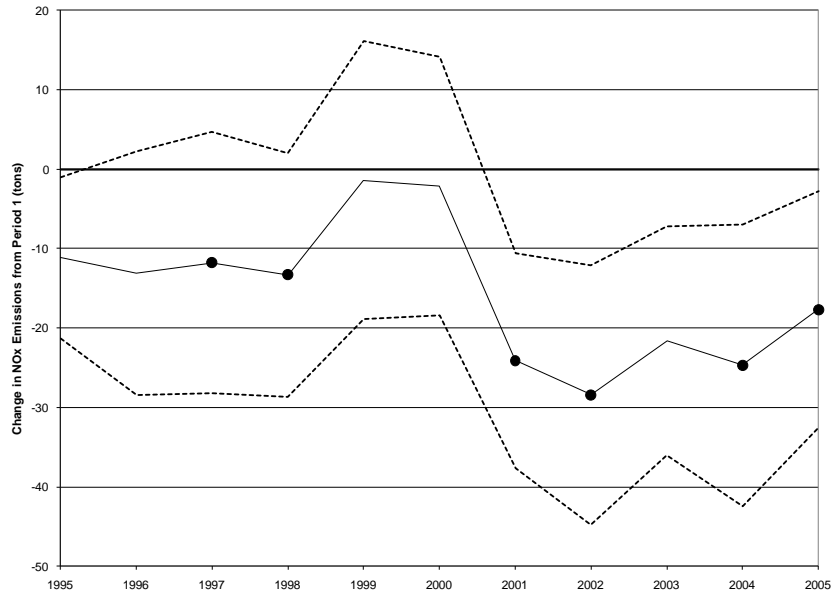


Figure A2: Average Cumulative Treatment Effect by Year (relative to Period 1 emissions), matching $m=3$.

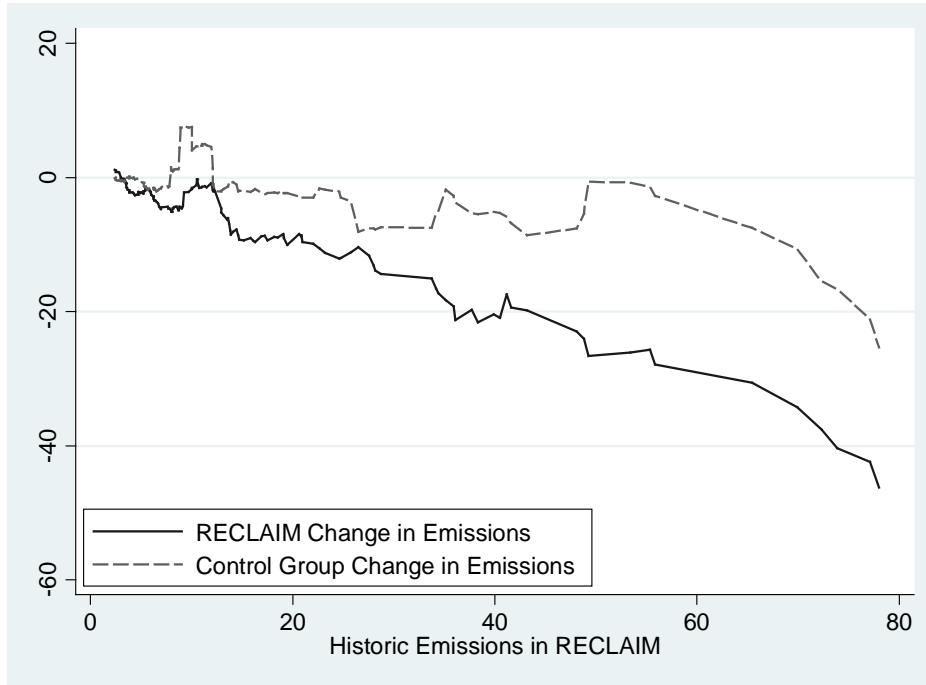


Figure A3: k-Nearest Neighbor Regression of Changes in Emissions from Period 1 to Period 4 in the RECLAIM and Control Groups on Period 1 Emissions. The sample is from the main results shown in Table 4.

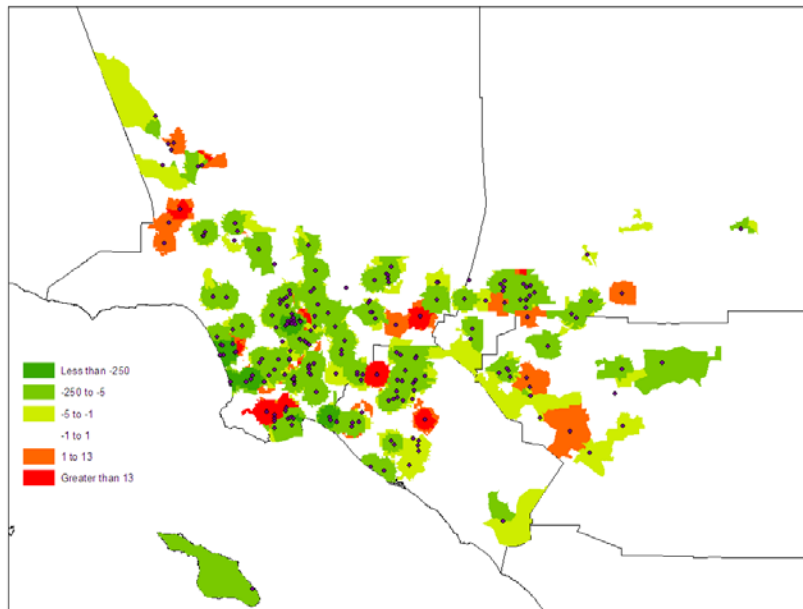


Figure A4: Difference between Actual and Counterfactual Command-and-Control Emissions in Period 4 in tons.

Table A1: Demographic Summary Statistics

Variables for RECLAIM facilities	n	mean	std dev	min	max
median income within 1 mile of a facility (in \$1000s) in 1989	211	35.7	11.7	10.2	80.8
percent black or Hispanic within 1 mile of facility (in 1990)	211	49%	27%	0%	99%
indicator of whether toxics were measured on site	211	28%	45%	0%	100%
% change in total county employment from period 1 to 4	211	2276%	1837%	1112%	6909%
% change in total county payroll from period 1 to 4	211	7617%	3008%	5626%	15300%
% change in total county establishments from period 1 to 4	211	1703%	667%	1287%	3549%
Indicator of coastal permits	211	70%			
Petroleum Refining	211	7%			
Stone, Clay, and Glass Products	211	9%			
Primary Metal Industries	211	9%			
Electric and Gas Services	211	17%			
Variables for control facilities	n	mean	std dev	min	max
median income within 1 mile of a facility (in \$1000s) in 1989	664	34.4	11.5	13.8	85.2
percent black or Hispanic within 1 mile of facility (in 1990)	664	41%	27%	0%	99%
indicator of whether toxics were measured on site	664	20%	40%	0%	100%
% change in total county employment from period 1 to 4	664	2689%	1689%	587%	12254%
% change in total county payroll from period 1 to 4	664	9343%	3249%	5626%	27731%
% change in total county establishments from period 1 to 4	664	1439%	827%	-161%	6038%
Indicator of coastal permits	664	70%			
Petroleum Refining	664	8%			
Stone, Clay, and Glass Products	664	9%			
Primary Metal Industries	664	8%			
Electric and Gas Services	664	16%			

Table A2: Robustness to Number of Neighbors

Panel A: Change in NO_x Emissions between Periods 1 and 4.

Dependent variable	m(1)	m(2)	m(3)	m(4)	m(5)
Levels	-27.19*** (8.74)	-23.98*** (8.07)	-20.59*** (7.63)	-18.52** (7.81)	-17.96** (8.10)
Logs	-0.38*** (0.11)	-0.26** (0.10)	-0.25*** (0.09)	-0.26*** (0.09)	-0.24*** (0.08)

Panel B: Change in NO_x Emissions between Periods 2 and 3.

Dependent variable	m(1)	m(2)	m(3)	m(4)	m(5)
Levels	-4.94 (7.08)	-9.18** (4.13)	-8.29** (3.85)	-10.18** (4.64)	-10.32** (4.06)
Logs	-0.29*** (0.07)	-0.26*** (0.06)	-0.26*** (0.06)	-0.23*** (0.06)	-0.22*** (0.05)

Notes: m(*n*) denotes the *n* neighbors matched. Panels report results for the base specifications. See Table 4 for notes.

Table A3: Robustness to Bias Adjustment

Panel A: Change in NO_x Emissions between Periods 1 and 4.

<u>Dependent variable</u>	<u>No bias adjustment</u>	<u>linear bias adjustment</u>	<u>quadratic bias adjustment</u>
levels	-25.02*** (7.63)	-20.59*** (7.63)	-17.79** (7.63)
logs	-0.32*** (0.09)	-0.27*** (0.09)	-0.25*** (0.09)

Panel B: Change in NO_x Emissions between Periods 2 and 3.

<u>Dependent variable</u>	<u>No bias adjustment</u>	<u>linear bias adjustment</u>	<u>quadratic bias adjustment</u>
levels	-9.60** (3.85)	-8.29** (3.85)	-9.42** (3.85)
logs	-0.26*** (0.06)	-0.26*** (0.06)	-0.26*** (0.06)

Notes: Panels report results for the base specifications. See Table 4 for notes.

Table A4: Robustness to Alternative Matching Specifications**Panel A: Change in NO_x Emissions between Periods 1 and 4.**

Description	Levels	Logs	RECLAIM	
			facilities	Controls
Base specification	-20.59** (7.63)	-0.25*** (0.09)	212	1222
Base specification + % minority	-25.00** (10.55)	-0.16* (0.08)	211	1191
Base specification + % income	-15.43 (11.04)	-0.16** (0.08)	211	1191
Base specification + 90employment	-7.34 (25.66)	0.11 (0.17)	80	332

Panel B: Change in NO_x between Periods 2 and 3 for Non-Electricity Facilities.

Description	Levels	Logs	RECLAIM	
			facilities	Controls
Base specification	-8.29** (3.85)	-0.26*** (0.06)	255	1577
Base specification + % minority	-6.76 (4.71)	-0.15* (0.08)	252	1493
Base specification + % income	-6.91 (4.83)	-0.17*** (0.05)	252	1493
Base specification + 90employment	6.79 (21.16)	0.15 (0.11)	94	431

Table A5: Average Treatment Effect using Propensity Score Matching

Panel A: Change in NO_x Emissions between Periods 1 and 4.

<u>Dependent variable</u>	<u>No bias adjustment</u>
levels	-24.81* (13.86)
logs	-0.27** (0.12)

Panel B: Change in NO_x Emissions between Periods 2 and 3.

<u>Dependent variable</u>	<u>No bias adjustment</u>
levels	-14.78*** (2.22)
logs	-0.28*** (0.02)

Table A6: Environmental Justice Results in Logs**Panel A: Change in log NO_x Emissions between Periods 1 and 4.**

	1	2	3	4	5	6	7
Treatment	-0.25 ** (0.10)	-0.21 * (0.10)	-0.20 ** (0.08)	-0.21 ** (0.09)	-0.20 ** (0.07)	-0.14 * (0.07)	-0.15 ** (0.06)
Treat * Period 1 NO _x	-0.13 ** (0.05)			-0.11 ** (0.05)	-0.11 ** (0.04)		-0.09 ** (0.03)
Treat * Income		-0.21 (0.42)		-0.18 (0.42)		0.26 (0.58)	0.21 (0.55)
Treat * %Minority			0.82 * (0.41)		0.67 (0.40)	0.96 * (0.51)	0.80 (0.45)
Period 1 NO _x	-0.35 ** (0.11)	-0.34 ** (0.11)	-0.35 ** (0.11)	-0.36 ** (0.11)	-0.36 *** (0.10)	-0.37 *** (0.10)	-0.38 *** (0.10)
Income		-0.25 (0.26)		-0.22 (0.25)		-0.48 (0.41)	-0.41 (0.39)
%Minority			-0.20 (0.31)		-0.14 (0.30)	-0.55 (0.46)	-0.45 (0.43)
R ²	0.32	0.34	0.34	0.34	0.34	0.34	0.35

Panel B: Change in log NO_x between Periods 2 and 3 for Non-Electricity Facilities.

	1	2	3	4	5	6	7
Treatment	-0.25 *** (0.04)	-0.23 *** (0.03)	-0.21 *** (0.04)	-0.23 *** (0.03)	-0.21 *** (0.05)	-0.19 *** (0.05)	-0.19 *** (0.05)
Treat * Period 1 NO _x	-0.02 * (0.01)			-0.02 (0.01)	-0.02 (0.02)		-0.01 (0.02)
Treat * Income		-0.03 (0.18)		-0.03 (0.18)		-0.16 (0.15)	-0.17 (0.16)
Treat * %Minority			0.03 (0.25)		0.01 (0.25)	-0.11 (0.18)	-0.13 (0.20)
Period 1 NO _x	-0.06 (0.05)	-0.07 (0.04)	-0.09 * (0.04)	-0.07 (0.04)	-0.09 * (0.04)	-0.09 ** (0.03)	-0.09 ** (0.04)
Income		-0.001 (0.096)		-0.001 (0.097)		-0.13 (0.09)	-0.13 (0.10)
%Minority			-0.20 (0.20)		-0.19 (0.21)	-0.32 (0.24)	-0.31 (0.25)
R ²	0.12	0.14	0.14	0.14	0.14	0.15	0.14

Notes: See notes in Table 7. Here the sample size is 838 and 1005 in Panels A and B, respectively.

Table A7: Average Treatment Effect using Nearest Neighbors Matching and Imputed Emissions Observations

Panel A: Change in NO_x Emissions between Periods 1 and 4.

Description	Levels	Logs	RECLAIM	
			facilities	Controls
Base specification	-20.59** (7.63)	-0.25*** (0.09)	212	1222
Specification w/ imputed emissions	-11.76** (4.76)	-0.13** (0.06)	373	5,324

Panel B: Change in NO_x between Periods 2 and 3.

Description	Levels	Logs	RECLAIM	
			facilities	Controls
Base specification	-8.29** (3.85)	-0.26*** (0.06)	255	1577
Specification w/ imputed emissions	-10.68* (6.41)	-0.21*** (0.06)	359	5,324

Notes: Panels report results for the base specifications. See Table 4 for notes.

Table A8: Deviations from Initial Permit Allocation**Panel A: Change in NO_x Emissions between Periods 1 and 4.**

Variable	(1)	(2)	(3)
Change in permit allocation	0.71*** (0.07)	0.69*** (0.07)	0.03 (0.13)
Income		0.88 (1.72)	0.53 (1.07)
% minority		1.02 (0.93)	0.48 (0.51)
Period 1 NO _x			-0.69*** (0.10)
Constant	2.40 (7.32)	-81.13 (100.83)	-38.52 (61.38)

Panel B: Change in NO_x between Periods 2 and 3.

Variable	(1)	(2)	(3)
Change in permit allocation	0.51*** (0.08)	0.49*** (0.08)	-0.11 (0.19)
Income		-0.46 (0.59)	-0.31 (0.47)
% minority		-0.06 (0.34)	-0.10 (0.26)
Period 1 NO _x			-0.27*** (0.08)
Constant	-3.24 (2.15)	16.09 (36.27)	15.19 (28.96)

Notes: Panels report results for the base specifications. See Table 4 for notes.

Table A9: Period 2 to 3 Results when Including Electric Facilities.

Panel A: Robustness to Control Group (Table 6).

Control Group	RECLAIM			
	Levels	Logs	facilities	Controls
Table 4 Results	-6.18 (5.06)	-0.16*** (0.06)	252	1,493
Exclude L.A. Facilities	-7.15 (5.47)	-0.19*** (0.07)	260	877
Exclude Northern CA	-12.75** (5.20)	-0.26*** (0.07)	268	1090
Severe Non-Attainment Only	-11.94** (5.34)	-0.15** (0.07)	257	541
Single Facility Only	-12.87** (5.56)	-0.20*** (0.06)	266	1027

Panel B: Environmental Justice Results (Table 7).

	1	2	3	4	5	6	7
Treatment	-4.86 (3.10)	-5.79 (3.27)	-4.03 (3.28)	-5.82* (3.15)	-4.19 (3.72)	-4.48 (2.67)	-4.94 (3.21)
Treat * Period 1 NO _x	-0.04 (0.02)			-0.04 (0.03)	-0.04 (0.03)		-0.04 (0.03)
Treat * Income		-0.51* (0.23)		-0.41* (0.19)		-0.60 (0.37)	-0.69* (0.32)
Treat * %Minority			0.18 (0.16)		0.08 (0.13)	0.01 (0.22)	-0.12 (0.18)
Period 1 NO _x	-0.32*** (0.07)	-0.32*** (0.06)	-0.31*** (0.06)	-0.32*** (0.08)	-0.31*** (0.07)	-0.32*** (0.07)	-0.31*** (0.08)
Income		0.39 (0.27)		0.40 (0.25)		0.15 (0.36)	0.25 (0.33)
%Minority			-0.22 (0.12)		-0.18 (0.12)	-0.19 (0.15)	-0.12 (0.15)
R ²	0.40	0.36	0.37	0.37	0.37	0.37	0.37