Online Appendix for "Drilling Like There's No Tomorrow: Bankruptcy, Insurance, and Environmental Risk."

Judson Boomhower

Contents

A Industry Background

A.1 Distributions of Production and Revenue

This table lists quintile cutoffs for output and revenue for the 8,235 operators that reported production during March, 1996– February, 2002. Annual production and revenue are twelve times the average monthly production across months during this period that the firm was in the industry. One barrel-of-oil equivalent is one barrel of oil or 6,000 cubic feet of natural gas. Price data are EIA Texas first purchase prices for oil and EIA Texas wellhead prices for natural gas. Dollar amounts are in 2010 dollars.

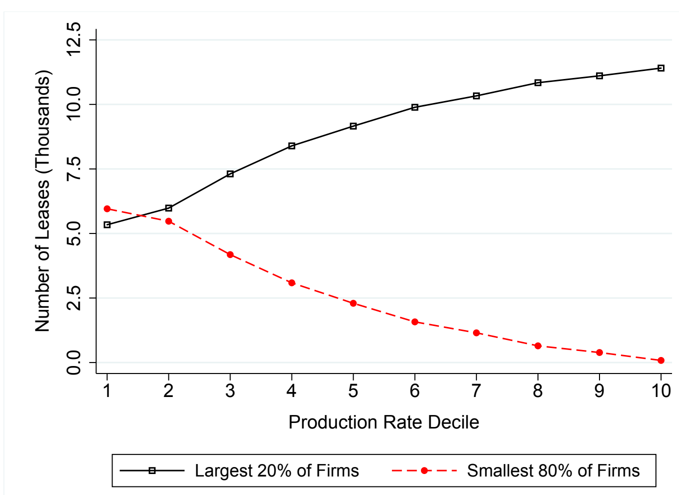
Appendix Table 1: Distributions of Annual Production and Revenue

Percentile	Production (BOE)	Revenue (Dollars)
Max	87,330,104	2,265,808,896
80	43,396	1,047,229
60	10,420	250,755
40	3,550	86,331
20	1,100	26,601

A.2 Specialization by Small and Large Firms

This figure shows how small firms specialize in low-producing leases. The horizontal axis shows deciles of production rate for all oil and gas leases in Texas in 2000. Dots show the number of leases in each decile operated by small and large firms.

Appendix Figure 1: Lease-level Production Rates, By Firm Size

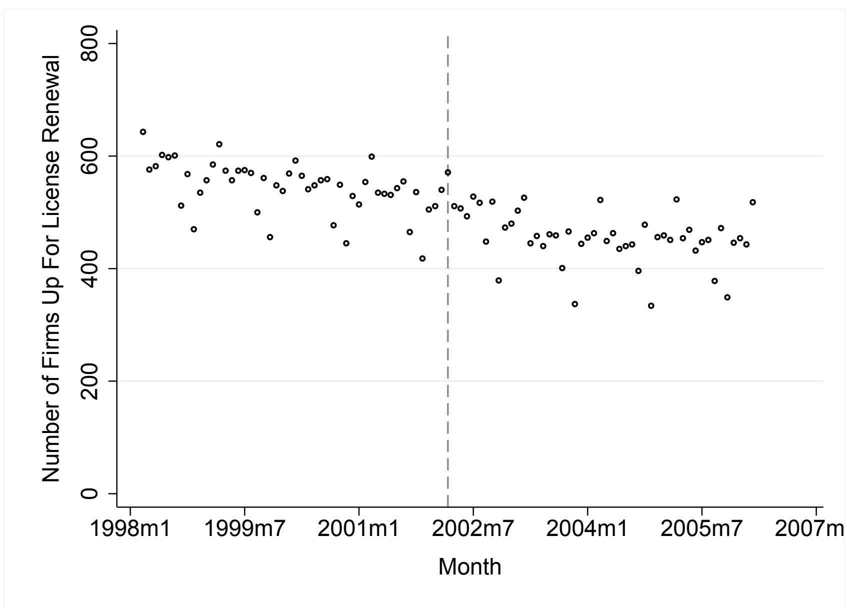


B Industry Participation Robustness Checks and Additional Results

B.1 Number of Firms up for License Renewal Each Month

Appendix Figure 2 shows the standard McCrary (2008) check for manipulation of the assignment variable. The figure shows the number of firms required to renew their annual operating license in each month around the implementation of the universal bond requirement. These months are assigned by the Texas Railroad Commission and cannot be manipulated by firms. The sample includes firms with oil or gas production during 1997–2006. The vertical dashed line indicates the implementation of the increased bond requirement in March, 2002. As expected, there is no evidence of firms manipulating their license renewal dates.

Appendix Figure 2: Number of Firms Up For License Renewal Each Month



B.2 Exit Regressions Robustness Checks

Appendix Table 2 reports additional RD estimates for the effect of the bond requirement on exit (as in Table 1 in the main text). The first two columns show local linear regression estimates with varied bandwidths. The center column shows results for a quadratic polynomial with a 36-month bandwidth. The final two columns show results for a cubic polynomial with 36- and 48-month bandwidths. All regressions control for output prices and month-of-year fixed effects.

Appendix Table 2: Exit Effects, Alternative Bandwidths and Polynomials

Specification	LLR	LLR	Quadratic	Cubic	Cubic
Bandwidth	15	9	36	48	36
$1[Implemented]$	0.058	0.055	0.058	0.045	0.049
	(0.013)	(0.019)	(0.014)	(0.016)	(0.015)
Constant	0.104	0.099	0.105	0.117	0.110
	(0.007)	(0.014)	(0.010)	(0.011)	(0.011)
N	15,226	9,077	35,964	48,619	35,964
Firms	6,691	6,301	8,003	8,854	8,003

Notes: This table reports additional RD estimates for the effect of the bond requirement on exit (as in Table 1). The first two columns show local linear regression estimates with varied bandwidths. The center column shows results for a quadratic polynomial with a 36-month bandwidth. The final two columns show results for a cubic polynomial with 36- and 48-month bandwidths. All regressions control for oil prices and month-of-year fixed effects. Standard errors are clustered by month.

B.3 Alternative Estimates for the Variance of the Effect on Exit

Appendix Table 3 replicates Table 1 in the main text using alternative variance estimators. Each panel shows standard errors, t-statistics, and observation numbers for a different variance estimator. For the bottom panel, the data are collapsed to a time series of monthly averages and standard errors are calculated using the Newey West HAC variance estimator with 6 lags in Columns (1) and (2) and 12 lags in Columns (3) and (4) (which include longer time series and thus support more lags).

Appendix Table 3: Alternative Variance Estimates for the Effect on Exit

	(1)	(2)	(3)	(4)
Coefficient	0.05331	0.06611	0.04760	0.04962
Standard Errors Clustered by Month				
SE	(0.01211)	(0.01253)	(0.00923)	(0.00897)
t	4.4	5.3	5.2	5.5
N	12,200	12,200	48,619	48,619
Standard Errors Clustered by Month and Operator				
SE	(0.01213)	(0.01254)	(0.00926)	(0.00900)
t	4.4	5.3	5.1	5.5
N	12,200	12,200	48,619	48,619
Newey-West Standard Errors, Time Series Regression				
SE	(0.01152)	(0.00969)	(0.00786)	(0.00801)
t	4.6	6.8	6.1	6.2
N	24	24	97	97

Notes: This table replicates Table 1 using alternative variance estimators for the coefficient of interest, $1[Implemented]$. In the bottom panel, the data are collapsed to a time series of monthly averages and standard errors are calculated using the Newey West HAC variance estimator with 6 lags in Columns (1) and (2) and 12 lags in Columns (3) and (4) (which include longer time series and thus support more lags). The time series regression is weighted by the number of firm-level observations in each month, in order to yield coefficient estimates identical to the microdata regression (these weights have no effect past the third decimal place).

B.4 Survival Regression

Appendix Table 4 estimates a survival regression following the discrete time transition model described in Cameron and Trivedi (2005). Each column in this table reports a separate logit regression of a binary outcome variable on year dummies. The outcome is zero before a firm exits and one in the year of exit. The sample is limited to 1997–2012, and to firms that were actively producing before 1997. Years are measured from March–February to match the policy implementation period, and there is one observation per firm per year. I report predicted exit probabilities (i.e., the estimated exit hazard) for each year. To compare this approach to Tables 1 and 2 in the main text, compare the exit hazard in 2002 to the sum of the constant term and the policy effect in Table 1 or 2. The overall hazard in 2002 is about 14.9%, closely matching Table 1 (16.1%). The next five columns report exit hazards by quintile of operator size. The estimated hazard rates for 2002 match Table 2 closely.

Appendix Table 4: Survival Regression

		Firm Size Quintile				
	All Firms	(1)	(2)	(3)	(4)	(5)
1998	7.7	10.7	9.0	7.4	6.0	4.0
	(0.3)	(0.9)	(0.8)	(0.7)	(0.7)	(0.5)
1999	8.4	10.4	10.4	9.1	6.5	6.1
	(0.4)	(0.9)	(0.9)	(0.8)	(0.7)	(0.7)
2000	6.7	10.0	8.4	6.0	5.0	5.3
	(0.3)	(1.0)	(0.9)	(0.7)	(0.6)	(0.7)
2001	9.2	16.5	9.9	8.2	6.4	7.0
	(0.4)	(1.2)	(1.0)	(0.9)	(0.7)	(0.8)
2002	14.9	28.3	22.1	13.5	9.2	7.1
	(0.5)	(1.7)	(1.4)	(1.1)	(0.9)	(0.8)
2003	5.5	8.4	6.6	5.3	4.7	4.3
	(0.4)	(1.2)	(1.0)	(0.8)	(0.7)	(0.7)
2004	5.8	9.4	7.4	5.3	4.1	4.8
	(0.4)	(1.3)	(1.1)	(0.8)	(0.7)	(0.7)
2005	5.4	6.3	6.4	5.1	5.6	4.7
	(0.4)	(1.2)	(1.0)	(0.8)	(0.8)	(0.7)
2006	5.0	6.0	5.1	5.8	4.2	4.6
	(0.4)	(1.2)	(1.0)	(0.9)	(0.7)	(0.7)
2007	4.6	8.5	4.2	4.3	4.4	3.6
	(0.4)	(1.4)	(0.9)	(0.8)	(0.7)	(0.7)
2008	4.3	4.8	5.6	6.1	2.6	3.3
	(0.4)	(1.1)	(1.1)	(1.0)	(0.6)	(0.6)
2009	3.3	3.8	3.1	4.2	3.0	2.7
	(0.3)	(1.0)	(0.8)	(0.8)	(0.6)	(0.6)

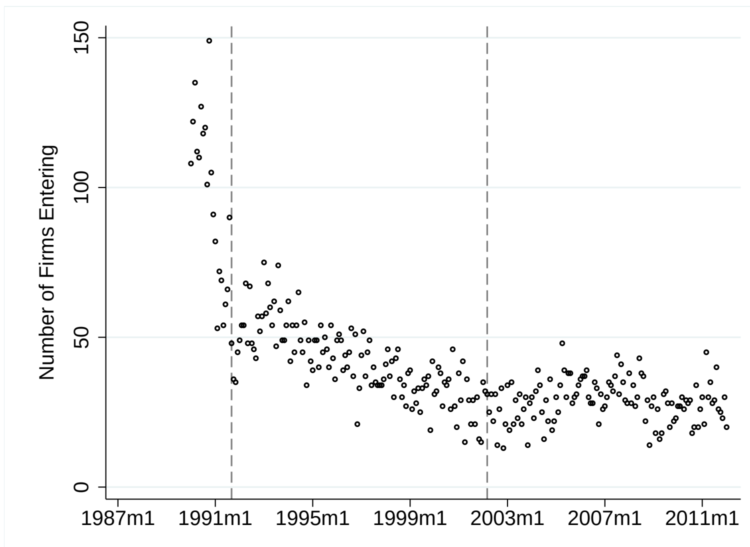
Notes: To compare this approach to Tables 1 and 2 in the main text, compare the exit hazard for 2002 in this table to the sum of the constant term and the policy effect in Table 1 or 2. The overall hazard rate in 2002 is about 14.9%, closely matching the results in Table 1 (16.1%). The predicted annual exit hazards by quintile of operator size also closely match Table 2.

This table estimates a survival regression following the discrete time transition model described in Cameron and Trivedi (2005). Each column in this table reports a separate logit regression of a binary outcome variable on year dummies. The outcome is zero before a firm exits and one in the year of exit. The sample is limited to 1997–2012, and to firms that were actively producing before 1997. Years are measured from March–February to match the policy implementation period, and there is one observation per firm per year. I report predicted exit probabilities (i.e., the estimated exit hazard) in each year along with standard errors. To compare this approach to Table 1 in the main text, compare the estimated exit hazards in 2002 to the sum of the constant term and policy effect in Table 1. The results are very similar.

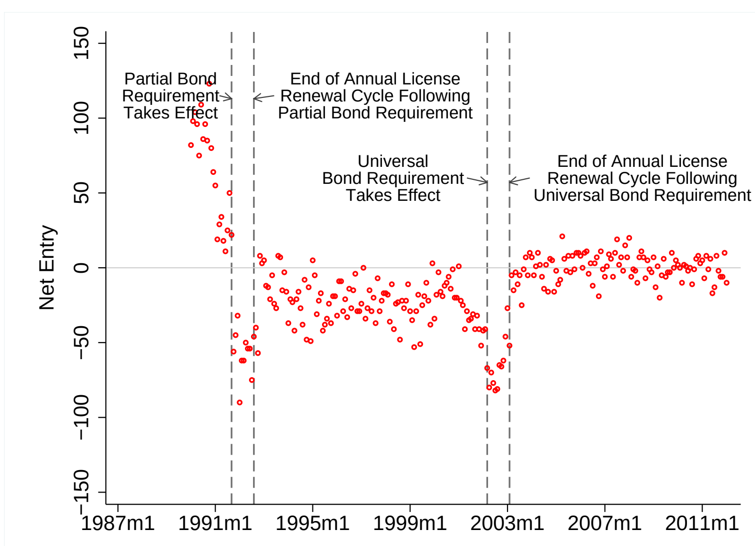
B.5 Entry and Net Entry

Appendix Figure 3 shows the number of firms entering the industry each month. Entry date is defined as the date of the firm's first annual operating license filing. The vertical dashed lines show September, 1991 and March, 2002. The sample includes all firms with oil or gas production from 1990 to 2010. Appendix Figure 4 shows the net change in the number of firms in the industry each month. Vertical dashed lines show Sep. 1991–Aug. 1992 and Mar. 2002–Feb. 2003.

Appendix Figure 3: Entry by Month



Appendix Figure 4: Net Entry (Entry - Exit) by Month



C Firm-Level Oil and Gas Output Robustness Checks and Additional Results

C.1 Comparing Firms by License Renewal Month

Appendix Table 5 compares the average output and number of leases across renewal month groups. Firms' assigned license renewal months are determined by the Railroad Commission and are not manipulable by firms. As expected given this assignment mechanism, there is no evidence of differences across groups.

Appendix Table 5: Comparing Firms by License Renewal Month

	(1)	(2)	(3)
Month	Number of Firms	Annual Production (BOE)	Number of Leases
January	645	45,829 (128,995)	12 (23)
February	682	51,335 (127,883)	13 (23)
March	719	41,923 (111,552)	12 (22)
April	648	33,806 (99,278)	11 (20)
May	645	43,090 (118,016)	11 (18)
June	664	37,721 (101,845)	11 (23)
July	667	38,777 (102,081)	11 (20)
August	676	39,364 (111,611)	10 (21)
September	572	42,830 (120,910)	12 (20)
October	667	33,463 (85,484)	11 (19)
November	542	46,018 (125,278)	12 (24)
December	611	47,812 (130,343)	14 (30)
F statistic		1.50	1.04
p-value		0.12	0.41

Notes: This table reports mean annual production and number of leases for firms according to their assigned license renewal month. Standard deviations are in parentheses. This table covers firms from their 1996 to 2001 license renewals. To reduce the influence of a few very large firms, I drop the largest 2.5% of firms in terms of annual production (for this table only). The F statistics and p-values are for a test of the null hypothesis that the mean is the same in every month.

C.2 Placebo Analysis

Appendix Table 6 shows a placebo test using data from three years before and three years after the policy implementation period. There is no measurable effect of license renewal on small firms' output in this placebo analysis.

Appendix Table 6: Effect of License Renewal on Production in Other Years

	(I) Implementation Year	(II) Adjacent Years
1[Bonded]*Quintiles 1 − 4	−0.048	−0.010
	(0.017)	(0.006)
N	45,539	281,742
Firms	4,467	6,140

Notes: This table reports estimates from two separate regressions showing the impact of renewing the annual operating license on small firms' oil and gas production in various years. Using the approach described for Table 4, similar samples are constructed for the three years before and the three years after the implementation of the bond requirement. Column (I) of this table is identical to Column (2) of Table 4. Column (II) of this table pools the three years prior to and three years after the implementation year. Standard errors are clustered at the operator level.

C.3 Event Study Figure

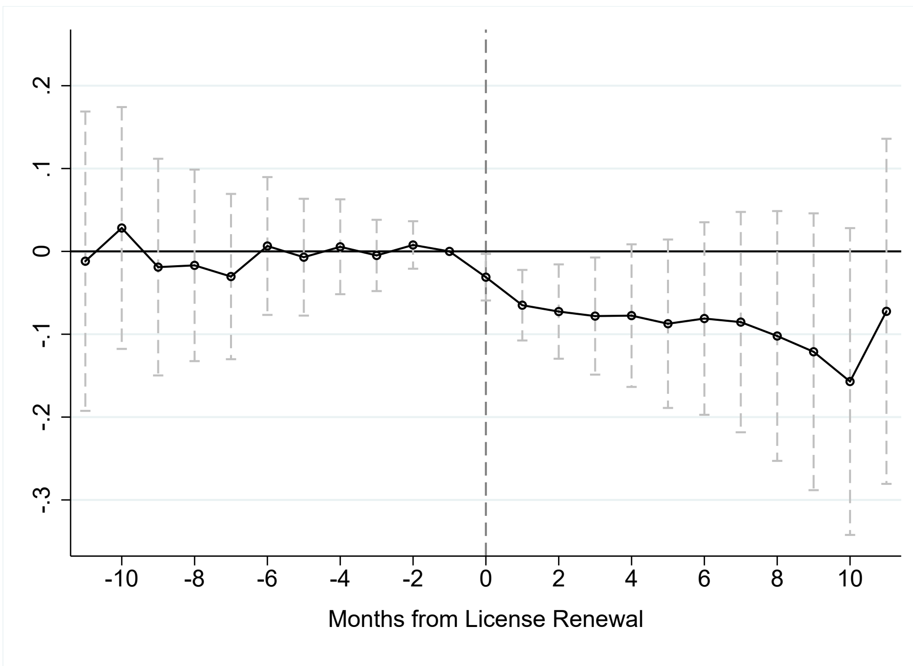
Appendix Figure 5 shows the effect of becoming bonded on small firms' oil and gas output over time. I plot estimated coefficients and 90% confidence intervals corresponding to event time indicators from an event study regression,

$$ln(y_{it}) = \sum_{k=-11}^{-2} \alpha_k 1[\rho_{it} = k]_{it} + \sum_{k=0}^{11} \alpha_k 1[\rho_{it} = k]_{it} + \tau_t + \epsilon_{it}. \tag{6}$$

The dependent variable y_{it} is oil and gas production for firm i in calendar month t. ρ denotes the event month defined so that $\rho = 0$ is the month in which the firm becomes bonded. The omitted category is $\rho = -1$, so the other coefficients are measured relative to the month before the firm becomes bonded. The regression also includes calendar month fixed effects τ_t. Unlike equation 5 in the main text, the fully dynamic event study in equation 6 cannot be estimated with firm fixed effects without additional normalizations due to the well-known "age-time-cohort" problem in event studies.[1] Following Borusyak and Jaravel (2016), I instead include firm-level random effects to increase precision.

The figure shows event month coefficients from a regression including the smallest 60% of firms. The individual event month coefficients are noisily estimated, which is not surprising; there are only a small number of observations to identify each individual event month dummy. However, there is a clear decrease in the point estimates at the time of bonding equal to about 7%. Before and after bonding, the trend in oil and gas production is approximately flat in event time.

Appendix Figure 5: Event Study Figure for Production By Small Firms



[1]Firm fixed effects are nested subsets of treatment cohort fixed effects. Within each event month, calendar time perfectly predicts treatment time (by definition) and thus calendar time effects and treatment cohort effects are not separately identified.

D Lease-level Effects Robustness Checks and Additional Results

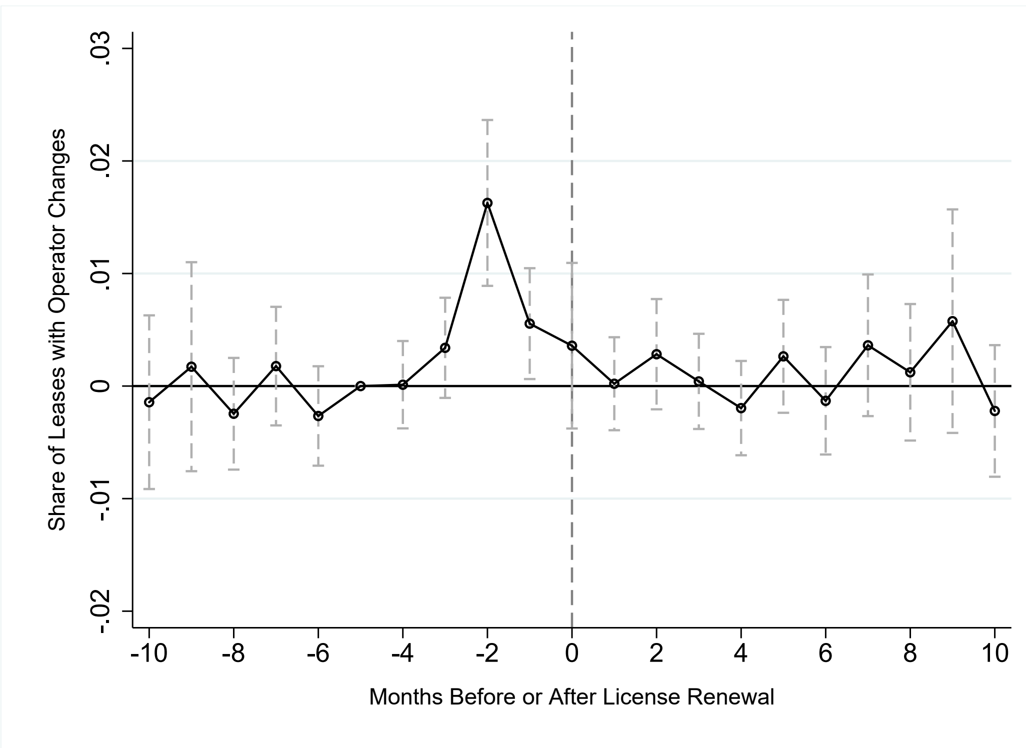
D.1 Lease-level Event Studies for Transfers and Shut-ins

It is also possible to implement the event study design from Section 6.2.2 to measure effects on transfers and shut-ins. Appendix Figure 6 shows event month coefficients from an event study regression for transfers. I use the specification described for Appendix Figure 5, except that observations are individual leases instead of firms. The dependent variable is $1[Transfer]$, an indicator variable equal to one in months when there is a change in the lease operator. The sample includes the implementation year (March 2003 to February 2002), and the smallest 80% of firms. The horizontal axis in the figure plots event month dummies that measure the time between the current month and the assigned license renewal month for the firm that owned the lease at the beginning of the year. The omitted event month in the regression is -5 (five months prior to license renewal).

Transfers were disproportionately likely during event months -2, -1, and 0. Since the bond requirement became binding on firms at their license renewal, this is consistent with firms releasing projects as they became bonded (which would have occurred in the 1–2 months before their compliance deadline). The sum of the excess transfers during event months -2 to 0 is approximately equal to the level of excess transfers reported in Table 5 in the main text.

The power of the event study design to detect shut-in effects at the individual lease level is limited. The firm-level effects in Section 6.2.2 represent a combination of transfer and shut-in decisions, often across multiple leases for each firm. Detecting changes in these outcomes separately at the individual lease level in an event study is more demanding of the data. Furthermore, while firms become subject to the bond requirement on a fixed date (the firm's license renewal month), leases can be transferred between firms and thus there is some noise in the determination of when any given lease should be considered subject to the policy. Shut-ins occur less frequently than operator transfers (as shown in Table 5), and are thus most difficult to measure with this approach. As might be expected given the nature of the data, the event study results for lease shut-ins are noisy and somewhat sensitive to specification choice. These results are available upon request.

Appendix Figure 6: Ownership Transfers Event Study



E Environmental Outcomes Robustness Checks and Additional Results

E.1 Alternative Measures of Well Orphaning

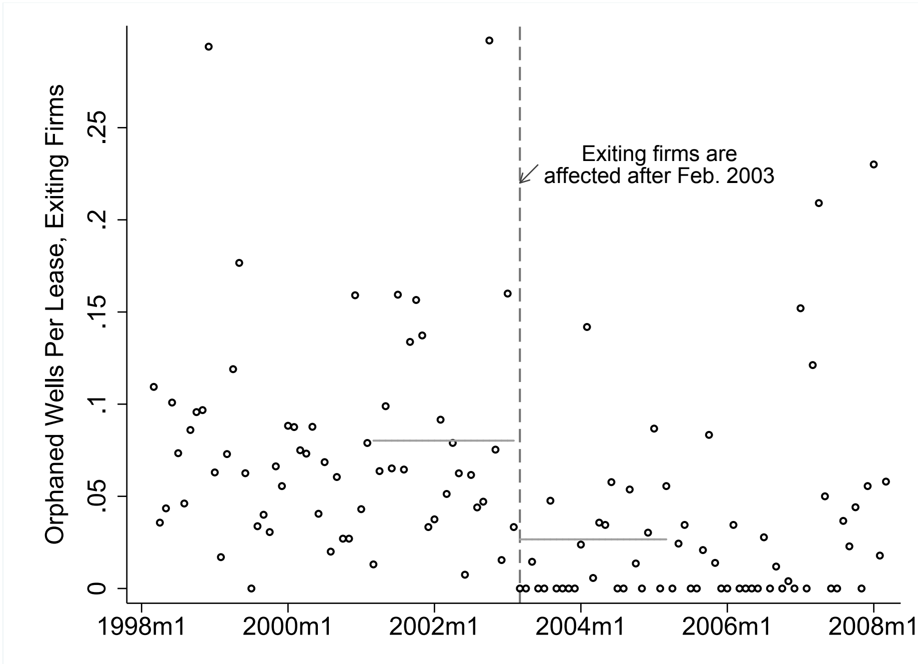
Appendix Figure 7 shows two other measures of well orphaning over time. The top panel shows the average number of orphan wells per lease operated among exiting firms each month. Prior to the bonding requirement, exiting firms orphaned about 0.08 wells per lease operated on average. There is a sharp decrease after the bond requirement to about 0.025 wells per lease. The bottom panel shows the total number of orphan wells left by firms exiting in each month. Prior to the bond requirement this was about 16 per month; after the requirement it falls sharply to about 3 per month.

E.2 Robustness Checks for Well Orphaning Regressions

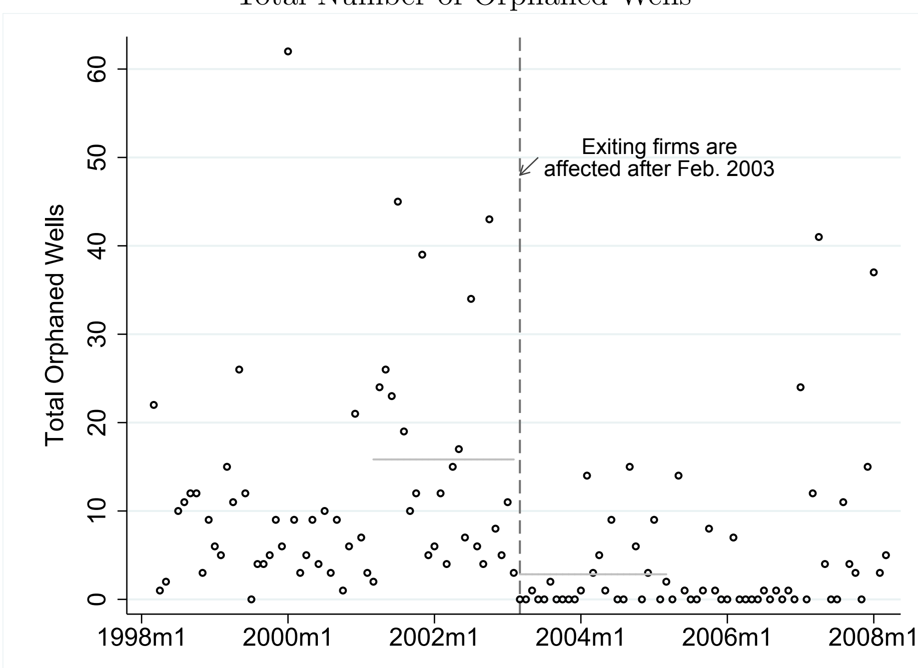
Appendix Table 7 shows alternative bandwidths and specifications for the regression estimate of the change in well orphaning. As in Table 6 in the main text, the dependent variable in all columns is the number of orphan wells per lease among exiting firms. The four sections labeled (I) through (IV) each use different sample radii in months around the bond requirement. Section (IV) also includes a time trend. In each section, I show results separately for the smallest 80% of firms and the largest 20% of firms. Results are similar across columns and similar to the results in Table 6 in the main text.

Appendix Figure 7: Alternative Measures of Well Orphaning

Orphan Wells Per Lease Operated



Total Number of Orphaned Wells



Notes: The top panel shows the average number of orphan wells per lease among exiting firms each month. The bottom panel shows the total number of wells orphaned each month. In the bottom panel, one pre-period outlier month with 110 orphan wells (June, 1998) is not shown.

Appendix Table 7: Change in Well Orphaning, Alternative Specifications

	(I)		(II)		(III)		(IV)	
	Small Firms	Large Firms	Small Firms	Large Firms	Small Firms	Large Firms	Small Firms	Large Firms
1[Bonded]	-0.062	-0.007	-0.060	-0.007	-0.060	-0.016	-0.064	-0.012
	(0.033)	(0.006)	(0.016)	(0.011)	(0.011)	(0.010)	(0.027)	(0.014)
Constant	0.092	0.007	0.089	0.017	0.086	0.026	0.096	0.011
	(0.027)	(0.006)	(0.014)	(0.008)	(0.010)	(0.007)	(0.024)	(0.011)
Firms	919	158	2,103	422	3,238	631	3,238	631
Bandwidth	12	12	36	36	60	60	60	60
Time Trend	No	No	No	No	No	No	Yes	Yes

Notes: This table reports the results of eight separate OLS regressions, following the specification in Table 6 in the main text. "Small firms" pools the smallest 80% of firms, while "large firms" includes the largest 20%. The bandwidths listed in each column are the sample radii in months around March, 2003. The time trend in the final two columns includes a linear trend interacted with $1[Bonded]$. Standard errors are clustered by month.

E.3 Regression Estimates for Violations and Blowouts

Appendix Table 8 shows regression estimates of the changes in rules violations and well blowouts. These are relatively infrequent events, so I report results from a count model along with OLS results. I use negative binomial regression. Results are similar for poisson regression. The regressions also include time trends as reported in the table. In each case, the dependent variable is normalized by an exposure variable reported in the table. For OLS the dependent variable is divided by the normalizing quantity. For negative binomial models, normalization is implemented by including the log of the normalizing quantity as a regressor with coefficient constrained to one. The percentage change in expected count for negative binomial regressions is $e^{\phi} - 1$, where ϕ is the regression coefficient.

Columns (1) – (3) focus on rules violations. These regressions use operator-level data and the normalizing quantity is the firm's number of leases. Column (1) shows OLS results, while column (2) shows negative binomial results. In both, the average change in the rate of rules violations following the policy is about 23%. For robustness, Column (3) shows results when violations recorded after the firm's final production date are included (these are dropped in the main analysis, as discussed in the Data Appendix). These additional violations have little effect on the estimates. Columns (4) – (6) report results for well blowouts. These regressions use aggregate statewide data at the monthly level because of challenges in accurately merging blowout records to individual operators, as discussed in the text. Column (4) shows OLS results, normalizing by the number of active drilling rigs as in Figure 8. Column (5) shows negative binomial results using the same normalization. Finally, column (6) shows negative binomial results normalizing by the number of active drilling permits from the Texas Railroad Commission. In each specification, the decrease in the rate of well blowouts following the policy is about 69%.

Appendix Table 8: Changes in Rules Violations and Well Blowouts

	Water Protection Rules Violations			Well Blowouts		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	NB	NB	OLS	NB	NB
$1[After]$	−0.00023	−0.25	−0.27	−0.0056	−1.18	−1.13
	(0.00012)	(0.12)	(0.14)	(0.0013)	(0.26)	(0.26)
% Change in Expected Count	−24%	−22%	−24%	−69%	−69%	−68%
Constant	0.00096	−7.18	−6.87	0.0082	−4.75	−5.89
	(0.00007)	(0.06)	(0.07)	(0.0008)	(0.13)	(0.13)
Time Trend	Linear	Exponential	Exponential	Linear	Exponential	Exponential
Normalization	Leases Operated	Leases Operated	Leases Operated	Active Rigs	Active Rigs	Drilling Permits
N	687,196	687,196	791,794	120	120	120

Columns (1)–(3) Notes: The dependent variable is the monthly count of violations of Statewide Rules 8 or 14, normalized by the number of leases operated. The sample includes operator-level observations from June, 1996 to June, 2006. $1[After]$ is an indicator variable equal to one after June, 2001. Column (1) shows OLS results. Column (2) shows negative binomial (NB) results. Column (3) shows NB results when months beyond the firm's final production date are included. Standard errors are clustered by month.

Columns (4)–(6) Notes: The dependent variable is the monthly count of well blowouts, normalized by the number of active drilling rigs or drilling permits. The sample includes aggregate statewide data at the monthly level from June, 1996, to June, 2006. Column (1) shows OLS results, normalizing by the number of active drilling rigs as in Figure 8 in the main text. Column (2) shows negative binomial (NB) results, also normalizing by the number of active drilling rigs. Column (3) shows NB results, normalizing by the number of active drilling permits from the Texas Railroad Commission.

E.4 Other Policy Changes

To consider the possible role of other Texas policy changes, Appendix Table 9 shows all rules that were implemented or amended by the RRC in 2001 and 2002. The first column shows the date that the proposed action was published for public comment in the Texas Register. The second column shows the date that the regulation took effect. The other RRC actions were primarily procedural, and it seems unlikely that any of them would have caused the observed changes in environmental outcomes.

E.5 Aggregate Effects

Appendix Table 10 reports changes in the total rate of orphan wells per lease and violations per lease. I calculate these using weighted versions of the regressions in Table 6 and Figure 7 in the main text. The weights are equal to the number of leases operated by the firm.

Appendix Table 9: Texas Regulations Implemented in 2001 and 2002

Action	Proposed	Implemented
Allows electronic filing of drilling permits	March 2001 (TXR 26 2257)	June 2001 (TXR 26 4088)
Clarifies wording of hazardous waste rules[1]	May 2001 (TXR 26 3431)	September 2001 (TXR 26 6870)
Extends existing tax credit for high-cost gas	June 2001 (TXR 26 4015)	August 2001 (TXR 26 6009)
Extends existing tax credit for marginal wells	July 2001 (TXR 26 3431)	September 2001 (TXR 26 6869)
Implements Senate Bill 310 (bonding)[2]	**August 2001** (TXR 26 5919)	**March 2002** (TXR 27 139)
Clarifies rules for assigning acreage to pooled units	October 2001 (TXR 26 7721)	January 2002 (TXR 27 150)
Clarifies rules for requesting end to unitization	November 2001 (TXR 26 9480)	February 2002 (TXR 27 906)
Clarifies rules for transporting oil and gas	January 2002 (TXR 27 547)	May 2002 (TXR 27 3756)
Clarifies rules for "swabbing" existing wells[3]	April 2002 (TXR 27 2666)	September 2002 (TXR 27 9149)

Notes: This table lists all rules changes for oil and gas producers implemented by the Texas Railroad Commission during 2001 and 2002. It is based on all rule introductions or amendments listed in the RRC Oil and Gas Division rules (Texas Administrative Code, Title 16, Part 1, Chapter 3). "TXR" refers to volume and page number in the Texas Register. The date proposed is the date that the rule was published as a "Proposed Rule" to allow for public comment. The date implemented is the date that the regulation was published as an "Adopted Rule".

[1]This was a technical change in wording to match federal law, changing the word "facility" to "site." The proposed rule states, "The language change is consistent with the way the commission has applied the rule in that the commission's intent and policy, since the initial adoption of §3.98 in 1996, has been to apply the provisions of subsection (e) to oil and gas waste generators. Therefore, no one will be affected that was not affected under the previous rule."

[2]SB 310 passed the Texas legislature in June 2001; the RRC rule implementing SB 310 was first published as a proposed rule in August, 2001. This version was withdrawn and a second proposed rule was published in November, 2001 (TXR 26 8937).

[3] Swabbing is a technique that involves pulling fluid through the well bore using a wire and cup assembly. This rule clarifies that swabbing is prohibited as an ongoing production method to extend the life of very old wells.

Appendix Table 10: Aggregate Changes in Orphan Wells and Rules Violations

	Orphan Wells		Rules Violations	
	All Firms	Smallest 80% of Firms	All Firms	Smallest 80% of Firms
1[Bonded]	-0.013	-0.074	-0.10	-0.42
	(0.006)	(0.017)	(0.02)	(0.08)
Constant	0.020	0.097	0.40	1.28
	(0.005)	(0.015)	(0.01)	(0.06)
Percent Change	**-65%**	**-76%**	**-25%**	**-33%**
Observations	2,522	2,103	413,819	304,592

Notes: This table reports the results of four separate regressions, following Table 6 and Figure 7 in the main text except that each observation is weighted by the number of leases operated by the firm. In columns (1) and (2), the dependent variable is orphan wells per lease. The sample includes firms exiting within a 36 month radius around March, 2003. 1[*Bonded*] is an indicator variable equal to one starting in March, 2003. Column (1) excludes 3 outlier observations corresponding to firms exiting during this period with greater than 2,500 leases. In columns (3) and (4), the dependent variable is the number of violations of Statewide Rules 8 or 14 per month. The sample includes a 36-month radius around June, 2001. 1[*Bonded*] is an indicator variable equal to one after June, 2001. Standard errors are clustered by month.

F Accounting for Related Operators

The Organization Report data identify some sets of operators as having related ownership. Of the 10,489 total operators, 1,175 are identified as being related to at least one other operator. Collapsing related operators into single groups leaves 9,888 firm groups. This section uses the collapsed firm definitions to reproduce key tables from the analysis. In each case, the results are very similar to those in the main text. I choose not to collapse related operators in the main results because it is not clear how to treat some operator-specific fields when collapsing. For example, if related operators have different assigned license renewal months, the analysis in Table 4 would require me to choose which renewal month to use for the collapsed firm group.

Appendix Table 11: Reproducing Table 2, Collapsing Related Operators

	(1)	(2)	(3)	(4)	(5)
	Quintile 1	**Quintile 2**	**Quintile 3**	**Quintile 4**	**Quintile 5**
1[*Implemented*]	0.098	0.109	0.054	0.036	0.014
	(0.040)	(0.018)	(0.024)	(0.021)	(0.015)
Constant	0.208	0.158	0.090	0.053	0.043
	(0.017)	(0.012)	(0.018)	(0.019)	(0.008)
N	1,849	2,053	2,217	2,422	2,496
Firm Groups	1,005	1,085	1,152	1,249	1,284

Notes: This table reproduces Table 2, collapsing related operators into single firms. For related operators, exit date is defined to be the most recent exit date among firms in the group. The size group cutoffs are the same as in the main text.

Appendix Table 12: Reproducing Table 5, Ignoring Transfers Between Related Operators

Firm Size Quintiles	(1) Lease-Year Observations	(2) Implementation Year Rate	(3) Baseline in Adjacent Years	(4) Excess (2) − (3)
	Panel A. Transfers, All Leases			
1–3	83,310	15.6%	9.0%	6.7%
4	86,442	10.8%	9.1%	1.7%
5	261,090	8.6%	8.9%	-0.2%
	Panel B. Transfers, High Quality Leases			
1–3	9,621	12.8%	3.9%	8.9%
4	11,505	10.5%	9.5%	1.0%
5	92,576	9.3%	9.5%	-0.3%

Notes: This table reproduces Panels A and B of Table 5, ignoring lease transfers between related operators.

Appendix Table 13: Reproducing Table 6, Collapsing Related Operators

		Firm Size Quintile				
	All Firms	(1)	(2)	(3)	(4)	(5)
1[Implemented]	−0.056	−0.088	−0.059	−0.050	−0.005	−0.016
	(0.014)	(0.047)	(0.022)	(0.019)	(0.025)	(0.012)
Constant	0.083	0.142	0.086	0.061	0.051	0.021
	(0.012)	(0.041)	(0.018)	(0.017)	(0.013)	(0.010)
Firm Groups	2,460	624	541	427	368	311

Notes: This table reproduces Table 6, collapsing related operators into single firms. For related operators, exit date is defined to be the most recent exit date among firms in the group. The size group cutoffs are the same as in the main text.

G Alternative Empirical Design: Difference-in-Differences

As an additional robustness check on the main empirical results, this section re-estimates the key results in the paper using an alternative empirical design. I use Louisiana oil and gas producers as a comparison group for difference-in-differences (DiD) estimates. The primary value of this DiD analysis is to confirm that outcomes in Louisiana do not jump discontinuously at the time of the Texas policy change. I find that the DiD estimates are similar to the estimates in the main text. This rules out concerns about mistakenly attributing the effects of a national or global-level shock to the Texas policy change.

While this is a useful robustness check, the empirical approach in the main text is preferable for estimating the effect of the policy. The Louisiana data are less granular than the Texas data, which leads to noisier results for some outcomes and also precludes the visually clear identification strategies employed in the main text. The annual nature of the Louisiana data is challenging given that the policy of interest took effect in the middle of a calendar year (in March). It also makes it impossible to use event study comparisons of production by bonded and unbonded firms in the same month during the implementation year. The DiD approach also requires its own identifying assumptions – most importantly, it requires the assumption that outcomes in Louisiana in 2003 and later years provide a valid counterfactual for what would have happened in Texas during those years. While the oil and gas sectors in the two states are similar, they are not identical. For example, the size compositions of the industries are somewhat different. Finally, while I have done my best to use comparable quantities across the two states, each state has its own record-keeping system and measurement may not be perfectly consistent.

Data for Louisiana come from the Louisiana Department of Natural Resources SONRIS database. See Appendix J (the data appendix) for details.

G.1 Exit

Louisana keeps data on oil and gas operators' filings of Form OR-1, which is required each year. As in Texas, I define an operator's exit date to be 365 days after the most recent OR-1 renewal. Unlike in Texas, where renewal dates are staggered throughout the calendar year, almost all Louisiana renewals (88%) occur in January. I thus have to collapse the analysis to the annual level. To match the timing of Texas' 12-month rollout from March 2002 to February 2003, I work with 12-month "policy years" that begin in March and end in February of the following calendar year.

Appendix Table 14 shows DiD estimates of the effect of the bond requirement on exit by oil and gas producers. Column (1) uses only Texas data, collapsed to the annual level in order to match the Louisiana data. The coefficient on 1[2002] is 0.066, which implies an increase in the exit rate during the implementation year of 6.6 percentage points. Column (2) brings in data for Louisiana and adds controls for year fixed effects, which can now be included due to the presence of a comparison group. The effect of the bond requirement is given by the coefficient on $1[2002] * 1[Texas]$, which is 0.075. This is slightly larger but very similar to the estimate using only Texas data.

These estimates can be compared to those in Table 1, Columns (1) and (2). In those specifications, the exit effect is found to be 0.053 and 0.066, respectively. These are quite similar to the difference-in-differences estimate. The difference-in-differences estimate is contained within the 95% confidence intervals of both of these estimates from the main text.

Appendix Table 14: Effect of the Bond Requirement on Exit, DiD Estimates

	Texas Only	Difference in Differences
	(1)	(2)
$1[2002] * 1[Texas]$	0.066	0.075
	(0.005)	(0.012)
$1[Texas]$		-0.033
		(0.004)
Constant	0.072	0.121
	(0.004)	(0.005)
Year Fixed Effects	No	Yes
Oil Price	Yes	No
Time Trend	Yes	No
Annual Observations	53,383	61,140
Texas Firms	9,095	9,095
Louisiana Firms	0	1,403

Notes: This table reports the results of two separate regressions. The sample includes data for Texas and Louisiana at the operator-year level from March, 1998 through February, 2006. To match the implementation period of the Texas policy, years are defined as March through February of the following calendar year (for example, "2002" corresponds to March 2002 through February 2003). The dependent variable is an indicator variable equal to one in the year of exit. $1[2002]$ is an indicator variable equal to one in 2002. $1[Texas]$ is an indicator variable equal to one for Texas operators. Column (1) controls for oil price and includes a linear time trend interacted with an indicator for years after the bonding requirement (following Table 1, column (2) in the main text). Column (2) includes year fixed effects with 2001 as the omitted year. Oil prices are average Texas first purchase prices during each twelve-month period, in hundreds of 2010 dollars. Standard errors are clustered by operator.

I now consider the exit effect by firm size. I assign Louisiana firms to size groups using the quintile cutoffs for Texas firms. Appendix Table 15 compares the size distributions of oil and gas producers in Texas and Louisiana. Column (1) shows that the Texas and Louisiana firm size distributions are comparable, but that Louisiana has slightly more large producers (as a share of firms). The merge success rate between the Louisiana production and license renewal data is lower for small firms than for large firms, which leads to a lower prevalence of small firms in the analysis dataset (Column (2)).

Appendix Table 15: Louisiana Firm Size Distribution Relative to Texas

	(1)	(2)	(3)
	Louisiana		Texas
Texas Size Quintile	All Firms	Merged to License Data	
1	21%	14%	20%
2	17%	14%	20%
3	15%	15%	20%
4	20%	23%	20%
5	27%	34%	20%

Appendix Table 16 shows the effect on exit by size of firm. The bottom two rows of the table show that there are fewer operators in Louisiana than Texas, so that dividing them into size groups means that a small number of Louisiana firms provide the counterfactual in each size group. Despite this, the DiD estimates by size are qualitatively and in most cases quantitatively similar to Table 2 in the main text. There is a large and statistically significant exit effect among small Texas producers that decreases monotonically with firm size. Large producers show no exit effect. The 95% confidence intervals for Quintiles 2 through 5 generally include the corresponding point estimates in Table 2, and vice versa. For Quintile 1, the DiD specification measures larger effects than those in the main text.

Appendix Table 16: Difference in Differences Estimates of Exit by Firm Size

	(1) Quintile 1	(2) Quintile 2	(3) Quintile 3	(4) Quintile 4	(5) Quintile 5
$1[2002] * 1[Texas]$	0.248	0.137	0.108	0.022	-0.027
	(0.031)	(0.042)	(0.029)	(0.026)	(0.023)
$1[Texas]$	-0.022	-0.043	-0.039	-0.036	-0.037
	(0.013)	(0.013)	(0.011)	(0.008)	(0.007)
Constant	0.181	0.151	0.115	0.103	0.104
	(0.017)	(0.016)	(0.013)	(0.010)	(0.009)
Annual Observations	8,469	9,280	10,374	12,071	13,314
Texas Firms	1,387	1,417	1,428	1,485	1,518
Louisiana Firms	150	149	172	269	390

Notes: This table reports estimates separately by size quintile for the regression specification shown in Column (2) of Appendix Table 14. All regressions include year fixed effects. Size cutoffs are the same as in the main text. Both Texas and Louisiana operators are assigned to size groups based on the Texas cutoffs. Standard errors are clustered by operator.

G.2 Oil and Gas Production

The monthly event study design in Section 6.2.2 in the main text leverages variation in the timing of Texas license renewals to compare bonded and unbonded firms in the same month, differencing out time-varying determinants of oil and gas output. One way to use data from another state in that analysis would be to include Louisiana producers in the event study as an additional control. However, the Louisiana production data are aggregated to the year-by-firm level, making this approach impossible. Instead, I use a simple difference in differences comparison of firms across states and years. The annual nature of the data and the small size of the Louisiana comparison group make it difficult to measure changes in output of the size implied in the analysis in the main text. Table 4 in the main text reports a 5% decrease in output on average for the smallest 80% of firms. The point estimate in the difference-in-differences specification with year fixed effects implies a 7% decrease, and so is similar. However, the confidence interval is quite wide (from a 17% decrease to a 3% increase), limiting the usefulness of this result. Detailed results for this regression are not reported.

G.3 Environmental Outcomes

Appendix Table 17 shows difference-in-differences estimates of the effect of the Texas bond requirement on the share of firms generating orphan wells. The first column shows estimates using Texas data only, collapsed to an annual level to match the Louisiana data. The second column shows difference-in-differences estimates. Because of the smaller size of the Louisiana dataset, and the incomplete merge rate between the license data and other Louisiana data, relatively few Louisiana operators are observed to exit during the sample, making for a noisy counterfactual comparison. I use a longer sample period for this regression than in the main text to increase precision (at the cost of relying on data further in time from the policy change). The difference-in-difference estimate of the change in orphan wells in Texas is -0.046, which is close to the Texas-only estimate of -0.047 in Column (1). Both of these estimates are statistically different from zero. Both also fall within the confidence interval of the estimate using the main empirical strategy, which is -0.039.[2]

Appendix Table 18 shows DiD estimates of the change in well blowouts. As in the main text, I focus on the aggregate time series of blowouts in each state because of challenges in correctly merging blowout records to individual firms. The dependent variable is the monthly number of blowouts divided by the number of drilling rigs active in the state during that month. Using only the Texas time series, the percentage decrease after the bond requirement is 66%. After including the Louisiana data, the difference in differences estimate is 51%. Again, the difference in differences estimate is within the 95% confidence interval of the main estimate.

[2]Because I do not observe the number of leases operated by Louisiana firms, I focus on an indicator outcome for whether firms orphaned wells, instead of orphan wells per lease as in Table 6 in the main text. The -0.039 coefficient here is for a corresponding regression using the specification in Table 6, with an indicator outcome for orphaning as the dependent variable.

Appendix Table 17: Change in Orphan Wells, DiD Estimates

	Texas Only (1)	Difference in Differences (2)
1[Bonded]	-0.047	-0.046
	(0.008)	(0.020)
1[Texas]		-0.009
		(0.012)
Constant	0.079	0.062
	(0.004)	(0.014)
Year Fixed Effects	No	Yes
Texas Firms	4,387	4,387
Louisiana Firms	0	820

Notes: This table reports the results of two separate regressions. The sample includes Louisiana and Texas oil and gas operators that exited between 1997 and 2009. The dependent variable is an indicator variable equal to one for operators that generate any orphan wells. 1[*Bonded*] is an indicator equal to one for Texas observations beginning in March, 2003.

Appendix Table 18: Change in Well Blowouts, DiD Estimates

	Texas Only (1)	Difference in Differences (2)
1[After]*1[Texas]	-0.0047	-0.0037
	(0.0007)	(0.0017)
% Change in Expected Count	**-66%**	**-51%**
1[Texas]		0.0008
		(0.0012)
Constant	0.0078	0.0066
	(0.0005)	(0.0029)
Time Fixed Effects	No	Yes
Texas Months	120	120
Louisiana Months	0	120

Notes: This table reports the results of two separate regressions. The sample includes state-level aggregate data for each month from June, 1996 to June, 2006. The dependent variable is the number of blowouts per active drilling rig in each state-month. Rig information is from the Baker-Hughes Historical Rig Count dataset. $1[After]$ is an indicator variable equal to one for Texas observations starting in June, 2001. The percentage change in expected count is the coefficient on $1[After] * 1[Texas]$ divided by the mean blowout rate in Texas during the six months prior to the policy change.

H Theory Appendix

H.1 Industry Equilibrium Without Bankruptcy

Define average project cost to include expected costs except payments to landowners,

$$AC_{ij}(k) = \frac{F_i + c(k) + k(x + h(x))}{k} \tag{4}$$

Free entry by production firms means that average project costs must be minimized in equilibrium. The relevant first order condition defines the AC-minimizing number of projects k^*,

$$k^* = \frac{F_i + c(k^*)}{c'(k^*)}$$

The project cost function $c(k)$ does not depend on site quality ρ. Thus, each firm type i will produce k_i^* projects regardless of site quality. The H-type firm produces more projects since $F_H > F_L$. The difference in F also means that the H-type firm has higher average costs than the L-type firm for every output level. From here, the main text describes the determination of payments to landowners, r_j.

The Minimum Project Quality Cutoff $\underline{\rho}$. The zero profit condition requires that the total revenue across all projects produced by the firm equal the sum of the firm's costs. For this to hold, the firm can only develop a project that yields less than its average cost $AC(k_i^*)$ if it can also produce another project in which it earns more than $AC(k_i^*)$. This is not possible. If the firm attempts to retain more than $AC(k_i^*)$ on any project, it will be outbid for that project by another firm that retains exactly $AC(k_i^*)$ on each of its projects. Thus, neither firm type can profitably produce projects with revenues below its average cost. The L-type firm has the lower average cost and so sets the location of $\underline{\rho}$.

Allocation of Project Types to Firm Types. The high-technology firm type dominates for project type j if and only if it generates larger per-project surplus (r) than the low-technology firm type,

$$pA_H\rho_j - \frac{F_H + c(k_H^*)}{k_H^*} - (x^H + h(x^H)) \geq pA_L\rho_j - \frac{F_L + c(k_L^*)}{k_L^*} - (x^L + h(x^L))$$

where x^H and x^L represent safety effort by type H and type L firms. In the absence of bankruptcy, $x^H = x^L = x^*$. Manipulating this inequality demonstrates how it depends on project quality,

$$p(A_H - A_L)\rho_j \geq \frac{F_H + c(k_H^*)}{k_H^*} - \frac{F_L + c(k_L^*)}{k_L^*} \tag{5}$$

The high-technology firm dominates when its additional output outweighs the difference in cost. Depending on the parameters, there is an interior cutoff quality $\bar{\rho}$ that partitions projects, or one firm type dominates on all projects.

H.2 Proposition 1

This argument is provided in the main text. The main text also mentions a second reason that the high-technology firm is less affected by the availability of bankruptcy. The larger sunk investment by the high-technology firm means that its efficient scale involves more projects. Because project outcomes are independent, operating a large number of projects reduces the likelihood that high-damage outcomes occur on a large share of the firm's projects. Intuitively, the probability that a firm will have accidents at all of its projects is higher when the firm has only one project compared to when it has many projects. Projects with low damage realizations help pay for projects with high damage realizations. This argument requires that expected environmental damages are less than the flow of quasi-rents from a project. When accidents are rare, this assumption is not restrictive.

H.3 Proposition 2

Low-technology firms produce projects with quality below $\underline{\rho}$. Equation 2 in the main text shows how limited liability lowers a judgment-proof firm's cost for any number of projects. Thus, it also lowers the minimum of average cost and shifts down the minimum cutoff quality for projects to be produced.

Low-technology firms produce some projects with quality above $\bar{\rho}$. By Proposition 1, $x_L \leq x_H \leq x^*$. The first inequality is strict unless both firms' revenues exceed worst-case damages. The second inequality is strict unless the H-type firm's revenue exceeds worst-case damages. Additionally, define $h^H(x)$ and $h^L(x)$ to be each firm's expected private costs from environmental damage when bankruptcy is available, as defined by the bracketed term at the right of Equation 2 in the main text. $h^L(x) \leq h^H(x)$, with equality only if both firms' revenues exceed worst case damages. With these changes, Equation 5 in this section becomes,

$$p(A_H - A_L)\rho_j \geq \frac{F_H + c(k_H^*)}{k_H^*} - \frac{F_L + c(k_L^*)}{k_L^*} + [(x^H + h^H(x^H)) - (x^L + h^L(x^L))]$$

The additional term is positive. And unless both firms' revenues are large enough to internalize all potential damages, it is strictly positive and the minimum project quality required for the H-type firm to win out increases.

H.4 Proposition 3

This argument is given in the text.

H.5 Relaxing the assumption that firms specialize

The presentation in the main text assumes that each firm specializes in one project quality type. If this assumption is eliminated, the profit function in Equation 1 becomes,

$$p\sum_{m=1}^{m=k}[A_i\rho_m - r_m] - c(k) - k(x + \mu(x)) - F_i \tag{6}$$

where $m = 1, ..., k$ indexes the projects produced by the firm. Average project costs are the same as in Equation 4. Firms choose to operate the same number of projects as in the main text, although those projects may represent a mix of project types. Landowner payments r are the same as before. Even when firms operate different types of projects, the payments to initial owners of project sites, r, are exactly the difference between project revenues (for that type of project) and average project costs (which do not depend on project quality). In order to pay any site owner more than this difference without earning negative long-run profits, a firm would have to pay another site owner less than this difference on another project. That is not possible in equilibrium because the firm will be outbid. Thus, the key results of the model are unchanged by relaxing this assumption. The reason to impose it in the main text is that it saves notation and simplifies the graphical analysis.

H.6 Capital Structure and Other Issues

The preceding sections do not address capital structure. One assumption is that firms cannot issue debt that is senior in repayment to accident damages. If so, any firm could eliminate liability exposure by issuing debt secured by all of the firm's assets, as in Che and Spier (2008). In the event of an accident, all assets would already be pledged to senior creditors. In the United States, this assumption is reasonable. For environmental costs in Texas oil and gas, the state has a lien against insolvent producers' assets that is senior to secured debt.[3] In other industries, it is difficult to foreclose on assets involved in environmental incidents, effectively subordinating a secured creditor's claim to environmental costs.[4]

[3]Texas Natural Resources Code Section 89.083.

[4]For example, a creditor who acquires a Superfund site via foreclosure faces a risk of being held liable as an owner, although legal opinions differ (Harkins, 1994; Murray and Franco, 2011).

I Back-of-the-envelope damage estimates

This section uses published estimates to value environmental damages from orphan wells. I focus on orphan wells because this is the most straightforward category of damages. Nevertheless, these values depend on many strong assumptions, and the specific numbers reported in this section should be interpreted cautiously. Appendix Table 19 summarizes the key parameters and the sources used. The remainder of this section discusses each category of damages in detail.

Groundwater Contamination Risk

Krupnick and Siikamaki (2014) use stated choice methods to measure willingness to pay to prevent oil and gas groundwater contamination. For Texas, they find that the average annual WTP to reduce the number of polluted water wells by 1,000 is $25 per household per year. This implies that aggregate willingness to pay to permanently avoid one contaminated water source is about $4 million. This calculation and other calculations in this appendix use a 5% discount rate.

Muehlenbachs et al. (2015) uses a revealed preference approach and finds a similar value. They measure the effects of oil and gas development on nearby homes that rely on groundwater wells. Their dataset does not include accidents, so their estimate reflects both a perceived risk of contamination and a cost of contamination. They find that, relative to similar homes with city-supplied water, homes with groundwater wells within 1.5 km of an oil or gas well lose 10–17% of their value after drilling. This is about $35,000.[5] The rate of confirmed groundwater contamination cases for new Pennsylvania natural gas wells is about 1%, so these home price effects imply costs of about $3.5 million per contaminated water well.[6]

Several older studies also report economic costs for groundwater contamination from a variety of pollutants, including industrial chemicals and agricultural discharge. These are reviewed in National Research Council (1997) and United States Congress Office of Technology Assessment (1984). I use the two more recent papers discussed above

[5] Groundwater-dependent homes lose $30,167 in average value; similar homes with city water connections gain $4,802, presumably reflecting the value of royalty payments. The sum of these effects is the total negative impact of perceived groundwater contamination risk.

[6] There were 154 incidents of drinking water contamination due to drilling in Pennsylvania from 2009–2014, and 16,108 new wells were drilled. Pennsylvania Department of Environmental Protection, 2015 Oil and Gas Annual Report.

because they are specific to onshore oil and gas and are more current.

While orphan wells create a serious risk of groundwater contamination, not every orphan well contaminates groundwater. Calculating expected damages requires the average probability that an orphan well results in groundwater contamination. Established estimates of this risk do not exist. As the best available benchmark, I use the number of officially confirmed cases of groundwater contamination from orphan wells and the total number of orphan wells to establish a lower bound on the likelihood of contamination. This is a lower bound because not all pollution events are detected and officially attributed. There were 24 confirmed incidents of groundwater contamination caused by orphan wells in Texas between 1993 and 2002 (Kell, 2011). The average stock of orphan wells during these years was about 1,159.[7] This implies a groundwater contamination risk of 0.21% per orphan well-year, or 2.1% across the assumed 10-year lifetime. I also consider a novel source of contamination from orphan wells that emerged during the decade following the bond requirement. Geologists and hydrologists have shown that existing orphan wells may be intersected by newly drilled, hydraulically fractured horizontal wells ("frac hits"), creating a conduit for upward migration. Brownlow et al. (2017) reports that orphan and other abandoned wells in Texas face a probability of at least 1.1% of interacting with a future fractured well. Combining these two sources of contamination risk yields expected discounted damages of $109,000. For discounting purposes I assume incidents occur at the midpoint of the orphan well's lifetime.

Climate Impacts of Methane Emissions

Methane is a potent greenhouse gas. Kang et al. (2016) reports mean methane emissions rates from orphan oil and gas wells in the United States. I value those emissions using the U.S. government's central estimate of climate damages from methane (Interagency Working Group on the Social Cost of Greenhouse Gases, United States Government, 2016). This yields discounted damages of $2,300 per orphan well.

[7] IOGCC (1996) and IOGCC (2000) report numbers of orphan wells in each state waiting to be plugged with state funds, according to the Interstate Oil and Gas Compact Commission. I use the 1996 number for 1993–1998 and the 2000 number for 1999–2002. Alternative historical numbers are available in annual RRC Oil Field Cleanup Program reports, but the definition of "orphan well" used in those historical reports includes many temporarily inactive operators and wells. The number of wells approved for state plugging better reflects the number of actual orphan wells during this period.

Public Financing of Well Plugging

Orphan wells may eventually be plugged by the state. This takes a number of years because of existing backlogs of orphan wells and limited agency resources. The cost of plugging to governments is also higher than it would be for the responsible producer. According to the Railroad Commission, "Unlike most operator well pluggings, the RRC's state well pluggers rarely know what will be encountered once a well is reentered for plugging... If records exist, they frequently provide insufficient data to determine the downhole conditions of the well... It is not uncommon for an operator to dispose of waste (junk) down the wellbore. Junk found in holes... can increase the anticipated plugging cost ten-fold."[8] In addition to these problems, public expenditures impose an efficiency cost determined by the marginal cost of public funds.

The table reports the average of reported well plugging costs from two different sources. STRONGER (2003) reports that average well plugging costs for Texas were $5,600 (in 2012 dollars). Reported expenditures on plugging and site remediation per well plugged were about $23,000 (based on Oil Field Cleanup Program reports from 2002–2013, not including program administrative costs). I use the average of these two numbers, which yields a plugging cost of $8,600 in present value terms.

Other Damages

The table omits some additional damages from orphan wells that are difficult to value. One of these is the explosion hazard. High-profile examples include home explosions in Pennsylvania due to methane from orphan wells accumulating in septic tanks and basements.[9] Interactions with new wells during drilling also pose an explosion risk. There is also aesthetic damage borne by landowners and neighbors due to abandoned, rusting oilfield equipment. Finally, states bear administrative costs of programs to remediate orphan wells. Omitting these difficult-to-quantify damages underestimates the economic costs of orphan wells.

[8] Texas Railroad Commission. 2000. "Well Plugging Primer", page 10.

[9] Kusnetz, Nicholas. "Deteriorating Oil and Gas Wells Threaten Drinking Water, Homes Across the Country." *ProPublica*. April 4, 2011.

Appendix Table 19: Back-of-the-Envelope Damages from Orphan Wells

Groundwater Contamination	
Cost of contamination	\$4,281,500[a]
Probability of leakage	2.1%[b]
Probability of drilling interaction	1.1%[c]
NPV Groundwater Damages	\$109,000
Methane Emissions to Atmosphere	
Emissions (tons)	2.72[d]
Climate Damages (\$/ton)	1,120[e]
NPV Methane Damages	\$2,300
State-financed Well Plugging	
Marginal Cost of Public Funds	1.3[f]
Plugging And Remediation Cost	\$14,300[g]
NPV Plugging Costs	\$8,600[h]
NPV Damages Per Orphan Well	\$119,900

[a] Texas household WTP to prevent contamination of one water source (Krupnick and Siikamaki, 2014), applied to 9,013,582 households in Texas (2014 American Community Survey).
[b] Calculated by the author based on Kell (2011); IOGCC (1996); IOGCC (2000). See text.
[c] Share of wells potentially interacting with hydraulic fracturing (Brownlow et al., 2017)
[d] Mean annual emissions for unplugged US oil and gas wells from Kang et al. (2016).
[e] Central estimate of climate damages from methane for 2015 from Interagency Working Group (2016), converted to year 2012 dollars.
[f] This assumption follows Bovenberg and Goulder (1996) and related papers.
[g] Average of, (1) reported per-well plugging cost from STRONGER (2003), and (2) observed per-well state spending on plugging and remediation from Texas Oil Field Cleanup Fund Annual Reports 2002–2013. Does not include program administrative costs.
[h] Present value of plugging cost 10 years in the future.

All costs are in 2012 dollars. Future costs are discounted at 5% per year, except climate costs which use discount rates from Interagency Working Group (2016). Orphan wells are assumed to persist for 10 years before being plugged by the state. Damages rounded to nearest \$100.

J Data Appendix

I construct a novel dataset on market structure and environmental outcomes based on several administrative databases from the Railroad Commission of Texas (RRC), the state agency that regulates oil and gas production.

Operators and Production

Oil and gas production data come from the RRC Production Database Query (PDQ) dataset, which reports monthly crude oil and natural gas production at the lease level. Every lease is identified by a unique combination of district id, lease id, and oil or gas indicator. The dataset also identifies the unique operator number of the firm operating the lease each lease-month. There are 257,318 leases with at least one month of non-zero production during 1993–2012, and 35,568,267 lease-month observations. I include casinghead gas (natural gas from wells that primarily produce oil) in natural gas production. I include condensate (a liquid petroleum product from gas wells) in crude oil production.

Additional information on operators comes from the RRC Organization Report dataset. All firms involved in the production of oil or natural gas must file an organization report annually by the anniversary date of the firm's first filing. I successfully merge all but 4 lease-month observations (out of 40 million) from the PDQ dataset to the Organization Report dataset based on unique operator id numbers. This yields a dataset with 15,029 oil and gas producers with at least six months of non-zero production during 1993–2012. Because my focus is on the 2002 universal bond requirement, I further limit the dataset to firms with at least one month of production during the ten years surrounding the policy implementation year (March 1997 – February 2008). This leaves 10,489 operators.

An additional field identifies firms with related ownership (for example, name changes). Of the 10,489 operators, 1,175 are reported as being related to at least one other firm. In a robustness check in the online appendix I group related observations together into 9,888 firm groups and reproduce the main results. The results are very similar to those in the main text. I choose not to collapse related operators in the main results because it is not clear how to treat some operator-specific fields when collapsing. For example, if related operators have different assigned license renewal months, the analysis in Table 4 would require me to choose which renewal month to use for the collapsed firm group.

The organization report dataset includes each operator's initial filing date, most recent filing date, and assigned month of the year for license renewal. For operators where the assigned renewal month is not provided, I use the calendar month of the initial filing date, since those dates determine renewal months. I define a firm's exit month as 12 months after its final license renewal. Specifically, I use 12 months after the final assigned license renewal month. Thus, a firm with a final observed filing date of Dec. 15, 2000 and an assigned renewal month of January would have an exit month of January, 2002 (12 months after its January, 2001 license renewal). As a quality check, I compare the filing month to the assigned renewal month for each operator. 91% of filing dates are within three months of the assigned renewal month; 73% occur in the assigned month or the two previous months.[10] In parts of the analysis where knowing the assigned license renewal month correctly is important (i.e., Figures 3–6, Tables 1–4, Table 6) I drop 899 operators (9%) with reported filing dates more than three months before or after their assigned license renewal month.

For Figure 3 only, I extend the sample back to 1990 (the earliest available year) by incorporating oil production data from the Final Oil and Gas Annuals (FOGA) datasets for 1990–1992. I do this because Figure 3 also addresses the 1991 partial bond requirement. The sample for that figure includes all operators appearing in either the FOGA or PDQ datasets. I confirm consistency of the FOGA and PDQ datasets by comparing FOGA and PDQ production reports for 1993 and 1994. For firms with final license renewal dates prior to 1994, I define exit month as 12 months after the final renewal filing.

Environmental Outcomes

Information on orphan wells comes from the RRC's publicly available Orphan Wells Database. I use the March 2014 version of the database. This dataset reports unplugged wells that have not produced for at least 12 months and for which the operator has not filed a license renewal in at least two years. The data include the well name, lease identifier, operator number, operator name, and the date of the operator's last P-5 license renewal. I successfully merge 100% of the orphan wells to the operator and production data based on unique operator numbers. The final dataset includes

[10] The insurance requirement was enforced according to the assigned renewal month, regardless of when firms filed their paperwork (as explained in the text).

3,404 orphan wells during 1993-2012. The Orphan Wells Database is not a comprehensive list of wells ever orphaned, because some orphan wells have been plugged by the state. Because I am primarily interested in the rate of well orphaning before and after a policy change, an incomplete list does not affect my analysis as long as the state did not preferentially plug wells orphaned just before or just after the rules changes. There is no reason to suspect this.

Information on field rules violations comes from the Railroad Commission's Severance Query Database. "Severance" is the RRC term for an enforcement action that shuts down production from a lease due to a rules violation. In the main analysis, I limit the data to severances for Field Rules Violations related to Statewide Rule 8 and Statewide Rule 14. Statewide Rule 8 governs water quality protection during drilling and production. Statewide Rule 14 requires that inactive wells be promptly plugged. This dataset contains the lease identifier, operator identifier, operator name, reason for the severance, date of warning letter, date of severance, and date the problem was resolved. I merge the severance data to the production data using unique operator identification numbers. I successfully merge 99.6% of Field Rules Violations since 1993. The final dataset includes 11,101 violations. About 13% of these violations are assessed after firms are no longer producing. I drop these violations in the main analysis in order to focus on effects during the period of active production. In the online appendix I show that dropping these observations has little effect on the estimates.

Data on well blowouts come from the RRC database of blowouts and well control problems. The March 2014 version includes 533 blowouts since 1990. These records are not consistently formatted. For 49.9% of blowout records, a unique drilling permit identifier is reported. I merge these records to drilling permit data, which report unique operator numbers. The remaining 50.1% of blowout records do not contain these identifiers and are thus difficult to merge reliably. I attempt to merge these records as well as possible using other fields. For 183 blowout records I identify an operator match using lease identifier numbers provided in both the blowout and monthly production data. I check these matches by comparing string fields that were not used for the match, such as the string field listing the name of the operator. For 65 of the remaining 84 blowouts, I identify a likely operator match using a fuzzy string match based on operator name and other fields.[11]

Additional Data

Data on operator-level bond choices were obtained through a public records request to the RRC. This dataset covers 1991–2012 and includes, at the operator-by-year level: the type of bond (surety bond, "Good Guy" option, etc.); the required bond amount; and the number and depth of wells.

Drilling data come from the RRC "Drilling Permit Master and Trailer" dataset. This dataset identifies all oil and gas wells drilled between 1991 and 2012. I follow Kellogg (2011) and Anderson et al. (2018) in working with the drilling data. The dataset identifies the date that each drilling permit was granted and, for wells that are completed, the date that the well began producing oil or gas. It also identifies spud-in dates (the date that drilling work began); however, as noted by Kellogg (2011), these dates are not reliably reported for a significant share of wells. I merge drilling permit data to the operator-level dataset based on unique operator numbers.

Louisiana Data for Alternative Empirical Strategy

Data for Louisiana come from the Louisiana Department of Natural Resources SONRIS database. This database reports annual production totals for oil and gas operators. It also reports information on oil and gas operators' filings of Form OR-1, which are required each year. As in Texas, I define an operator's exit date to be 365 days after the most recent OR-1 renewal. Unlike in Texas, where renewal dates are staggered throughout the calendar year, almost all Louisiana renewals (88%) occur in January. I thus have to collapse the analysis to the annual level. To match the timing of Texas' 12-month rollout from March 2002 to February 2003, I work with 12-month "policy years" that begin in March and end in February of the following calendar year.

I successfully merge 55% of the firms in the production data to the OR-1 database. The reason for this low merge rate is unclear, but the available data on the most recent license filing appear to be incomplete for some firms. I proceed using the subset of Louisiana records that merge successfully. I have also run an alternative

[11] I use the *reclink* routine for Stata (Blasnik, 2010). Fields used are the first 6 letters of operator name, full operator name, name of the oil/gas field, lease name, and lease number. Weights on matches were 1, 10, 5, 3, 10, and 8, respectively. Weights on mismatches were 15, 8, 8, 1, 4, and 1.

analysis where I define exit dates for Louisiana firms based on the last observed year of oil or gas production instead of based on license renewal. This alternative avoids the need to merge to the license renewal data, at the cost of not being consistent with the approach used for Texas. This alternative approach leads to similar results. The Louisiana data include a field for Initial Date of Operations, but this field is poorly reported: this field is missing for 39% of operators. Instead of using this field to define entry dates, I define the entry date as the first year in which the operator appears in the annual oil and gas production data, or 1989 for operators entering before 1989. Since my analysis focuses on 1998–2006, the left-censored entry dates do not affect the analysis.

Data on well blowouts in Louisiana also come from the SONRIS database. To focus on onshore production, I include all Louisiana blowouts at wells with a valid Public Lands Survey System "Township" designation. The number of active drilling rigs in Louisiana each month comes from the Baker Hughes Historical Rig Count dataset. I include rig activity for "land" and "inland water" areas. Finally, the Louisiana Department of Natural Resources maintains a list of orphan wells. This list includes operator identifiers which I use to merge the orphan well data to the exit and production data.

References

Anderson, Soren T., Ryan Kellogg, and Stephen W. Salant, "Hotelling under pressure," *Journal of Political Economy*, 2018.

Blasnik, Michael, "Reclink: Stata module to probabilistically match records," 2010.

Borusyak, Kirill and Xavier Jaravel, "Revisiting event study designs," Mimeo, Harvard University 2016.

Bovenberg, A Lans and Lawrence H Goulder, "Optimal environmental taxation in the presence of other taxes: general-equilibrium analyses," *The American Economic Review*, 1996, *86* (4), 985–1000.

Brownlow, Joshua W., Joe C. Yelderman, and Scott C. James, "Spatial risk analysis of hydraulic fracturing near abandoned and converted oil and gas wells," *Groundwater*, 2017, *55* (2), 268–280.

Cameron, A Colin and Pravin K Trivedi, *Microeconometrics: methods and applications*, Cambridge University Press, 2005.

Che, Yeon-Koo and Kathryn E. Spier, "Strategic judgment proofing," *The RAND Journal of Economics*, 2008, *39* (4), 926–948.

Harkins, Robert M, "Lender liability under superfund: New rules-old rules-no rules," *Environs: Environmental Law & Policy Journal*, 1994, *17*, 18.

Interagency Working Group on the Social Cost of Greenhouse Gases, United States Government, "Addendum to Technical Support Document on Social Cost of Carbon for Regulatory Impact Analysis under Executive Order 12866: Application of the Methodology to Estimate the Social Cost of Methane and the Social Cost of Nitrous Oxide," Technical Report 2016.

Interstate Oil and Gas Compact Commission, "Produce or Plug: The Dilemma over the Nation's Idle Oil and Gas Wells," 1996.

_ , "Produce or Plug: A Study of Idle Oil and Gas Wells," 2000.

Kang, Mary, Shanna Christian, Michael A. Celia, Denise L. Mauzerall, Markus Bill, Alana R. Miller, Yuheng Chen, Mark E. Conrad, Thomas H. Darrah, and Robert B. Jackson, "Identification and characterization of high methane-emitting abandoned oil and gas wells," *Proceedings of the National Academy of Sciences*, 2016, *113* (48), 13636–13641.

Kell, Scott, "State Oil and Gas Agency Groundwater Investigations And Their Role in Advancing Regulatory Reforms-A Two-State Review: Ohio and Texas," Technical Report, Oklahoma City: The Ground Water Protection Council 2011.

Kellogg, Ryan, "Learning by drilling: Interfirm learning and relationship persistence in the Texas oilpatch," *The Quarterly Journal of Economics*, 2011, p. 39.

Krupnick, Alan and Juha Siikamaki, "Would you pay to reduce the risks from shale gas development? Public attitudes in Pennsylvania and Texas," *Resources*, 2014, *185*, 38–43.

McCrary, Justin, "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of Econometrics*, 2008, *142* (2), 698 – 714.

Muehlenbachs, Lucija, Elisheba Spiller, and Christopher Timmins, "The housing market impacts of shale gas development," *American Economic Review*, 2015, *105* (12), 3633–59.

Murray, Milissa A and Sandra Franco, "Treatment of environmental liabilities in bankruptcy," in James B Witkin, ed., *Environmental Aspects of Real Estate and Commercial Transactions*, American Bar Association, 2011.

National Research Council, "Valuing ground water: Economic concepts and approaches," Technical Report, Committee on Valuing Ground Water 1997.

STRONGER, *State Review of Oil and Natural Gas Environmental Regulations: Texas State Review*, Environmental Protection Agency and Interstate Oil and Gas Compact Commission, 2003.

United States Congress Office of Technology Assessment, "Protecting the Nation's Groundwater from Contamination," Technical Report OTA-O-233 1984.