9 Abortion access and risky sex

Jonathan Klick and Thomas Stratmann

INTRODUCTION

Is risky sex subject to the law of demand? To an economist, such a question is trivial, while the non-economist is likely to think the question is absurd. According to the economist, all goods are subject to the law of demand, and risky sex is a good; therefore, we should expect to see the demand for risky sex declining as its cost increases. Non-economists, however, are likely to scoff at such a notion. Sex in general and risky sex in particular is driven by emotions, and hormones, but rational cost-benefit analysis is likely to be absent even on the margin.

As an empirical matter, examining the sensitivity of risky sex incidence to changes in costs and benefits is tricky. First, there is no reliable data on the incidence of risky sex. For many years, there are no real data available at all. Even when data have been collected, however, there are serious concerns about the data's integrity. Second, it is hard to quantify the inherently subjective costs and benefits associated with sexual activities. Further, even if some metric were available, unobserved heterogeneity across individuals would likely generate statistical identification problems.

However, in a series of papers, we have developed partial solutions to both of these problems, allowing us to identify the relationship between risky sex behavior and at least one major cost of risky sex – the risk of unwanted pregnancy. In this chapter, we describe this work and provide some extensions of it that strengthen the conclusion that risky sex activity does increase when the costs of that sex decline.

This work has value beyond mere prurient interests. Primarily, it underscores the universality of basic microeconomic intuitions. Namely, incentives matter. They matter when you are deciding whether to buy toilet paper. They matter when you decide whether to smoke a cigarette. They matter when you contemplate having casual sex and whether you decide to use a condom while having sex.

ABORTION AS A COST SHOCK

While the costs and benefits of engaging in risky sex are fairly idiosyncratic, one cost stands out in this context – the cost of an unwanted pregnancy. The magnitude of this cost, however, will exhibit heterogeneity. For example, a financially comfortable couple might perceive an unwanted pregnancy as being lower cost than will a couple engaging in casual sex, and a teenage girl may perceive the cost as being even higher yet. However, most individuals will view an unwanted pregnancy as generating some cost, almost by definition.

From an empirical standpoint, the magnitude of the individual's subjective cost is unobservable. Further, even if it were observable, it would be difficult to use that metric to estimate the causal relationship between cost and the demand for risky sex, because there are likely to be a number of confounds that are unknown to the researcher that are correlated with both the perception of the cost of an unwanted pregnancy and other elements of the demand for risky sex. For example, individuals with high discount rates may really enjoy risky sex and have low estimates of the cost of an unwanted pregnancy. Or, perhaps, individuals who are very risk averse may not enjoy risky sex as much, and they will expect a greater loss of utility arising from the possibility of an unwanted pregnancy resulting in financial burdens. Generally, there is no way to account for these kinds of confounds, leaving the researcher with an omitted variables bias in the attempt to isolate the causal effect of cost on risky sex behavior.

The legalization of abortion, as well as subsequent changes in abortion access, however, provides a shock to the cost of an unwanted pregnancy. That is, once abortion becomes available, many individuals will lower their estimate of the cost of an unwanted pregnancy, since abortion allows for the termination of that pregnancy. To be sure, the cost will not decline to zero, since having an abortion will generate both financial and psychic costs of its own, but at least some individuals will expect those costs to be lower than the cost of continuing the pregnancy. In this way, abortion is a kind of insurance policy or ex post birth control.[1] If legalization and subsequent abortion policy changes are orthogonal to the unobservable confounds, these policy changes can be used as an instrument for the cost of risky sex in order to determine the causal relationship between cost and behavior. While it is not possible to be certain that these abortion law changes are conditionally orthogonal (which is required for drawing a causal inference), the proliferation of econometric studies of the effects of changing abortion laws suggests that many economists do believe the adoption of these laws is sufficiently random for use in a wide range of social and economic applications.[2]

GONORRHEA AS A PROXY FOR RISKY SEX

Data on sexual behavior are limited and unreliable. The path-breaking data collection of Alfred Kinsey was limited in geographic scope and periods covered, as well as its statistical sophistication.[3] More recent surveys are also limited in how frequently they ask questions regarding risky sex behaviors. Perhaps more important, there are reasons to be suspicious of self-reported indicators on risky sex behavior, as survey respondents may systematically under-report their actions due to shame or systematically over-report in order to boast. Since there is no way for researchers to independently validate the answers of even a subsample of respondents, normal methods of bias correction are not possible in this context.

However, Klick and Stratmann (2003) note that sexually transmitted disease (STD) data potentially provide a suitable proxy for risky sex since STDs are likely to be a monotonic function of risky sex behavior. Further, at least in the US, the Centers for Disease Control track many STDs systematically, providing a high-quality comprehensive data source. Among STDs, gonorrhea has many attractive properties for use in this context.

1 See, for example, Levine and Staiger (2002).
2 For an overview of this explosion of research, see Klick (2004).
3 Ibid.

Specifically, the latency period of gonorrhea is relatively short compared with other STDs, ensuring that diagnosis quickly follows infection. Also, as compared to some other STDs, gonorrhea is easily treated, and, once treated, it cannot be transmitted again until the individual gets infected again. These characteristics imply that the timing of gonorrhea diagnosis is very closely related to the timing of gonorrhea infection, which in turn is very closely related to the timing of risky sex behavior.

THE EFFECT OF LEGALIZATION IN THE US

Abortion on demand was legalized nationally in 1973 by the Supreme Court in its *Roe v Wade* decision. However, California had previously legalized abortion on demand in 1969, and Alaska, Hawaii, New York, and Washington followed suit in 1970. Using legalization as a shock to the cost of risky sex, Klick and Stratmann (2003) estimated the relationship between these shocks and gonorrhea incidence in a sample of the 50 states and District of Columbia (DC)[4] during the period 1963–80.

By controlling for state fixed effects (to net out the baseline gonorrhea rate in each state) and year dummy variables (to account for any non-linear national trends), as well as a host of control variables, in their regression, Klick and Stratmann (2003) provide a natural quasi-experimental research design of the hypothesis that risky sex increases when the cost of unwanted pregnancy declines.[5]

Klick and Stratmann's (2003) Table 3 suggests that the effect of abortion legalization on the incidence of gonorrhea per 100,000 state residents was to increase it by 82 cases. This represents an increase of just under 27 percent on a population mean of 305 cases per 100,000 population. We report that these results are statistically significant at $p < 0.01$. The results, while slightly smaller in magnitude, are qualitatively similar if we allow for individual state-level linear trends. Results do not change if the sample is restricted to 1963–75 to focus on the period where law changes occurred without adding extra post-change observations.

However, in retrospect, advances in panel data econometrics suggest that we may have been too liberal in our statistical inference. Specifically, subsequent work on serial correlation in panel data applications suggests that t statistics may be incorrect if researchers do not account for the fact that many policy variables exhibit inertia and many outcomes of interest are also serially correlated, generating dependence that needs to be accounted for when performing inference. That is, if there is positive dependence in a model's error terms, t statistics will be biased upwards, while negative dependence will generate a downward bias.[6]

A commonly used correction for this problem is to cluster standard errors on the cross-

---

[4] There is some debate as to whether DC was a de facto legalizer pre-*Roe*. See Klick and Stratmann (2003), fn. 13 on this point. Note that none of the following results are affected by coding DC as a legalizer or by dropping the DC observations.

[5] Weighted least squares was used to estimate the model using state population as the weight. This is appropriate to handle heteroskedasticty, wherein the more populous states are likely to generate lower sampling variation.

[6] See Bertrand et al. (2004).

Table 9.1 Estimates allowing for state-level clustering

	1963–1980		1963–1975	
	(2)	(2) with Trends	(2)	(2) with Trends
Abortion legal	81.71**	61.38**	72.97*	43.39**
	(38.11)	(26.90)	(38.03)	(19.21)
Controls	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
State effects	Yes	Yes	Yes	Yes
State trends	No	Yes	No	Yes

Note: All regressions include controls for education, income, transfer payments, percentage of population made up of 15–34 year olds, percentage of population that is black, alcohol consumption, and no-fault divorce as described in Klick and Stratmann (2003). (2) with trends is not the same specification presented in that paper since it includes all of the control variables, but (2) is identical to the column 2 specification. All regressions are estimated with population weights.
** $p < 0.05$ for null hypothesis of zero effect in a two-tailed test.
* $p < 0.10$ for null hypothesis of zero effect in a two-tailed test.

Source: Klick and Stratmann (2003; Table 3).

sectional unit, which in this case would be the state unit. If Klick and Stratmann's (2003) primary regressions are re-estimated, allowing for clustering on the state level, we find the results presented in Table 9.1.

Once this dependence is accounted for, the estimated abortion effect is still statistically significant. However, concerns have been raised about a failure to allow for dependence in the cross-section as well as in the time series for an individual unit.[7] This concern may be particularly acute here since the timing of changes occurs, more or less, in two clusters: the 1969–70 legalization and then the national legalization in 1973.

Table 9.2 provides the same estimates allowing for multi-way clustering within a state and within a year. The estimated standard errors increase slightly in the trend models, but still remain statistically significant at better than the 10 percent level. In the no trend models, the standard errors actually decline.

Another concern has been raised regarding the asymptotic properties of the test statistics using clustering to account for dependence.[8] Specifically, while the Bertrand et al. (2004) results suggested that clustering "works" if one has 50 states upon which to cluster, this is an artifact of their applications. The sufficient number of clusters is determined by the data-generating process and is a priori unknown. A more conservative approach is offered by the wild cluster bootstrap, which estimates an empirical test statistic distribution by taking the data and imposing the null of no effect of the variable of interest, generating a synthetic outcome sample that is orthogonal to the treatment variable (in this case, abortion legalization). The method then resamples from the synthetic outcomes, sampling all observations from a given cluster to maintain any dependence present in the

7 See Cameron et al. (forthcoming).
8 See Cameron et al. (2008).

Table 9.2 Estimates allowing for multi-way clustering

	1963–1980		1963–1975	
	(2)	(2) with Trends	(2)	(2) with Trends
Abortion legal	81.71**	61.38*	72.97**	43.39*
	(35.18)	(33.60)	(36.82)	(22.78)
Controls	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
State effects	Yes	Yes	Yes	Yes
State trends	No	Yes	No	Yes

Note: All regressions include controls for education, income, transfer payments, percentage of population made up of 15–34 year olds, percentage of population that is black, alcohol consumption, and no-fault divorce as described in Klick and Stratmann (2003). (2) with trends is not the same specification presented in that paper since it includes all of the control variables, but (2) is identical to the column 2 specification. All regressions are estimated with population weights. Standard errors are clustered on state and on year using the procedure discussed in Cameron et al. (forthcoming).
** $p < 0.05$ for null hypothesis of 0 effect in a two-tailed test.
* $p < 0.10$ for null hypothesis of 0 effect in a two-tailed test.

Source: Klick and Stratmann (2003; Table 3).

original sample, adding a mean zero random error term and then regressing the synthetic outcomes on the treatment variable and all control variables. By construction, the estimate of the treatment effect is centered on zero. The original t statistic is then compared to this synthetic t distribution to determine how extreme the estimated result is relative to the variation observed in the synthetic distribution. This method has been shown to allow for valid inference even when the number of clusters is small.

Using this approach on the specifications presented above, in the 1963–80 sample, if state trends are not included, the abortion legalization effect is statistically significant ($p < 0.08$) if symmetry is imposed on the significance test (i.e., magnitude of upper critical value = magnitude of lower critical value). In the more sensible approach of placing equal portions of the Type 1 error in each tail (i.e., do not enforce symmetry on the empirical distribution), the effect is statistically significant at the 5 percent level.

The results reject the null of no effect even more soundly once state-level trends are included. Specifically, in the case of symmetric critical values, $p < 0.015$. If symmetry is not imposed, and equal density is placed beyond each critical value, the null of no effect is rejected ($p < 0.01$).

Klick and Stratmann's (2003) results hold up against these more modern inference techniques. However, the causal interpretation of our results relies on the assumption that legalization was truly a shock. That is, the timing of legalization must be conditionally independent. While this seems to be a reasonable assumption for the 1973 legalization, the earlier legalizations may be more questionable. New York and California particularly may have experienced changing social beliefs that led to both more risky sex and abortion legalization. While the results including state trends help mitigate this possibility, the causal interpretation of these results should be made with caution since the background trends could have been more complex than the linear trends can account for.

PARENTAL INVOLVEMENT LAWS

To address the potential endogeneity of the abortion law changes, Klick and Stratmann (2008) exploit changes in parental involvement laws which affect one group of decision-makers in a state (young women who have not reached the age of majority), but leave a useful comparison group unaffected (older women). Parental involvement laws require a minor wishing to get an abortion either to involve their parents in the decision, through notification or consent requirements, or to seek judicial approval to bypass that involvement. Clearly, when these laws go into effect, the psychic or transaction costs of obtaining an abortion rise, increasing the cost of an unwanted pregnancy. If laws are endogenous to changes in background beliefs, leading a state to pass such laws and to the residents of the state independently engaging in less risky sex, the estimate of the law effects will be biased, and no causal interpretation can be made. However, this endogeneity story should affect the behavior of younger and older women alike, so the availability of a comparison group of similar women who are unaffected by the law directly allows us to control for these unobservable state-level background changes.

We first implement a differences-in-differences strategy where we simply control for the average relationship between the gonorrhea rates of affected and unaffected women within a state. If this relationship is comparable across states, this approach will eliminate the omitted variable bias. We also allow for state-specific trends in the gonorrhea incidence of minor women to mitigate this bias as well. We find evidence of the negative relationship between these restrictions on abortion access for both Hispanic women and white women. In both cases, we find large (21 percent increase in gonorrhea due to the law change for Hispanics and 12 percent for whites) and statistically significant effects. The results for black women, while consistent in the sign of the effect, are neither relatively large nor statistically significant. In the results presented in Klick and Stratmann (2008), we account for serial dependence through clustering. However, once again, there may be dependence among the observations across states within a given year as well. For example, if individuals engage in relationships across state lines and legal changes have some spatial correlations, there will be positive dependence. Changes in the way Centers for Disease Control (CDC) collects its data year to year could also generate dependence.

In Table 9.3, we present the Hispanic and white results from Klick and Stratmann (2008), allowing for multi-way clustering in both the state and year dimensions. As seen above, the standard errors grow for both sets of results, implying positive dependence. In fact, the white results are no longer statistically significant even at the 10 percent level once the multi-way clustering is accounted for, reducing our confidence in the white results.

If we apply the wild cluster bootstrap routine discussed above, the results fare better. Evenly dividing the Type 1 error between the tails, the involvement law coefficient in the regression using data on Hispanic women is statistically significant at $p < 0.05$. If we perform a single-tailed test of the hypothesis that the parental involvement law coefficient is zero or greater, we can reject this hypothesis at $p < 0.015$.

Using the data for white women and the wild cluster bootstrap, if we evenly distribute the Type 1 error across the two tails, we reject the hypothesis that the parental involvement law generates zero effect at $p < 0.05$. If we perform the one-tailed test, we reject the hypothesis that the effect is zero or greater at $p < 0.02$.

Table 9.3 Estimates allowing for multi-way clustering

	Hispanic	White
Involvement law	−12.05	−9.54
	(6.51)*	(6.08)
Controls	Yes	Yes
Year effects	Yes	Yes
State effects	Yes	Yes
State trends	Yes	Yes

Note: The dependent variable is the number of gonorrhea cases among women aged 19 and below in a given state for a given year. All regressions include controls for the within-state gonorrhea rate among women aged 20 and above as described in Klick and Stratmann (2008). All regressions are estimated with population weights. Standard errors are clustered on state and on year using the procedure discussed in Cameron et al. (forthcoming).
** $p < 0.05$ for null hypothesis of zero effect in a two-tailed test.
* $p < 0.10$ for null hypothesis of zero effect in a two-tailed test.

Source: Klick and Stratmann (2008; Table 2).

Klick and Stratmann (2008) also estimate a complete triple differences model, using the within-state comparison group of unaffected women and include state-specific non-linear trends in gonorrhea incidence. This very demanding specification suggests that changing state-level unobservables are not driving the estimated involvement law effect.

However, perhaps the strongest evidence for a causal effect of the abortion laws on behavior comes from exploiting that fact that a number of states reverse their involvement laws via state courts or state attorneys general. If unobserved changes in state social beliefs drive both the adoption of the laws and changes in risky sex behavior, when relatively independent state courts or officials throw the laws out, we should observe a continuation of the gonorrhea decline among minors since, in this story, the law's enforcement is irrelevant. In fact, we find that gonorrhea rates among minors return back to pre-law baselines, supporting the causal interpretation of our estimated effects.

DIAGNOSIS EFFECTS

An additional concern arises when interpreting our results as providing causal evidence for abortion access leading to behavioral changes. Klick and Stratmann (2003) suggest that these results may simply be picking up changes in the diagnosis of gonorrhea as opposed to a change in underlying incidence (and, by extension, behavior). Specifically, because abortion providers generally test for STDs prior to performing an abortion, if more (fewer) people get abortions, more (less) of the underlying cases will be identified even if there is no change in the true incidence. Yoeli (2007) provides some evidence for this effect.[9]

In the 2003 paper, we assert that if this diagnosis effect were driving our results, we should see differential effects for women and men, since men do not seek abortions

[9] Available at http://www.yoeli.net/std.pdf.

before or after legalization. We provide evidence suggesting that the change in gonorrhea incidence for men is not statistically different from that observed among women when abortion is legalized. A problem with this argument, however, is that women who test positive for gonorrhea may inform their sexual partners, who will in turn get tested, leading to a parallel diagnosis effect among men. We cannot rule out this possibility.

However, in the 2008 paper, there is evidence against this diagnosis claim that we failed to highlight. Specifically, in our specifications that include a control for when the involvement law is enjoined, we find that gonorrhea rates converge back to the pre-law baseline. This is inconsistent with a diagnosis story. That is, if abortion restrictions simply lead to less testing but no change in behavior, the stock of gonorrhea cases will grow because they are no longer diagnosed and treated. Once the law is enjoined, and young women begin to get abortions again, the incidence should reflect this increase as a positive value for the coefficient on the enjoin variable. We do not observe a positive coefficient, suggesting that the stock of gonorrhea cases has not grown during the restricted period.

CONCLUSION

While no individual empirical design is perfect, Klick and Stratmann (2003 and 2008) provide complementary designs that have different strengths and weaknesses, using different sample periods and different legal changes, and yet converge on similar results. Namely, changes in abortion access appear to lead to changes in an indicator of risky sex behavior in the predicted directions. When abortion access improves, individuals appear to engage in more risky sex. When access worsens, individuals engage in less risky sex. This supports the notion that individuals respond to incentives even in this context. Emotions, hormones, and other a-rational effects do not tell the whole story, even among teenagers.

In addition to validating the use of rational choice analysis in non-traditional contexts, these results have practical applications as well. Namely, they strengthen the case for incorporating price theory intuitions in epidemiological models of disease spread, as suggested in Kremer (1996). The general practice in these models is to assume behavior that is essentially static. These results suggest that predictive and analytical gains could be made by simply taking incentive effects into account. With improved models, presumably public policy interventions can be targeted more effectively with respect to STDs.

As for evaluating abortion policy itself, it is not clear that these results have much to add to the debate. Any welfare analysis will hinge on fundamentally normative decisions regarding the costs and benefits of risky sex. Also, because these studies cannot distinguish between the possibility that the results are driven by less fastidious condom (or other barrier methods that protect against pregnancy) use or less sex generally, even analysts willing to make normative evaluations would not have enough information to evaluate abortion policy on the basis of this research.

REFERENCES

Bertrand, Marianne, Esther Duflo, Sendhil Mullainathan. "How Much Should We Trust Differences-in-differences Estimates?" *Quarterly Journal of Economics,* 119(1): 249–75 (2004).

Cameron, Colin, Jonah Gelbach, and Douglas Miller. "Bootstrap-based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, 90: 414–27 (2008).
Cameron, Colin, Jonah Gelbach, and Douglas Miller. "Robust Inference with Multi-way Clustering," *Journal of Business and Economic Statistics* (forthcoming).
Klick, Jonathan. "Econometric Analyses of U.S. Abortion Policy: A Critical Review," *Fordham Urban Law Journal*, 31: 751–82 (2004).
Klick, Jonathan and Thomas Stratmann. "The Effect of Abortion Legalization on Sexual Behavior: Evidence from Sexually Transmitted Diseases," *Journal of Legal Studies*, 32(2): 407–34 (2003).
Klick, Jonathan and Thomas Stratmann. "Abortion Access and Risky Sex Among Teens: Parental Involvement Laws and Sexually Transmitted Diseases," *Journal of Law, Economics, and Organization*, 24(1): 2–21 (2008).
Kremer, Michael. "Integrating Behavioral Choice into Epidemiological Models of the AIDS Epidemic," *Quarterly Journal of Economics*, May: 549–73 (1996).
Levine, Phillip and Douglas Staiger. "Abortion as Insurance." NBER Working Paper No. 8813 (2002).
Yoeli, Erez. "Does Birth Control Promote Promiscuity or Improve Health Care? Evidence from STDs." Working Paper (2007).