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ONLINE APPENDICES

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Appendix 1. Proofs

The social loss from the liability rule without monitoring is $L_L = \delta\Lambda$. The social loss from the property rule without monitoring is $L_P = [1 - (1 - \beta)^N]\Pi + (1 - \beta)^N \delta(\Lambda - \lambda_P)$. The social loss from regulation with monitoring is $L_R = [1 - (1 - \beta)^N](\Pi - \pi_R) + \delta(\Lambda - \lambda_R) + m$. The social loss from regulation without monitoring is $\text{Ł}_R = \delta\Lambda + [1 - (1 - \beta)^N](1 - \delta)\Pi$.

A1.1. Proof of Lemma 1

If $\beta = \delta = 0$ then $L_L = L_P = \text{Ł}_R = 0 \leq L_R = m$.

A1.2. Proof of Lemma 2

If $\beta > \delta = 0$ then $L_L = 0$ while $L_P = \text{Ł}_R = [1 - (1 - \beta)^N]\Pi > 0$ and $L_R = [1 - (1 - \beta)^N](\Pi - \pi_R) + m > 0$.

A1.3. Proof of Proposition 1

$L_L \leq L_P$ if and only if $\delta \leq \tilde{\delta} \equiv (\Pi/\Lambda)[1 - (1 - \beta)^N]/[1 - (1 - \beta)^N (1 - \lambda_P/\Lambda)]$.

A1.4. Proof of Proposition 2

Regulation without monitoring is dominated because $\text{Ł}_R \geq L_L$, with strict inequality unless $\beta = 0$ or $\delta = 1$. Regulation with monitoring has $\partial L_R/\partial\delta \leq \partial L_L/\partial\delta$. It also has $\partial L_R/\partial\delta \geq \partial L_P/\partial\delta$ if and only if $(1 - \beta)^N \leq (\Lambda - \lambda_R)/(\Lambda - \lambda_P)$. Suppose this condition is satisfied.

Then $L_R \geq \min\{L_L, L_P\}$ for all $\delta \geq 0$ if and only if $L_R \geq L_L = L_P$ for $\delta = \tilde{\delta}$, namely if and only if: $m \geq \bar{m} \equiv \Pi[1 - (1 - \beta)^N][\lambda_R/\Lambda + \pi_R/\Pi - 1 - (1 - \beta)^N (1 - \lambda_P/\Lambda)(1 - \pi_R/\Pi)]/[1 - (1 - \beta)^N (1 - \lambda_P/\Lambda)]$. The ranking of L_L and L_P is as in Proposition 1.

If instead $m < \bar{m}$, then $L_R < \min\{L_L, L_P\}$ if and only if $\{[1-(1-\beta)^N](\Pi-\pi_R)+m\}/\lambda_R \equiv \tilde{\delta}_1 < \delta < \tilde{\delta}_2 \equiv \{[1-(1-\beta)^N]\pi_R - m\}/[\Lambda-\lambda_R-(1-\beta)^N(\Lambda-\lambda_P)]$. Otherwise, $L_L < L_R < L_P$ for $\delta < \tilde{\delta}_1$, while $L_L > L_R > L_P$ for $\delta > \tilde{\delta}_2$.

A1.5. Generalization of Proposition 2

Suppose that $(1-\beta)^N > (\Lambda-\lambda_R)/(\Lambda-\lambda_P)$ and thus $\partial L_R/\partial\delta < \partial L_P/\partial\delta \leq \partial L_L/\partial\delta$. If $m \geq \bar{m}$, then $L_L < L_P < L_R$ for $\delta < \tilde{\delta}$, while $L_P < \min\{L_L, L_R\}$ for $\tilde{\delta} < \delta < \tilde{\delta}_2$, and $L_R < L_P < L_L$ for $\delta > \tilde{\delta}_2$. If instead $m < \bar{m}$, then $L_P \geq \min\{L_L, L_R\}$ for all $\delta \geq 0$. $L_L < L_R$ if and only if $\delta < \tilde{\delta}_1$.

A1.6. Proof of Proposition 3

The expected cost of pollution to the owners is:

$$C_L = \int_0^{b\rho_b/\rho_c} c\,dF(c) + (1-\rho_c+\delta\rho_c)\int_{b\rho_b/\rho_c}^{b(1-\rho_b)/(1-\rho_c)} c\,dF(c) + \delta\int_{b(1-\rho_b)/(1-\rho_c)}^{\infty} c\,dF(c)$$

under the liability rule, or:

$$C_R = \left[1-\rho_c+(1-\beta)^N\rho_c\right]\int_0^{b\rho_b/\rho_c} c\,dF(c) + (1-\rho_c)\int_{b\rho_b/\rho_c}^{b(1-\rho_b)/(1-\rho_c)} c\,dF(c) + \delta(1-\rho_c)\int_{b(1-\rho_b)/(1-\rho_c)}^{\infty} c\,dF(c)$$

under regulation, such that:

$$\begin{aligned} C_R\,\partial C_L/\partial\delta - C_L\,\partial C_R/\partial\delta = {} & \rho_c\left\{\left[1-\rho_c+(1-\beta)^N\rho_c\right]\int_0^{b\rho_b/\rho_c} c\,dF(c) + (1-\rho_c)\int_{b\rho_b/\rho_c}^{b(1-\rho_b)/(1-\rho_c)} c\,dF(c)\right\} \\ & \times\int_{b\rho_b/\rho_c}^{b(1-\rho_b)/(1-\rho_c)} c\,dF(c) + \rho_c\left[(1-\beta)^N\int_0^{b\rho_b/\rho_c} c\,dF(c) + (1-\rho_c)\int_{b\rho_b/\rho_c}^{b(1-\rho_b)/(1-\rho_c)} c\,dF(c)\right]\int_{b(1-\rho_b)/(1-\rho_c)}^{\infty} c\,dF(c) \geq 0 \end{aligned}$$

and therefore $\partial[(C_L-C_R)/C_L]/\partial\delta = (C_R\partial C_L/\partial\delta - C_L\,\partial C_R/\partial\delta)/(C_L)^2 \geq 0$.

A1.7. Generalization of Proposition 3

The expected benefit of activity to the polluter is:

$$B_L = b\{F(b\rho_b/\rho_c) + (1-\rho_b+\delta\rho_b)[F(b(1-\rho_b)/(1-\rho_c)) - F(b\rho_b/\rho_c)] + \delta[1-F(b(1-\rho_b)/(1-\rho_c))]\}$$

under the liability rule, or:

$$B_R = b\{[1-\rho_b+(1-\beta)^N\rho_b]F(b\rho_b/\rho_c) + (1-\rho_b)[F(b(1-\rho_b)/(1-\rho_c)) - F(b\rho_b/\rho_c)] + \delta(1-\rho_b)$$

$[1 - F(b(1 - \rho_b)/(1 - \rho_c))]\}$

under regulation, such that:

$$B_R\, \partial B_L/\partial\delta - B_L\, \partial B_R/\partial\delta = b^2\rho_b\{[1 - \rho_b + (1 - \beta)^N\rho_b]F(b\rho_b/\rho_c) + (1 - \rho_b)[F(b(1 - \rho_b)/(1 - \rho_c)) - F(b\rho_b/\rho_c)]\} \times [F(b(1 - \rho_b)/(1 - \rho_c)) - F(b\rho_b/\rho_c)] + b^2\rho_b\{(1 - \beta)^N F_1 + (1 - \rho_b)[F(b(1 - \rho_b)/(1 - \rho_c)) - F(b\rho_b/\rho_c)]\}[1 - F(b(1 - \rho_b)/(1 - \rho_c))] \geq 0$$

and therefore $\partial[(B_L - B_R)/B_L]/\partial\delta = (B_R\partial B_L/\partial\delta - B_L\, \partial B_R/\partial\delta)/(B_L)^2 \geq 0$.

A1.8. Proof of Corollary 1

The total expected cost of pollution to the owners is C_L when all polluters are subject to the liability rule. When a share $1 - \alpha$ of polluters switch to being subject to regulation, the total expected cost of pollution to the owners becomes $\alpha\, C_L + (1 - \alpha)\, C_R$. The decline in total harm from pollution, as a fraction of initial harm, equals $(1 - \alpha)(C_L - C_R)/C_L$ such that $\partial[(1 - \alpha)(C_L - C_R)/C_L]/\partial\alpha = -(C_L - C_R)/C_L \leq 0$, given that:

$$C_L - C_R = \rho_c\left\{\left[1 - (1 - \beta)^N\right]\int_0^{b\rho_b/\rho_c} c\,dF(c) + \delta\int_{b\rho_b/\rho_c}^{\infty} c\,dF(c)\right\} \geq 0.$$

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Appendix 2. Theoretical Extensions

A2.1. Inadvertent Harm

In our baseline model we have assumed for simplicity that P's action causes a certain harm c to the owners, known ex ante both to them and to the polluter. In reality, however, the connection between pollution and the ensuing harm to affected owners is often less direct and less certain. In many cases, a polluter's actions do not directly cause harm to the owners, but merely expose the owners to a risk of harm. P's deliberate choice is not whether to harm each O_i, but rather whether to adopt precautions that would prevent or reduce the risk of the owners being harmed inadvertently as a by-product of his risky activities. Our model applies identically to this setting, with a slightly more complicated exposition.

Formally, if P acts without abatement he exposes each O_i to the risk of suffering harm c_i, which is a random variable with expectation $\mathrm{E}(c_i) = c/N$ conditional on the characteristics of a particular case. Abatement shifts down the distribution of harm c_i, reducing its expectation to $(1 - \rho_c)c/N$. The conditional expectation c of the total social harm that pollution risks causing in a particular case is itself another random variable with cumulative distribution $F(c)$. Its realization for a particular case is privately known to the polluter P and every owner O_i. The court eventually observes the true realization of harm c_i, to each O_i, but its amount is always disputable.

The timeline of the model is the following.

Stage 0. The legislator sets the legal rule protecting the owners and chooses whether to mandate a monitoring system to indisputably measure abatement.

Stage 1. The conditional expectation of social costs c and P's ability to subvert the court are realized and privately observed by P and each O_i. The parties have a chance to bargain and write a contract, but each O_i is unable to join the bargaining table with probability β. The ability to bargain is drawn independently across owners. Bargaining is otherwise efficient among the parties able to join the negotiation.

Stage 2. P chooses whether to act and, if he acts, whether to abate.

Stage 3. If P acts, harm c_i to each O_i is realized. The court assesses facts and penalties are enforced.

Under the property rule, each O_i is entitled to enjoin the polluter's activity. Hence, if P acts he suffers a large penalty $f > b$, which does not depend on any fact-intensive verification of whether any owners were harmed by his risky actions, how many, or how much. Under the liability rule, if P acts he must pay damages to each O_i equal to assessed harm, hence equal to the true realization of harm c_i if P cannot subvert the court, or to zero if he can. Under regulation, each O_i is entitled to enjoin P from acting without abatement, but only to compensation for any harm suffered if P acts with abatement.

This more complicated version of the model is identical to our simpler baseline. Whether the consequences of P's action are certain as in the baseline, or residually uncertain as in this extension, damages fairly assessed when the court is not subverted perfectly align his incentives with aggregate efficiency; damages assessed by a subverted court can never provide any incentives; while large fixed penalties always provide complete deterrence.

A2.2. Disputable Activity

In our baseline model we have assumed that the polluter's action is indisputable, so monitoring is not required to enforce the property rule. However, in some cases monitoring could be needed to make action itself indisputable. For instance, with multiple factories, the indisputable presence of effluents in a body of water does not suffice to prove that any one factory polluted the water. Then a subverted court could refuse to apply the sanction f to a powerful polluter, ruling that he is not responsible for pollution and the ensuing harm to the owners. Enforcing the property rule would then require adoption of the monitoring technology.[1]

[1] In addition, legislation might be required to clarify that every owner is always entitled to be spared from the polluter's activity, and not only when the court deems his activity to be harmful. Otherwise a subverted court might grant that the polluter is indisputably responsible for his polluting activity, yet still refuse to enforce the property rule by denying that his polluting activity is harmful to the owners. The need for such legislative intervention would be immaterial in our model, since we assume it is costless to choose the rule that protects the owners.

Needless to say, this alternative scenario would imply that the property rule is less attractive, since the social loss it induces must increase by the cost of necessary monitoring (m). However, our baseline results would be qualitatively unchanged. To state the analogues of our main results, we switch terminology so that "the property rule" now denotes the property rule with monitoring.

Proposition A1. Suppose that the polluter's activity is disputable unless the monitoring technology is adopted. Then the liability rule yields greater social surplus than the property rule if and only if court subversion is unlikely enough: $\delta < \tilde{\delta}' \equiv \{[1 - (1 - \beta)^N]\Pi + m\}/[\Lambda - (1 - \beta)^N(\Lambda - \lambda_P)]$.

Proof. The social loss from the liability rule (without monitoring) is $L_L = \delta\Lambda$. The social loss from the property rule (with monitoring) is $L'_P = [1 - (1 - \beta)^N]\Pi + (1 - \beta)^N\delta(\Lambda - \lambda_P) + m$. Thus, $L_L < L'_P$ if and only if $\delta < \tilde{\delta}'$. ■

Intuitively, the comparison between the liability rule and the property rule is the same as in our baseline scenario if and only if the monitoring technology is costless ($\lim_{m\to 0} \tilde{\delta}' = \tilde{\delta}$). As the monitoring cost increases, the property rule becomes less and less appealing ($\partial\tilde{\delta}'/\partial m > 0$). Thus, the liability rule dominates the property rule for all $\delta \leq 1$ if monitoring is costly enough that $m \geq [1 - (1 - \beta)^N](\Lambda - \Pi) + (1 - \beta)^N\lambda_P$.

Proposition A2. Suppose that the polluter's activity is disputable unless the monitoring technology is adopted, and that $(1 - \beta)^N \leq (\Lambda - \lambda_R)/(\Lambda - \lambda_P)$.

If adopting the monitoring technology is costly enough that $m > \bar{m}' \equiv \Pi[\lambda_R/\Lambda + \pi_R/\Pi - 1 + (1 - \beta)^N(1 - \lambda_P/\Lambda)(1 - \pi_R/\Pi)]\,[1 - (1 - \beta)^N]/[1 - \lambda_R/\Lambda - (1 - \beta)^N(1 - \lambda_P/\Lambda)]$, then the efficiency-maximizing rule is the liability rule if court subversion is rare enough that $\delta < \tilde{\delta}'$ and the property rule if court subversion is common enough that $\delta > \tilde{\delta}'$.

If adopting the monitoring technology is cheap enough that $m < \bar{m}'$, then the efficiency-maximizing rule is the liability rule if court subversion is rare enough that $\delta < \tilde{\delta}_1$, the property rule if court subversion is frequent enough that $\delta > \tilde{\delta}'_2 \equiv [1 - (1 - \beta)^N]\pi_R/[\Lambda - \lambda_R - (1 - \beta)^N(\Lambda - \lambda_P)]$, and regulation for intermediate levels of court subversion: $\tilde{\delta}_1 < \delta < \tilde{\delta}'_2$.

Proof. The social loss from the liability rule (without monitoring) is $L_L = \delta\Lambda$. The social loss from the property rule (with monitoring) is $L'_P = [1-(1-\beta)^N]\Pi + (1-\beta)^N \delta(\Lambda - \lambda_P) + m$. The social loss from regulation (with monitoring) is $L_R = [1-(1-\beta)^N](\Pi - \pi_R) + \delta(\Lambda - \lambda_R) + m$. Thus, $L_R \geq \min\{L_L, L'_P\}$ for all $\delta \geq 0$ if and only if $L_R \geq L_L = L_P$ for $\delta = \tilde{\delta}'$, namely if and only if: $m \geq \bar{m}'$. If instead $m < \bar{m}$, then $L_R < \min\{L_L, L'_P\}$ if and only if $\tilde{\delta}_1 < \delta < \tilde{\delta}'_2$. Otherwise, $L_L < L_R < L'_P$ for $\delta < \tilde{\delta}_1$, while $L_L > L_R > L'_P$ for $\delta > \tilde{\delta}'_2$. ■

Intuitively, when enforcing the property rule requires monitoring, the range of monitoring costs for which regulation is not a dominated option expands ($\bar{m}' > \bar{m}$), and so does the range of court subversion for which regulation is the most efficient option ($\tilde{\delta}'_2 = \lim_{m\to 0} \tilde{\delta}_2 > \tilde{\delta}_2$).

A2.3. Contract Enforcement

In our baseline model we have considered the case of torts. We turn here to the parallel case of contract enforcement, which can also be subverted by the strong. Just as the property rule is less vulnerable to subversion than the liability rule, a parallel remedy in contract enforcement is specific performance. With this remedy, instead of accepting a breach of contract and mandating the payment of damages in compensation for its effects, the court simply orders a party to complete its performance of the contract as originally stipulated, refraining from any breach or fully correcting it. To establish the parallel between contracts and property, we keep the same symbols and describe how the basic logic applies to contract enforcement, when breach of contract yields a deterministic benefit b to the breaching party but imposes a stochastic cost c on the counterparty—which we assume now to be a single individual ($N = 1$). The timeline follows.

Stage 0. The legislator sets the legal rule enforcing contracts.

Stage 1. O and P can sign a contract that, if performed, generates a baseline surplus $u > 0$ which can be split between them. If the parties do not sign a contract, payoffs are normalized to zero.

Stage 2. P's ability to subvert the court and the cost c that O would suffer from a breach is realized and privately observed by the parties. P chooses whether to breach.

Stage 3. If P breaches, the court assesses facts and penalties are enforced.

Whether P can subvert the court is known at the time of the breach, but not when the contract is originally signed. We do not allow renegotiation in stage 2, so P decides whether to breach unilaterally, in the shadow of the legal rules that governs enforcement of the original contract.

In stage 3, O can sue P for breach of contract. So long as specific performance can be enforced by courts, P can be forced to remedy the breach, which costs him $f > b$, since he must forego any benefits of breach and pay the costs to remedy the previous action. With contractual damages, P has to pay the cost of the damages to O, namely c if he cannot subvert the court, and 0 if he can.

If the court is not subverted, contractual damages lead to efficient breach, which raises the baseline surplus. Instead, specific performance always prevents efficient breach, because we assumed no recontracting in stage 2. The baseline surplus is then the total surplus generated by the contract. If the court cannot be subverted, contractual damages thus lead to the textbook first-best outcome and specific performance does not. If the court can be subverted, however, the flexibility of contractual damages enables a strong P to persuade the court that damages are negligible. As noted by Cooter (2008, p. 1128) "the final advantage of specific performance concerns corruption," because "damages allow judges to vary the award over a continuous range, which makes disguising bribes easier." In this case, breach does not lead to large penalties imposed on the strong.[2]

Contract is always breached in this case, and the surplus is reduced by $\mathrm{E}(c) - b$. For contracts, it is natural to assume that that $\mathrm{E}(c) > b$ (hence $\Lambda > \Pi$). If an action yields positive expected surplus ($b > \mathrm{E}(c)$),

[2] Dunworth and Rogers (1996) find that larger corporations typically outperform all other parties in contract disputes, including smaller businesses. Galanter (2001) similarly finds that corporations overwhelmingly defeat individuals in contract litigation.

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the contract would be written ex ante so that the action constitutes correct performance rather than breach. Because specific performance is not subverted, breach does not occur and surplus is not eroded.

The logic of this reinterpretation is identical to that of our core model. Contractual damages dominate specific performance when the probability of judicial subversion is low. Specific performance, like the property rule, dominates when the probability of court subversion is high.

In the famous case of *Peevyhouse v. Garland Coal & Mining Co.* (382 P.2d 109, Okla. 1962), the Oklahoma Supreme Court ruled that the coal miner did not have to honor its contractual promise to perform remedial work in order to restore the small farmers' property after strip mining. Instead, it merely owed damages for nonperformance. The trial jury, taking into account both the diminished value of the property and the cost of remediation, awarded damages of $5,000. On appeal, the Supreme Court reduced damages to $300, ruling the cost of remediation immaterial. Many legal scholars celebrate this outcome as the triumph of economic efficiency over bleeding-heart sentimentalism, since the coal company paid damages rather than the high cost of restoring the land (Posner 2009). Some, however, are concerned with the inequity of the outcome (Kennedy and Michelman 1980; Maute 1995).

In our framework, the matter is not just the inequity of the outcomes, but also their inefficiency. Enforceable Coasian contracts against the strong—if specific performance indeed enables them—improve efficiency because they encourage bargaining between a weak owner and a strong polluter.

Proposition A4. A contract enforced by specific performance is always signed. A contract enforced by compensatory damages is signed if and only if $\delta < (u + \Pi)/\Lambda$.

Proof. A contract enforced by specific performance is never breached, so it yields surplus $u > 0$. A contract enforced by compensatory damages is breached whenever breach is efficient or a powerful P can subvert the court. Thus, it yields expected surplus $u + \Pi - \delta\Lambda$, which is positive if and only if $\delta < (u + \Pi)/\Lambda$. ■

In equilibrium, when the remedy for breach is damages rather than specific performance, many contracts are avoided. The strong contract with the strong, the weak with the weak, and gains from trade between parties of different legal strengths are not realized. The trust necessary for parties to make Coasian bargains disappears when justice is subverted.

Proposition A4 has implications for the theory of the firm. If the weak and the strong have difficulty writing enforceable arm's length contracts, there is an added case for combining into a single firm. If a supplier has less legal power than its customer, a contract dispute may end up being resolved in favor of that powerful customer. With vertical integration of the two, this contracting problems can be avoided (Klein, Crawford, and Alchian 1978; Grossman and Hart 1986). This argument mirrors the classic claim that non-verifiable contracting requirements help explain the boundaries of the firm, but it emphasizes that institutional quality rather than verifiability determines the optimal firm size.

A good deal of recent empirical research considers closely related issues, both across countries with different legal institutions, and within countries. For example, Antras, Desai, and Foley (2009) show empirically that weak investor protection, which may capture the legal disadvantage of a foreign firm compared to a domestic trading partner, limits the activities of multinational firms as well as foreign direct investment. Boehm and Oberfield (2018) show directly for Indian manufacturing that firms in states with more congested courts, and therefore less effective contract enforcement, exhibit more vertical integration and less purchasing of inputs from outsiders. Less directly, our logic can help explain why newer, smaller firms are more likely to enter in markets where there an abundance of small-scale suppliers (Glaeser and Kerr 2009; Glaeser, Kerr and Ponzetto 2010). These existing smaller suppliers are less likely to enjoy a legal advantage than larger incumbents.

The absence of contracts between parties with asymmetric power may explain why damages have come to be preferred in many of the common-law legal debates on contract enforcement, while property

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rules are still preferred to secure rights of possession.[3] Whereas asymmetric contracts can be avoided in equilibrium (albeit with a loss of gains from trade), the same does not apply to torts. A poor farmer can decide not to contract with a powerful firm, but that firm can still damage his crops or even take his land.

While we have focused for simplicity on the classic case of deliberate breach, as in *Peevyhouse*, many real-world cases of contractual breach are inadvertent. The key decision by P is not whether to breach, but how much to invest in reducing the risk of a breach (Kornhauser 1983; Craswell 1988; Bebchuk and Png 1999). Our analysis also applies to such inadvertent breaches, so long as it remains possible to remedy them by specific performance. Our model is exactly unchanged if we consider the simple case of a binary choice of the level of care. The contract yields the baseline surplus u if P performs his obligation with a high degree of care.[4] Choosing a low degree of care instead yields a saving b in P's cost of performance, but it creates a risk of breach whose expected cost to O equals c.

When courts are not subverted, our model then replicates Bebchuk and Png's (1999) result that specific performance is dominated by expectation damages: it induces excessively high care by P, reducing total surplus from the first best $u + \Pi$ to u. However, Proposition A4 highlights that specific performance comes into its own when courts can be subverted: then damages induce insufficient care, so much so that they can destroy all surplus from the contract and lead O to refrain from signing it.[5]

[3] Schwartz (1979) prominently advocates specific performance because of under-compensation with damages.
[4] In this setting, a high degree of care may mean either fully avoiding inadvertent breach, or remedying it ex post whenever it proves unavoidable ex ante.
[5] Our model could also be extended to include investment in reliance by O. When courts are not subverted, specific performance induces over-reliance (Kornhauser 1983; Bebchuk and Png 1999). However, court subversion implies that damages induce under-reliance, which may reach the extreme of no reliance whatsoever---i.e., no contracting.

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Appendix 3. Data Discussion

In what follows and in the main text we classify agricultural counties using SIC codes from the 1972 County Business Patters Data. We consider agricultural any employment or establishment listed under the "01-09" codes in the 1972 CBP.

A3.1. Calculation of Z Scores

In our primary analysis we use all of the available pollution data collected by Keiser and Shapiro (2019) for each county in each year in our sample. For each pollutant in our sample we calculate an annual *Z* score for each pollutant × county as:

$$Z_{itp} = (L_{itp} - \bar{L}_p)/\sigma_p$$

where L_{itp} is the level of pollutant p in county i in year t. $\bar{L}_p$ denotes the average level of that pollutant across all county × year observations in our sample, while σ_p is the standard deviation in the level of that pollutant across the sample. In addition to our primary outcome we calculate a *Z* score that sums across the set of pollutants that the CWA defines as "conventional" and is particularly focused on: i.e., Biochemical Oxygen Demand, Fecal Coliforms, Total Suspended Solids, and pH.

A3.2. Comparison of Z Scores to Raw Pollution Levels

In Figures A1 and A2 we compare the ranking of how polluted a given county × year pair is, using our preferred measure of the summed *Z* score with rankings based on the *Z* score for individual pollutants (A1) and the raw levels of individual pollutants (A2). Across all six pollutants we consider (Dissolved Oxygen Deficit, Total Suspended Solids, Total Dissolved Solids, pH, Fecal Coliforms and Biochemical Oxygen Demand) our measure compares favorably to the individual pollutant *Z* scores and the raw levels.

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A3.3. Pre-Trends

In Table A3 we present two measures of pre-trends in water pollution in treated counties. In the first column we show the coefficient on t from the regression: $y_{ijt} = \beta_0 + \beta_1 t + \gamma_i$, where y_{ijt} is the Z score of pollution in county i in state j and year t and γ_i is a county fixed effect. We include all the counties and years prior to 1972 in this estimate. In Column 2 we present the estimates of ω_τ from the regression: $y_{ijt} = \sum_{\tau \in T} \omega_\tau Y_\tau + \gamma_i$, where Y_τ is an indicator for year τ in the set $T = \{1962, 1963, 1964, 1965, 1966, 1967, 1968, 1969, 1970, 1971, 1972\}$, γ_i is a county fixed effect and 1972 is the base year.

Neither measure suggests strong pre-trends prior to 1972. In the continuous measure the estimated linear time trend is both close to zero and insignificant. In the dummy approach only the coefficient on 1964 is significant and across the set of years the coefficients oscillate around zero.

A3.4. Event Studies

The trends in Figures 2 – 4 in the main text suggest that the CWA led to substantial reductions in pollution in treated counties. Moreover, these reductions were larger in non-agricultural and in corrupt counties, consistent with the predictions of our theory. In order to show more clearly that the effects we find are not the result of preexisting trends, we estimate several "event study" specifications of the form:

$$y_{ijt} = E_{it} \times \sum_{\tau \in T} \omega_\tau Y_\tau + \gamma_i, \qquad \text{(A1)}$$

where E_{it} is an indicator for whether the CWA became enforceable in county i after 1972. Y_τ is an indicator for year τ in the set $T = \{1962, 1964, 1966, 1968, 1970, 1972, 1974, 1976, 1978, 1980\}$ and γ_i is a county fixed effect. The coefficients $\omega_{\tau \in T}$ summarize trends in the average pollution level in treated and non-treated counties relative to a baseline year chosen to be 1971, the year prior to the passage of the CWA.

Figure A5 presents the coefficients for $\omega_{\tau \in T}$ from Equation A1. The estimated coefficients indicate that while pollution levels were stable relative to their 1971 baseline in both treated and non-treated counties prior to 1972, in treated counties pollution levels begin to trend downward substantially after the passage of the CWA in 1972. Non-treated counties, by contrast, show no significant change in the levels after 1972.

To examine the impact of treatment in agricultural and non-agricultural counties we modify Equation A1 to be:

$$y_{ijt} = A_i \times \sum_{\tau \in T} \omega_\tau Y_\tau + \gamma_i, \tag{A2}$$

where A_i is an indicator for whether county i is agricultural. We estimate Equation A2 on the sample of treated counties. Figure A6 presents the results. Both agricultural and non-agricultural counties have consistent levels of pollution leading up to 1971. After passage of the CWA pollution falls faster and further in the non-agricultural counties. This is consistent with Empirical Prediction 4: the initial enforcement of the CWA, via NPDES permits, was more effective in reducing pollution in non-agricultural counties because a greater share of the pollution in these counties came from point source pollution and was therefore covered by the new regulation while the majority of pollution in agricultural counties remained subject to the less effective liability regime.

We test the hypothesis that pollution falls more in corrupt counties after passage of the CWA by modifying Equation A1 to be:

$$y_{ijt} = C_j \times \sum_{\tau \in T} \omega_\tau Y_\tau + \gamma_i + \delta_j t, \tag{A3}$$

where C_j is an indicator for whether county i is in a corrupt state j and δ_j is a state-specific linear time trend. Figure A7 reports the coefficient estimates. Prior to treatment, pollution levels in non-corrupt counties are stable relative to the 1971 baseline. In corrupt counties the levels are noisily estimated but have no significant trend through 1970. Beginning in 1972 pollution begins to decline in both corrupt and non-corrupt counties. With the caveat that the estimates remain noisy, they appear to fall faster and further for corrupt counties relative to non-corrupt counties.

A3.5. Robustness Checks

We estimate several variations of the main specification in the paper. Starting with Table A4, we estimate the main specification retaining data from 1973 and 1974. Our point estimates are smaller and less precise, as would be expected given mis-assignment in treatment, but consistent with our main results. Table A5 reports estimates when we include counties that contain facilities that receive permits between 1975 and 1985 and classify them as untreated. Again, as expected our estimates are smaller and less precise, but consistent with the main results. When we allow treatment to vary flexibly across the sample—in Table A6, a county is considered treated in the year its first facility receives a permit regardless of whether that year is 1973 or 1974—the overall treatment effect remains significant in specifications leaving out year fixed effects. Adding year fixed effects reduces the size of the estimate but it remains negative. Our estimates of the differential treatment effect across agricultural and non-agricultural counties also shrink but remains significant. Focusing only on the subset of pollutants defined as "conventional," our estimates become much less precise (Table A7). We still find the expected negative and significant ($p < 5\%$) effects of treatment in our preferred specification with year fixed effects. The differential impact across agricultural and non-agricultural counties shows the expected pattern but is significant only when we define counties as agricultural based on establishments rather than employment (Column 5). When we examine the effect of treatment on the full set of pollutants using establishments to determine agricultural status we find very similar results to those reported in the paper using employment (Table A8).

In Table A9 we mimic Table 3 from the main text but measure corruption using the continuous measure of convictions per capita rather than a binary determination of whether a location is corrupt based on the level of convictions relative to the mean. Using both levels and logs we find that additional convictions strengthen (make more negative) the impact of treatment but only the specification in levels is significant. In Table A10 we combine Table 3 from the text and Table A9 but measure corruption using the number of newspaper subscriptions per capita in 1972 from Gentzkow, Shapiro and Sinkinson (2011). This measure takes the number of newspaper subscriptions in a state in 1972, calculates a per capita rate using

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population data from the BLS and assigns corruption status to a state for the whole sample period based on the number of subscriptions per capita in 1972.

The pattern of results using newspaper subscriptions is similar to that when we use convictions but more imprecise. Given that newspaper subscriptions are a noisy measure of government transparency we should expect our estimates to be noisier. In general Table A10 suggests that corrupt counties do have more pollution prior to 1972 (Column 1).[6] The positive, but insignificant coefficients in Columns 2 and 3 suggest that places with more newspapers reduce pollution less after treatment than those with fewer papers. Conversely, the negative coefficient in Column 4 suggests that when we assign a binary indicator based on whether the number of newspaper subscriptions is above or below the mean the places that are corrupt reduce pollution more after treatment than those that are considered not corrupt.

In Table A11 we consider alternative specifications that test the robustness of our definition of agricultural counties. In the first, reported in column 1, we replace our binary definition of agricultural and non-agricultural counties with a continuous measure using the share of employment in agriculture in the county in 1972. This is the same underlying measure that is used to create the binary indicator. Note that the binary variable in the main text indicates that a county is non-agricultural – somewhere the CWA should have been more effective – while a higher value of the continuous measure indicates that a county is more agricultural – somewhere the CWA should be less effective – and so the signs on the estimated coefficients should be opposite. Because a large share of counties in our sample have no agricultural employment in 1972 and these may be fundamentally different from those that do have agricultural employment, we include a dummy for counties that have no agricultural employment. Our results in this specification are noisy but consistent with the results reported in Table 2 in the main text. Our point estimate does have the opposite sign and suggests that increasing a county's share of employment in agricultural in 1972 by 1 percentage point (1.2 SD) results in average pollution levels after passage of the CWA that are 0.37 standard

[6] Note that for the continuous measures the expected signs on the coefficients should be opposite for newspapers than for convictions because more papers indicates less corruption while more convictions indicates more corruption.

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deviations higher relative to counties with less agricultural employment in 1972. This suggests that the CWA was less effective in areas with higher levels of agricultural employment.

As an alternative to the continuous measure in column 2 we divide counties that have a non-zero level of agricultural employment into terciles based on their level of agricultural employment. We then use indicators for each tercile, as well as the dummy for zero agricultural employment, to consider the effectiveness of the CWA in those counties with higher levels of agricultural employment relative to those counties with the lowest non-zero levels of agricultural employment. Consistent with the results in column 1 we find that counties in the third tercile, those with the highest levels of employment in agriculture, have substantially smaller reductions in pollution after the passage of the CWA.

Finally, we directly test the robustness of our threshold in the main specification by replicating Equation 2 that produced the results in Table 2, column 4 of the main text but varying the threshold level of agricultural employment at which a county is classified as agricultural. In the main text we consider any county whose share of agricultural employment exceeds the 75th percentile of the distribution across all counties as agricultural. Here we vary this threshold from the 65th percentile to the 85th percentile. The results reported in columns 3-7 suggest our conclusion that the impact of the CWA was larger in non-agricultural counties is broadly robust to the threshold used to define agricultural counties.

A3.6. Replication of Keiser and Shapiro (2019) Trends

In Table A12 we replicate Table 1 from Keiser and Shapiro (2019) showing the trends in Dissolved Oxygen Deficit, Biochemical Oxygen Demand, Fecal Coliforms and Total Suspended Solids. In Columns 1 and 2 we report Keiser and Shapiro's estimates with significance stars. In Columns 3 and 4 we report the results we get estimating their equations on pollutant specific Z scores in our county × year data. While the magnitudes differ due to our use of Z scores, the general patterns are the same. Overall, all four pollutants trend down over the sample period but, similar to their results, in three of the four cases the trend is steeper prior to 1972 than after. In the fourth case we find no evidence of a change in the trend.

A3.7. Placebo Tests

In Table A13 we replicate Table 2 from the main text but assume that the CWA becomes enforceable in 1971 and 1972 instead of 1973 and 1974 (in Table A14 we assume the CWA becomes enforceable in 1975 and 1976). If the effect we measure in Table 2 is truly due to the CWA becoming enforceable in 1973 and 1974, assigning treatment in earlier and later years (as we do in Table A14) should result in estimated effects that are smaller and less precise. The results in Table A13 and A14 fit this pattern and give us confidence that the effect we measure in the main text is due to the receipt of an NPDES permit and not to other contemporaneous trends. In Table A13 we find smaller coefficients across all specifications relative to those in Table 2 and all except Column 1 are statistically insignificant. In Table A14 we again find smaller coefficients relative to Table 2.

In Table A15 we compare the effect if we only look at counties treated in 1975 to that estimated in Table 2. Recalling that a county is treated in the year the first facility in that county receives an NPDES permit, more counties are treated in 1973 and 1974 than in 1975. Given the weaker treatment in 1975 we expect the estimated effects of treatment using only 1975 to be smaller as well. Table A15 confirms our expectations. The pattern of treatment effects is the same as we find using 1973 and 1974 but the effect sizes are much smaller and less significant.

In Figures A8 and A9 we randomize treatment and county agricultural status before estimating Equations 1 and 2 from the text 1,000 times. Doing so gives us an approximation of the likelihood of observing our coefficient values under the null of a zero true effect. In Figure A8 we show the distribution of the estimates of the impact of the CWA becoming enforceable (equivalent to Column 3 in Table 2) with our estimated effect indicated by the dashed red line. As can be seen from the figure, our observed effect is in the far tail of the distribution of effects when treatment is assigned randomly making it highly unlikely that our results would be observed if the true effect was zero. In Figure A9 we show the joint distribution of the estimated impact of the CWA becoming enforceable in agricultural (on the y axis) and non-agricultural counties (on the x axis). The figure is equivalent to Column 4 in Table 2. Our joint estimates, indicated by the red dot, are well outside the joint distribution of the estimates from randomization, again suggesting that the probability of observing our results under the null of no effect is extremely small.

A3.8. Predicting Treatment Timing

To understand better whether there is meaningful selection in which counties contain facilities that receive NPDES permits in 1973 and 1974 compared to later years, we try to predict the timing of when the CWA becomes enforceable in the counties in our sample. We use data on county economic conditions in 1972 and regress a set of county characteristics, including employment levels in different industries in 1972 and a state fixed effect, on a dummy for whether the county was treated from 1973 to 1974 or later. We present the results in Table A16. In general, it appears more polluted areas were treated sooner – this is expected as an area necessarily had to have a polluter in order to receive a permit – and areas with more mining activity are treated sooner. Our other measures of economic activity do not appear to significantly predict treatment timing.

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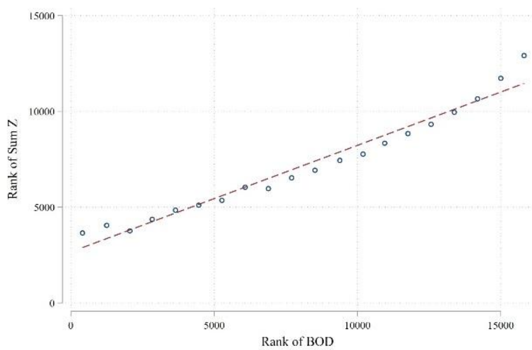
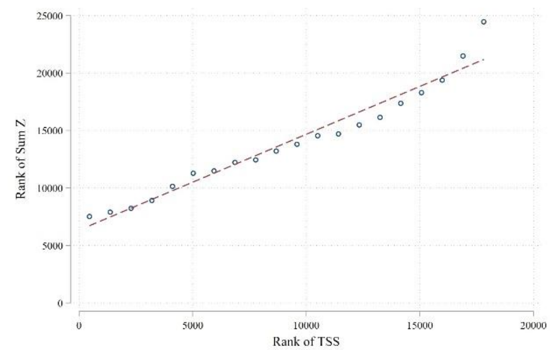
Figure A1: Comparison of Summed *Z* Score Ranks and Pollution Ranks

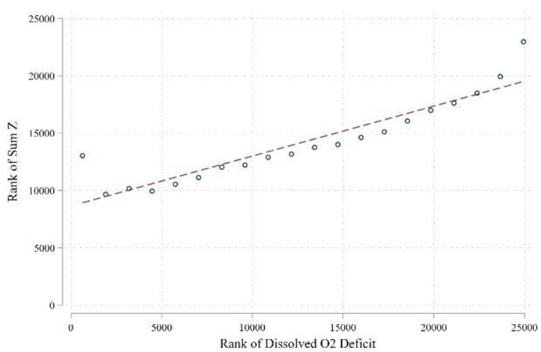
(a) Biochemical Oxygen Demand



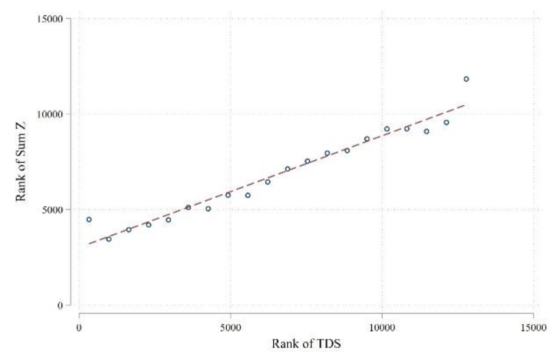
(b) Total Suspended Solids



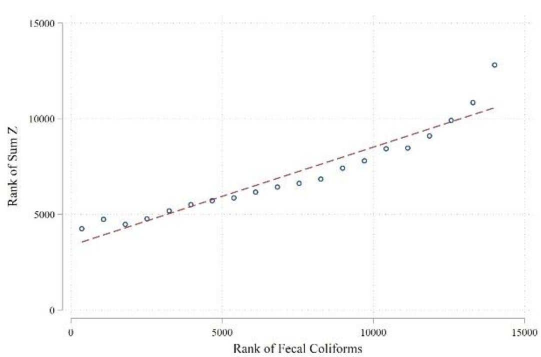
(c) Dissolved Oxygen Deficit



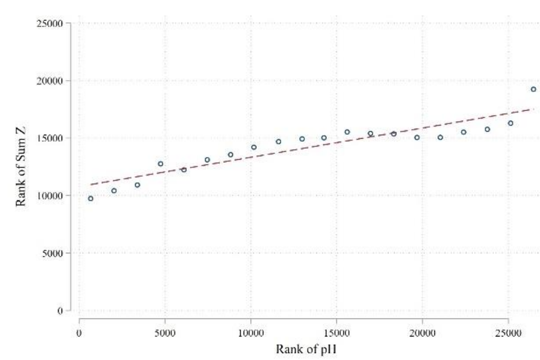
(d) Total Dissolved Solids



(e) Fecal Coliforms



(f) pH



Notes: We report here the bin scatter plots of the rank of county-year observations based on their summed *Z* score across all pollutants for which we have a reading in that county-year compared to the rank based on their level of six individual pollutants. *Z* scores are calculated for each pollutant in each year (negative *Z* scores indicate lower than average pollution). To calculate summed *Z* scores we sum the *Z* scores across all pollutants for which we have data in a county-year.

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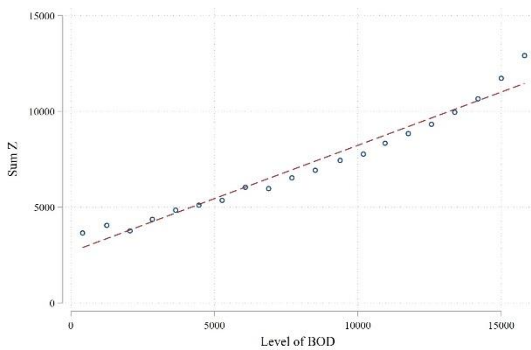
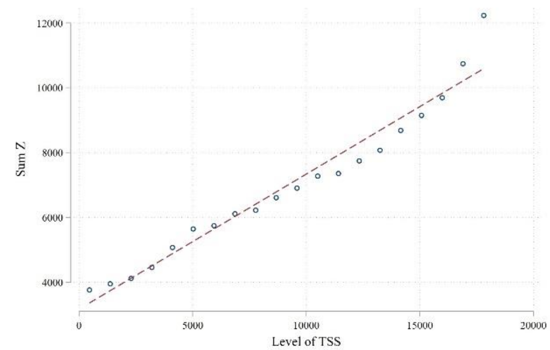
Figure A2: Comparison of Summed Z Score and Pollution Levels

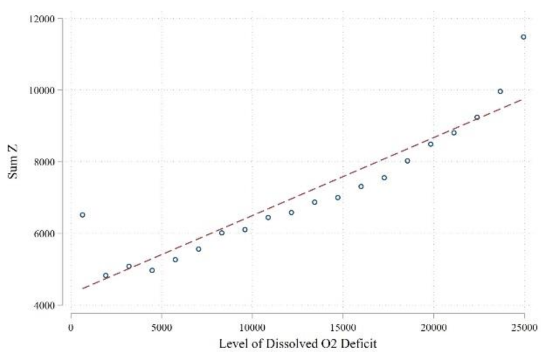
(a) Biochemical Oxygen Demand



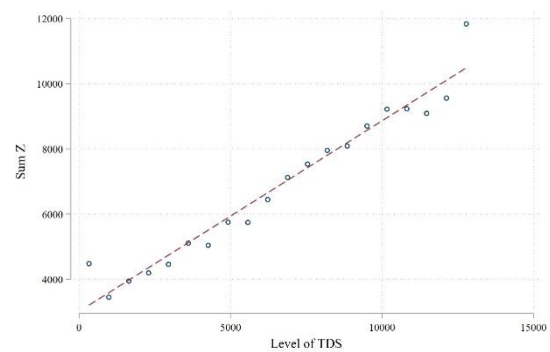
(b) Total Suspended Solids



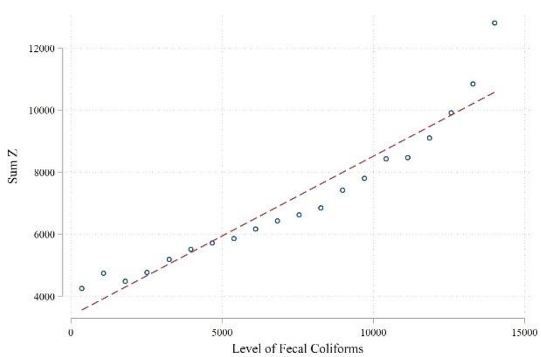
(c) Dissolved Oxygen Deficit



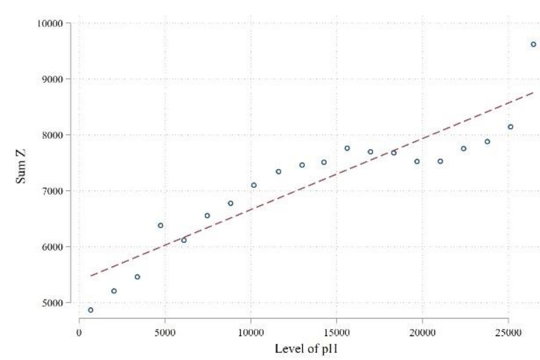
(d) Total Dissolved Solids



(e) Fecal Coliforms



(f) pH



Notes: We report here the bin scatter plots of the rank of county-year observations based on their summed *Z* score across all pollutants for which we have a reading in that county-year compared to the raw level of pollution for six individual pollutants at the same county × year level. *Z* scores are calculated for each pollutant in each year (negative *Z* scores indicate lower than average pollution). To calculate summed *Z* scores we sum the *Z* scores across all pollutants for which we have data in a county-year.

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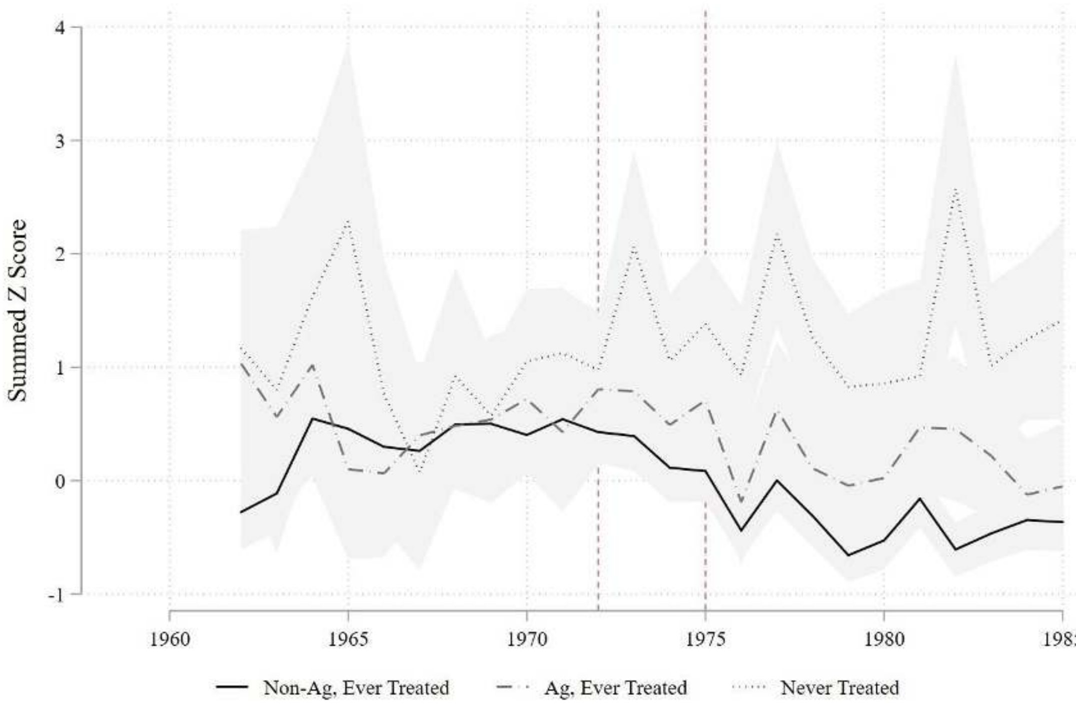
Figure A3: Pollution Trends: Agricultural and Non-Agricultural



Notes: The figure replicates figure 3 from the main text but defines agricultural counties based on agricultural establishments instead of employment. The figure shows the annual trend in the average summed *Z* score of water pollution for treated agricultural and non-agricultural counties and all non-treated counties. *Z* scores are calculated for each pollutant in each year (negative Z scores indicate lower than average pollution) and then *Z* scores for all pollutants are summed by county-year. Averages across counties are calculated by year. Treated counties are those that contain a facility that receives a NPDES permit in 1973 or 1974. Counties that contain a facility that receives a permit after 1974 are dropped. Non-treated counties do not contain a facility that received a NPDES permit between 1972 and 1985. Agricultural counties are those whose share of establishments in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Non-treated agricultural and non-treated non-agricultural counties are pooled in the non-treated group. The grey shaded area is the 95% confidence interval for the mean summed *Z* score.

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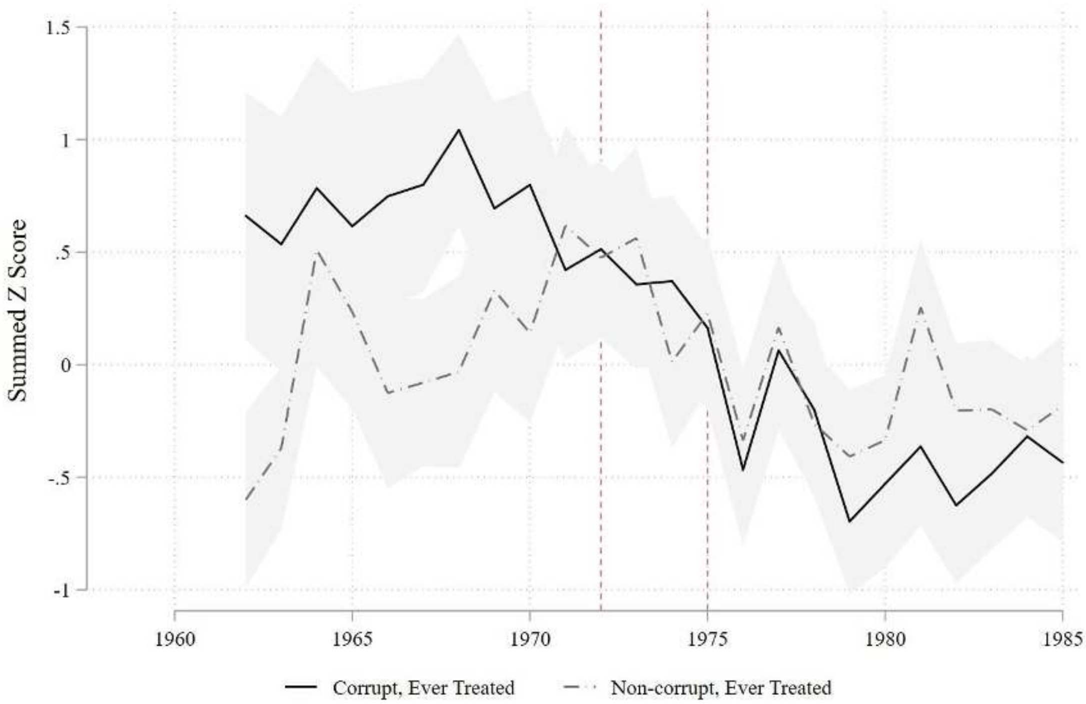
Figure A4: Pollution Trends: Corrupt and Non-Corrupt



Notes: This figure replicates figure 4 from the main text but defines corruption based on per capita newspapers subscriptions instead of conviction rates. The figure shows the annual trend in the average summed *Z* score of water pollution for corrupt and non-corrupt counties that are treated. *Z* scores are calculated for each pollutant in each year (negative *Z* scores indicate lower than average pollution) and then *Z* scores for all pollutants are summed by county-year. Averages across counties are calculated by year. Treated counties are those that contain a facility that receives a NPDES permit in 1973 or 1974. Counties that contain a facility that receives a permit after 1974 are dropped. Non-treated counties do not contain a facility that received a NPDES permit between 1972 and 1985. Corrupt counties are those in states where the number of newspaper subscriptions per 1,000 residents in 1972 is below the mean. The grey shaded area is the 95% confidence interval for the mean summed *Z* score.

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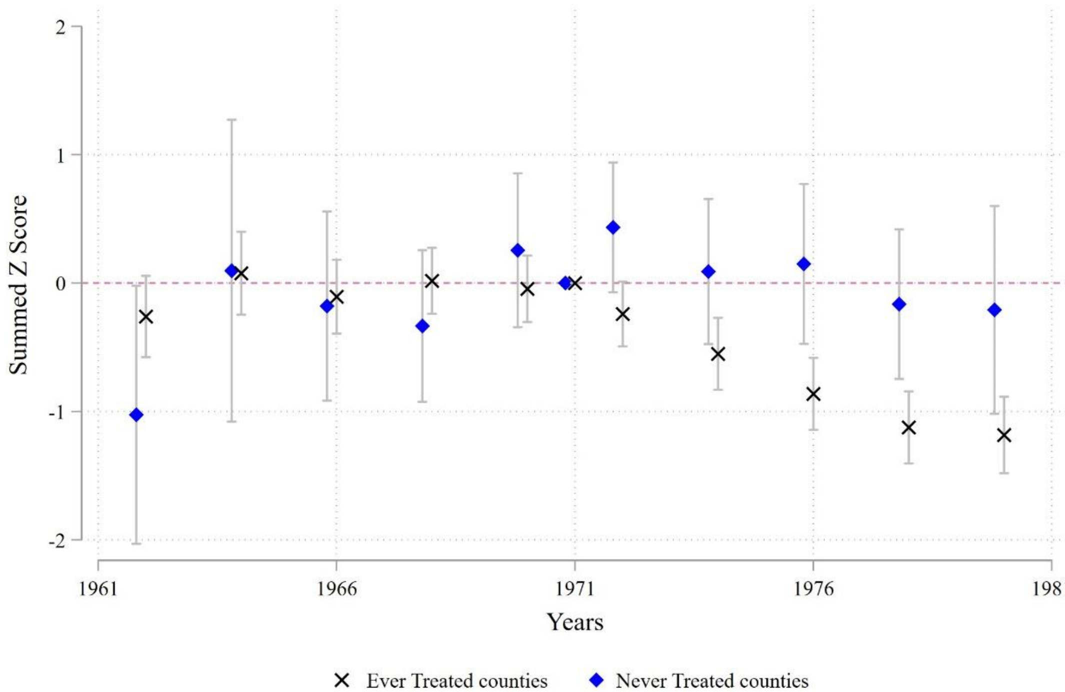
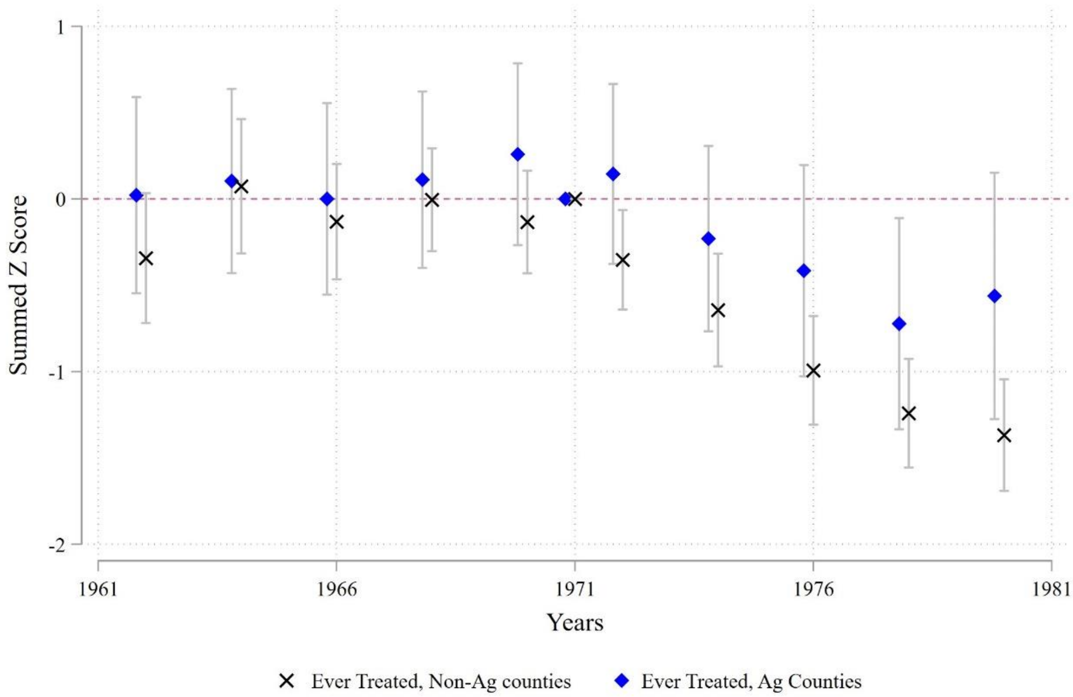
Figure A5: Event Study Estimates: Treated vs. Non-Treated



Notes: This figure displays the response of pollution levels by county to the passage of the CWA. Each point is the estimated ω_τ coefficient from the regression described in Equation A1. The treated series shows the estimates on the sample of counties in which the CWA becomes enforceable in 1973 or 1974. Non-treated shows the estimates in the sample counties in which the CWA does not become enforceable between 1972 and 1985. The omitted year is the year prior to the passage of the CWA, 1971. The dark grey bars indicate the 95% confidence interval on the estimates, clustered at the county level.

Figure A6: Event Study Estimates: Agricultural vs. Non-Agricultural



Notes: This figure displays the response of pollution levels by county in treated agricultural and non-agricultural counties to the passage of the CWA. Each point is the estimated ω_τ coefficient from the regression described in Equation A2. Agricultural counties are those whose share of employment in agriculture in 1972 was above the 75th percentile of its distribution across all counties. The omitted year is the year prior to the passage of the CWA, 1971. The dark grey bars indicate the 95% confidence interval on the estimates, clustered at the county level.

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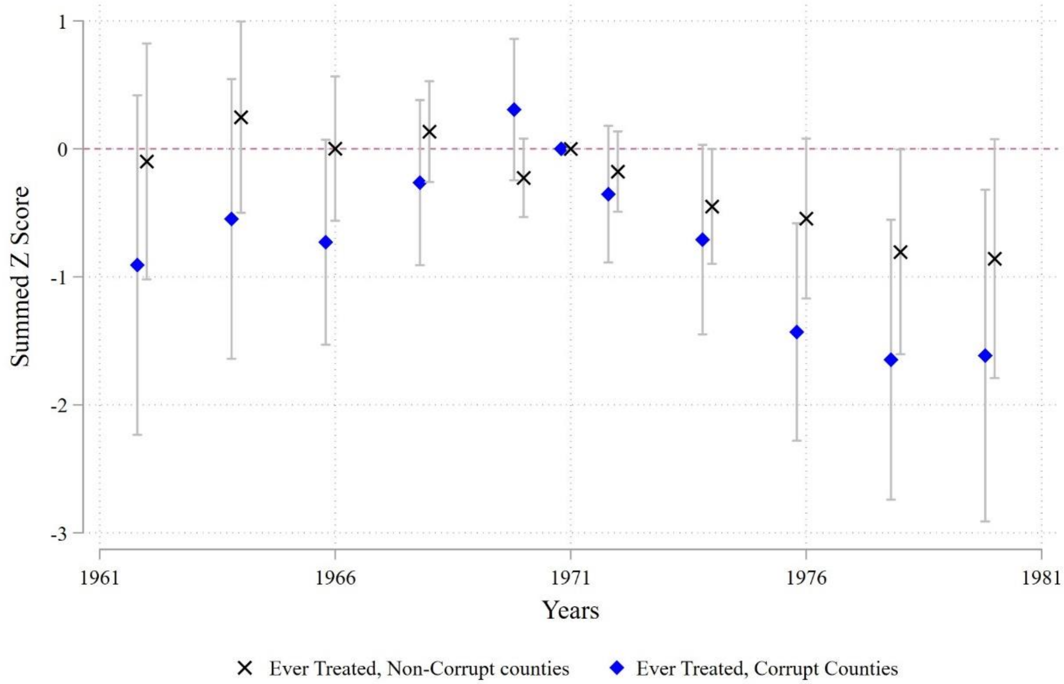
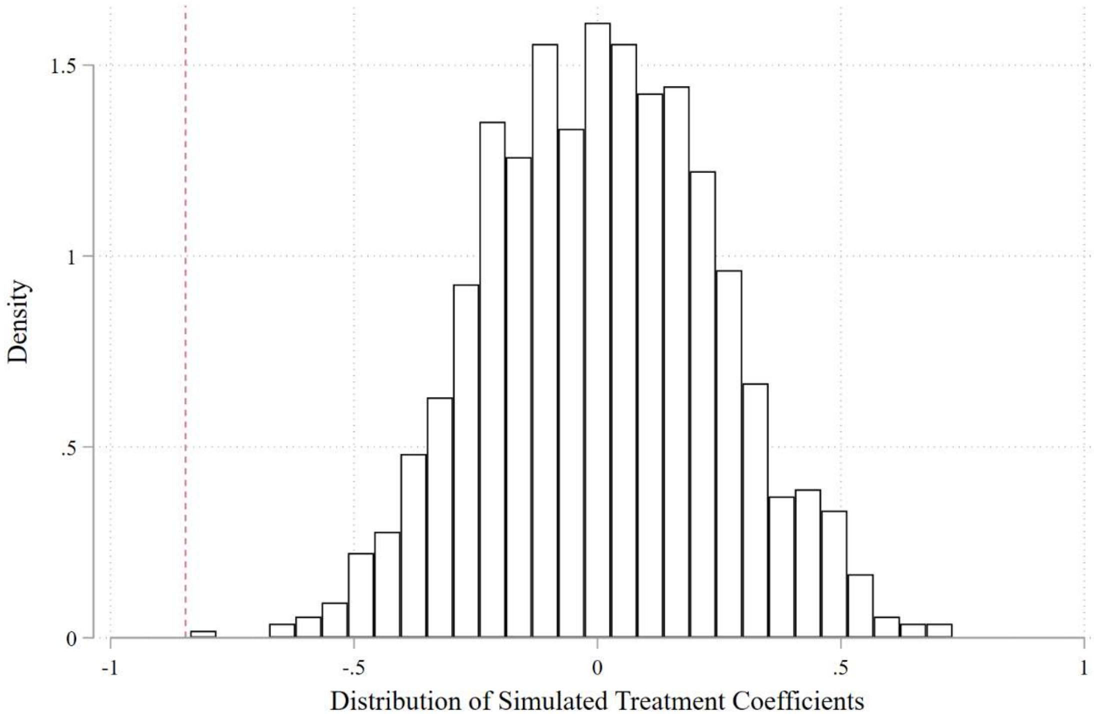
Figure A7: Event Study Estimates: Corrupt vs. Non-Corrupt



Notes: This figure displays the response of pollution levels by county in treated corrupt and noncorrupt counties to the passage of the CWA. Each point is the estimated ω_τ coefficient from the regression described in Equation A3. Corrupt counties are those in states where the average number of federal convictions per 10,000 state residents is above the mean. Conviction rates are calculated as the average of the annual number of convictions per capita over the years 1976-1985. The dark grey bars indicate the 95% confidence interval on the estimates, clustered at the county level.

Figure A8: Distribution of Randomized Treatment Placebo Estimates



Notes: This figure displays the distribution of the estimates of the impact of the CWA becoming enforceable from Equation 1 when we randomly assign treatment to counties and re-estimate the equation 1,000 times. The vertical red dashed line indicates our estimate of the impact of the CWA becoming enforceable given the actual pattern of enforcement that we report in Column 3 of Table 2.

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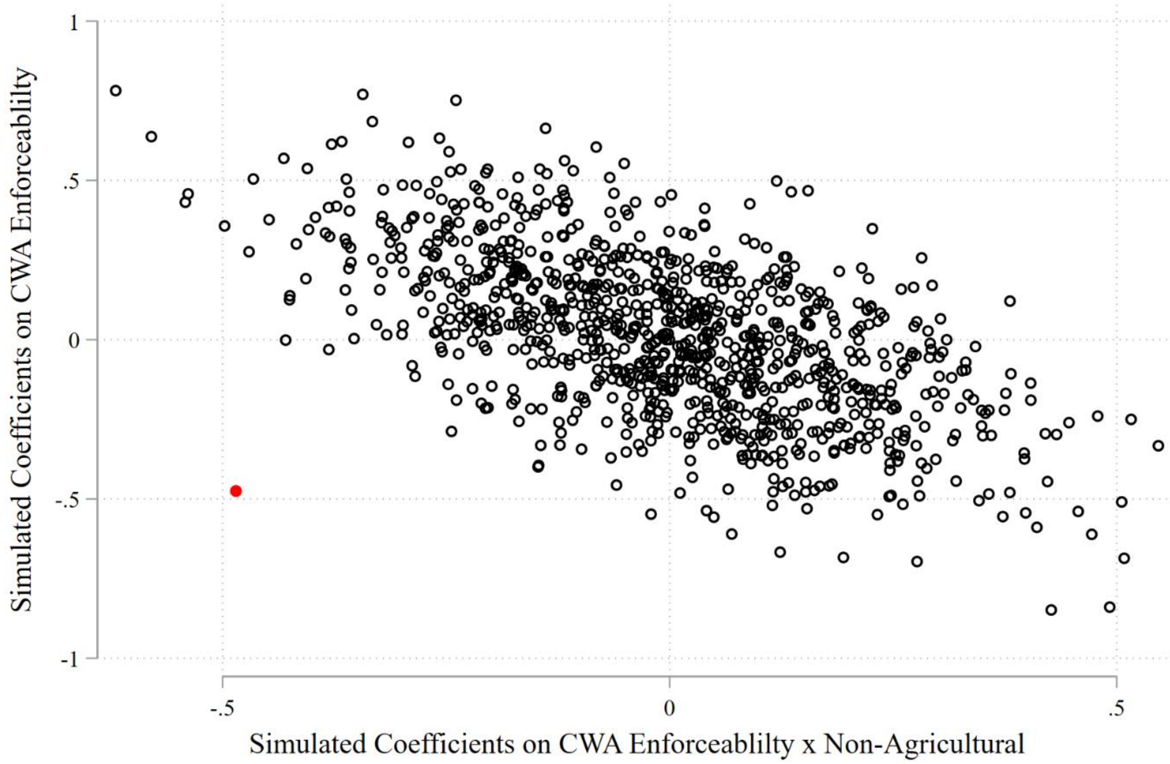
Figure A9: Joint Distribution of Randomized Treatment Placebo Estimates



Notes: This figure displays the joint distribution of the estimates of the impact of the CWA becoming enforceable and the impact of the CWA becoming enforceable in non-agricultural counties from Equation 2 when we randomly assign treatment and agricultural status to counties and re-estimate the equation 1,000 times. The red point indicates our estimate of the CWA becoming enforceable and the impact of enforceability in non-agricultural counties given the actual pattern of enforcement that we report in Column 4 of Table 2.

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Table A3: Pre-Trends in Treated Counties

	(1)	(2)
Year	0.012	
	(0.016)	
Year = 1962		−0.154
		(0.171)
Year = 1963		−0.077
		(0.155)
Year = 1964		0.426**
		(0.186)
Year = 1965		−0.068
		(0.172)
Year = 1966		−0.067
		(0.168)
Year = 1967		0.101
		(0.144)
Year = 1968		0.200
		(0.130)
Year = 1969		0.200
		(0.145)
Year = 1970		0.136
		(0.134)
Year = 1971		0.200
		(0.136)
N	7,641	7,641
R^2	0.66	0.66
County FE	Yes	Yes

Notes: Column 1 reports the coefficient on a continuous Year variable regressed on the *Z* score of pollution in treated counties in a specification with county fixed effects in a sample from 1962 to 1972. Column 2 reports the coefficients on a series of dummies for each year from 1962 to 1972 with 1972 set as the base year and county fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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Table A4: Difference-in-Differences Results – Retain 1973 and 1974

	(1)	(2)	(3)	(4)
CWA Enforceable	−0.505***	−0.502***	−0.396**	−0.117
	(0.100)	(0.100)	(0.170)	(0.233)
CWA Enforceable × Non-Agricultural				−0.361*
				(0.195)
N	28,589	28,589	28,589	28,589
R^2	0.54	0.54	0.55	0.55
County FE	Yes	Yes	Yes	Yes
Year FE			Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes

Notes: This table replicates Table 2 from the text but retains all data from 1973 and 1974 and includes them in the regressions. Columns 1–3 report the difference-in-differences specification described in Equation 1 with and without year fixed effects and with and without controls. Column 4 reports the additional difference between agricultural and non-agricultural counties described in Equation 2. We consider the CWA enforceable starting in the year the first facility in a county receives its first NPDES permit if that is prior to 1975. Counties that only contain facilities that receive their first permit between 1975 and 1985 are dropped. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1985. Agricultural counties are those whose share of employment in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Difference-in-Differences Results – Retain post-1974

	(1)	(2)	(3)	(4)
CWA Enforceable	−0.599***	−0.586***	−0.359**	−0.016
	(0.120)	(0.118)	(0.161)	(0.244)
CWA Enforceable × Non-Agricultural				−0.457**
				(0.224)
N	32,434	32,434	32,434	32,434
R^2	0.57	0.57	0.57	0.57
County FE	Yes	Yes	Yes	Yes
Year FE			Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes

Notes: This table replicates Table 2 from the text but retains counties that contain a facility that receives a permit between 1975 and 1985 and considers them untreated. Columns 1–3 report the difference-in-differences specification described in Equation 1 with and without year fixed effects and with and without controls. Column 4 reports the additional difference between agricultural and non-agricultural counties described in Equation 2. We consider the CWA enforceable starting in the year the first facility in a county receives its first NPDES permit if that is prior to 1975. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1975. Agricultural counties are those whose share of employment in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A6: Difference-in-Differences Results – Time-Varying Treatment

	(1)	(2)	(3)	(4)
CWA Enforceable	-0.420^{***}	-0.418^{***}	-0.083	0.202
	(0.085)	(0.085)	(0.109)	(0.170)
CWA Enforceable × Non-Agricultural				-0.362^{**}
				(0.165)
N	36,442	36,442	36,442	36,442
R^2	0.56	0.56	0.56	0.56
County FE	Yes	Yes	Yes	Yes
Year FE			Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes

Notes: This table replicates Table 2 from the text but classifies all counties as treated in the year the first facility within them receives an NPDES permit. Columns 1–3 report the difference-in-differences specification described in Equation 1 with and without year fixed effects and with and without controls. Column 4 reports the additional difference between agricultural and non-agricultural counties described in Equation 2. We consider the CWA enforceable starting in the year the first facility in a county receives its first NPDES permit. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1985. Agricultural counties are those whose share of employment in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Difference-in-Differences Results – Conventional Pollutants

	(1)	(2)	(3)	(4)	(5)
CWA Enforceable	0.018	0.020	-0.245^{**}	-0.158	-0.384
	(0.048)	(0.048)	(0.107)	(0.125)	(0.357)
CWA Enforceable × Non-Agricultural				-0.113	-0.568^{**}
				(0.086)	(0.265)
N	24,531	24,531	24,531	24,531	25,455
R^2	0.49	0.49	0.49	0.49	0.56
County FE	Yes	Yes	Yes	Yes	Yes
Year FE			Yes	Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes

Notes: This table replicates Table 2 from the text but for the set of pollutants the CWA defined as conventional and was particularly focused on: Biochemical Oxygen Demand, Fecal Coliforms, Total Suspended Solids and pH. Columns 1–3 report the difference-in-differences specification described in Equation 1 with and without year fixed effects and with and without controls. Columns 4–5 reports the additional difference between agricultural and non-agricultural counties described in Equation 2. We consider the CWA enforceable starting in the year the first facility in a county receives its first NPDES permit if that is prior to 1975. Counties that only contain facilities that receive their first permit between 1975 and 1985 are dropped. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1985. Agricultural counties are those whose share of employment in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Column 4 reports results defining agricultural counties based on employment and Column 5 report results using establishments. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Difference-in-Differences Results – Establishments

	(1)	(2)	(3)	(4)
CWA Enforceable	−0.689***	−0.683***	−0.846***	−0.384
	(0.129)	(0.128)	(0.272)	(0.357)
CWA Enforceable × Non-Agricultural				−0.568***
				(0.265)
N	25,455	25,455	25,455	25,455
R^2	0.55	0.55	0.55	0.55
County FE	Yes	Yes	Yes	Yes
Year FE			Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes

Notes: This table replicates Table 2 from the text but defines agricultural counties based on establishments instead of employment. Columns 1–3 report the difference-in-differences specification described in Equation 1 with and without year fixed effects and with and without controls. Column 4 reports the additional difference between agricultural and non-agricultural counties described in Equation 2. We consider the CWA enforceable starting in the year the first facility in a county receives its first NPDES permit if that is prior to 1975. Counties that only contain facilities that receive their first permit between 1975 and 1985 are dropped. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1985. Agricultural counties are those whose share of establishments in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Convictions per Capita – Continuous

	(1)	(2)
CWA Enforceable	−0.052	−1.412
	(0.222)	(0.938)
CWA Enforceable × Convictions, p.c.	−16.816**	
	(7.903)	
CWA Enforceable × ln(Convictions, p.c.)		−0.246
		(0.229)
N	28,589	28,589
R^2	0.55	0.55
County FE	Yes	Yes
Year FE	Yes	Yes
State-Specific Linear Time Trend	Yes	Yes
Controls	Yes	Yes

Notes: The table reports $y_{ijt} = \beta\, CWA_{it} + \omega\, CWA_{it} \times Conviction\ Rate_j + \gamma_i + \delta_j t + \psi_t$. y_{ijt} is the summed *Z* score across all pollutants in county *i* in state *j* and year *t*. CWA_{it} is an indicator for whether the CWA was enforceable in county *i* in year *t*. We consider the CWA enforceable starting in the year the first facility in a county receives its first NPDES permit if that is prior to 1975. Conviction rates for each state are calculated as the average of the annual number of federal convictions per capita over the years 1976-1985. Controls include total employment, manufacturing employment and mining employment at the county level and rates of college attendance at the state level. Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A10: Convictions per Capita – Newspapers

	(1)	(2)	(3)	(4)
Corrupt	0.310			
	(0.714)			
CWA Enforceable		−0.832***	−9.139	−2.407*
		(0.285)	(6.542)	(1.216)
CWA Enforceable × Corrupt.		−0.029		
		(0.440)		
CWA Enforceable × Newspapers, p.c			0.006	
			(0.004)	
CWA Enforceable × ln(Newspapers, p.c.)				1.481
				(1.170)
N	9,106	25,455	25,455	25,455
R^2	0.02	0.56	0.56	0.56
County FE		Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-Specific Linear Time Trend		Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table replicates Table 3 in the main text but defines corruption based on newspapers per capita. It adds the continuous measures of corruption as in Table A9, again using newspapers per capita instead of conviction rates. Column 1 reports $y_{ijt} = \beta\ Corrupt_j + \psi_t$. Column 2 reports $y_{ijt} = \beta\ CWA_{it} + \omega\ CWA_{it} \times Corrupt_j + \gamma_i + \delta_j t + \psi_t$. Columns 3 and 4 report $y_{ijt} = \beta\ CWA_{it} + \omega\ CWA_{it} \times Subscription\ Rate_j + \gamma_i + \delta_j t + \psi_t$. y_{ijt} is the summed Z score across all pollutants in county i in state j and year t. CWA_{it} is an indicator for whether the CWA was enforceable in county i in year t. We consider the CWA enforceable starting in the year the first facility in a county receives its first NPDES permit if that is prior to 1975. $Corrupt_j$ is an indicator for whether a county is in a corrupt state (1 = Yes). Corrupt states are those where the number of newspaper subscriptions per 1,000 residents in 1972 is below the mean. Controls include total employment, manufacturing employment and mining employment at the county level and rates of college attendance at the state level. Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A11: Difference-in-Differences Results with Varying Agricultural Thresholds

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CWA Enforceable	−1.126***	−1.107***	−0.764**	−0.58**	−0.475	−0.453	−0.286
	(0.304)	(0.307)	(0.31)	(0.322)	(0.334)	(0.345)	(0.383)
CWA Enforceable × Ag. Employment	0.377*						
	(0.226)						
CWA Enforceable × Ag. Emp., Tercile 2		0.087					
		(0.263)					
CWA Enforceable × Ag. Emp., Tercile 3		0.65**					
		(0.277)					
CWA Enforceable × Non-Agricultural, p65			-0.13				
			(0.197)				
CWA Enforceable × Non-Agricultural, p70				−0.389*			
				(0.212)			
CWA Enforceable × Non-Agricultural, p75					−0.485**		
					(0.224)		
CWA Enforceable × Non-Agricultural, p80						−0.492**	
						(0.241)	
CWA Enforceable × Non-Agricultural, p85							−0.662**
							(0.293)
N	25,455	25,455	25,455	25,455	25,455	25,455	25,455
R^2	0.56	0.56	0.56	0.56	0.56	0.56	0.56
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummy for Zero Agricultural Employment	Yes	Yes	No	No	No	No	No

Notes: The table reports the results of the primary specification reported in Table 2, column 4 in the main text. However, here we vary how we measure whether a county is agricultural. In column 1 we use the continuous measure of agricultural employment share in 1972. In column 2 we classify counties with non-zero employment in agriculture into terciles based on their share of employment in agriculture in 1972 and include indicators for terciles two and three. In both columns 1 and 2 we include a dummy for counties with zero agricultural employment. In columns 3-7 we return to using an indicator for whether a county is agricultural but we vary the threshold at which we consider a county agricultural. For example, in column 3 we consider counties whose share of employment in agriculture in 1972 was above the 65th percentile of the distribution across all counties. In column 4 we set the threshold at the 70th percentile and so on. Column 5 uses the same threshold, the 75th percentile, as in the baseline results in Table 2, column 4 of the main text. We consider the CWA enforceable starting in the year the first facility in a county receives its first NPDES permit if that is prior to 1975. Counties that only contain facilities that receive their first permit between 1975 and 1985 are dropped. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1985. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A12: Replication of Keiser and Shapiro (2019)

	KS Trend	KS Trend Break	Trend	Trend Break
Dissolved O_2 Deficit				
Year	-0.24^{***}	-1.027^{***}	-0.013^{***}	-0.009^{**}
			(0.001)	(0.004)
Year × Post1972		0.834^{***}		−0.006
				(0.005)
Biochemical O_2 Demand				
Year	-0.065^{***}	-0.124^{***}	-0.020^{***}	-0.022^{***}
			(0.002)	(0.005)
Year × Post1972		0.062^{***}		0.003
				(0.006)
Fecal Coliforms				
Year	-81.097^{***}	-255.462^{***}	-0.023^{***}	-0.063^{***}
			(0.003)	(0.015)
Year × Post1972		179.134^{**}		0.044^{***}
				(0.015)
Total Suspended Solids				
Year	-0.915^{***}	−1.113*	-0.009^{***}	-0.023^{***}
			(0.002)	(0.005)
Year × Post1972		0.203		0.018^{***}
				(0.006)

Notes: This table compares the results in Keiser and Shapiro (2019) Table I to our results running their trend specification with our county-level pollution Z scores. The first column reports the coefficients Keiser and Shapiro (2019) find in a regression of pollutant levels on a year trend. Column 2 reports their coefficients in a regression of pollutant levels on year and an interaction of year and a post-1972 dummy. Columns 3 and 4 replicate their regression specifications with our county level data using the Z score of the named pollutant as the outcome. Key differences between the data we use and those in Keiser and Shapiro (2019): their observation is at the monitor-day-hour level so they can include monitor fixed effects. We only include county fixed effects. Further, they include cubic polynomials to control for season and time of day. Our data is an average over all seasons and times of day. They report coefficients on pollution levels while we report coefficients in terms of the Z score of the pollutants. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A13: Difference-in-Differences Results – Placebo Treatment in 1971-1972

	(1)	(2)	(3)	(4)
CWA Enforceable	−0.328*	−0.280	−0.258	0.128
	(0.197)	(0.200)	(0.212)	(0.506)
CWA Enforceable × Non-Agricultural				−0.479
				(0.528)
N	26,164	26,164	26,164	26,164
R^2	0.55	0.55	0.56	0.56
County FE	Yes	Yes	Yes	Yes
Year FE			Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes

Notes: This table replicates Table 2 from the text but assigns treatment in 1971 and 1972 as a placebo test. Columns 1–3 report the difference-in-differences specification described in Equation 1 with and without year fixed effects and with and without controls. Column 4 reports the additional difference between agricultural and non-agricultural counties described in Equation 2. In the placebo test we assume that treatment follows the same pattern observed in the data but we consider treated counties treated beginning in 1971-1972 as opposed to 1973-1974. We still drop counties that only contain facilities that receive their first permit between 1975 and 1985. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1985. Agricultural counties are those whose share of establishments in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Difference-in-Differences Results – Placebo Treatment in 1975-1976

	(1)	(2)	(3)	(4)
CWA Enforceable	−0.550***	−0.541***	−0.165	−0.295
	(0.117)	(0.116)	(0.172)	(0.320)
CWA Enforceable × Non-Agricultural				−0.415**
				(0.204)
N	25,378	25,378	25,378	25,378
R^2	0.55	0.55	0.55	0.55
County FE	Yes	Yes	Yes	Yes
Year FE			Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes

Notes: This table replicates Table 2 from the text but assigns treatment in 1975 and 1976 as a placebo test. Columns 1–3 report the difference-in-differences specification described in Equation 1 with and without year fixed effects and with and without controls. Column 4 reports the additional difference between agricultural and non-agricultural counties described in Equation 2. In the placebo test we assume that treatment follows the same pattern observed in the data but we consider treated counties treated beginning in 1975-1976 as opposed to 1973-1974. We still drop counties that only contain facilities that receive their first permit between 1975 and 1985. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1985. Agricultural counties are those whose share of establishments in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Difference-in-Differences Results – Counties Treated in 1975

	(1)	(2)	(3)	(4)
CWA Enforceable	−0.435*	−0.422*	−0.355	−0.031
	(0.237)	(0.236)	(0.293)	(0.399)
CWA Enforceable × Non-Agricultural				−0.421
				(0.375)
N	7,986	7,986	7,986	7,986
R^2	0.55	0.55	0.56	0.56
County FE	Yes	Yes	Yes	Yes
Year FE			Yes	Yes
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes

Notes: This table replicates Table 2 from the text but assigns treatment in 1975 as a placebo test. Columns 1–3 report the difference-in-differences specification described in Equation 1 with and without year fixed effects and with and without controls. Column 4 reports the additional difference between agricultural and non-agricultural counties described in Equation 2. In the placebo test we drop counties treated in 1973 or 1974 and assign treatment to those counties that contain a facility that receives an NPDES permit in 1975. We drop counties that only contain facilities that receive their first permit between 1976 and 1985. We consider the CWA non-enforceable in counties that do not contain a facility that receives a permit by 1985. Agricultural counties are those whose share of establishments in agriculture in 1972 was above the 75th percentile of its distribution across all counties. Controls include total employment, manufacturing employment and mining employment. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Predicting Treatment Timing

	(1)
Sum *Z* Score	0.008^{**}
	(0.003)
Total Employment	-0.0826
	(0.0761)
Manufacturing Employment	0.232
	(0.19)
Mining Employment	3.889^{***}
	(1.006)
Agricultural Employment	0.0011
	(0.002)
N	2,113
R^2	0.41

Notes: This table reports the results of an OLS regression predicting whether a county contained a facility that received an NPDES permit prior to 1974. Our outcome is a dummy for whether the county contains such a facility and we include state fixed effects. Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.