Left in the Dust? Environmental and Labor Effects of Rural-Urban Water Sales

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Abstract

Growing urban populations and shifting precipitation patterns under a changing climate motivate the flexible use of markets to reallocate water in arid regions. To understand the effects of these markets, we examine the United States' largest ever agriculture-to-urban water transfer, from Imperial County to San Diego County, California. A general equilibrium water trade model is used to illustrate the trade-off between job preservation and environmental protection trade policies. Using a synthetic control and event study approaches, we find initial declines in agricultural output and labor under fallowing, which protected environmental water. Policy changes increasing the intensity of agricultural water use subsequently decreased inflows to the Salton Sea, exposing areas of fine-silted lakebed, creating additional dust. Dust-related air pollutants, PM10 and PM2.5, increase during the relevant period while placebo non-dust pollutants, Ozone and NO_2, do not.

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1 Introduction

The choices governments make about trade policy are often better explained by political considerations than economic theory (Baldwin, 1989). The loss of domestic jobs in certain industries creates a constituency for imposing tariffs and reducing the inflow of goods (Rodrik, 1995). While academic work on these topics has often examined international trade between countries, similar interactions between political economy and trade policy can exist sub-nationally when local jurisdictions regulate trade. In the United States, counties and public water management organizations enact policies to restrict the trade of water to preserve the use of the resource in local agricultural economies (Hanak and Dyckman, 2003; Hanak, 2003, p.viii; Edwards and Libecap, 2015).

In the western United States, water property rights and market mechanisms are used to reallocate water. Due to historic allocations, as much as 80% of western US water is currently used in agriculture, but demand for water for urban and environmental uses is rapidly increasing. A similar reallocation challenge exists in many arid and semi-arid regions throughout the world. Water trades between agricultural and urban areas provide a potential solution, because trade benefits the buyer and seller who are directly involved, with large potential gains (Grafton et al., 2012; Hagerty, 2019).

Despite this, local opposition to the liberalization of water markets has been strong, focusing on the potential loss of jobs in the originating region (Mann and Wüstemann, 2008; Holcombe and Sobel, 2001). Permanent transfers of water may limit future economic development in the area of origin and lead to out-migration, although negative outcomes in exporting regions are more limited when sellers receive substantial benefits and do not sell a large portion of their water (Rosegrant, 1997; Rosegrant and Ringler, 2000).

Negative environmental outcomes have also emerged as a criticism of trade in natural resources (Chichilnisky, 1994; Brander and Taylor, 1998; Copeland and Taylor, 2009). Key case studies in bison and fisheries have pointed to a direct, deleterious effect on renewable resource stocks due to export when foreign markets open (Taylor, 2011; Eisenbarth, 2018). However, there is a relatively small literature examining the connection between trade and resource use (Copeland et al., 2021). Market transfers of water, especially those that move water from one basin to another, appear to generate negative externalities in the originating region, including reductions in water quality, water availability, and in-stream flows (Howe et al., 1990).

In this study, we examine the largest ever agriculture-to-urban water transfer in the United States. The transfer of water used in Imperial County, California for agricultural irrigation generated large gains from trade when sold to San Diego County Water Authority (SDCWA). However, the Imperial Irrigation District (IID), a publicly controlled district which managed the water supply, initially resisted making the transfer, fearing declines in local agricultural activity. Under national and state government pressure, in 2003 IID began a program that paid farmers to voluntarily fallow fields, reducing agricultural production and making water available for transfer.

Initial fallowing programs contained provisions to ensure water would continue to flow to the Salton Sea, a shallow saline lake maintained by water flows from IID. However, IID opposed fallowing because it reduced agricultural output and jobs. Eventually, the fallowing program and its associated ecosystem flow protections were phased out. By 2016, 50% of the water being transferred to San Diego was generated through conservation programs which reduced Salton Sea flows, and lake levels have declined rapidly (Fogel et al., 2020; IID, 2018). The exposed lakebed playa contains fine dust particles, which led to increased dust pollution (Jones and Fleck, 2020).

We build on the trade literature to develop a general equilibrium water trade model that demonstrates the relative effect of free trade in water under *fallow-transfer* and *unrestricted-transfer* policy regimes. The model suggests trade policy can play an important role in the incidence of trade on labor and environmental outcomes. Under fallowing, environmental water flows are conserved but factor prices cannot adjust, resulting in migration out of the local economy. Under conservation, labor markets can adjust more flexibly, but water in the system, and corresponding ecosystem benefits, decline.

We employ a synthetic control approach to compare Imperial County, the area of origin, to other California counties that did not engage in large-scale water exports. We find that under the fallow-trade policy agricultural acreage declined, with corresponding decreases in agricultural labor.[1] The water transfer initially had little impact on the Salton Sea, but as the fallowing retention program was phased out in favor of conservation, dust-related air pollutants, PM10 and PM2.5, increased. This result has not been documented previously. Placebo non-dust air pollutants, Ozone and NO_2, do not see corresponding increases. We subsequently compare the synthetic control approach to standard difference-in-differences and event study methods, using all California counties as a counterfactual, as well as using the subset of counties that receive nonzero weights in the synthetic control analysis.

Our work demonstrates the necessity of jointly examining water trade outcomes and policy in general equilibrium, with implications for the political economy of policy choice. This insight is missing from much of the literature on water trade. Namely, the additional loss of jobs under a fallowing policy, which protects water flows to ecosystems, is viewed negatively by key local constituencies due to its inflexibility and labor market distortions. The elimination of this policy led to corresponding decreases in environmental water flows, imposing additional health costs on the water exporting region.

[1]Between 2003 and 2017, San Diego provided $30 million to IID for economic mitigation of the agricultural labor and related effects. All results in the paper are inclusive of any effects of this transfer.

The paper proceeds with details on the empirical setting in section 2. The general equilibrium model of a regional economy is introduced in section 3. Section 4 presents the empirical approach and section 5 the data used in the analysis. Results are reported in section 6 and section 7 concludes. The appendix contains supplementary materials related to data collection, empirical analysis, and robustness checks.

2 Background

The Colorado River is the largest water source in the southwestern United States. Its waters are divided between seven states, two countries, and many tribal nations (Pulwarty et al., 2005). California's allocation of Colorado River water of 4.4 million acre-feet was the result of a 1922 agreement that divided up 15 million acre-feet of Colorado River water.[2] In the early 2000s, California's ongoing use was around 5.2 million acre-feet.

Imperial County is one of the top agricultural producing counties in the United States, with agricultural production and processing estimated to contribute $4.5 billion and 24,429 jobs to the local economy (Ortiz and Dessert, 2017). The largest single user of Colorado River water is the Imperial Irrigation District (IID), which has rights to divert 2.6 million acre-feet through the All-American Canal, just north of the border with Mexico. The geographic region of study is shown in figure 1.

Facing pressure to cut back on its excess diversions, primarily due to Arizona's development of its share of Colorado River water, the state of California and US federal government pressured IID to transfer water to the San Diego County Water Authority (SDCWA), allowing overall reductions in Colorado River diversions while maintaining water supply to what had become the country's seventh largest city. While initially opposed to transferring water out of the local agricultural economy, IID reached an agreement in 2003, the Quantification Settlement Agreement (QSA), transferring up to 200,000 acre-feet/year of water from Imperial County to San Diego County for 35-70 years; in 2020, the agreement transferred 190,000 acre-feet of water with payments totaling $129 million.

The agreement is commonly described as the largest agriculture-to-urban water transfer in the United States' history. Soon after its ratification in October 2003, IID began a fallowing program that paid farms to halt irrigation on certain fields, reducing agricultural production and making water available for transfer. A consensus has emerged that the result of the transfers was a decline in inflows to the Salton Sea:

> *"The Salton Sea is shrinking primarily because regional water policy—indirectly—is providing it a significantly smaller share of water from the Colorado River [...] To generate the water for transfer and sale, Imperial Irrigation District*

[2] An acre-foot (AF) is 326,000 gallons and is enough to supply 1-2 California households with water each year, meaning 4.4 million acre-feet could supply water for up to 22 million people.

engaged in several activities to reduce the amount of water used for irrigation, including the fallowing of agricultural lands in the Imperial Valley early in the program, to be followed up later by improved irrigation efficiency. Both water-saving approaches conveyed the known side effect of drastically reducing inflows to the Salton Sea." (Fogel et al., 2021, p.22)

Transfers began in 2003 and as noted above, the policy regime changed over time. Initially, IID made water available to transfer by fallowing fields. Under this program, the district ensured that return flows, water that prior to fallowing had left the field and flowed into the Salton Sea, were maintained. This meant that all reductions in water use came from the agricultural sector, reducing crop acreage but not Salton Sea inflows. The total water conserved under the various programs implemented by IID, including transferred water and Salton Sea mitigation water, is shown in figure 2.

In hydro-economic modeling, fallowing programs have been shown to result in more inflow to the Salton Sea than direct lease programs that generate conservation (Levers et al., 2019). However, fallowing removes agricultural land from production, and these programs have been referred to derisively as "buy-and-dry." Opposition to fallowing programs was strong in IID prior to agreement, and the 2003 QSA included a planned phase-out of fallowing starting in 2014. The agreement also ramped up transfers in 2013.

Like the Salton Sea, other terminal lakes have seen agricultural diversions reduce water inflows, leading to dust pollution, including the Aral Sea, Lake Urmia, Owens Lake, and the Great Salt Lake (Wurtsbaugh et al., 2017). Dust pollution has been documented as affecting human health through the increase in particulate concentrations (Griffin and Kellogg, 2004). Prior to an investment in dust mitigation of over $2 billion dollars by the City of Los Angeles, the Owens Dry Lake was the largest source PM10 in the United States (Kittle, 2000). Atmospheric PM2.5 due to dust storms has been shown to decrease birth weight and increase infant mortality (Jones, 2020). In the Salton Sea, decreases in lake elevation induced changes in PM2.5 during the period 1998-2014 and led to increases in respiratory mortality (Jones and Fleck, 2020).

3 Water Trade Model

We develop a general equilibrium representation of a regional economy with three sectors: an agricultural sector (A), a manufacturing sector (M), and a water-based ecosystem service sector (S). We derive a general set of results showing the changes in the originating region as a result of regional water trade. Then, we examine two policy scenarios corresponding to our empirical setting, fallow-trade and unrestricted-trade, to arrive at predictions.

Among the three sectors, the agricultural sector is the domain sector with the largest share of labor. Assume regional water availability, W, follows the equation of motion:

$$\frac{dW}{dt} = \dot{W} = \bar{\sigma} - f(W) - W_A - W_M \tag{1}$$

where $\bar{\sigma}$ is water inflow; $f(W)$ is water outflow, which is a function of the amount of water; and W_A and W_M are the amount of water used in the agricultural and manufacturing sectors, respectively.

Each sector requires unskilled and/or skilled labor, U and L, respectively. We assume that skilled labor can only be hired in the agricultural and manufacturing sectors, while unskilled labor can be hired in both the agricultural and ecosystem service sectors (e.g., services related to the natural system). Labor is fully employed and assumed to be freely mobile across sectors, implying $\bar{L} = L_A + L_M$ and $\bar{U} = U_A + U_S$. The agricultural sector produces output using skilled labor, unskilled labor, and water, while the manufacturing sector uses skilled labor and water. The ecosystem service sector only uses unskilled labor and water that remains in the system. Hence, the amount of water withdrawn from the ecological system is $\bar{W} = W_A + W_M$.[3] Production technologies are represented by the following production functions:

$$Q_A = Q_A(U_A, L_A, W_A) \tag{2}$$

$$Q_M = Q_M(L_M, W_M) \tag{3}$$

$$Q_S = Q_S(U_S, W) = U_S W \tag{4}$$

where all neoclassical assumptions are maintained for production functions in (2) and (3).[4]

We can add a second regional economy that works in a similar way, albeit with a potentially different equation of motion and different ecosystem, agricultural and manufacturing production functions. Since our focus is on the water selling region, we only define the production function of this region in what follows.

Given a perfect competition assumption, the zero-profit condition implies:

$$\alpha_{L_A}\gamma_L + \alpha_{W_A}\gamma_W + \alpha_{U_A}\gamma_U = \overline{P_A} \tag{5}$$

$$\alpha_{U_S}\gamma_U = \overline{P_S} \tag{6}$$

$$\alpha_{L_M}\gamma_L + \alpha_{W_M}\gamma_W = \overline{P_M} \tag{7}$$

[3]This model can be modified to include land and capital as specific factors. However, this modification has no bearing on our analysis and does not add much to the theoretical foundation that this section provides.

[4]The specific production function for ecosystem service sector is commonly used for resource sectors (see Schaefer, 1957; Brander and Taylor, 1998).

$\overline{P_A}$, $\overline{P_S}$, and $\overline{P_M}$ denote the constant price of agricultural, service, and manufacturing output, respectively. Constancy of these prices are due to these regional economies being small relative to the global economy. γ_L and γ_U are the wage rates for skilled and unskilled labor, respectively; γ_W is the water price; and α_{L_i}, α_{U_i}, and α_{W_i} are the respective per-unit amount of skilled labor, unskilled labor, and water in sector i. Moreover, the full employment condition implies:

$$\alpha_{U_A}Q_A + \alpha_{U_S}Q_S = \bar{U} \tag{8}$$

$$\alpha_{L_A}Q_A + \alpha_{L_M}Q_M = \bar{L} \tag{9}$$

$$\alpha_{W_A}Q_A + \alpha_{W_M}Q_M = \bar{W} \tag{10}$$

Differentiating equation (5), where $\hat{x} = \frac{dx}{x}$ denotes the proportional change in variable x, produces:

$$\theta_{L_A}\widehat{\gamma_L} + \theta_{U_A}\widehat{\gamma_U} + \theta_{W_A}\widehat{\gamma_W} = \widehat{\overline{P_A}} - (\theta_{L_A}\widehat{\alpha_{L_A}} + \theta_{U_A}\widehat{\alpha_{U_A}} + \theta_{W_A}\widehat{\alpha_{W_A}}) \tag{11}$$

where θ_{j_i} is the factor j's cost share in sector i, e.g., $\theta_{L_i} = \frac{\gamma_L \alpha_{L_i}}{\overline{P_i}}$, $\theta_{U_i} = \frac{\gamma_U \alpha_{U_i}}{\overline{P_i}}$, $\theta_{W_i} = \frac{\gamma_W \alpha_{W_i}}{\overline{P_i}}$. $\widehat{\overline{P_A}}$ is the (potential) exogenous proportional change in the price of agricultural output. In equilibrium, $\theta_{L_A}\widehat{\alpha_{L_A}} + \theta_{U_A}\widehat{\alpha_{U_A}} + \theta_{W_A}\widehat{\alpha_{W_A}} = 0$. Thus, we obtain:

$$\theta_{L_A}\widehat{\gamma_L} + \theta_{U_A}\widehat{\gamma_U} + \theta_{W_A}\widehat{\gamma_W} = \widehat{\overline{P_A}} \tag{12}$$

Using the definition of α_{U_S}, i.e., $\alpha_{U_S} = \frac{U_S}{Q_S}$, and equations (4) and (6), we get:

$$\gamma_U = W \cdot \overline{P_S} \tag{13}$$

By totally differentiating equation(13), we have:

$$\widehat{\gamma_U} = \widehat{\overline{P_S}} + \widehat{W} \tag{14}$$

A similar derivation yields:

$$\theta_{L_M}\widehat{\gamma_L} + \theta_{W_M}\widehat{\gamma_W} = \widehat{\overline{P_M}} \tag{15}$$

Re-writing equations (13)-(15) in matrix form:

$$\begin{bmatrix} \theta_{U_A} & \theta_{W_A} & \theta_{L_A} \\ 0 & \theta_{W_M} & \theta_{L_A} \\ 1 & 0 & 0 \end{bmatrix} \cdot \begin{bmatrix} \widehat{\gamma_U} \\ \widehat{\gamma_W} \\ \widehat{\gamma_L} \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \\ \widehat{W} \end{bmatrix} \tag{16}$$

where we maintain $\overline{P_A} = \overline{P_S} = \overline{P_M} = 0$, due to our small regional economy assumption.

Solving the system of equations in (16), we obtain:

$$\widehat{\gamma_U} = \widehat{W} \tag{17}$$

$$\widehat{\gamma_W} = -\frac{\theta_{U_A}\theta_{L_M}}{\theta_{W_A}\theta_{L_M} - \theta_{L_A}\theta_{W_M}}\widehat{W} \tag{18}$$

$$\widehat{\gamma_L} = \frac{\theta_{U_A}\theta_{W_M}}{\theta_{W_A}\theta_{L_M} - \theta_{L_A}\theta_{W_M}}\widehat{W} \tag{19}$$

The sign of $\widehat{\gamma_W}$ and $\widehat{\gamma_L}$ is determined by the sign of $\theta_{W_A}\theta_{L_M} - \theta_{L_A}\theta_{W_M}$. Rewriting this equation using the factor shares, we have:

$$\begin{aligned}
\theta_{W_A}\theta_{L_M} - \theta_{L_A}\theta_{W_M} &= \frac{\gamma_W \alpha_{W_A}}{\overline{P_A}} \cdot \frac{\gamma_L \alpha_{L_M}}{\overline{P_M}} - \frac{\gamma_L \alpha_{L_A}}{\overline{P_A}} \cdot \frac{\gamma_W \alpha_{W_M}}{\overline{P_M}} \\
&= \frac{\gamma_L \gamma_W}{\overline{P_A}\,\overline{P_M}} \left(\alpha_{W_A}\alpha_{L_M} - \alpha_{L_A}\alpha_{W_M}\right) \\
&= \frac{\gamma_L \gamma_W}{\overline{P_A}\,\overline{P_M}} \left(\frac{W_A}{Q_A}\frac{L_M}{Q_M} - \frac{L_A}{Q_A}\frac{W_M}{Q_M}\right) \\
&= \frac{\gamma_L \gamma_W L_M L_A}{\overline{P_A}\,\overline{P_M} Q_A Q_M} \left(\frac{W_A}{L_A} - \frac{W_M}{L_M}\right)
\end{aligned}$$

Given that the agricultural sector is more water intensive than the manufacturing sector (i.e., $\frac{W_A}{L_A} > \frac{W_M}{L_M}$), then $\theta_{W_A}\theta_{L_M} - \theta_{L_A}\theta_{W_M} > 0$. We next find changes in the wage rates and water price for the water exporting region under two scenarios.

3.1 Fallow-Transfer

Under the fallow-transfer policy, water transfers must be removed directly from consumptive use in agriculture, so that overall water in the system remains unchanged, i.e., land is fallowed and conserved water is exported, less some amount to maintain the amount of water that remains in the system. This implies that $\widehat{W} = 0$. It follows from equations (17)-(19) that $\widehat{\gamma_U} = \widehat{\gamma_W} = \widehat{\gamma_L} = 0$, i.e., factor prices do not change. Moreover, it follows from equation (10) that under this policy, $dW_A = d\bar{W}$, which amounts to the transfer, implying that $dW_M = 0$. Factor prices and proportions must remain unchanged, as does water intensity (in terms of unskilled labor) in agriculture and manufacturing. Hence, $dL_M = 0$ and $dL_A = dW_A$, and from equation (9) it is apparent that skilled labor must migrate out of the regional economy as water is exported. Similarly, to maintain the water-to-unskilled labor ratio in the agricultural sector, $dU_A = dW_A$, unskilled labor moves to ecosystem services sector and/or moves out of the regional economy. Therefore, under the fallow-transfer policy, labor moves out of the system and the non-ecosystem services part of the economy (mainly agriculture) shrinks.

3.2 Unrestricted-Transfer

Now suppose the water transfer takes place without being tied to a particular sector. We have $\widehat{W} < 0$ under this scenario. Then, it follows from equations (17)-(19) that $\widehat{\gamma_U} < 0$, $\widehat{\gamma_L} < 0$, $\widehat{\gamma_W} > 0$. From these results, we make the following predictions:

(i) Water trading raises the return to water in the water exporting region;

(ii) Water trading reduces the returns to skilled and unskilled labor in the water exporting region.

If the predictions hold and the amount of water that remains in the system decreases, we expect an increase in water values—and therefore the marginal productivity of water—and a decrease in both skilled and unskilled labor in the agricultural sector. Furthermore, if water is transferred out of the system, the size of the resource providing the ecosystem service will decline.

Under the fallow-transfer policy, water is maintained in the ecosystem (by design), while the unrestricted-transfer policy leads to a decrease in system water (and thus ecosystem services). While both policies lead to reductions in employment, the unrestricted policy allows factor prices to change, leading to more water intensive production (i.e., a lower water-to-labor ratio) since wages fall for both types of labor. In this sense, the untied policy is less distortionary because there is no need for the displacement of labor.

4 Empirical Framework

In this section, we describe our empirical strategy to quantify the impact of the QSA on economic and ecological outcomes of Imperial County—the treatment unit. The plausible identification of a treatment effect requires the specification of suitable control units that represent a counterfactual scenario. In our case, all the remaining counties in California—that are not affected by the QSA and that have not experienced water transfers of similar magnitude due to any other policy or agreement—serve as potential controls. Further, granted that the QSA was sighed on October 16, 2003, the post-treatment period encompasses a period starting from 2004.

4.1 A Synthetic Control

Standard comparative case study methods, such as difference-in-differences, assume that all available control units are similar to the treatment unit (in terms of observable and unobservable characteristics) in the pre-intervention period, and thus assign an equal weight to all control units in the analysis. In practice, however, it is unlikely that any given control unit can fully match the treatment unit in all or most of its attributes in the pre-intervention period.

The synthetic control method adopted in this study avoids the above limitations by relying upon a data-driven procedure to obtain a suitable counterfactual unit (Abadie and Gardeazabal, 2003; Abadie et al., 2010; Abadie, 2021). A synthetically composed control unit (i.e., synthetic control) is a weighted average of available control units (i.e., donor units), with weights determined based on how closely the attributes of each control unit approximate those of the treatment unit in the pre-intervention period. This allows the synthetic control unit to match the treatment unit during the pre-treatment period better than any single control unit.

The synthetic control method is a two-step procedure. In the first step, using pre-intervention data on the characteristics of treatment and control units, the optimal sets of weights are estimated. A synthetic control unit is then constructed using these weights as the weighted average of outcomes of control units. A plausible synthetic control tracks the pre-intervention time path of a treatment unit's outcome as closely as possible. The counterfactual outcomes are recovered in the second step by taking the weighted average of outcomes of control units for the post-intervention period. The treatment effect is then measured by taking the difference between the predicted (counterfactual) outcome for the synthetic control and the actual outcome of the treatment unit during the post-intervention period.

Similar to Abadie et al. (2010), let Y_{it} be the outcome of interest for county i, for $i = 1, ..., N+1$, in period t, for $t = 1, ..., T_0+1, ..., T$. Suppose the treatment county corresponds to $i = 1$, while the remaining N counties constitute the donor pool. Also, assume that the policy/intervention takes place in period T_0+1, so that the pre-treatment period covers $1, ..., T_0$ and the post-treatment period encompasses $T_0+1, ..., T$. Let Y_{it}^{noQSA} be the outcome of interest for county i at time t if county i is not exposed to the treatment (i.e., QSA) up to time t, for $i = 1, ..., N+1$ and $t = 1, ..., T_0+1, ..., T$. Let Y_{it}^{QSA} be the outcome for county i at time t if county i is exposed to the treatment in periods $T_0+1, ..., T$. The implicit assumption here is that the QSA has no effect on outcomes before it is implemented, i.e., $Y_{it}^{QSA} = Y_{it}^{noQSA}$ for $i = 1, ..., N+1$ and $t = 1, ..., T_0$. The treatment effect for county i at time t is measured by $\delta_{it} = Y_{it}^{QSA} - Y_{it}^{noQSA}$. Given only the first county ($i = 1$) is exposed to the treatment by assumption, the main estimator for the treatment effect boils down to:

$$\delta_{1t} = Y_{1t}^{QSA} - Y_{1t}^{noQSA} \text{ for } t > T_0 \tag{20}$$

Notice that, while Y_{1t}^{QSA} is observable for the post-intervention period ($t > T_0$), the counterfactual outcome Y_{1t}^{noQSA} is not, which renders the above equation ill-posed. Our methodology allows us to replace unobservable Y_{1t}^{noQSA} with a synthetically composed outcome.

The main requirement placed on a synthetic control unit is that it closely approximates all relevant attributes of the treatment unit in the *pre-treatment period*. The attributes can include both the outcome variable of interest and other covariates. Let $\mathbf{Z}_i$ be an $r \times 1$ vector of observed explanatory variables of the outcome variable of interest, for $i = 1, ..., N+1$.[5] Let $\tilde{Y}_i^{\mathbf{K}} = \sum_{t=1}^{T_0} k_t Y_{it}$ be a linear combination of pre-treatment outcomes for county i, where $\mathbf{K} = (k_1, ..., k_{T_0})'$ is the set of weights. One can consider M sets of $\mathbf{K}$-type weights, i.e., $\mathbf{K}_1, ..., \mathbf{K}_M$, to obtain M linear combinations of the outcome variable.[6]

Consider an $N \times 1$ vector of weights $\mathbf{W} = (w_2, ..., w_{N+1})'$, with $w_i \geq 0$ and $w_2 + ... + w_{N+1} = 1$, that are assigned to N control units to produce a weighted average of control units—a synthetic control. Different control unit weights, $\mathbf{W}$'s, produce potentially different synthetic controls. Given that a plausible synthetic control should closely mimic the treatment unit (in terms of all relevant attributes) during the pre-intervention period, the optimal weights $\mathbf{W}^* = (w_2^*, ..., w_{N+1}^*)'$ must thus satisfy:

$$\sum_{i=1}^{N+1} w_i^* \mathbf{Z}_i = \mathbf{Z}_1$$

$$\sum_{i=1}^{N+1} w_i^* \tilde{Y}_i^{\mathbf{K}_1} = \tilde{Y}_1^{\mathbf{K}_1}$$

$$\vdots$$

$$\sum_{i=1}^{N+1} w_i^* \tilde{Y}_i^{\mathbf{K}_M} = \tilde{Y}_1^{\mathbf{K}_M}$$

Since there often exists no set of weights for which the above conditions hold exactly, Abadie et al. (2010) suggest selecting the optimal weights by minimizing the overall discrepancy in the attributes of the treatment and synthetically composed units, given by:

$$||\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}|| = \sqrt{(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})'\mathbf{V}(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})} \tag{21}$$

where $\mathbf{X}_1 = (\mathbf{Z}_1', \tilde{Y}_i^{\mathbf{K}_1}, ..., \tilde{Y}_i^{\mathbf{K}_M})'$ is a $(r+M) \times 1$ vector of pre-treatment period attributes of the treatment unit; $\mathbf{X}_0$ is a $(r+M) \times N$ matrix, with the jth column of $(\mathbf{Z}_j', \tilde{Y}_j^{\mathbf{K}_1}, ..., \tilde{Y}_j^{\mathbf{K}_M})'$, of pre-treatment period attributes of the control units; and $\mathbf{V}$ is a $(r+M) \times (r+M)$ symmetric and positive semidefinite matrix that weighs the variables in $\mathbf{X}_1$ and $\mathbf{X}_0$ based on their predictive power on the outcome. The optimal set of weights for $\mathbf{W}$ and $\mathbf{V}$ are determined using a numerical search method by minimizing the mean square prediction error (MSPE) given in equation (21).

[5]See appendix B for the list of covariates considered for each outcome variable.

[6]The M linear combinations of the outcome variable allow for controlling for unobservable common confounders that vary over time (Abadie et al., 2010), which improves upon standard difference-in-differences method that can control for unobservable confounders that are time-invariant.

The synthetic control unit is constructed using the optimal weights $\mathbf{W}^* = (w_2^*, ..., w_{N+1}^*)'$. The post-intervention values of the synthetically composed outcome can then replace the (unobservable) counterfactual outcome Y_{1t}^{noQSA} in equation (20), producing the estimator for the treatment effect:

$$\hat{\delta}_{1t} = Y_{1t}^{QSA} - \sum_{i=2}^{N+1} w_i^* Y_{it} \text{ for } t > T_0 \tag{22}$$

To draw inferences on statistical significance of the measured treatment effect, a series of falsification tests need to be conducted (Abadie, 2021). Specifically, the treatment status is systematically assigned to each control unit in the donor pool, which is equivalent to treating control units with a placebo. The test carries out synthetic control analysis to measure the "treatment" (placebo) effect.[7] The estimated treatment effect for the exposed county ($\hat{\delta}_{1t}$, for $t > T_0$) is considered statistically significant if it is unusually large in magnitude relative to the "treatment" effects estimated for the unexposed counties in the post-treatment period. In contrast, if several unexposed counties can reproduce the effect that is comparable to that of the exposed unit, the treatment effect for the exposed unit is then deemed to be not statistically significant.[8]

An alternative technique to evaluate the statistical significance of the measured treatment effect is by the ratio of the post-intervention root mean square prediction error (RMSPE) to the pre-intervention RMSPE. Given that the control units are not exposed to treatment, the post-intervention RMSPE (i.e., the square root of average discrepancy between actual and synthetic outcomes for the post-intervention period) for control units should, in theory, be similar to the pre-intervention RMSPE (i.e., the square root of average discrepancy between actual and synthetic outcomes for the pre-intervention period), thus producing a relatively small ratio. On the other hand, for the treatment unit, the difference between actual and synthetic outcomes will be more pronounced during the post-intervention period if a treatment effect is truly present, thus producing a larger post-intervention RMSPE and, in turn, a larger overall ratio. As such, the estimated treatment effect for the treatment county ($\hat{\delta}_{1t}$, for $t > T_0$) is considered statistically significant if the treatment county has one of the few large post/pre RMSPE ratios. In particular, a treatment unit with a significant treatment effect would appear at or near

[7]In order to ensure that the synthetic control method produces a plausible synthetic control for each control unit in the donor pool, we consider control units for which the method produces, at least, as good a fit as that for a treatment unit in the pre-treatment period. Specifically, in our inferences from falsification tests, we consider control counties with pre-intervention RMSPEs that are less than or equal to twice that of a treatment unit (Abadie et al., 2010). This helps us refine our inferences as placebo units with poor pre-intervention fit could increase inference uncertainty in the post-intervention period.

[8]Nonzero values of the placebo effects can be attributed to broader factors (e.g., economic, regional, environmental, etc.) that affect both the treatment and control units. Therefore, the falsification test allows one to distinguish the true treatment effect from other, more common factors.

the top when all post/pre RMSPE ratios were listed in descending order.[9]

4.2 Difference-in-Differences

For comparison purposes, we also conduct an event study analysis using a difference-in-differences (DID) framework. We first estimate a standard panel DID specification given by:

$$Y_{it} = \alpha \left(\mathbf{1}[Imperial]_i \times \mathbf{1}[Post\text{-}Intervention]_t\right) + \mathbf{Z}_{it}\boldsymbol{\beta} + \mu_i + \lambda_t + \varepsilon_{it} \tag{23}$$

where Y_{it} is the outcome variable of interest for county i at time t; $\mathbf{1}[Imperial]_i$ is an indicator that equals 1 if county i represents Imperial County and zero otherwise; $\mathbf{1}[Post\text{-}Intervention]_t$ is an indicator variable that equals 1 if year t is in the post-intervention period (i.e., 2004-2018) and zero otherwise; $\mathbf{Z}_{it}$ is a vector of explanatory variables, which is similar to that under synthetic control; μ_i and λ_t are fixed county and year effects, respectively; and ε_{it} is the idiosyncratic error term. The average treatment effect (ATE) is captured by the parameter α. In cases where an outcome variable of interest is continuous in nature (e.g., harvested acres, labor employment, etc.), equation (23) is estimated using a standard panel linear model with two-way fixed effects. While, in cases where an outcome variable is count (e.g., PM10 days, PM2.5 days, etc.), we estimate the above equation using a fixed-effects (FE) Poisson regression under the panel generalized linear model framework.[10]

The DID estimation framework relies on several identifying assumptions. First, and foremost, conditional on observable factors the trends of an outcome variable of interest in Imperial County and the control counties would be similar in the absence of the QSA (treatment). This is broadly known as the parallel (common) trends assumption. We test the validity of this assumption rigorously by performing event study analysis (Yip, 2018; Bartik et al., 2019), as discussed in the following section. We complement this analysis by also performing DID estimation using solely control counties that receive a nonzero weight in the synthetic control analysis.[11] The main advantage of a *synthetic-control-*

[9] The disadvantage of RMSPE test is that it does not distinguish between positive and negative deviations in the post-intervention period when ranking post/pre RMSPE ratios of the treatment and placebo units. So, for instance, a treatment unit may present a large negative effect in the post-intervention period (i.e., placebo units do not produce similar negative effect), but such effect may not necessarily be found to be significant according to RMSPE test (i.e., post-pre RMSPE ratio of the treatment unit may not appear at or near the top of the ranking) if there are placebo counties that produce large (cumulative) positive effect in the post-intervention period. Hence, a caution should be exercised when interpreting RMSPE test results.

[10] For robustness, we have also estimated count outcome variable models using (i) a standard panel linear model with two-way fixed effects and (ii) a FE negative binomial regression under the panel generalized linear model framework. The results, which are available upon request, were largely unaltered.

[11] The list of control counties that receive a nonzero weight from the synthetic control analysis for each outcome variable is provided in appendix table D1. The corresponding DID analysis using only control counties selected by the synthetic control analysis is provided in appendix C.

informed DID is the selection of more suitable control counties (i.e., control counties that closely mimic the treatment county in the pre-treatment period) for the DID analysis. Its disadvantage, however, is a (substantial) reduction in the degrees of freedom, which thus entails exercising caution in interpreting the results from this approach.

Second, the water transfer from Imperial County to SDCWA should not affect agricultural and environmental outcomes in other Californian counties. If, for instance, the QSA depressed agricultural employment in Imperial County and, simultaneously, boosted agricultural employment (due to, for instance, migration flows) in control counties, then the measured employment effect could, at best, serve as the upper bound of the true effect. If such agricultural and/or environmental "leakage" effects do exist, they are likely limited to neighboring counties, particularly, to Riverside and San Diego counties (see figure 1). So, conducting empirical analysis without theses two control counties should potentially alleviate this concern. As discussed in section 5, we exclude Riverside County from our analysis due to a water transfer. Given that San Diego County does not receive a nonzero weight in the synthetic control analysis (see appendix table D1), this concern does not affect our synthetic control analysis. For our DID analysis, the synthetic-control-informed DID discussed above serves as a robustness check. The results are qualitatively similar (see appendix C).

Bertrand et al. (2004) raise concerns about serial correlation, specifically how failure to account for it can lead to spurious inferences in the DID context, and suggest computing standard errors that are robust to serial correlation. Serial correlation becomes potentially an important issue with long time series. Accordingly, we report standard errors clustered at the county level that are robust to both heteroskedasticity and serial correlation. For FE Poisson regressions, we report cluster-robust standard errors as recommended by Cameron and Trivedi (2009) to control for potential overdispersion.

4.3 Event Study

The advantage of event study analysis is twofold. First, it provides an internal validity check on the parallel (common) trends assumption of DID estimation. If the trends of an outcome variable of interest are parallel between Imperial County and the control counties in the pre-intervention period, then such parallel trends would likely be maintained in the post-intervention period, had the policy not been implemented. Event study analysis offers an opportunity to visually evaluate whether differential pre-trends pose a challenge to causal inference. Second, event study analysis allows for the study of the evolution (possible lead/lag) of the treatment effect over time.

The event study is constructed by replacing $\mathbf{1}[Imperial]_i \times \mathbf{1}[Post\text{-}Intervention]_t$ in

(23) with a full set of $\mathbf{1}[Imperial]_i \times \mathbf{1}[Year]_t$ interaction terms, for $t = 1, ..., T_0 + 1, ..., T$:

$$Y_{it} = \sum_t \alpha_t \left(\mathbf{1}[Imperial]_i \times \mathbf{1}[Year]_t\right) + \mathbf{Z}_{it}\boldsymbol{\beta} + \mu_i + \lambda_t + \varepsilon_{it} \tag{24}$$

where $\mathbf{1}[Year]_t$ is an indicator that equals 1 in year t and zero otherwise. The parameters of interest are α_t, for $t = 1, ..., T_0 + 1, ..., T$, which quantify the difference in an outcome variable of interest between Imperial County and control counties in year t, relative to the reference year (i.e., 2003). Depending on the nature of an outcome variable, equation (24) is estimated using either a standard panel linear model with two-way fixed effects or a FE Poisson regression under the panel generalized linear model framework. The 95% confidence bounds for the estimates of α_t are obtained using standard errors discussed in section 4.2.

For each outcome variable, we produce two different event studies. In the first event study, we include all the available control counties in the estimation. As noted earlier, this assumes that all control units are similar to the treatment unit in the pre-intervention period, which may not be necessarily true. In the second event study, we limit the control counties to only those that receive nonzero weight in the synthetic control analysis. Although this allows for the selection of more suitable control counties for the analysis, it comes at a cost of reduced sample size.[12]

5 Data

We create a yearly county-level panel on crop production, labor, and ambient air quality. The study variables are described in detail in appendix A. Crop production statistics come from the annual report of USDA's National Agricultural Statistics Service's California Field Office and are available for 1980-2018. We use annual harvested acreage as the outcome variable in the analysis of the effects of the water transfer on agriculture. We also obtain several agriculture-related control variables from this dataset, including cattle value, alfalfa hay value, lettuce value, melon value, and other vegetable values. Lettuce and melons are included as the most valuable specialty crops in Imperial County.

Labor employment and earnings variables are available for 1992-2018 from the Quarterly Workforce Indicators (QWI). The QWI is a product from the United States Census Bureau based on the Longitudinal Employer-Household Dynamics (LEHD) survey that provides quarterly employment statistics data at the county-NAICS (2- and 3-digit) code level (Abowd and Vilhuber, 2011).[13] We create measures of average employment and

[12]The event study analysis using only control counties selected by the synthetic control analysis is provided in appendix C.

[13]This is the best estimate of employment available at this spatial and industry scale, but the extent to which it is able to fully capture the important role of undocumented workers in California's economy, especially for agricultural labor, is not clear (see, for instance, Borjas, 2017).

earnings for skilled (above high-school education) and unskilled (high school education and below) employees. For agricultural labor data, we choose the *Agriculture, Forestry, Fishing and Hunting* sector—NAICS two-digit code 11—to investigate the aggregate labor effect of the QSA. For robustness, we also perform analysis using NAICS subsector 111 (*Crop Production*) labor data, given that this sector is potentially affected by the QSA fallowing program.

Additional predictors to control for local economic development include farm proprietor's income and employment, wages and salaries, and proprietor's income and employment, and are obtained from Bureau of Economic Analysis (BEA). Agricultural labor ratios—the ratio of male-to-female labor in the agricultural sector, the ratio of white-to-Hispanic labor in the agricultural sector, and the ratio of low-to-high skill workers in the agricultural sector—are obtained from the LEHD.

Measures of air quality are sourced from the United States Environmental Protection Agency's Air Quality System (AQS). We collect data on air quality index "bad days" for key pollutants: PM10 for the period between 1980-2018; PM2.5 for 1998-2018; and Ozone and NO_2 for 1994-2018. The data period for each pollutant reflects the data availability.

Our donor (control unit) pool is composed of the remaining counties in California. To avoid potential confounding effects, we exclude from our donor pool four California counties (Yuba, Stanislaus, San Joaquin, and Riverside) as these counties engaged in somewhat sizable water transfers over the course of the study period.[14] Depending on the specification as well as outcome variable examined, there is also a loss of a few control counties due to missing observations, as detailed under table and figure notes. Further, unlike a difference-in-differences method, a synthetic control approach requires data to be balanced for the construction of a counterfactual outcome. This explains a possible difference in control counties used by the two methods.

For the summary statistics of variables, as well as the list of controls, included in the analysis of each outcome variable, see appendix B.

6 Results

We test the effect of the water transfer on Imperial County using a synthetic control and DID-style event study. To infer statistical significance using the synthetic control approach, we prefer the use of gap plots, which show the pre- and post-divergence of a synthetic control relative to the observed values of the treatment unit. The measured treatment effect is considered statistically significant if the gap plot for the treatment county lies below the gap plots for the placebo units (for a negative treatment effect) *or* above the gap plots for the placebo units (for the positive treatment effect) for *at least*

[14] For a comprehensive review of water transfers in California, see Hanak and Stryjewski (2012), particularly the study's technical appendix.

one post-intervention year.[15] We place lower importance on RMSPE tests, as discussed in section 4.1, but include RMSPE tests in the appendix (see table D2).

For the more traditional difference-in-differences analysis, we construct event study charts to examine yearly differences. Statistical significance is determined if the confidence interval for any year lies entirely above or below zero. However, with a single treatment county and many potential control counties, the estimated effect will be sensitive to the choice of counterfactual counties. For all outcome variables, we compare the results of using all available California counties with results using just the counties which received a nonzero weight in the synthetic control analysis.

A summary of the results of all the synthetic control analyses is provided in table 1, which shows the mean, minimum, and maximum gap between synthetic and observed outcomes for the post-intervention years.[16] Employment and crop production measures appear to behave as anticipated, with decreases in acres harvested and employment. PM10 and PM2.5 days, key measures of dust pollution, both increase. For each of the nine measures included in the table, we perform a synthetic control analysis, two difference-in-differences analyses, and two event study analyzes. In this section, we highlight key findings, leaving the results of the remaining analyses to the appendix (see appendix C).

We begin by examining the effect of the water transfer on harvested acres and agricultural employment. Figure 3 shows the results of the synthetic control analyses of harvested acres (left panel), high-skill employment in the agricultural sector (middle panel), and low-skill employment in the agricultural sector (right panel). All three figures reveal a large, negative gap emerging post-QSA for the observed values for Imperial County, relative to the synthetic control. The gap plots show several years where the treatment effect is larger than any placebo, suggesting the results are statistically significant. Moreover, the findings are largely consistent for employment in the crop production sector (see appendix figures C5 and C7). At the end of the treated period (i.e., 2015-2018), the low-skill crop labor gap decreases, and Imperial County is no longer below the placebo gap plots. This outcome is consistent with the transition from a fallow-transfer to an unrestricted-transfer policy at that time.

Although the placebo tests offer some measure of statistical significance, they do not provide traditional statistical tests or confidence intervals for the estimated coefficients. To bound our point estimates, we turn to the difference-in-differences results, provided for harvested acres, high-, and low-skill employment in table 2. These results suggest large point estimates and large confidence intervals. The estimate for Harvested Acres shows an average post-treatment reduction of around 76,000 acres of harvested cropland, which is in the ballpark the number of acres fallowed in several years. A 95% confidence interval

[15] That is, the measured treatment effect (gap) for the treatment unit does not need to lie above/below the gap plots for the placebo units in the entire post-intervention period for statistical significance.

[16] For additional details of our synthetic control analyses (e.g., county weights and RMSPE tests), see appendix D.

around the point estimate further improves the coverage of the estimated treatment effect, suggesting that the synthetic counterfactual and difference-in-differences approaches pick up the treatment effect reasonably well. The DID analysis for Harvested Acres using only control counties that receive nonzero weight from the synthetic control analysis produces qualitatively similar results (see appendix table C1).[17]

The remaining columns of table 2 provide the result of the difference-in-differences regression of the effect of the transfer program on labor. The regressions including all comparison counties are statistically significant and suggest job losses on the order of 1,367 and 2,323 in the high-skill and low-skill, respectively, categories of the agricultural sector; and 320 and 438 in the high-skill and low-skill, respectively, categories in the crop production sector. However, the magnitude of the point estimates declines when only those counties selected by the synthetic control approach are utilized, and the results are no longer statistically significant (see appendix tables C3-C6). This is partly attributable to a significant reduction in the sample size. Therefore, we conclude that we have some, but not overwhelming, evidence of job losses because of the water transfers, which can be partially explained by the change in policy designed to reduce labor losses.

The event study plots in figure 4 add further clarity to our base DID results by presenting the dynamics of the treatment effects (see appendix figures C6 and C8 for the crop production sector). First, given that the estimates largely hover around the zero line in the pre-treatment period (particularly for Harvested Acres, Ag High-Skill Employment, Crop High-Skill Employment, and Crop Low-Skill Employment), this suggests that these variables share similar trends between Imperial County and the control counties in the pre-treatment period, which provides evidence for the parallel (common) trends assumption of the DID framework.[18] Second, the evolution of the treatment effect for each outcome variable in the post-treatment period closely resembles that of our synthetic control output: we observe a distinct downward trend in agricultural production and labor immediately after the transfer.

To understand the change in air pollution in Imperial County, we repeat our empirical analysis on dust-related (PM10 and PM2.5) and non-dust (Ozone and NO_2) air pollutants. As demonstrated in figure 2, the large decreases in flows to the Salton Sea did not occur until the implementation of non-fallow conservation programs, around 2014. Figure 5 shows that dust-related air pollutants, PM2.5 and PM10, increase dramatically in this year, relative to the synthetic Imperial County, and stay high through 2018. The PM10 gap plot shows that Imperial County has a larger divergence in 2014, and every

[17]The crop reductions come primarily from reductions in hay/alfalfa acreage post-2004 (see appendix figure C2), which we would expect as these are the low-value crops. The DID analysis on hay/alfalfa acreage uncovers a statistically significant negative effect of the transfer (see appendix figure C2).

[18]Event study analyses using only control counties selected by the synthetic control analysis produce qualitatively similar (at times, improved) results, though we remain cognizant of a reduced sample size in this body of analysis. See appendix figures C1, C3, C4, C6, and C8.

subsequent year, relative to all other placebo counties. The PM2.5 gap plot indicates that from 2014 onwards Imperial County is among the counties with the largest positive divergence, although it is never the largest.

Turning to the difference-in-differences analysis, summarized in table 3, there appear to be large and statistically significant increases in dust-related air pollution. To test the robustness of these results, we compare the dust-related pollutants to air pollutants attributable to other factors. Because our causal story is that the water transfer exposed additional lakebed playa, resulting in air pollution, pollutants like ozone and NO_2, which are generally caused by the combustion of fossil fuels, should not increase with PM2.5 and PM10. The results in table 3 suggest that, indeed, these placebo pollutants do not appear to increase. In fact, ozone and NO_2 days slightly decline, perhaps owing to reduced agricultural activity in the region.[19]

Figure 6 shows the event study plots for the dust-related air-quality measures (top panel) and the placebo air pollutants (bottom panel). It is apparent that the pre-treatment period estimates are mostly statistically insignificant (more so for PM2.5, Ozone, and NO_2), which means the treatment unit was no different than control units prior to the transfer.[20] Similar to the synthetic control results, the PM10 and PM2.5 figures show a sharp uptick in pollution days starting in 2014, while the placebo pollutants actually decrease near the end of the sample, consistent with our DID estimates. As illustrated in figure 7, the synthetic control analysis for ozone corroborate our findings from DID and event study analysis. But it fails to find a plausible convex combination of the donors for NO_2, as is evident from panel A of the figure, where a synthetic control fails to closely track the treatment unit in the pre-treatment period. Hence, we rely on DID and event study analysis for inferences about NO_2.

7 Conclusion

This paper explores the causal effect of the largest agriculture-to-urban water transfer in US history. A general-equilibrium representation of a regional economy with an ecosystem service sector is constructed to demonstrate how outcomes in the exporting region depend on the type of trade policy implemented. Initially, Imperial County adopted a fallowing policy, which set water flowing into the natural system at approximately the same level as before the transfers. The model demonstrates that this type of policy can lead to job losses in the water exporting region. Later, Imperial County allowed transfers of water through the intensification of consumptive use, which the model suggests will lead

[19] The DID analyses using only control counties selected by the synthetic control analysis produce qualitatively similar results for the study's air pollutants, though the effect of the sample size is manifested in the significance of the estimates. See appendix tables C7-C10.

[20] Analogous results are obtained using only control counties selected by the synthetic control analysis. See appendix figures C9-C12.

to degradation of the ecosystems services sector, but may be a popular policy decision because it allows more flexibility for labor market adjustments.

To test the intuition of the model we use synthetic control, difference-in-differences, and event study analysis. Our results suggest an immediate loss of harvested acres and agricultural-sector employment as the fallowing program is implemented. Around 2014, when fallowing and offset program ends, we see a significant increase in dust-related air pollution in Imperial County.

The political economy of trade plays a key role in policy choice. In Imperial County, the switch out of a fallowing-based transfer program appears to have been made due to political pressure. Preserving the ecosystem services sector via a fallowing program with offset water flows to the Salton Sea provides broad public good benefits in terms of limiting dust-related air pollution. However, when this policy ends, a narrow set of benefits accrues to concentrated economic interests, especially farm-related businesses and agricultural labor. Thus, both the magnitude and distribution of these benefits are important factors to understanding the endogenous choice of water trade policy and its outcomes. When these types of distributional effects are present in the choice of policy, it is important to examine the environmental justice implications. While causal analysis of the health effects of dust-based air pollution is beyond the current scope of work, we did examine the age-adjusted asthma rates in Imperial County relative to others in California (see appendix E). While we were not able to obtain pre-QSA data, the results do suggest high rates of asthma in Imperial County.

While we attribute some reductions in employment and environmental damage to the water transfer, this paper is by no means a full benefit-cost analysis. The primary benefits of the transfer come from payments for water, and from the surplus of water users in high-value urban areas. The gains from trade of these types of transfers are likely to be quite large, given the limited marginal value of water in irrigated agricultural production relative to urban consumption (Grafton et al., 2012). The key point, though, is that environmental and labor-market costs fall on parties who may not receive these gains. Thus, there may be political opposition to these types of transfers and without proper planning, such transfers raise environmental justice concerns.

Political opposition stalled the transfer finally undertaken in the QSA for nearly 20 years before it was finally ratified (Edwards and Libecap, 2015), and the opposition has continued to this day. Growing concerns about high rates of asthma around the Salton Sea, recently raised by journalists, suggest dust pollution from the desiccated lakebed will need to be addressed going forward via investment in mitigation or additional dedicated water flows to the sea. Our work demonstrates the increase in dust pollution is attributable in part to the choice of transfer policy, linked to the political economy of trade.

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Table 1: Estimated Annual Treatment Effects from Synthetic Control Analysis.

	Mean	Min	Max
Harvested Acres (in thousands)	-57.84	-251.53	-10.41
Ag High-Skill Labor Employment	-493.46	-605.79	-250.15
Ag Low-Skill Labor Employment	-1,372.08	-1,806.62	-836.88
Crop High-Skill Labor Employment	-279.26	-381.39	-196.77
Crop Low-Skill Labor Employment	-628.40	-954.27	-347.97
PM10 Days	27.45	-27.90	109.61
PM2.5 Days	12.61	-25.08	101.37
Ozone Days	-17.25	-116.83	58.99
NO_2 Days	24.86	-9.96	57.59

Notes: Mean/min/max annual treatment effect is obtained by taking the average/minimum/maximum of differences between the treatment outcome and its synthetic counterpart (i.e., measured treatment effect) for the post-intervention period (2004-2018). Employment measures are for the ag sector (NAICS=11) and the crop sector (NAICS=111).

Table 2: Difference-in-Differences Estimates for Agricultural Output and Labor.

	Harvested Acres	High-Skill Labor Employment		Low-Skill Labor Employment	
		Ag	Crop	Ag	Crop
Treatment Effect	-75.5588***	-1,367.0360***	-320.0928***	-2,323.6270***	-438.2687***
	(23.8476)	(391.3947)	(119.5283)	(361.6651)	(139.5125)
Observations	1,555	1,050	1,008	1,049	1,029
R^2	0.0324	0.5986	0.4357	0.4487	0.4268
F Statistic	4.8697***	102.3863***	50.7392***	55.8084***	49.9867***

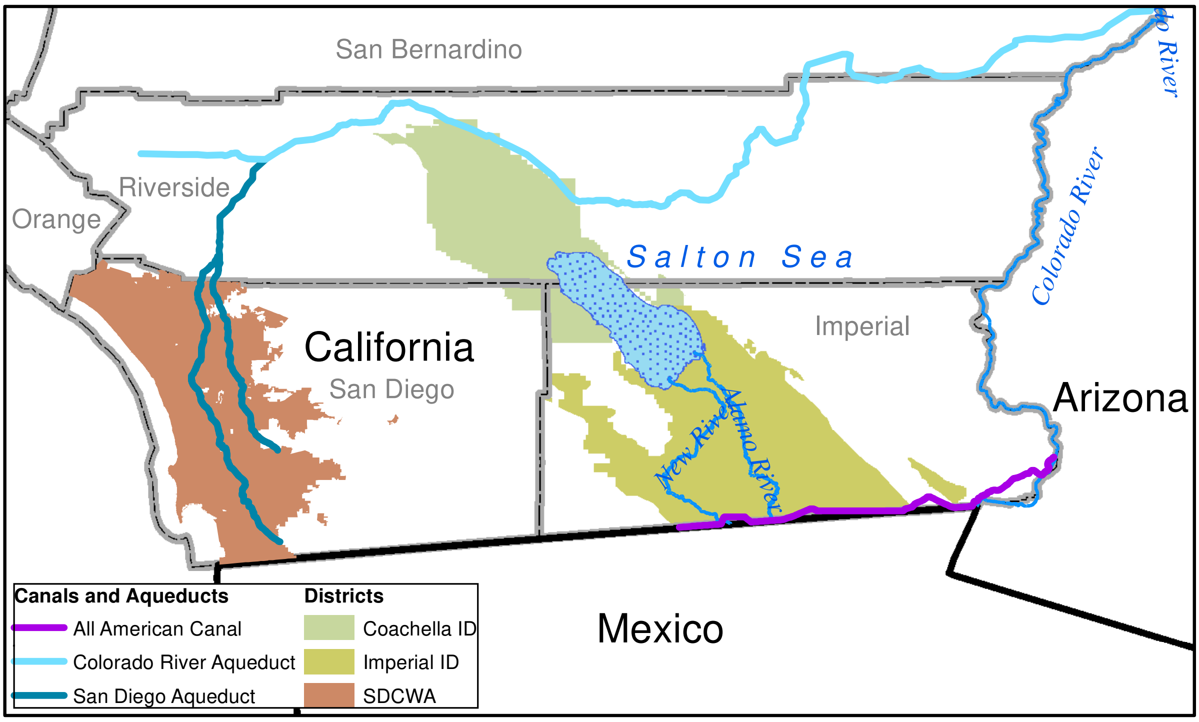
Notes: See appendix C for complete estimation results, including the list of control variables included in the analysis of each outcome variable. All models control for county and year fixed effects. Models use all available control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Harvested Acres is measured in thousands. Employment measures are for the ag sector (NAICS=11) and the crop sector (NAICS=111). Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Table 3: Difference-in-Differences Estimates for Air Quality Measures (FE Poisson Regression).

	Dust-Related Air Quality Measures		Air Quality Placebo Measures	
	PM10 Days	PM2.5 Days	Ozone Days	NO_2 Days
Treatment Effect	0.6971***	1.0241***	-0.1497***	-0.1027***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Observations	1,876	1,069	1,276	1,276
Log-likelihood	-10,807.38	-16,410.72	-12,753.68	-8,639.59

Notes: See appendix C for complete estimation results, including the list of control variables included in the analysis of each outcome variable. All models control for county and year fixed effects. Models use all available control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

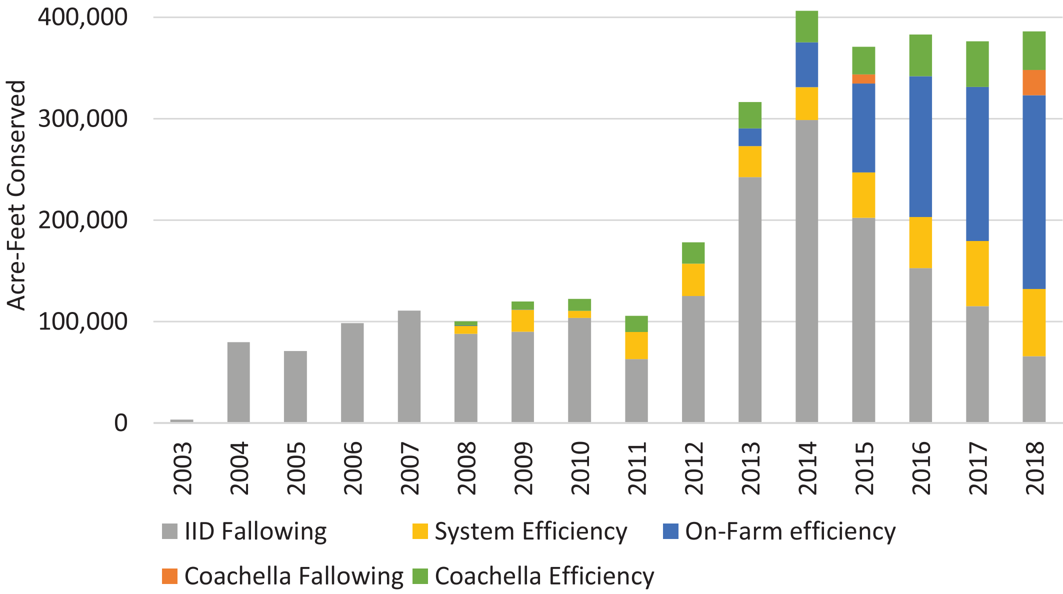
Figure 1: Map of Study Region.



Notes: Imperial (and Coachella) Irrigation Districts receive water from the Colorado River via diversions just north of the Mexican border into the All-American Canal. Water sold to San Diego is diverted upstream from this diversion into the Colorado River Aqueduct and then the San Diego Aqueduct. Imperial diversions into the All-American Canal are reduced to correspond to the amount of water transferred.

Source: Author created map made using data from the State of California and the US Census Bureau.

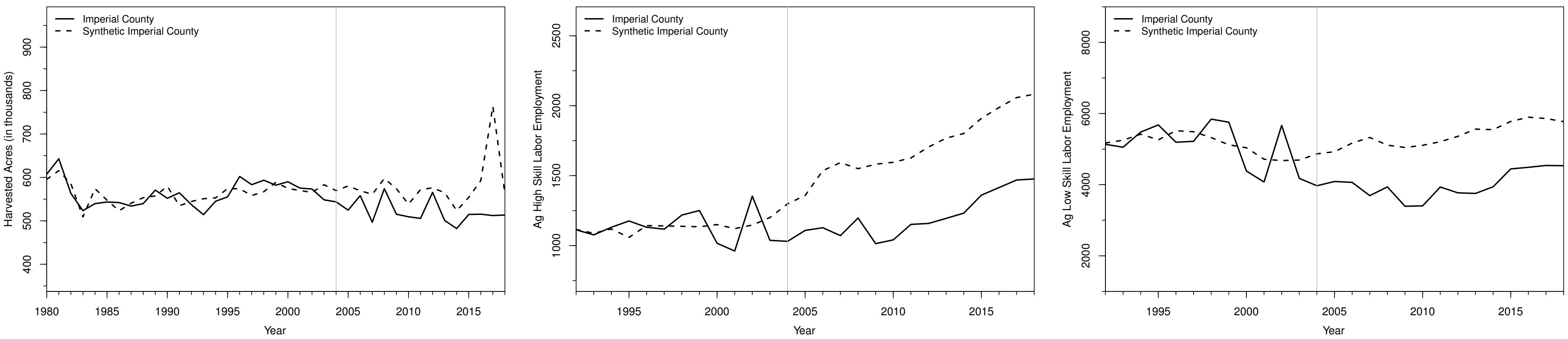
Figure 2: Imperial Irrigation District Water Conservation.



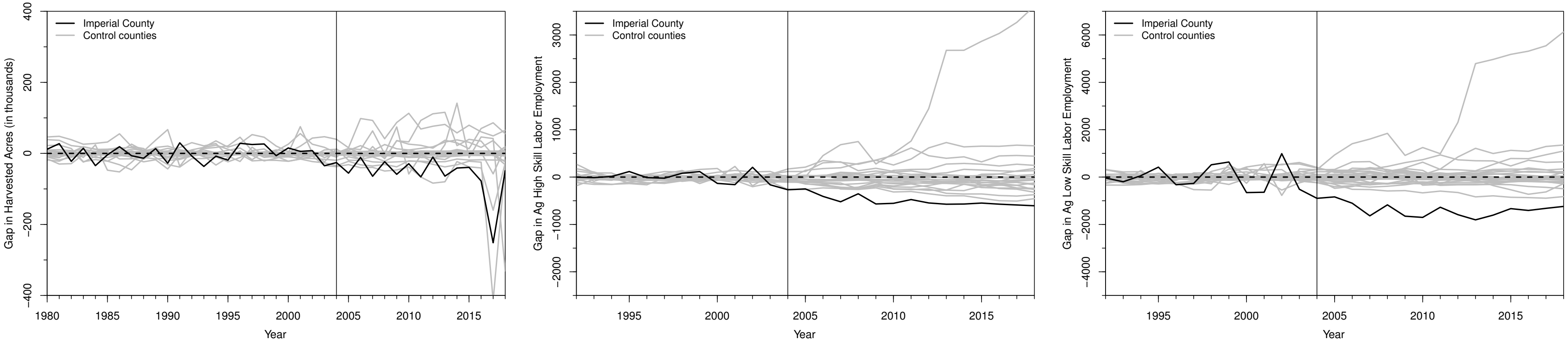
Notes: Estimates of the amount of water IID conserved by type. Conserved water may be credited for transfer to SDCWA or for Salton Sea mitigation. Figure is based on author simplification of figure "IID QSA annual conserved water summary" (IID, 2018, p.69). Coachella numbers are estimated amounts.

Figure 3: Imperial County Agricultural Production and Labor.

Panel A: Synthetic Control Output.

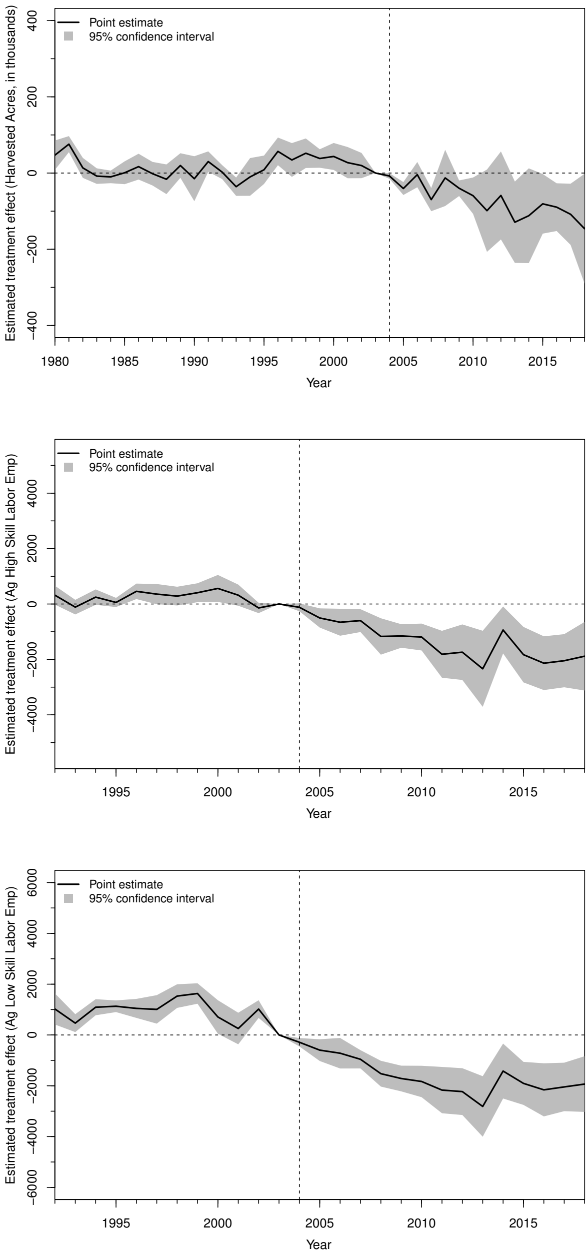


Panel B: Falsification Tests.



Notes: Graphical summary of synthetic control output for Harvested Acres (left), Ag High-Skill Employment (middle), and Ag Low-Skill Employment (right). Panel A shows the time path realized by Imperial County and the synthetic Imperial County. Panel B shows the falsification test results of the estimated treatment effect for Imperial County along with placebo effects for control units. Donor pool for Harvested Acres and Labor Employment consists of 29 and 30, respectively, control counties. To refine inferences from falsification tests, we consider control counties with pre-intervention RMSPEs that are less than or equal to twice that of a treatment unit (Abadie et al., 2010). Employment measures are for the ag sector (NAICS=11). The vertical line represents the QSA effective year.

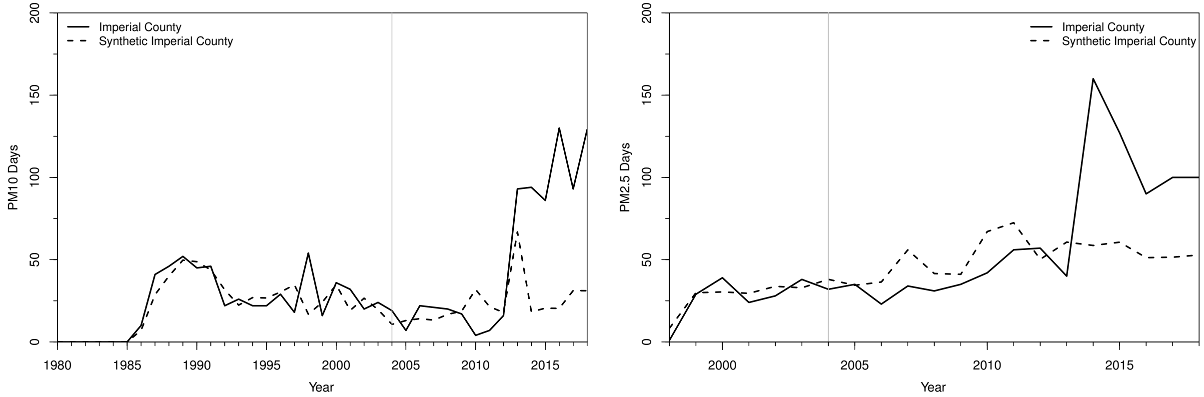
Figure 4: Imperial County Agricultural Production and Labor Event Studies.



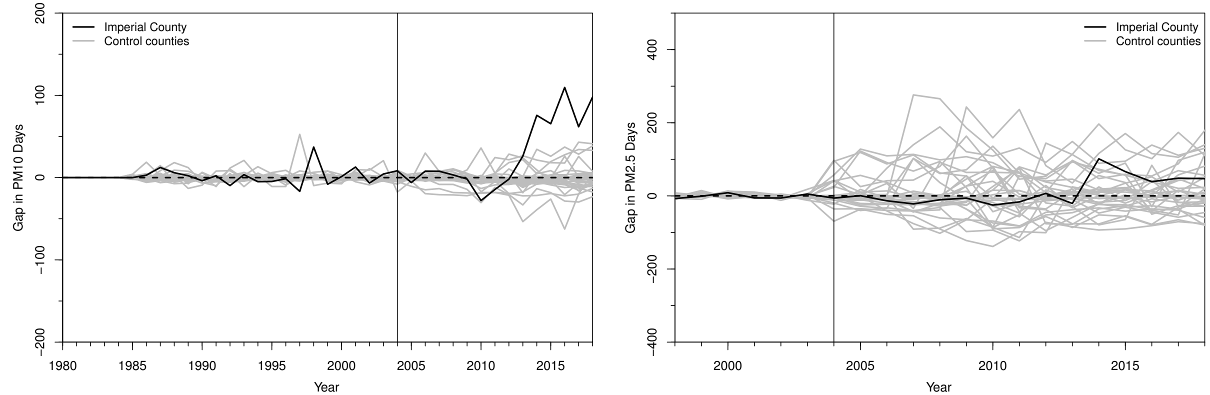
Notes: Event study analysis for Harvested Acres (top), Ag High-Skill Employment (middle), and Ag Low-Skill Employment (bottom). See appendix B for the list of control variables included in the analysis of each outcome variable. All models control for county and year fixed effects. Models use all available control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. The confidence bounds are obtained using robust standard errors. Employment measures are for the ag sector (NAICS=11). The vertical line represents the QSA effective year.

Figure 5: Imperial County Dust-Related Air Quality.

Panel A: Synthetic Control Output.



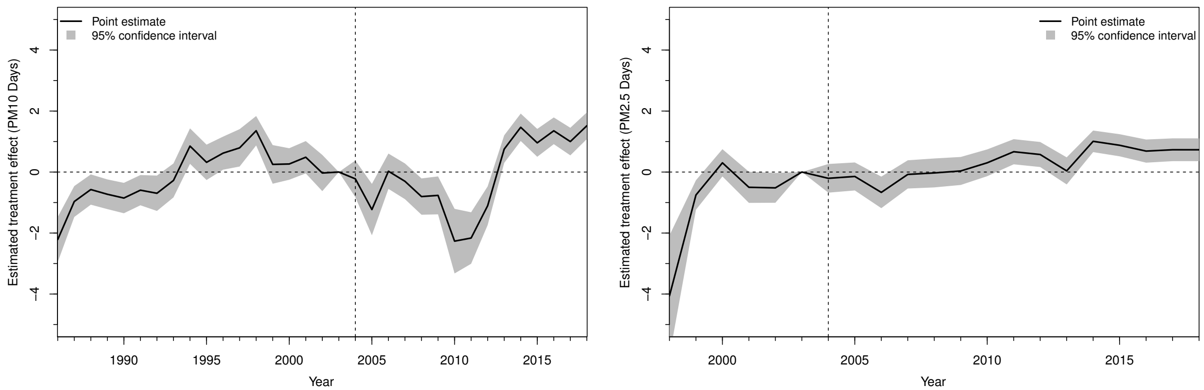
Panel B: Falsification Tests.



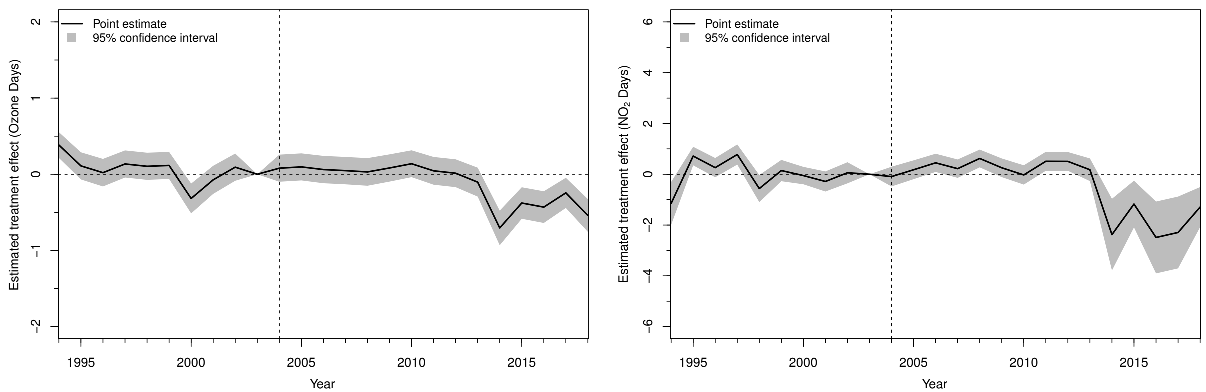
Notes: Graphical summary of synthetic control output for PM10 Days (left) and PM2.5 Days (right). Panel A shows the time path realized by Imperial County and the synthetic Imperial County. Panel B shows the falsification test results of the estimated treatment effect for Imperial County along with placebo effects for control units. Donor pool for PM10 Days and PM2.5 Days consists of 35 and 49, respectively, control counties. To refine inferences from falsification tests, we consider control counties with pre-intervention RMSPEs that are less than or equal to twice that of a treatment unit (Abadie et al., 2010). The vertical line represents the QSA effective year.

Figure 6: Imperial County Air Quality Event Studies (FE Poisson Regression).

Panel A: Dust-Related Air Quality Measures.



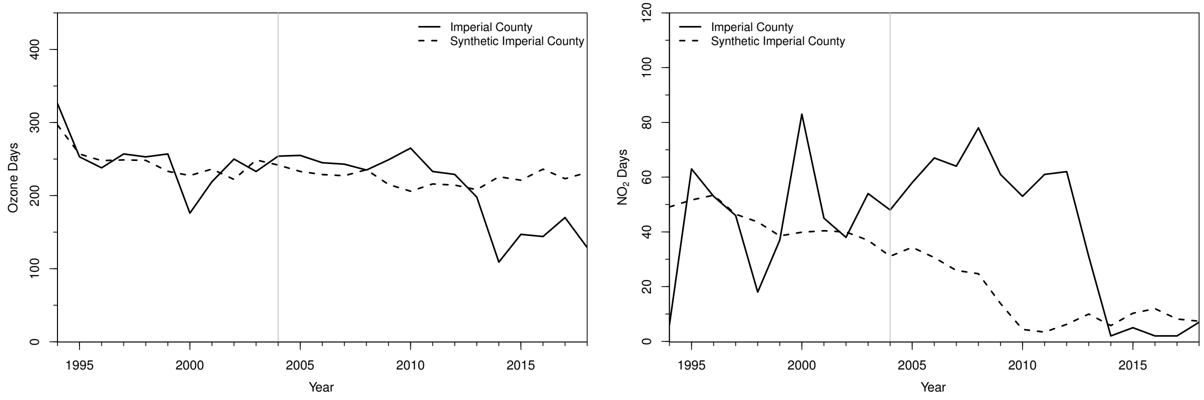
Panel B: Air-Quality Placebo Measures.



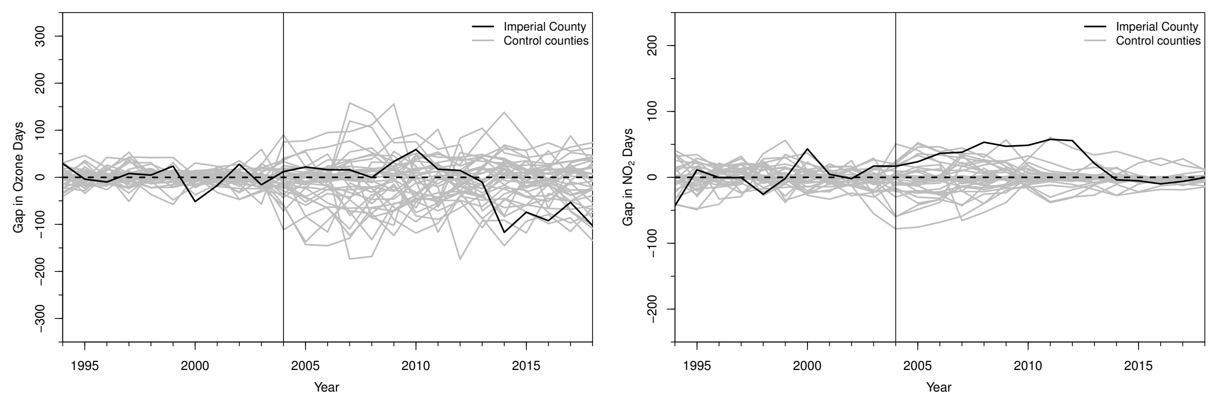
Notes: Event study analysis for PM10, PM2.5, Ozone, and NO_2 Days. See appendix B for the list of control variables included in the analysis of each outcome variable. All models control for county and year fixed effects. Models use all available control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. The vertical line represents the QSA effective year.

Figure 7: Imperial County Air-Quality Placebo Measures.

Panel A: Synthetic Control Output.



Panel B: Falsification Tests.



Notes: Graphical summary of synthetic control output for Ozone Days (left) and NO_2 Days (right). Panel A shows the time path realized by Imperial County and the synthetic Imperial County. Panel B shows the falsification test results of the estimated treatment effect for Imperial County along with placebo effects for control units. Donor pool for both variables consists of 48 control counties. To refine inferences from falsification tests, we consider control counties with pre-intervention RMSPEs that are less than or equal to twice that of a treatment unit (Abadie et al., 2010). The vertical line represents the QSA effective year.

Appendix:

Left in the Dust? Environmental and Labor Effects of Rural-Urban Water Sales

Muyang Ge, Sherzod B. Akhundjanov, Eric C. Edwards, and Reza Oladi

A Data Description

Table A1: Variable Descriptions and Sources.

Outcome Variables	Source	Period	Notes
Harvested acreage	USDA	1980-2018	Data from annual crop report compiled by the California County Agricultural Commissioners (CCAC) providing detailed annual agricultural production data at the county level. https://www.nass.usda.gov/Statistics_by_State/California/Publications/AgComm/index.php
Skilled labor employment	Census Bureau	1992-2018	Quarterly employment is the estimate of the number of jobs that are held on both the first and last day of the quarter with the same employer. Our measure is the mean of the four quarters in a year.
Unskilled labor employment	Census Bureau	1992-2018	Quarterly employment is the estimate of the number of jobs that are held on both the first and last day of the quarter with the same employer. Our measure is the mean of the four quarters in a year.
Bad air pollution days	EPA	Varies (see text)	Bad Days is based on author's calculation. Bad days = (Unhealthy for Sensitive Groups Days + Unhealthy Days + Very Unhealthy Days + Hazardous Days)/AQI sampling days. Calculated separately for PM2.5, PM10, Ozone, and NO_2. https://www.epa.gov/criteria-air-pollutants/naaqs-table
Predictors	**Source**	**Periods**	**Notes**
Farm proprietors' income	BEA	1980-2018	Income (in $ millions) received by sole proprietorships and partnerships that operate farms (excludes income received by corporate farms).
Farm proprietors' employment	BEA	1980-2018	Employment of sole proprietors and noncorporate partners in the farm industry in thousands of jobs.
Wage and salary employment	BEA	1980-2018	Average annual number of full-time and part-time jobs (thousands of jobs).
Wage and salary	BEA	1980-2018	Aggregation of county wages and salaries (in billions of dollars).
Proprietors' employment	BEA	1980-2018	Proprietors' income is the current-production income (including income in kind) of sole proprietorships, partnerships, and tax-exempt cooperatives. Includes farm proprietors' and nonfarm proprietors' employment (in number of jobs).
Proprietors' income	BEA	1980-2018	The proprietor's income is in billions of dollars. https://apps.bea.gov/regional/histdata/releases/1117lapi/index.cfm

Table A1: (*Continued*).

White ag labor ratio	Census Bureau	1992-2018	Author calculation using LEHD number of stable jobs in agricultural sector, white employees over total employment.
Male ag labor ratio	Census Bureau	1992-2018	Male employees over total employment.
Hispanic ag labor ratio	Census Bureau	1992-2018	Hispanic employees over total employment.
High school or higher ag labor ratio	Census Bureau	1992-2018	Employees with high school degree or higher over total employment.
Annual cattle values	USDA	1980-2018	Data from annual crop report compiled by the California County Agricultural Commissioners (CCAC) providing detailed annual agricultural production data at the county level.
Annual alfalfa hay values	USDA	1980-2018	Data from annual crop report compiled by the California County Agricultural Commissioners (CCAC) providing detailed annual agricultural production data at the county level.
Annual lettuce values	USDA	1980-2018	Data from annual crop report compiled by the California County Agricultural Commissioners (CCAC) providing detailed annual agricultural production data at the county level.
Annual melons values	USDA	1980-2018	Data from annual crop report compiled by the California County Agricultural Commissioners (CCAC) providing detailed annual agricultural production data at the county level.
Annual other vegetable values	USDA	1980-2018	Data from annual crop report compiled by the California County Agricultural Commissioners (CCAC) providing detailed annual agricultural production data at the county level.

B Summary Statistics of Variables Used in Each Outcome Variable Analysis

Table B1: Summary Statistics for Variables Used in the Analysis of Harvested Acres.

Variable	Sample	Mean	Std. Dev.
Harvested Acres	1,555	188.68	265.08
Farm Proprietors' Income	1,555	119.20	223.43
Farm Proprietors' Employment	1,555	1.44	1.52
Wage and Salary Employment	1,555	222.99	498.23
Wage and Salary	1,555	8,542.49	19,204.46
Proprietors' Employment	1,555	52.67	100.84
Proprietors' Income	1,555	1,523.13	2,773.80
Annual Cattle Values	1,555	44,888.45	93,886.92
Annual Alfalfa Hay Values	1,555	22,449.45	45,380.42
Annual Vegetable Values	1,555	119,730.20	342,839.10

Notes: (Unbalanced) data includes observations for 51 counties (the treatment county and 50 control counties) for 1980-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables are in thousands.

Table B2: Summary Statistics for Variables Used in the Analysis of Ag High-Skill Labor Employment.

Variable	Sample	Mean	Std. Dev.
Ag High Skill Labor Employment	1,050	1,398.96	2,135.29
Farm Proprietors' Income	1,050	145.17	258.64
Farm Proprietors' Employment	1,050	1.37	1.43
Wage and Salary Employment	1,050	217	406.52
Wage and Salary	1,050	10,205.03	20,609.87
Proprietors' Employment	1,050	56.89	98.14
Proprietors' Income	1,050	1,904.59	3,027.42
White Ag Labor Ratio	1,050	0.85	0.04
Male Ag Labor Ratio	1,050	0.70	0.07
Hispanic Ag Labor Ratio	1,050	0.49	0.15
High School or Higher Ag Labor Ratio	1,050	0.53	0.09
Annual Cattle Values	1,050	54,654.46	109,409.30
Annual Alfalfa Hay Values	1,050	26,059.64	51,161.19
Annual Vegetable Values	1,050	149,702.70	403,854.40

Notes: (Unbalanced) data includes observations for 49 counties (the treatment county and 48 control counties) for 1992-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables, except Ag High Skill Labor Employment, White Ag Labor Ratio, Male Ag Labor Ratio, Hispanic Ag Labor Ratio, and High School or Higher Ag Labor Ratio, are in thousands.

Table B3: Summary Statistics for Variables Used in the Analysis of Ag Low-Skill Labor Employment.

Variable	Sample	Mean	Std. Dev.
Ag Low Skill Labor Employment	1,049	2,856.66	4,443.42
Farm Proprietors' Income	1,049	145.3	258.72
Farm Proprietors' Employment	1,049	1.37	1.43
Wage and Salary Employment	1,049	217.2	406.66
Wage and Salary	1,049	10,214.64	20,617.34
Proprietors' Employment	1,049	56.94	98.17
Proprietors' Income	1,049	1,906.36	3,028.32
White Ag Labor Ratio	1,049	0.85	0.04
Male Ag Labor Ratio	1,049	0.70	0.07
Hispanic Ag Labor Ratio	1,049	0.49	0.15
High School or Higher Ag Labor Ratio	1,049	0.53	0.09
Annual Cattle Values	1,049	54,703.48	109,450.00
Annual Alfalfa Hay Values	1,049	26,084.40	51,179.29
Annual Vegetable Values	1,049	149,845.40	404,020.50

Notes: (Unbalanced) data includes observations for 49 counties (the treatment county and 48 control counties) for 1992-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables, except Ag Low Skill Labor Employment, White Ag Labor Ratio, Male Ag Labor Ratio, Hispanic Ag Labor Ratio, and High School or Higher Ag Labor Ratio, are in thousands.

Table B4: Summary Statistics for Variables Used in the Analysis of Crop High-Skill Labor Employment.

Variable	Sample	Mean	Std. Dev.
Crop High Skill Labor Employment	1,008	687.40	889.37
Farm Proprietors' Income	1,008	151.08	262.31
Farm Proprietors' Employment	1,008	1.42	1.44
Wage and Salary Employment	1,008	225.66	412.64
Wage and Salary	1,008	10,616.88	20,934.10
Proprietors' Employment	1,008	59.08	99.56
Proprietors' Income	1,008	1,978.46	3,067.70
White Ag Labor Ratio	1,008	0.84	0.04
Male Ag Labor Ratio	1,008	0.70	0.07
Hispanic Ag Labor Ratio	1,008	0.50	0.14
High School or Higher Ag Labor Ratio	1,008	0.53	0.09
Annual Cattle Values	1,008	56,513.84	111,275.20
Annual Alfalfa Hay Values	1,008	27,007.97	51,994.56
Annual Vegetable Values	1,008	155,902.30	411,021.80

Notes: (Unbalanced) data includes observations for 48 counties (the treatment county and 47 control counties) for 1992-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables, except Crop High Skill Labor Employment, White Ag Labor Ratio, Male Ag Labor Ratio, Hispanic Ag Labor Ratio, and High School or Higher Ag Labor Ratio, are in thousands.

Table B5: Summary Statistics for Variables Used in the Analysis of Crop Low-Skill Labor Employment.

Variable	Sample	Mean	Std. Dev.
Crop Low Skill Labor Employment	1,029	1,468.99	2,045.55
Farm Proprietors' Income	1,029	148.07	260.46
Farm Proprietors' Employment	1,029	1.40	1.43
Wage and Salary Employment	1,029	221.25	409.55
Wage and Salary	1,029	10,406.86	20,770.27
Proprietors' Employment	1,029	57.98	98.84
Proprietors' Income	1,029	1,940.81	3,047.41
White Ag Labor Ratio	1,029	0.85	0.04
Male Ag Labor Ratio	1,029	0.70	0.07
Hispanic Ag Labor Ratio	1,029	0.50	0.15
High School or Higher Ag Labor Ratio	1,029	0.53	0.09
Annual Cattle Values	1,029	55,585.80	110,323.20
Annual Alfalfa Hay Values	1,029	26,516.94	51,577.75
Annual Vegetable Values	1,029	152,728.60	407,396.30

Notes: (Unbalanced) data includes observations for 49 counties (the treatment county and 48 control counties) for 1992-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables, except Crop Low Skill Labor Employment, White Ag Labor Ratio, Male Ag Labor Ratio, Hispanic Ag Labor Ratio, and High School or Higher Ag Labor Ratio, are in thousands.

Table B6: Summary Statistics for Variables Used in the Analysis of PM10 Days.

Variable	Sample	Mean	Std. Dev.
PM10 Days	1,876	12.81	33.65
Days with AQI	1,876	334.62	80.13
Median AQI	1,876	48.54	21.53
Farm Proprietors' Income	1,876	107.37	210.63
Farm Proprietors' Employment	1,876	1.32	1.43
Wage and Salary Employment	1,876	283.48	657.8
Wage and Salary	1,876	11,782.76	30,366.88
Proprietors' Employment	1,876	71.55	172.14
Proprietors' Income	1,876	2,297.81	6,217.10

Notes: (Unbalanced) data includes observations for 54 counties (the treatment county and 53 control counties) for 1980-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables, except PM10 Days, Days with AQI, and Median AQI, are in thousands.

Table B7: Summary Statistics for Variables Used in the Analysis of PM2.5 Days.

Variable	Sample	Mean	Std. Dev.
PM2.5 Days	1,069	83.46	74.02
Days with AQI	1,069	343.73	69.67
Median AQI	1,069	47.60	18.27
Farm Proprietors' Income	1,069	135.89	258.48
Farm Proprietors' Employment	1,069	1.14	1.20
Wage and Salary Employment	1,069	294.78	670.70
Wage and Salary	1,069	15,596.14	36,842.41
Proprietors' Employment	1,069	83.92	201.75
Proprietors' Income	1,069	3,208.93	7,788.22

Notes: (Unbalanced) data includes observations for 54 counties (the treatment county and 53 control counties) for 1998-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables, except PM2.5 Days, Days with AQI, and Median AQI, are in thousands.

Table B8: Summary Statistics for Variables Used in the Analysis of Ozone Days.

Variable	Sample	Mean	Std. Dev.
Ozone Days	1,276	227.11	101.21
Days with AQI	1,276	341.68	73.15
Median AQI	1,276	47.30	18.31
Farm Proprietors' Income	1,276	124.86	242.79
Farm Proprietors' Employment	1,276	1.19	1.28
Wage and Salary Employment	1,276	287.71	660.73
Wage and Salary	1,276	14,361.13	34,802.35
Proprietors' Employment	1,276	80.22	193.24
Proprietors' Income	1,276	2,935.39	7,303.44

Notes: (Unbalanced) data includes observations for 54 counties (the treatment county and 53 control counties) for 1994-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables, except Ozone Days, Days with AQI, and Median AQI, are in thousands.

Table B9: Summary Statistics for Variables Used in the Analysis of NO_2 Days.

Variable	Sample	Mean	Std. Dev.
NO_2 Days	1,276	26.42	45.47
Days with AQI	1,276	341.68	73.15
Median AQI	1,276	47.30	18.31
Farm Proprietors' Income	1,276	124.86	242.79
Farm Proprietors' Employment	1,276	1.19	1.28
Wage and Salary Employment	1,276	287.71	660.73
Wage and Salary	1,276	14,361.13	34,802.35
Proprietors' Employment	1,276	80.22	193.24
Proprietors' Income	1,276	2,935.39	7,303.44

Notes: (Unbalanced) data includes observations for 54 counties (the treatment county and 53 control counties) for 1994-2018. Omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. All the variables, except NO_2 Days, Days with AQI, and Median AQI, are in thousands.

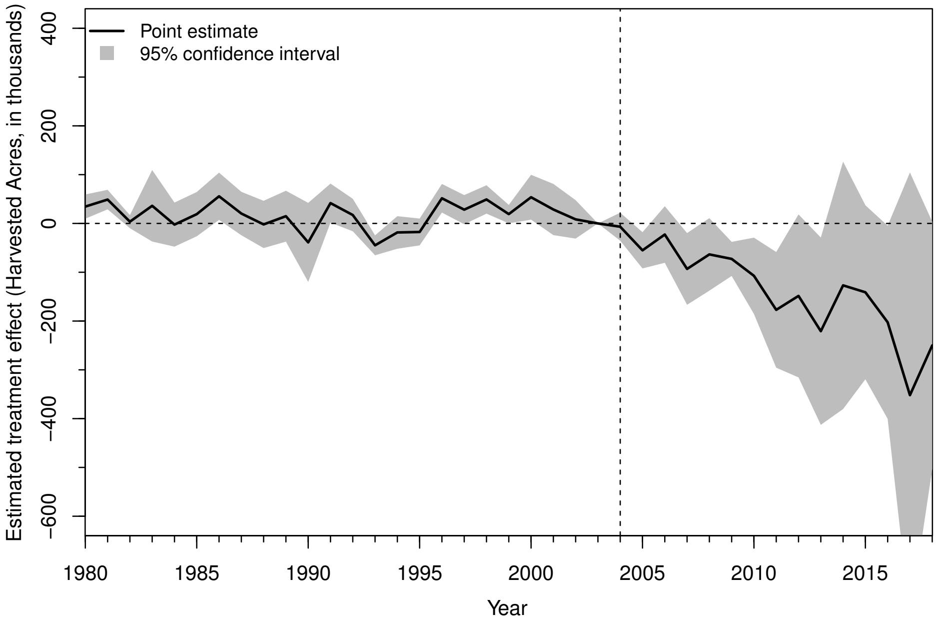
C Additional Empirical Results

Table C1: Difference-in-Differences Analysis for Harvested Acres.

	(1)	(2)
1(Imperial)×1(Post-intervention)	-75.5588***	-101.5805***
	(23.8476)	(33.9260)
Farm Proprietors' Income	-0.0129	-0.0353
	(0.0349)	(0.0760)
Farm Proprietors' Employment	12.2460	-40.9849***
	(20.6599)	(12.9161)
Wage and Salary Employment	-0.0418	2.2353**
	(0.0416)	(1.0953)
Wage and Salary	0.0001	-0.0401*
	(0.0002)	(0.0206)
Proprietors' Employment	0.0113	0.1774
	(0.0777)	(2.6029)
Proprietors' Income	-0.00005	0.0135
	(0.0014)	(0.0525)
Annual Cattle Values	0.0001	0.0002
	(0.0002)	(0.0001)
Annual Alfalfa Hay Values	0.0001	0.00001
	(0.0002)	(0.0002)
Annual Vegetable Values	0.00003*	0.0001***
	(0.00002)	(0.00002)
Observations	1,555	234
R^2	0.0324	0.1947
F Statistic	4.8697***	4.3512***

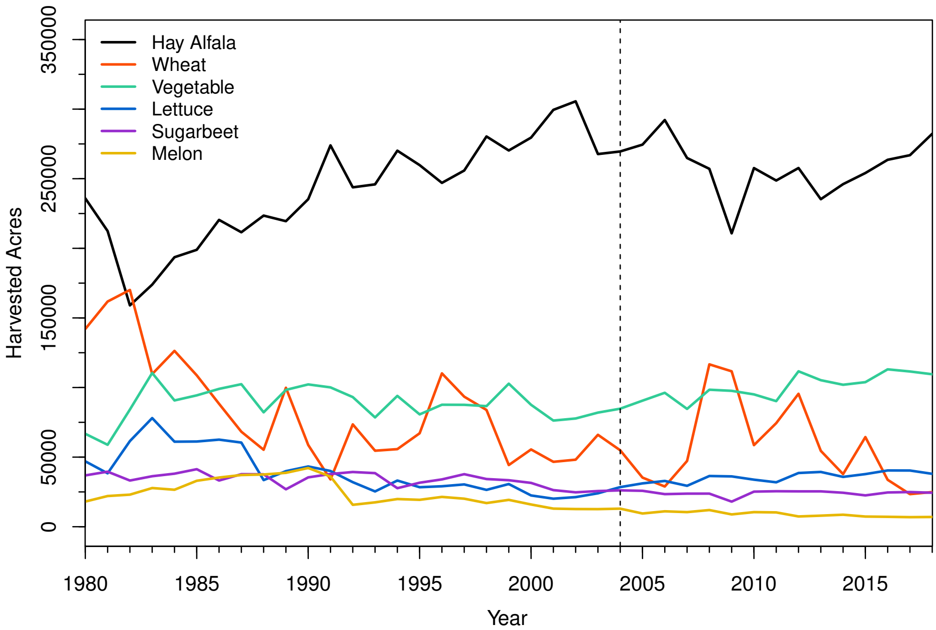
Notes: Model 1 uses all available (50) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Harvested Acres is measured in thousands. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Figure C1: Event Study Analysis for Harvested Acres.



Notes: Event study analysis for Harvested Acres using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. The model controls for county and year fixed effects. The confidence bounds are obtained using robust standard errors. The vertical line represents the QSA effective year.

Figure C2: Harvested Acres by Crop Type in Imperial County.



Notes: Data from annual crop report compiled by the California County Agricultural Commissioners (CCAC). The vertical line represents the QSA effective year.

Table C2: Difference-in-Differences Analysis for Hay Alfalfa Acres.

	(1)	(2)
1(Imperial)×1(Post-intervention)	-14.7916^{*}	-48.1613^{***}
	(7.8049)	(3.6182)
Farm Proprietors' Income	-0.0226^{***}	-0.0597^{***}
	(0.0081)	(0.0016)
Farm Proprietors' Employment	-1.1685	10.1109^{***}
	(3.4083)	(2.3190)
Wage and Salary Employment	-0.0189	-0.7917^{***}
	(0.0137)	(0.0162)
Wage and Salary	0.0001	0.0228^{***}
	(0.0001)	(0.00003)
Proprietors' Employment	-0.0090	-5.0185^{***}
	(0.0288)	(0.1206)
Proprietors' Income	-0.0001	0.0024^{***}
	(0.0005)	(0.0002)
Annual Alfalfa Hay Values	0.0004^{***}	0.0004^{***}
	(0.0001)	(0.00001)
Observations	1,834	78
R^2	0.2117	0.7932
F Statistic	58.2751^{***}	14.3866^{***}

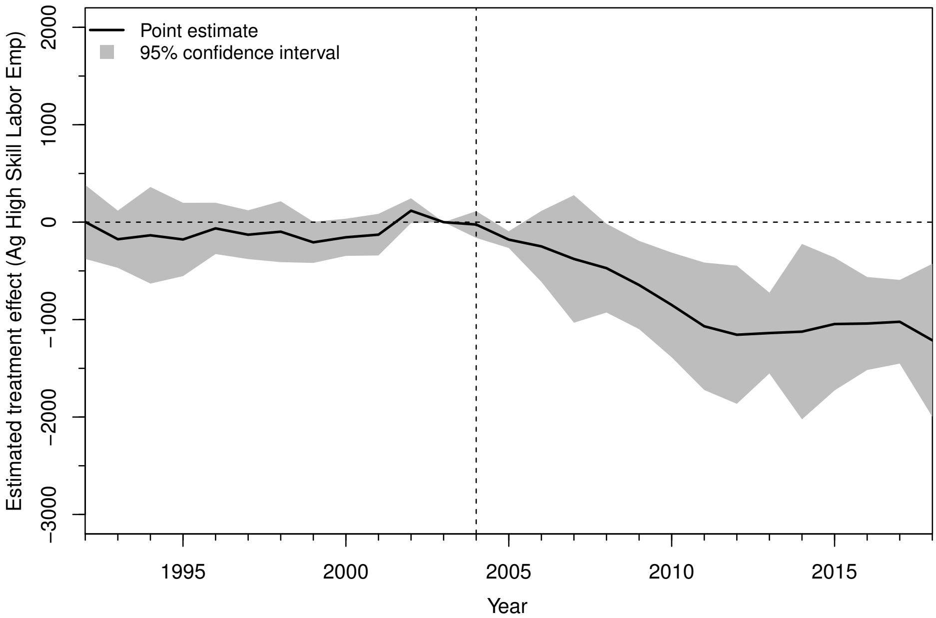
Notes: Model 1 uses all available (52) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis. Both models control for county and year fixed effects. Hay Alfalfa Acres is measured in thousands. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Table C3: Difference-in-Differences Analysis for Ag High-Skill Labor Employment.

	(1)	(2)
1(Imperial)×1(Post-intervention)	-1,367.0360***	86.9032
	(391.3947)	(93.7862)
Farm Proprietors' Income	1.7902**	1.1420***
	(0.8163)	(0.0637)
Farm Proprietors' Employment	149.8136	-692.1584***
	(384.4494)	(183.9810)
Wage and Salary Employment	3.4769	14.4508
	(2.2658)	(10.0252)
Wage and Salary	-0.0107**	-0.0291
	(0.0052)	(0.1491)
Proprietors' Employment	-5.3897*	49.7737*
	(3.0171)	(26.4864)
Proprietors' Income	0.0076	-0.8106***
	(0.0389)	(0.1285)
White Ag Labor Ratio	-2,303.4760**	-9,837.8240**
	(1,013.6630)	(4,380.8940)
Male Ag Labor Ratio	-656.3264	-3,452.4110***
	(930.0653)	(939.5769)
Hispanic Ag Labor Ratio	-979.6626	7,358.6510***
	(873.4222)	(2,314.2420)
High School or Higher Ag Labor Ratio	2,023.6130	9,280.2650**
	(1,396.6070)	(4,204.5420)
Annual Cattle Values	0.0028*	-0.0011***
	(0.0017)	(0.0003)
Annual Alfalfa Hay Values	-0.0018	0.0001
	(0.0019)	(0.0007)
Annual Vegetable Values	0.0023***	0.0003*
	(0.0008)	(0.0002)
Observations	1,050	108
R^2	0.5986	0.9076
F Statistic	102.3863***	44.9236***

Notes: Model 1 uses all available (48) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Figure C3: Event Study Analysis for Ag High-Skill Labor Employment.



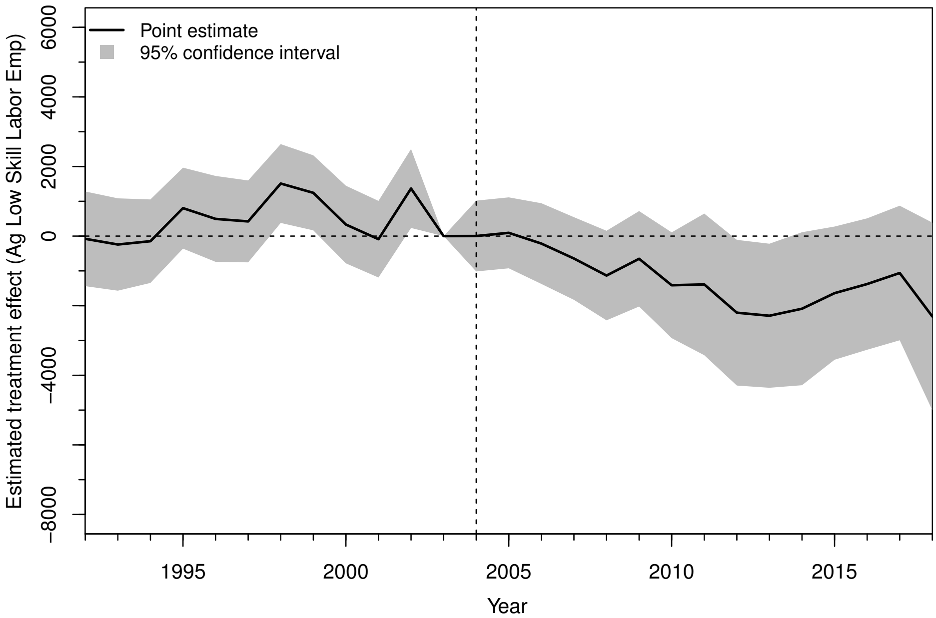
Notes: Event study analysis for Ag High-Skill Labor Employment using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. The model controls for county and year fixed effects. The confidence bounds are obtained using robust standard errors. Employment measure is for the ag sector (NAICS=11). The vertical line represents the QSA effective year.

Table C4: Difference-in-Differences Analysis for Ag Low-Skill Labor Employment.

	(1)	(2)
1(Imperial)×1(Post-intervention)	-2,323.6270***	-769.1221***
	(361.6651)	(185.9512)
Farm Proprietors' Income	2.2219**	1.6630***
	(0.9720)	(0.5251)
Farm Proprietors' Employment	876.8601	238.4546
	(551.9801)	(152.5152)
Wage and Salary Employment	4.1706	47.7105***
	(2.7683)	(12.5571)
Wage and Salary	-0.0149**	-1.1076***
	(0.0062)	(0.1641)
Proprietors' Employment	-7.0656*	282.3479***
	(3.6523)	(19.2937)
Proprietors' Income	0.0055	-0.5685
	(0.0485)	(0.4475)
White Ag Labor Ratio	-1,597.3660	-33,550.5000***
	(1,127.8540)	(3,360.5480)
Male Ag Labor Ratio	-1,470.3720	-10,910.6400*
	(974.4953)	(5,763.6920)
Hispanic Ag Labor Ratio	-29.5041	22,865.2700***
	(981.3061)	(3,628.1840)
High School or Higher Ag Labor Ratio	2,262.3560	15,344.5700***
	(1,825.9140)	(5,393.1060)
Annual Cattle Values	0.0023	-0.0015
	(0.0020)	(0.0011)
Annual Alfalfa Hay Values	-0.0012	0.0008
	(0.0028)	(0.0005)
Annual Vegetable Values	0.0021*	-0.0010**
	(0.0011)	(0.0004)
Observations	1,049	108
R^2	0.4487	0.8421
F Statistic	55.8084***	24.3877***

Notes: Model 1 uses all available (48) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Figure C4: Event Study Analysis for Ag Low-Skill Labor Employment.



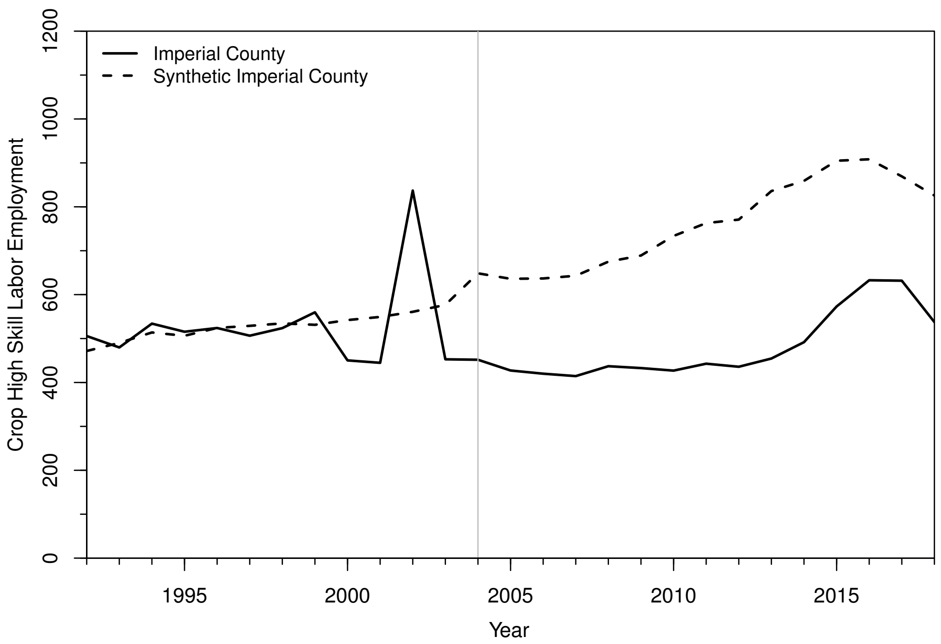
Notes: Event study analysis for Ag Low-Skill Labor Employment using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. The model controls for county and year fixed effects. The confidence bounds are obtained using robust standard errors. Employment measure is for the ag sector (NAICS=11). The vertical line represents the QSA effective year.

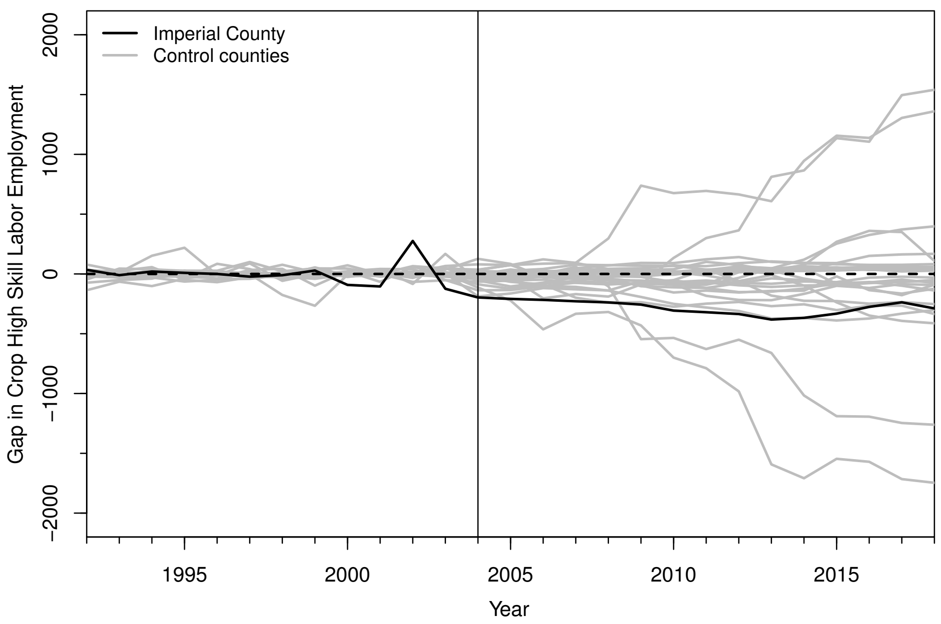
Table C5: Difference-in-Differences Analysis for Crop High-Skill Labor Employment.

	(1)	(2)
1(Imperial)×1(Post-intervention)	-320.0928***	-67.9849
	(119.5283)	(56.4572)
Farm Proprietors' Income	0.6911*	0.2257
	(0.3871)	(0.2664)
Farm Proprietors' Employment	134.4385	101.3437**
	(131.6754)	(42.6406)
Wage and Salary Employment	1.9767**	-20.6808***
	(0.9502)	(2.7785)
Wage and Salary	-0.0058**	0.4844***
	(0.0025)	(0.1227)
Proprietors' Employment	-0.7314	50.1366***
	(1.2684)	(14.1521)
Proprietors' Income	-0.0055	-0.1041
	(0.0193)	(0.2022)
White Ag Labor Ratio	-1,658.2800***	1,776.1150
	(576.6743)	(1,392.5900)
Male Ag Labor Ratio	-488.4528	-3,008.2560***
	(522.5314)	(1,015.9870)
Hispanic Ag Labor Ratio	-216.5619	2,850.9250**
	(502.1189)	(1,300.1720)
High School or Higher Ag Labor Ratio	895.6604	2,471.6420
	(990.6768)	(2,160.7160)
Annual Cattle Values	-0.0007	-0.0008*
	(0.0008)	(0.0004)
Annual Alfalfa Hay Values	-0.0002	-0.0002
	(0.0008)	(0.0003)
Annual Vegetable Values	0.0009***	0.0005***
	(0.0003)	(0.0001)
Observations	1,008	135
R^2	0.4357	0.9344
F Statistic	50.7392***	91.5996***

Notes: Model 1 uses all available (47) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

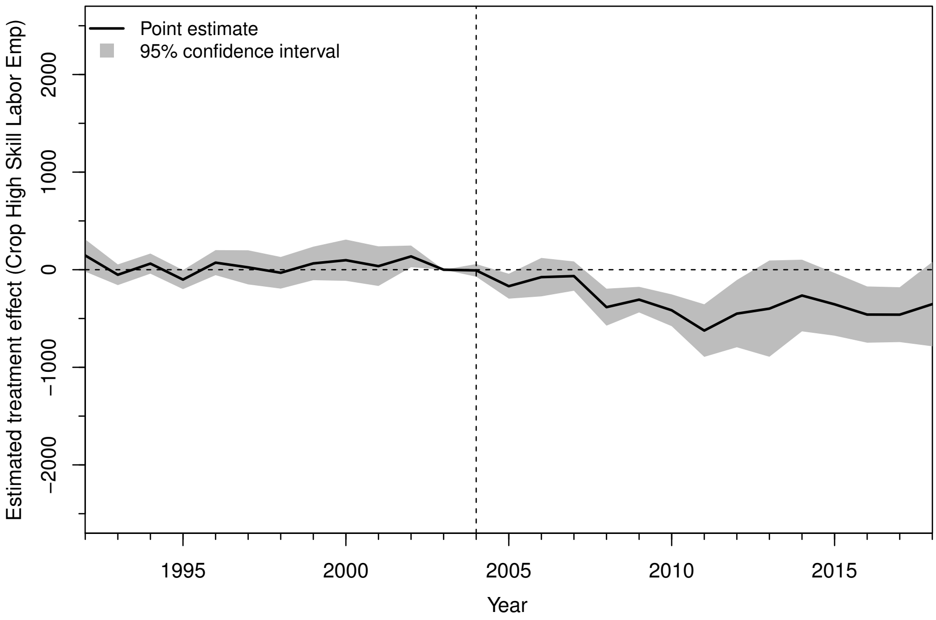
Figure C5: Synthetic Control Analysis for Crop High-Skill Labor Employment.

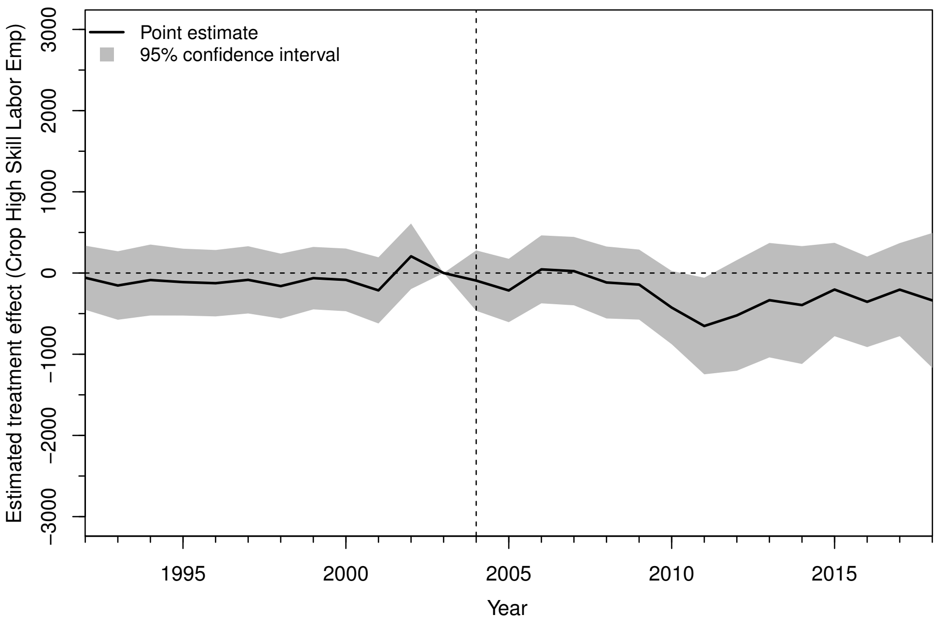




Notes: Graphical summary of synthetic control output for Crop High-Skill Employment. Top panel shows the time path realized by Imperial County and the synthetic Imperial County. Bottom panel shows the falsification test results of the estimated treatment effect for Imperial County along with placebo effects for control units. Donor pool for Crop High-Skill Employment consists of 29 control counties. To refine inferences from falsification tests, we consider control counties with pre-intervention RMSPEs that are less than or equal to twice that of a treatment unit (Abadie et al., 2010). Employment measure is for the crop sector (NAICS=111). The vertical line represents the QSA effective year.

Figure C6: Event Study Analysis for Crop High-Skill Labor Employment.





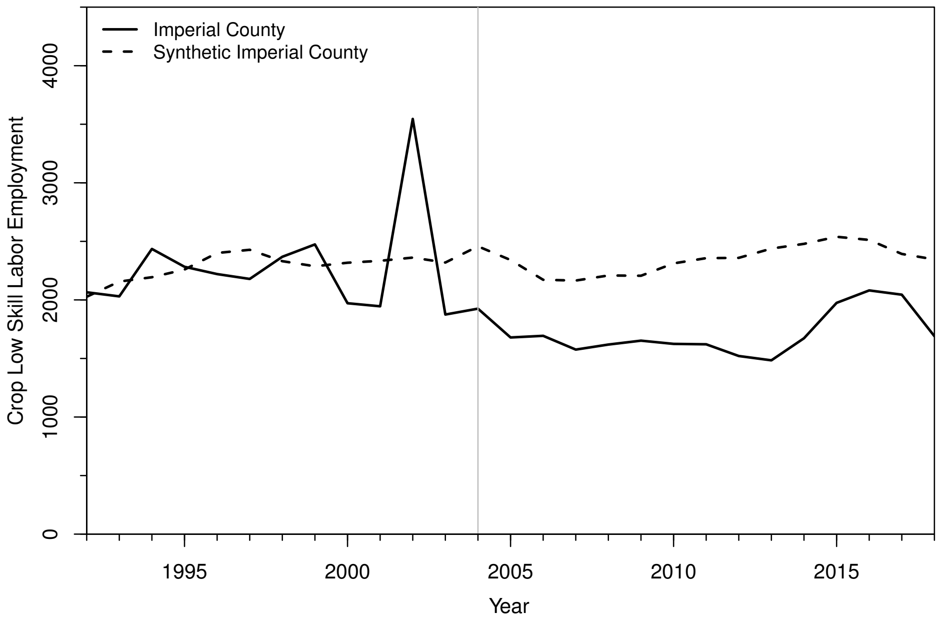
Notes: Event study analysis for Crop High-Skill Labor Employment. Top panel shows the estimated treatment effect for Imperial County using all available control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Bottom panel shows the estimated treatment effect for Imperial County using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. Both models control for county and year fixed effects. The confidence bounds are obtained using robust standard errors. Employment measure is for the crop sector (NAICS=111). The vertical line represents the QSA effective year.

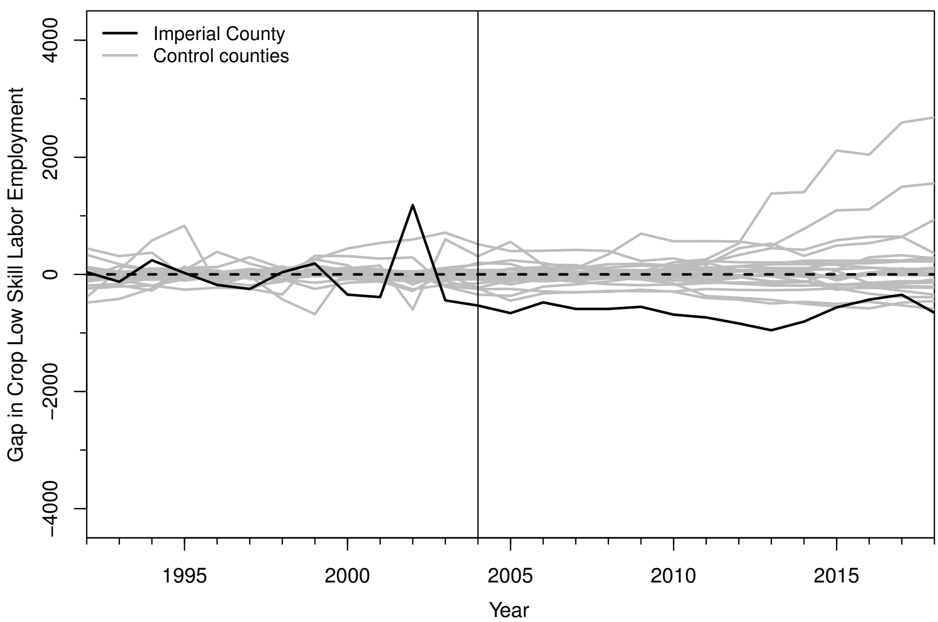
Table C6: Difference-in-Differences Analysis for Crop Low-Skill Labor Employment.

	(1)	(2)
1(Imperial)×1(Post-intervention)	-438.2687***	-283.5757
	(139.5125)	(284.9373)
Farm Proprietors' Income	0.7666	-1.2171**
	(0.5543)	(0.4775)
Farm Proprietors' Employment	663.2295***	316.9246*
	(215.0754)	(176.6794)
Wage and Salary Employment	2.7993***	7.8799
	(0.9868)	(13.7148)
Wage and Salary	-0.0107***	-0.0112
	(0.0023)	(0.1929)
Proprietors' Employment	-0.4435	-138.8576***
	(1.3554)	(50.3148)
Proprietors' Income	-0.0225	0.5034
	(0.0267)	(0.3829)
White Ag Labor Ratio	-1,125.7700*	-2,812.7910
	(619.8188)	(6,668.7190)
Male Ag Labor Ratio	-430.6889	-11,240.4400***
	(583.9036)	(4,266.9500)
Hispanic Ag Labor Ratio	426.8016	9,121.5260*
	(686.8160)	(4,689.3630)
High School or Higher Ag Labor Ratio	379.5046	6,653.9970
	(1,124.4850)	(7,907.4080)
Annual Cattle Values	-0.0020**	-0.0013
	(0.0009)	(0.0008)
Annual Alfalfa Hay Values	0.0006	0.0036***
	(0.0020)	(0.0014)
Annual Vegetable Values	0.0006*	0.0020***
	(0.0004)	(0.0004)
Observations	1,029	135
R^2	0.4268	0.7533
F Statistic	49.9867***	19.6349***

Notes: Model 1 uses all available (48) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

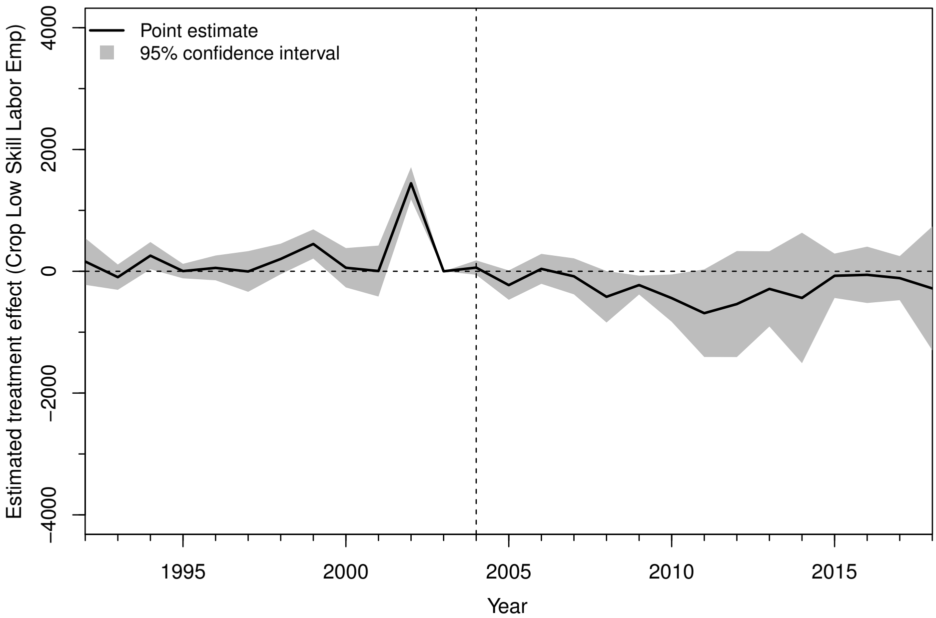
Figure C7: Synthetic Control Analysis for Crop High-Skill Labor Employment.

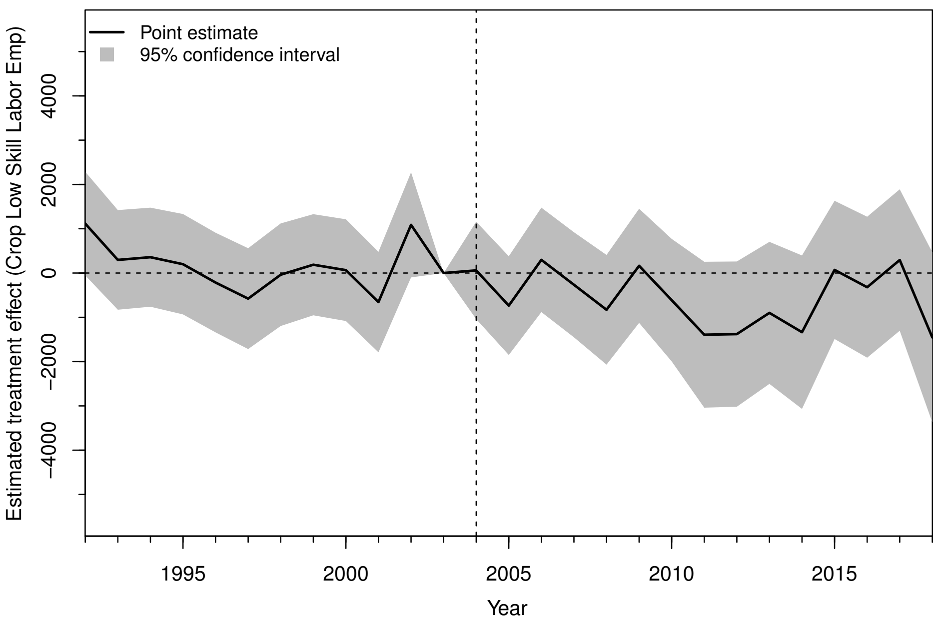




Notes: Graphical summary of synthetic control output for Crop Low-Skill Employment. Top panel shows the time path realized by Imperial County and the synthetic Imperial County. Bottom panel shows the falsification test results of the estimated treatment effect for Imperial County along with placebo effects for control units. Donor pool for Crop High-Skill Employment consists of 29 control counties. To refine inferences from falsification tests, we consider control counties with pre-intervention RMSPEs that are less than or equal to twice that of a treatment unit (Abadie et al., 2010). Employment measure is for the crop sector (NAICS=111). The vertical line represents the QSA effective year.

Figure C8: Event Study Analysis for Crop Low-Skill Labor Employment.





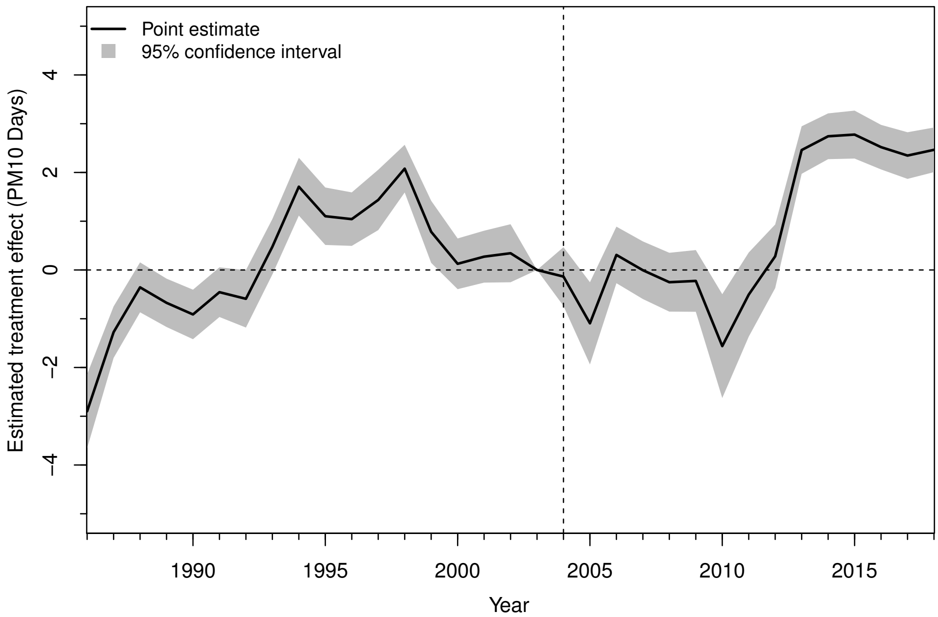
Notes: Event study analysis for Crop Low-Skill Labor Employment. Top panel shows the estimated treatment effect for Imperial County using all available control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Bottom panel shows the estimated treatment effect for Imperial County using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. Both models control for county and year fixed effects. The confidence bounds are obtained using robust standard errors. Employment measure is for the crop sector (NAICS=111). The vertical line represents the QSA effective year.

Table C7: Difference-in-Differences Analysis for PM10 Days (Fixed Effects Poisson Regression).

	(1)	(2)
1(Imperial)×1(Post-intervention)	0.6971***	0.9463
	(0.0000)	(2.3664)
Days with AQI	0.0003	0.0012
	(0.0013)	(0.0060)
Median AQI	-0.0437***	-0.0502*
	(0.0135)	(0.0243)
Farm Proprietors' Income	0.0014***	0.0021
	(0.0004)	(0.0073)
Farm Proprietors' Employment	0.7180***	0.6606
	(0.0000)	(0.9997)
Wage and Salary Employment	0.0011	0.0174
	(0.0018)	(0.5060)
Wage and Salary	0.0000	0.0004
	(0.0000)	(0.0057)
Proprietors' Employment	0.0121**	-0.0515
	(0.0053)	(0.2799)
Proprietors' Income	-0.0002**	-0.0021
	(0.0001)	(0.0037)
Observations	1,876	264
Log-likelihood	-10,807.38	-3,095.025

Notes: Model 1 uses all available (53) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Figure C9: Event Study Analysis for PM10 Days (Fixed Effects Poisson Regression).



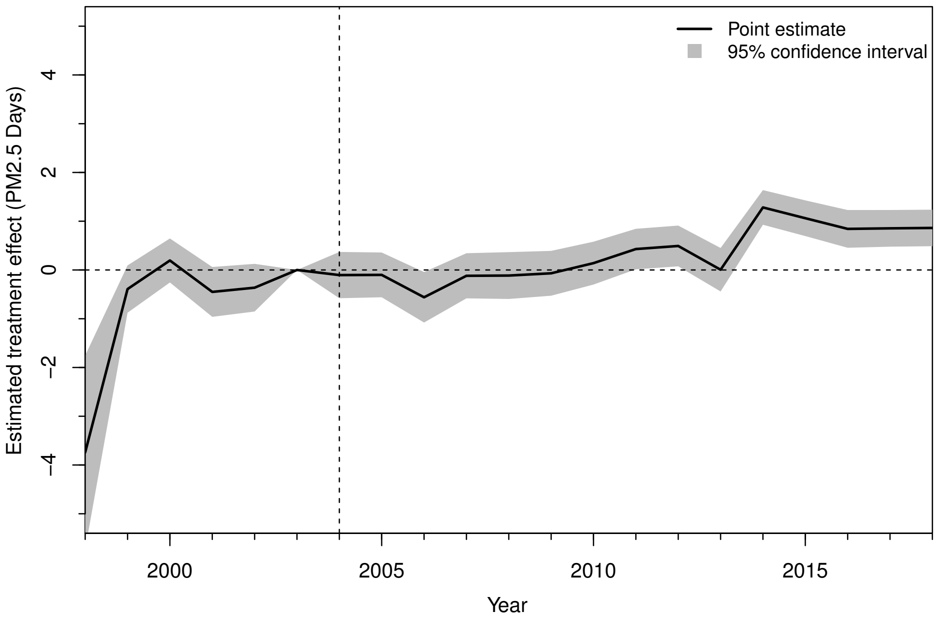
Notes: Event study analysis for PM10 Days using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. The model controls for county and year fixed effects. The vertical line represents the QSA effective year.

Table C8: Difference-in-Differences Analysis for PM2.5 Days (Fixed Effects Poisson Regression).

	(1)	(2)
1(Imperial)×1(Post-intervention)	1.0241***	0.9757***
	(0.0000)	(0.0002)
Days with AQI	0.0017***	-0.0764***
	(0.0006)	(0.0006)
Median AQI	0.0438***	0.0183*
	(0.0065)	(0.0101)
Farm Proprietors' Income	0.0004*	-0.0003
	(0.0003)	(0.0012)
Farm Proprietors' Employment	-0.9694***	-0.0570***
	(0.0000)	(0.0020)
Wage and Salary Employment	-0.0020***	-0.0199
	(0.0006)	(0.0290)
Wage and Salary	0.0000***	0.0003
	(0.0000)	(0.0005)
Proprietors' Employment	0.0009	-0.0067
	(0.0008)	(0.0481)
Proprietors' Income	0.0000	0.0004
	(0.0000)	(0.0005)
Observations	1,069	105
Log-likelihood	-16,410.72	-900.2285

Notes: Model 1 uses all available (53) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Figure C10: Event Study Analysis for PM2.5 Days (Fixed Effects Poisson Regression).



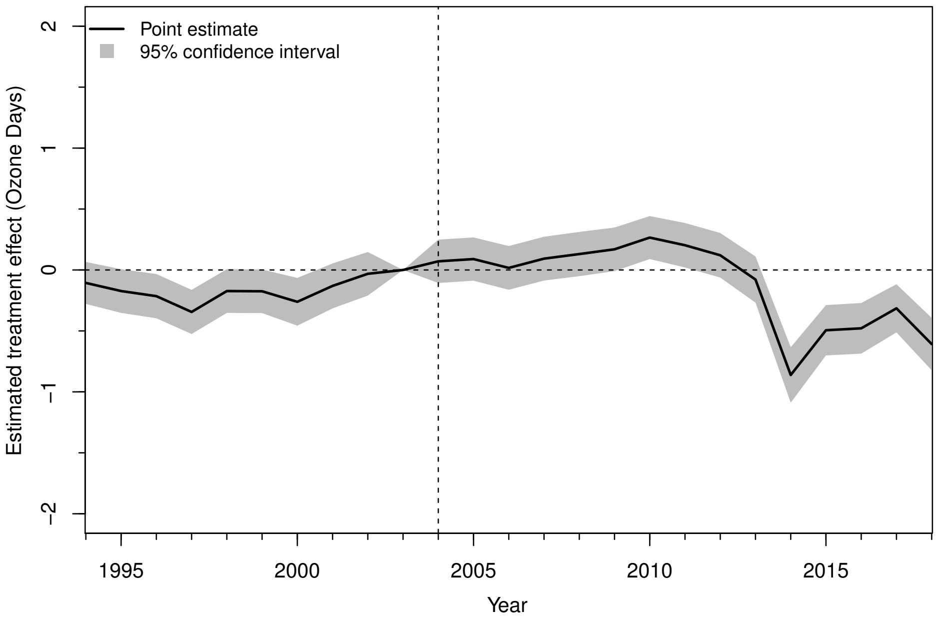
Notes: Event study analysis for PM2.5 Days using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. The model controls for county and year fixed effects. The vertical line represents the QSA effective year.

Table C9: Difference-in-Differences Analysis for Ozone Days (Fixed Effects Poisson Regression).

	(1)	(2)
1(Imperial)×1(Post-intervention)	-0.1497***	0.1023***
	(0.0000)	(0.0047)
Days with AQI	0.0083***	0.0150
	(0.0019)	(0.0602)
Median AQI	-0.0020	0.0183*
	(0.0034)	(0.0103)
Farm Proprietors' Income	-0.0002**	0.0003
	(0.0001)	(0.0023)
Farm Proprietors' Employment	0.0717***	0.3530***
	(0.0000)	(0.0006)
Wage and Salary Employment	0.0002	-0.0068
	(0.0002)	(0.1151)
Wage and Salary	0.0000	0.0000
	(0.0000)	(0.0016)
Proprietors' Employment	-0.0004	0.0383
	(0.0004)	(0.2193)
Proprietors' Income	0.0000*	-0.0002
	(0.0000)	(0.0010)
Observations	1,276	150
Log-likelihood	-12,753.68	-3,048.978

Notes: Model 1 uses all available (53) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Figure C11: Event Study Analysis for Ozone Days (Fixed Effects Poisson Regression).



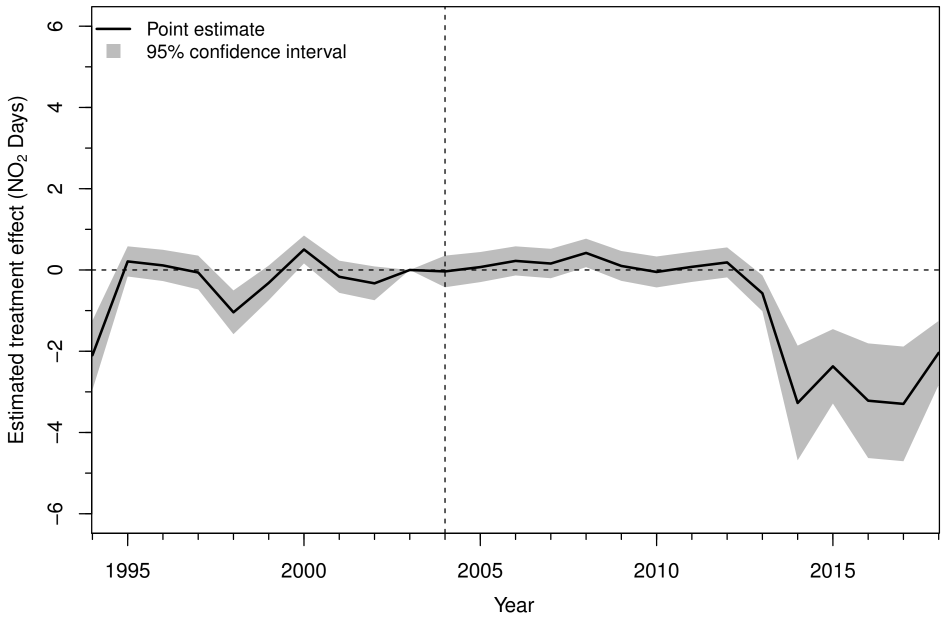
Notes: Event study analysis for Ozone Days using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. The model controls for county and year fixed effects. The vertical line represents the QSA effective year.

Table C10: Difference-in-Differences Analysis for NO_2 Days (Fixed Effects Poisson Regression).

	(1)	(2)
1(Imperial)×1(Post-intervention)	-0.1027***	-0.1221
	(0.0000)	(0.1345)
Days with AQI	0.0057***	-0.0782
	(0.0021)	(8.6883)
Median AQI	-0.0548***	-0.0580
	(0.0117)	(0.1632)
Farm Proprietors' Income	-0.0024***	-0.0011
	(0.0006)	(0.0032)
Farm Proprietors' Employment	0.9390***	1.6944***
	(0.0001)	(0.0648)
Wage and Salary Employment	0.0007	0.0116
	(0.0013)	(0.1369)
Wage and Salary	0.0000***	0.0000
	(0.0000)	(0.0034)
Proprietors' Employment	-0.0001	-0.0417
	(0.0043)	(0.4127)
Proprietors' Income	0.0000	-0.0002
	(0.0000)	(0.0016)
Observations	1,276	100
Log-likelihood	-8,639.586	-612.4266

Notes: Model 1 uses all available (53) control counties, where omitted counties include those with missing observations and/or significant water transfer (due to other policy/agreements) during the study period. Model 2 uses only control counties that receive nonzero weight in the synthetic control analysis (see table D1). Both models control for county and year fixed effects. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Figure C12: Event Study Analysis for NO_2 Days (Fixed Effects Poisson Regression).



Notes: Event study analysis for NO_2 Days using only control counties that receive nonzero weight in the synthetic control analysis (see table D1). See appendix B for the list of control variables included in the analysis. The model controls for county and year fixed effects. The vertical line represents the QSA effective year.

D Additional Estimation Output for Synthetic Control Analysis

Table D1: Donor County Weights for Each Outcome Variable Analysis.

Donor	Weight	Donor	Weight
Harvested Acres		*PM10 Days*	
Kern	0.346	Mendocino	0.314
Siskiyou	0.224	Glenn	0.259
Tulare	0.197	Butte	0.234
Monterey	0.142	Colusa	0.116
Kings	0.091	Mono	0.040
		Kern	0.028
Ag High Skill Labor Employment		Kings	0.007
Kings	0.516		
Merced	0.270	*PM2.5 Days*	
Colusa	0.215	Inyo	0.662
		Fresno	0.220
Ag Low Skill Labor Employment		Monterey	0.057
Kings	0.530	San Bernardino	0.057
Merced	0.324		
Fresno	0.146	*Ozone Days*	
		Inyo	0.542
Crop High Skill Labor Employment		Kern	0.156
Lassen	0.542	Monterey	0.116
Merced	0.222	Del Norte	0.114
Kings	0.124	Mono	0.072
Monterey	0.112		
		NO_2 Days	
Crop Low Skill Labor Employment		Inyo	0.640
Colusa	0.705	San Bernardino	0.301
Monterey	0.191	Marin	0.058
Merced	0.078		
Tulare	0.026		

Notes: County weights are obtained by performing synthetic control analysis separately for each outcome variable of interest. Reported counties are those that receive nonzero weights in the analysis.

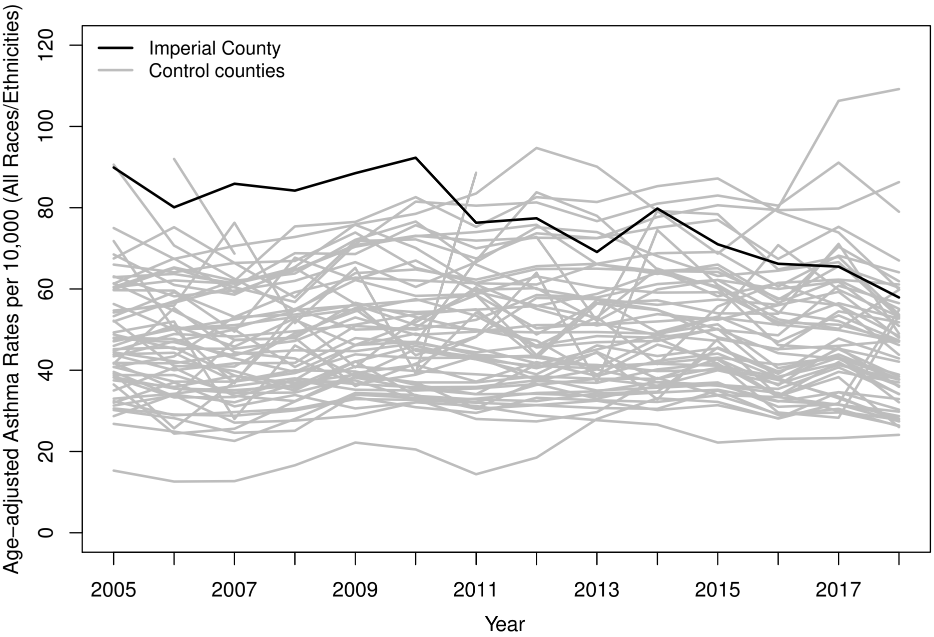
Table D2: RMSPE tests.

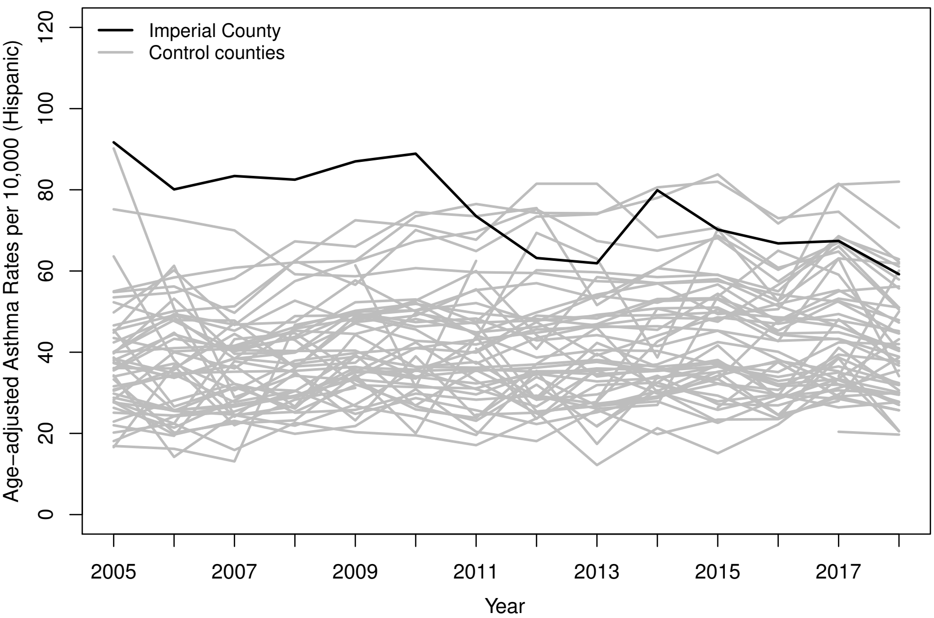
Outcome Variable	Post/Pre RMSPE Ratio	Max / Min	Treatment Unit Rank / # of All Valid Units
Harvested Acres	3.79	4.01 / 0.48	3 / 22
Ag High Skill Labor Employment	4.57	19.46 / 1.02	7 /23
Ag Low Skill Labor Employment	2.73	10.95 / 0.42	10 / 28
Crop High Skill Labor Employment	2.92	40.80 / 1.23	24 / 27
Crop Low Skill Labor Employment	1.56	11.28 / 0.52	18 / 26
PM10 Days	5.06	10.91 / 0.64	8 / 33
PM2.5 Days	6.49	1,786.54 / 2.69	25 / 32
Ozone Days	2.37	13.44 / 0.61	26 / 39
NO_2 Days	1.58	64,478,918.91 / 0.17	13 / 46

Notes: Post/Pre RMSPE ratio indicates the ratio of the post-intervention RMSPE to the pre-intervention RMSPE for the treatment unit. Max/Min indicates the maximum/minimum ratio of the post-intervention RMSPE to the pre-intervention RMSPE from among treatment and control units. Treatment unit rank is the rank of the ratio of the post-intervention RMSPE to the pre-intervention RMSPE for the treatment unit when all the ratios (both for treatment and control units) are ordered in descending order. To refine inferences from falsification tests, we consider control counties with pre-intervention RMSPEs that are less than or equal to twice that of a treatment unit (Abadie et al., 2010).

E Asthma Trends

Figure E1: Age-adjusted asthma rates (per 10,000) in Imperial County and control counties between 2005-2018.





Notes: Top panel shows age-adjusted asthma rates for all races/ethnicities. Bottom panel shows age-adjusted asthma rates for Hispanic/Latino population.