Writing Sample

The attached writing sample is my final-year economics thesis. This is a causal inference paper first-authored by me with two other co-authors. We attempted to estimate how variations in population sizes across US states causally impacted these states' capacity to produce patents.

The project was devised and implemented by me, along with help with literature review and data gathering by my two co-authors. (Group submission was a course requirement.)

This 9-page sample (not including this page and references) omits a subsection on robustness tests in the original paper. Sections and subsections such as Introduction; Theoretical Framework and Literature; Institutional Setting; and Results were shortened, while others consolidated, in order to reduce length. Minor edits were also made for correctness or clarity. A copy of the original paper can be downloaded at https://arxiv.org/abs/2211.00410.

ECON 4200 Senior Seminar in Economics and Finance
Population and Technological Growth: Evidence from *Roe v. Wade*

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1 Introduction

Some economists have argued that a greater population base causes higher technological growth and therefore higher per capita income, whether that is due to network effects such as intellectual contact and specialization spurring innovation or due to the need to sustain a larger population. Meanwhile, others have argued that per capita income decreases as population grows, due to competition over a fixed set of resources and greater dependency. Furthermore, it is commonly known that cross-sectional empirical evidence shows countries with higher population growth having lower income. This paper will attempt to validate the first position by providing evidence that a greater population leads to more technological growth in the form of patent production.

We find that for cohorts born between 1931-1984, a higher starting population at birth is correlated with higher patents per thousand residents between 1996-2012. In order to rule out the endogeneity of fertility decisions and estimate the causal effect of cohort births[1] on patent production, we exploit the heterogeneous impact of the US Supreme Court's ruling on *Roe v. Wade*, which ruled most abortion restrictions unconstitutional. Our identifying assumption is that states which had not liberalized their abortion laws prior to *Roe* would experience a negative birth shock of greater proportion than states which had undergone pre-*Roe* reforms. We estimate the difference-in-difference in births and use estimated births as an exogenous treatment variable to predict patents per capita. Our results show that one standard deviation increase in cohort's starting population (70,608 births) increases per capita patents by 0.23 (which is 24 percent of the outcome's standard deviation). These results suggest that at the margins, increasing fertility can increase patent production. Insofar as patent production is a sufficient proxy for technological growth, increasing births has a positive impact on technological growth.

This paper builds on two fields of research: the first is theories on the relationship between population and innovation, whose contributors include Michael Kremer, Simon Kuznets, and David Weil. Second, we add to the study of determinants of innovation in the US by Bell et al. Here, we would also like to acknowledge Bell et al. for making the data set of their study open-source and allowing this instance of alternative use. We should also note that although our research uses changes in abortion policy as an exogenous change in births, this paper and its results *do not* pertain to the issue of abortion itself. Insofar as we prove that fertility has a positive impact on technological growth (which we will argue is the case), we establish that states have good reason to promote births. We are silent on the optimality of abortion policy as a natalist tool for increasing technological growth.

Our paper will proceed as follows. Section 2 discusses the background of our research. We will review the relevant theoretical literature on population, technological growth, and income. Section 3 describes the various data sets we combine and use, and estimate the correlation between births and patents. Section 4 covers the institutional setting of abortion laws in the US and attempts to justify the use of the *Roe* ruling as an exogenous shock in births. In Section 5, we will describe our methodology for and results from estimating the causal effect of births on patents. We will also analyze where our methodology falls short and discuss its major limitations. Section 6 concludes.

2 Theoretical Framework and Literature

Our hypothesis that population and technology have a positive relationship follows Kremer's model and evidence on this subject. Kremer offers two main views. First, he co-opts the "Malthusian assumption that technology limits population" (Kremer 1993, 681) to argue that high population forces the adoption of "new" technology

[1] We use cohort starting population and cohort births interchangeably throughout this paper.

to replace "old" technology (i.e. technology insufficient for supporting a given level of population). Research productivity, under this view, would depend on the level of existing population and we should see proportionality between the growth of these two variables (Kremer 1993, 681-682). To support this, Kremer shows that eras with greater population bases also have higher population growth rates. In other words, because of the positive effect of population base on technological growth, humanity has been able to afford super-exponential population growth. Second, Kremer rejects the view that subsequent rises in income would have reduced efforts to invent new technologies (Kremer 1993, 684). Instead, he argues that research productivity depends positively on income (Kremer 1993, 687). That high population without income is insufficient for achieving technological growth also explains why densely populated countries such as China had (as of 1990) low research productivity.

Kremer's model and results contradict the general view that population growth reduces per capita income. Thomas Malthus has argued that larger populations will eventually fail due to the inadequacy of resources. Kremer's arguments are also contrary to economic growth models such as the Solow and Harrod-Domar models, which both predict that societies with higher population growth will see lower levels of per capita income (Williamson 2014, 222-5; 248-9; Ray 1998, 51-6). More recently, Weil has argued that as the populations of developing countries age, increasing fertility could actually lead to less per capita income in the short-term as dependency increases (Weil 1999, 253).[2] Galor and Weil (1999) have argued that even if Kremer is correct that a greater population base leads to more technological growth, this growth will subsequently reward investments in human capital that will (i) have a greater role driving subsequent technological growth and therefore (ii) lead households to prioritize the quality of children over quantity, explaining low levels of population growth in developed economies. Our task is to argue that the population-induced growth is still significant even in the context of a developed economy. However, it is beyond the scope of this paper to compare the size of population effects to the size of human capital investment's effect on innovation.

Similar to Kremer, Kuznets has argued that productivity per capita, and by extension innovation per capita, should increase with a larger population. All else equal, the productivity per capita of a country with higher population should increase as higher population density allows for greater division of labor and specialization, and the "possibility of more intensive intellectual contact" (Kuznets 1960, 325-327). Furthermore, Kuznets notes that a growing population increases the domestic market size; in other words, a greater population acts as a greater financial incentive for productivity growth. Such network effects suggest that population growth and patent production is non-linear (i.e. greater populations see higher patents per capita and exponentially more patents), which is precisely the hypothesis we aim to prove in the US context.

3 Data and Baseline Results

3.1 Data

Table 1. Descriptive Statistics by Cohort, State, and Year (unless otherwise specified)

Statistic	N	Mean	St. Dev.	Min	Max
Patents granted per 1,000	41,578	0.887	0.976	0.000	12.751
Age	41,578	47.390	15.395	20	80
Population by Year	41,578	60,075.030	71,862.380	1,710	504,530
Births by Cohort, State	41,578	64,779.690	70,608.410	1,223	998,198
Per Capita Income by Cohort, State	41,578	3,032.193	3,060.466	122	19,701

In order to estimate the effect of births on innovation, we assemble, construct, and merge several data sets. We use Bell et al.'s open-source panel data on patent outcomes. The panel data tracks patent outcomes across three units of observations. First, patent outcomes are tracked for each cohort born between 1916-1984; as patent outcomes are clustered by cohort (rather than at an individual level), cohorts are also the main unit of observation. Second,

[2] There is also a strand of literature on demographic transition which argues that as economic development improves, income increases, and child mortality decreases, households require less children as an investment into old-age security and therefore desire less children. However, here, the focus is on the relationship from income to population, rather than from population to income. We focus on the latter as our main interest is in determining what drives technological (and therefore economic) growth. For more, see Robert J. Barro and Gary S. Becker. "Fertility Choice in a Model of Economic Growth." *Econometrica* 57 (1989): pp. 481-501.

patent outcomes for each cohort are reported once per year, between 1996-2014. For example, we separately know how many patents the 1970 cohort were granted in 1996, 1997, and so forth. The data set only includes cohorts aged 25-80 each year. We track outcomes by year because a given cohort's propensity to produce patents is highly dependent on the cohort's age. Finally, patent outcomes for each cohort in each year are reported separately for each US state. To extend on the previous example, one row in the data set tells us how many patents were granted in 1996 to the 1970 cohort in California (versus, say, in Oregon). Patents granted in a year refer to patents that were applied in that year and subsequently granted even if the latter occurs in later years. Because the data only captures patent records from 1996-2014, patents applied in latter years that are granted after 2014 are not included as granted. Thus, compared to USPTO national data, the aggregate of patents granted in our data set tapers after 2011 (Figure 1). We have estimated our results without observations in 2012-2014 and found no significant difference. We therefore retain all years of observations despite the discrepancy.

Figure 1

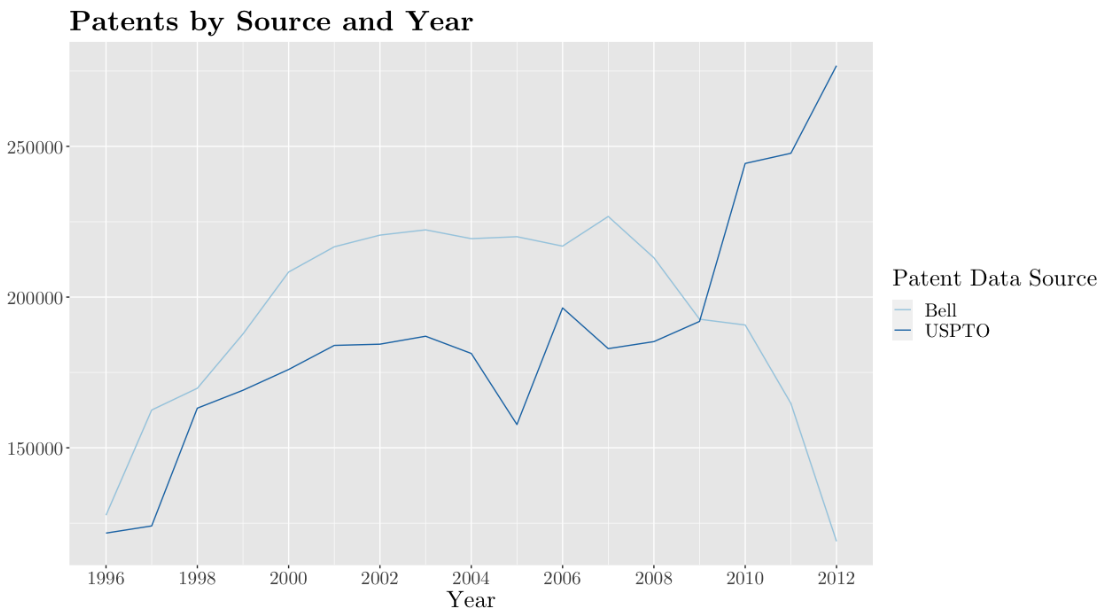


Table 1 reports summary statistics. Our outcome of interest is patents granted per thousand people by cohort, state, and year. The mean of the outcome is 0.887. We use patents adjusted by population as an outcome as a proxy for research productivity. For two groups with a given number of people, if group A is granted more patents than B, then group A has higher research productivity. Other variables of interest from the data set include each cohort's estimated population in each year in a given state. If having a larger cohort population at birth causes the cohort to later create more patents, all else equal, then a larger population base would be causing higher research productivity. The mean count of residents by cohort, state is 60,075.

We then combine patent outcomes with birth statistics tracked by the CDC's Vital Statistics of the United States reports. Births are reported by state and by cohort. On average, 64,780 children are given birth to in each cohort by state. This provides us with an estimate of a cohort's population at birth, which not only is essential to our estimation of population effects on patent production for each cohort, but also allows us to compare how much each cohort's population has changed since birth. Because birth data is available starting in 1931, we limit our initial sample of cohorts accordingly. We also merge per capita personal income by state published by the Federal Reserve, so as to control for income's impact on each state's average reproductive decisions.

3.2 Model Specification

We first estimate the following equation of patents per thousand people on the determinants of patent production:

$$p_{i,s,t} = \text{Birth}_{i,s}\delta + \text{Migrate}_{i,s,t}\beta + \text{Inc}_{i,s}\rho + \text{Age}_{i,t}\alpha + \text{Age}^2_{i,t}\lambda + \gamma_t + \epsilon_{i,s,t}$$

Here, $p_{i,s,t}$ refers to patents granted per thousand people to an observation, which is a given cohort i living in state s in the year t. $Birth_{i,s}$ refers to the number of births in a state for a given cohort year. $Migrate_{i,s,t}$ refers to the ratio of current residents in an observation to $Birth_{i,s}$. The way to interpret the ratio is that since the starting population for each cohort is fixed throughout its lifetime, differences between the current resident count and births is most likely attributable to migration, either among states or in and out of the US. This allows us to attempt to hold constant the effect of job-motivated relocations or high-skilled immigration on an observation's patent production.

$Inc_{i,s}$ refers to a state's per capita personal income recorded for the year during which a cohort is born. Controlling for an observation's state per capita income at birth has two purposes: (i) it allows us to control for reproductive choice differences across states that are driven by the differences in the ability to afford childrearing and (ii) it can imprecisely control for differences in human capital investment driven by income that can lead to different patent production outcomes. Unfortunately, including more recent income would lead to over controlling, as a state's per capita income in year t is some function of labor and capital input and technology; controlling for per capita income in year t would essentially be controlling for the outcome. The same applies to most time-variant economic variables, where $t > i$.

Given that patents granted quadratically rise and fall with a cohort's age, we include $Age_{i,t}$ as a quadratic term to control for shortfalls in patents granted to earlier and later cohorts that are likely due to their being too old and young, respectively. Finally, γ_t are year dummies. We do not add cohort or state fixed effects, as the variation in birth levels are *across cohorts and states.* Controlling for cohorts, i.e. looking at variation within cohorts, would not be sensible. The same applies to state fixed effects.

3.3 OLS Results

We estimate the aforementioned model and its simpler variations using OLS (see Table 2). We estimate heteroskedasticity-robust standard errors. A cohort's starting population is significantly and positively correlated with patents granted to that cohort. The significant and positive relationship of births is robust across various specifications. Cohorts with larger populations tend to be granted more patents per capita. Using the full model's coefficient, increasing births by one standard deviation (70,608) predicts an increase of 0.15 in patents per thousand (or 15 percent of the outcome's standard deviation). This is of course not causal: this relationship may be driven by a co-moving variable increasing both patent production and births, such as parents' research productivity. Another explanation could be that younger cohorts simply produce less patents on one hand, while younger cohorts are also smaller in population due to exogenous declines in birth nationally on the other. This would make births and outcomes correlated by coincidence. To infer causality, we turn to exogenous birth shocks in the next section.

We should note a few more things from the basic estimation. As expected, the more a cohort's population swells as a result of migration, the more likely it is that it receives more patent grants per person. This could either be due to the network effects of high population, or the states having high research productivity attracting more residents. That state's per capita income at birth is significantly negative on patents granted is surprising, though the coefficient relatively lacks economic insignificance. One possible reason is that holding a cohort's starting population and migration fixed, income of the state from the cohort year has little impact on that cohort's present patent production. Another reason is likely data structure. On one hand, state per capita income is higher in the years of birth of younger cohorts. On the other, younger cohorts also have less lifetime patent production between 1996-2012 due to their being younger.

4 Causal Inference with Abortion Law as a Fertility Shock

4.1 Institutional Setting

In January of 1973, the Supreme Court of the United States ruled in the case *Roe v. Wade* that women have a right to seek an abortion without state interference in the first trimester of their pregnancy. Before this ruling,

Table 2. Relationship Between Patents Granted per Capita and Birth

	Dependent variable:		
	Patents per 1,000		
	(1)	(2)	(3)
Births	0.000002***	0.000002***	0.000002***
	(0.0000001)	(0.0000001)	(0.0000001)
Migration		0.10***	0.10***
		(0.005)	(0.005)
State Per-Capita Income at Birth		−0.00003***	−0.00004***
		(0.000002)	(0.000002)
Year Fixed Effects	No	No	Yes
Age Controls	Yes	Yes	Yes
Heteroskedasticity-robust Wald stat	5288.7***	3270.6***	771.47***
Observations	41,578	41,578	41,578
R^2	0.20	0.20	0.22
Adjusted R^2	0.20	0.20	0.22
Residual Std. Error	0.88 (df = 41574)	0.87 (df = 41572)	0.86 (df = 41556)

*p<0.1; **p<0.05; ***p<0.01

around 1970, approximately two-fifths of states had already undergone liberalization reforms to their abortion laws as part of a series of criminal law reforms recommended by the legal profession at the time. We take advantage of the ruling as a heterogeneous negative shock on births which impacted states which had yet to undergo reforms, more than states where abortion law was already reformed. We assume both the pre-*Roe* reforms and the *Roe* ruling itself were sufficiently exogenous. In this sub-section, we briefly describe the state of abortion law before the *Roe* ruling to justify our assumption.

According to Boonstra et al. (2006) from the Guttmacher Institute, there were 17 states which had liberalized abortion laws in some form of another. Before these reforms, abortions were generally criminalized at all stages of pregnancy across states (Buell 1991, 1787). 13 of the 17 states expanded exceptions which made abortions permissible; under the reforms, pregnancies could generally be terminated if they threatened the mother's health, could result in a child with severe disability, or were the consequence of rape or incest (Tyler 1983, 245). The remaining four states decriminalized abortions and made them available to women who requested them (subject to physician approval), regardless of justification (Buell 1991, 1798-9; Tyler 1983, 247). Table 3 shows the dates of reforms for which there are records.

We determine that the reforms were exogenous because they were initiated by the American Law Institute, which was publishing a Model Penal Code, a series of criminal law reforms, one of whose many subjects included abortion law (Buell 1991, 1796; Tyler 1983, 245; Boonstra et al. 2006, 12). The Model Penal Code was influential and generally adopted by states without partisan disagreement. To this end, we estimate a probability model of a state having bans on abortion (i.e. not having undergone reform) with logistic regression. The vote share data is obtained from UC Santa Barbara's American Presidency Project. The results are in Table 4. The status of having undergone reform is not predictable by voting behavior. Although state per capita income in 1971 as a predictor has moderate statistical significance, it is not economically significant.

Separately, the Supreme Court's ruling expanded abortion access far beyond what was legislatively achieved, as it declared that the right to privacy included the right to an abortion and left the judgement of abortion's suitability during the first trimester solely to the physician. A latter ruling found that features shared by most reformed laws pre-*Roe* violated this right, e.g. the requirement of two physicians jointly approving the abortion (Tyler 1983, 248; 249). Furthermore, an overwhelming majority of public opinion as of 1969 was opposed to permitting abortions for households which would have financial difficulties raising children or for those which simply did not prefer additional children (Blake 1971, 541). For this reason, the liberalization of abortion by *Roe* appeared to be the result of jurisprudence that was uncorrelated with the pre-*Roe* appetite for legislative reforms or public opinion at the time. We therefore believe that *Roe* as an event satisfies the assumption of exogeneity.

Table 3. Timing and Type of Abortion Law Reforms by State

	State	Reform_Year	Reform
1	Alaska	1970	Decriminalization
2	Arkansas	1969	Expanded exceptions
3	California	1967	Expanded exceptions
4	Colorado	1967	Expanded exceptions
5	Delaware		Expanded exceptions
6	Florida		Expanded exceptions
7	Georgia		Expanded exceptions
8	Hawaii	1970	Decriminalization
9	Kansas		Expanded exceptions
10	Maryland		Expanded exceptions
11	New Mexico	1969	Expanded exceptions
12	New York	1965	Expanded exceptions; decriminalization in 1970
13	North Carolina	1969	Expanded exceptions
14	Oregon	1970	Expanded exceptions
15	South Carolina		Expanded exceptions
16	Virgina		Expanded exceptions
17	Washington	1970	Expanded exceptions; decriminalization in 1971

The table combines information on abortion law before Roe from Boonstra et al. (2006), Buell (1991), Gold (2003), Milman (1970), and Tyler (1983).

Table 4. Determinants of Abortion Bans

	Dependent variable:
	Probability of State Keeping Abortion Banned
	Logit
Vote Share for Rep., 1968 Presidential	−6.467
	(7.927)
Vote Share for 3rd Parties, 1968 Presidential	−8.993
	(7.290)
Vote Share for Rep., 1972 Presidential	−0.571
	(8.902)
State Per-Capita Income in 1970	−0.002**
	(0.001)
Constant	12.396**
	(5.957)
Observations	50
Log Likelihood	−27.724
Akaike Inf. Crit.	65.449

*p<0.1; **p<0.05; ***p<0.01

4.2 Specification: DID

We estimate the DID of patents per thousand with the following model:

$$p_{i,s,t} = (\text{Ban}_s \times \text{Roe}_i)\theta_1 + \text{Ban}_s\theta_2 + \text{Roe}_i\theta_3 + \text{Migrate}_{i,s,t}\beta + \text{Inc}_{i,s}\rho + \text{Age}_{i,t}\alpha + \text{Age}^2_{i,t}\lambda + \gamma_t + \epsilon_{i,s,t}$$

The model is the same as our original model, except that *Births* is replaced by an interaction term of two dummies, where $Ban_s = 1$ for states s which banned abortions up until Roe, and $Roe_i \forall i \geq 1973$.

Separately we also use the interaction term as an instrumental variable, with which we estimate the following first-stage equation of a two-stage least squares (2SLS) estimation:

$$\text{Births}_{i,s} = (\text{Ban}_s \times \text{Roe}_i)\ell_1 + \text{Ban}_s\ell_2 + \text{Roe}_i\ell_3 + \eta_{i,s}$$

The resulting $\widehat{Births}_{i,s}$ is used to to estimate the original model. As it will become clear, we felt it was necessary to isolate the impact of *Roe* on cohort starting sizes specifically. This is the case because as we will see, even if the two groups of states followed the parallel trends before *Roe*, we think it is possible that they might have diverged after *Roe*. This likely would have been the case due to the cumulative effect of policy changes that occurred at the state, and not to mention county and municipal level, after *Roe*.

5 Results

Column 1 of Table 5 displays the results from the vanilla difference-in-difference model. Contrary to our expectation, a cohort born in a state with pre-*Roe* abortion bans (which should have experienced a negative shock in fertility) were not granted less patents per thousand people later in their lifetime. In fact, the estimated coefficient is weakly positive, which suggests that non-reform states on average, compared to reform states, saw slightly greater positive change in patents per capita after *Roe*.

Table 5. Estimating Patents Granted per Capita on Births using Roe v. Wade and Pre-Roe Bans

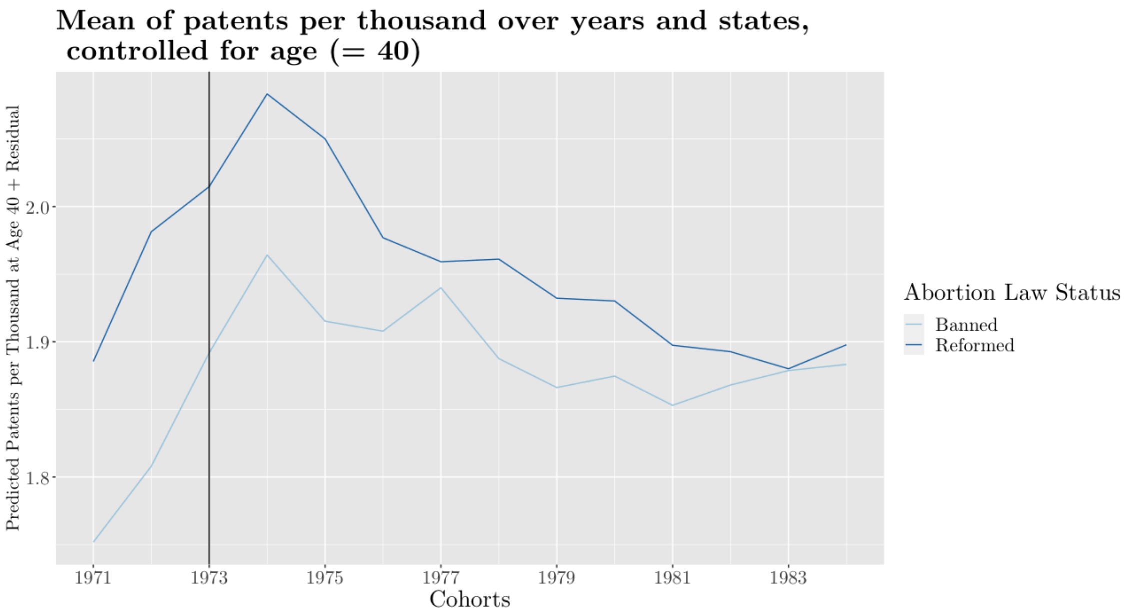
	Dependent variable:		
	Patents per 1,000 DID	Births 2SLS First-stage	Patents per 1,000 2SLS Second-stage
	(1)	(2)	(3)
Post-Roe × Pre-Roe Ban	0.12^{**}	$-2{,}675.00$	
	(0.06)	(4,346.54)	
Births			0.000003^{***}
			(0.000001)
Migration	0.19^{***}		0.23^{***}
	(0.03)		(0.03)
State Per-Capita Income at Birth	0.00005^{***}		0.00002^{***}
	(0.000005)		(0.00001)
Year Fixed Effects	Yes	No	Yes
Age Controls	Yes	No	Yes
Heteroskedasticity-robust Wald stat		51.971^{***} (df = 3; 9692)	
Observations	9,696	9,696	9,696
R^2	0.29	0.02	0.29
Adjusted R^2	0.29	0.02	0.29
Residual Std. Error	0.69 (df = 9672)	69,035.17 (df = 9692)	0.69 (df = 9674)

*p<0.1; **p<0.05; ***p<0.01

To understand these results better, Figure 2 plots mean of patents per capita over states and years in each group. Mean patents per capita over years here is the prediction–from estimating said outcome as a quadratic function of cohort's mean age–where every cohort is assumed to be age 40, plus the residual from said estimation. The outcome is then averaged across the control and treatment groups, respectively.

We observe two things: first, parallel trends held in the first two years. The optimistic interpretation is that the effect of pre-*Roe* reforms (mostly from before 1970) had already realized their effects on pre-1971 cohorts, and therefore we see cohorts from reformed states following the same trends as cohorts from non-reformed states. The pessimistic interpretation is that those reforms had yet to realize their impact and did not until *after Roe*, which would violate parallel trends. Assuming parallel trends of births hold for now, the second observation is that not only did non-reform states (i.e. the states "treated" by the *Roe* shock) not see a drop in patent outcomes,

Figure 2



they actually saw a rise in patent per capita outcomes relative to reform states. This would explain the positive interaction coefficient. This could be explained by the violation of parallel trends more broadly, and more specifically by non-birth factors that occurred predominantly to one of two groups after *Roe*.

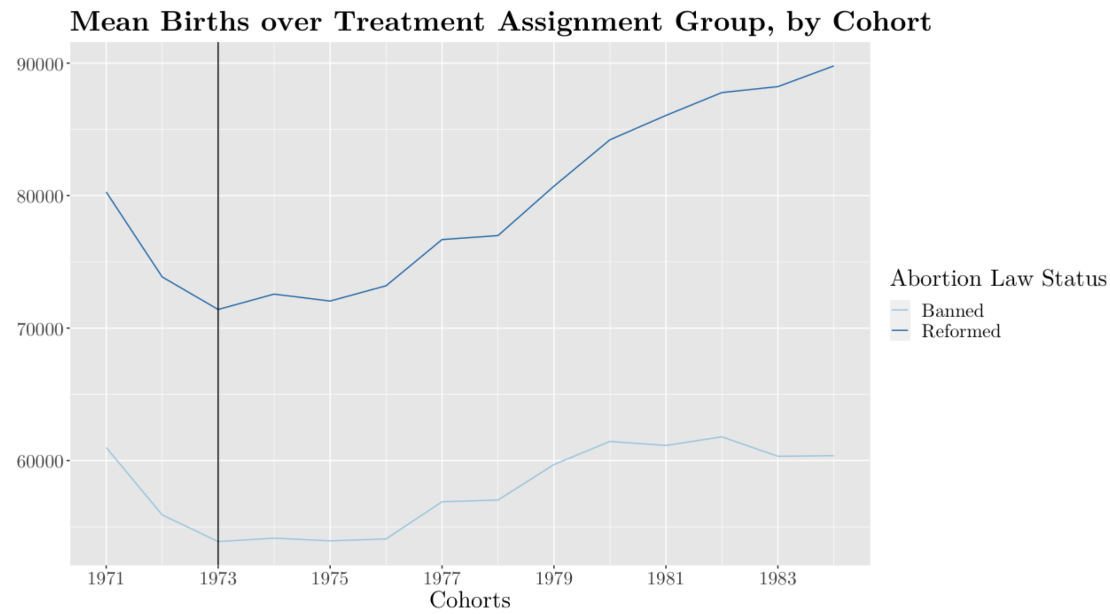
Parallel trends at-large likely did not hold in the post-*Roe* period. For this reason, we need an approach that isolates the effect of the *Roe* ruling on births exclusively. We therefore turn to the results from using difference-in-difference after *Roe* as an instrumental variable for estimating births as the treatment variable in a 2SLS setup. This would hopefully allow us to limit the scope of the parallel trends assumption to births only. Assuming the assumption holds, we can obtain a causal estimate of population effects on innovation from this setup.

The results are also shown in Table 5. Column 2 shows that the DID interaction term has a negative sign as we expect, but lacks statistical significance. The heteroskedasticity-robust Wald test statistic of the first-stage regression suggests that our IVs are not weak. However, most of the statistical significance of the predictors can be attributed to the dummy for classifying reform and non-reform states, rather than the interaction term. To understand the strength of the *Roe* IVs better, we plot births by cohorts and abortion law status in Figure 3.

First, we can immediately see that reform states began with a higher level of births, which explains the strength of the *Roe* IVs as a whole at predicting births. Second, we also see that non-reform states did experience less births after *Roe*, as we hypothesized. Third, birth trends in non-reform and reform states for cohorts after *Roe* did not diverge until after 1978. This might be attributable to reforms happening too close to the *Roe* ruling and their delayed impact, as we discussed earlier in our diagnosis of the DID results. Even if the pre-*Roe* abortion law reforms were exogenous, if their negative effects on birth did not fully realize until after *Roe*, then *Roe*'s effect on the non-reform states woukd be underestimated (as the delayed effect of the pre-*Roe* reforms in the reform states would have driven up the average number of births pre-*Roe*, leaving the change in births in reform states greater, and therefore the change in births in non-reform states relatively smaller). We experiment with shifting the post-*Roe* cutoff to 1978, and the interaction term's heteroskedasticity-robust t-value in the first-stage regression does indeed become stronger, going from -0.62 to -1.56.

Our discussion from the previous section suggests that our IV is likely to underestimate the negative effect of births on non-reform states. Nonetheless, the second-stage regression estimates larger cohorts to have a significant and positive effect on patents outcome. One standard deviation increase in births is predicted to increase patents per capita by 0.23 (or 24 percent of one standard deviation of the outcome). Another indicator suggesting a causal estimate is that the coefficient for state per capita income at a cohort's birth is positive, which aligns with our expectation (i.e. richer cohorts produce more patents due to exposure to parents with high research productivity

Figure 3



or higher levels of human capital investment) and suggests that if the effect of income were captured by births in the basic estimation (which would bias our estimate of population's effect), this is no longer the case.

6 Conclusion

This paper has presented evidence on how a larger population base affects technological growth by using the US Supreme Court's decision to legalize abortion for the first trimester nationwide in 1973 as a negative shock on births which disproportionately affected states which had not relaxed abortion laws prior to the ruling. To justify both the ruling and the status of pre-*Roe* reforms as exogenous, we showed that states adopted reforms to their abortion law as one part in a series of criminal law reforms suggested by legal professionals in the 1960s, and that the Supreme Court's ruling was more far-reaching than most of these reforms or public opinion at the time. Using DID of the ruling as an IV for births, we have found that increasing births by one standard deviation increases patents per capita by 0.24 standard deviation. The results are consistent with Michael Kremer's theory that a larger population base causes higher levels of technological growth. We have analyzed why our instrumental variable lacks statistical significance and attribute this to the effect of pre-*Roe* reforms being realized in a delayed manner in the period which coincided with the *Roe* ruling's shock on births, leading to an underestimation of *Roe's* relative impact on births in non-reform states. We have also analyzed why our simple DID estimates have returned estimates with signs contrary to our hypothesis.

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