

Abortion Context and Women's Contraceptive Use

In their article “State Abortion Context and U.S. Women's Contraceptive Choices, 1995–2010” [2015, 47(2):71–82, doi:10.1363/47e23015], Josephine Jacobs and Maria Stanfors use data from the 1995 and 2010 cycles of the National Survey of Family Growth to examine associations between state abortion policies and women's contraceptive use patterns, and assess whether these changed over time in response to increased abortion restrictions. The authors conclude that “women living in states with more restrictive abortion contexts tend to use highly effective contraceptives.” This provocative statement implies a causal relationship between abortion restrictions and contraceptive use, but does not correspond with the authors' findings. The authors also conclude that increases in abortion restrictiveness are not associated with changes in use of highly effective methods. However, we have several concerns about their framework, their measures of state abortion restrictions and, in turn, their assessment of the extent to which these restrictions changed over time.

The rational choice framework used by the authors assumes that the costs of unintended pregnancy were greater for women in states with more restrictive abortion laws and that, as a consequence, more women in these states were motivated to use highly effective contraceptive methods (sterilization, the IUD or the pill). Therefore, several of the primary independent variables took into account whether a woman resided in a state with any of several restrictive abortion laws in 1995 and 2010. However, a closer look at the authors' classification scheme (Appendix Table 1) suggests that they overstate the extent to which state laws were hostile to abortion in 1995 and that they inaccurately assess change over time.

Using their source material (NARAI's *Who Decides?*^{1,2}), we found that some states were misclassified. In the article, a state was considered “less hostile” if it had 0–2 abortion restrictions, “moderately hostile” if it had three restrictions and “hostile” if it had four or more. California apparently was classi-

fied as moderately hostile in 1995 because of three policies: a pre-*Roe* abortion ban, a law requiring parental consent for a minor's abortion and a restriction on abortion at 20 weeks' gestation. However, none of these was in effect in 1995. The pre-*Roe* abortion ban and the parental consent requirement had been struck down by courts, and the 20-week restriction had been ruled unenforceable by the attorney general. In the absence of those restrictions, California should be classified as less hostile. Similarly, the authors considered Texas hostile; however, we could find only three restrictions in the source material, which would mean classifying it as moderately hostile. We found similar issues for the counts in 2010. For example, in at least some states (California, Kansas, Nevada and New Hampshire), it appears that the authors counted policy changes that occurred prior to 1995 in their assessment of state policy changes for the 1995–2010 period. These examples of misclassification in both study years raise concerns about the classification of all states, as well as the extent to which the authors accurately measured change over time in the subsequent analysis.

The authors generated three sets of multivariate models examining associations between use of effective contraceptives and different measures of abortion context. In the first, they find that women living in states with low access to abortion services (a measure that took into account the proportion of women in the state living in a county without a provider) were slightly more likely than those living in states with high or medium access to use highly effective methods, rather than no method. However, in the state fixed effects models, which took into account unobserved state-level determinants, this association disappeared. The same pattern is observed in models that examined associations between contraceptive use and other measures of abortion context—abortion hostility and three specific abortion restrictions (mandatory delay, parental consent and Medicaid funding restrictions).

Because of the misclassifications mentioned above, it is unclear if the initial associations between abortion restrictions and contraceptive use are valid. Still, the fact that the associations disappear in the models that take state effects into account should have led the authors to conclude that women living in states with more restrictive abortion contexts were at no greater or lesser risk of using highly effective methods than those in less restrictive states. Instead, they conclude the opposite. They argue that the most likely explanation for a lack of an association between changes in abortion policy and contraceptive use is that “the states introducing...restrictive legislation already [had] significant limitations on abortion in place, and that women living in these states already [had] adjusted their behavior to these restrictions.” In doing so, they overlook a more simple explanation: that abortion restrictions have no causal relationship with the use of highly effective contraceptive methods.

We also suspect that the authors' rational choice framework led them to overlook more obvious explanations for some of the associations (or lack thereof) in their analyses. Their rational choice model assumes that women had full knowledge of their state's abortion laws and of the effectiveness of various contraceptive methods. However, research has demonstrated that individuals have imperfect knowledge about both.^{3–5} Moreover, if highly effective methods are not easily accessible to women, then perfect knowledge is of little use. During the time period covered by the study, the most effective contraceptive methods had regular monthly costs (in the case of the pill) or high up-front costs (in the case of the IUD and sterilization); as a result, for some poor and low-income women, less effective methods were the only option. We contend that a more appropriate model for attempting to explain how state policies potentially impact contraceptive use would have taken into account factors that are intended to directly affect this outcome. The authors explain that the fixed effects models “took into consideration that states

may differ regarding important unobserved state-level determinants of contraceptive use.” In this case, we suspect that the state fixed effects models controlled for factors such as state differences in availability of publicly funded family planning services, health care infrastructure and poverty. Once these were taken into account, associations between abortion context and use of effective contraceptive methods disappeared.

While it is possible that abortion policies influence women’s contraceptive use patterns, this hypothesis is not supported by the analyses by Jacobs and Stanfors. We encourage the authors to more carefully consider their conclusions in light of the above criticisms.

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Jacobs and Stanfors reply:

We would like to thank Rachel Jones and Elizabeth Nash for their letter regarding our article. We are grateful that they took the time to make a number of points that warrant further discussion and clarification. In particular, they raise concerns about whether our conclusions imply causality, about our measures of state abortion restrictions and how these have changed over time, and about our use of a rational choice framework. We address each of these concerns below.

We agree that our study did not find a causal relationship between abortion context and contraceptive use; indeed, we state this and discuss the issue of causality in our Limitations section, on page 80. In this section, we conclude that as a result of our methodological choices, “we did not attempt to infer causality in a strict sense.” Instead, we found a cross-sectional association that was not consistent in the difference-in-differences model with state fixed effects.

There are a number of possible explanations for the inconsistencies between the cross-sectional and difference-in-differences models. One, as Jones and Nash point out, is that there simply is no effect of abortion context on contraceptive behavior and that the association we see is due to state-level factors that we have not accounted for. We state this quite clearly in our Discussion, on page 80, where we note that “the associations we identified may be attributable to other time-invariant state-level variables—for example, other reproductive health policies or women’s attitudes.” However, we also provide an alternative explanation, noting that identification of difference-in-differences models relies on the states that changed over time. If transitioning states did not experience significant contextual changes, or if the amount of time that passed between the policy changes and the survey was long enough so that women could adjust to the changes—conditions that were present in our analysis—one might not expect to see a significant effect of the policy changes in our models. The importance of policy timing has been noted in the literature examining the impact of no-fault divorce laws, which demonstrates immediate, but not long-run, effects on divorce rates in the United States.^{1,2} We felt that pointing out these possible explanations for our findings was just as important as pointing out the possibility that there is no effect.

This leads to another concern that Jones and Nash discuss, the accuracy of our abortion context classification. Jones and Nash highlight specific states that they claim are misclassified. According to the criteria we outline on page 73, however, these states are not misclassified. For example, Jones and Nash note that we classified California as moderately hostile in 1995 because of three policies that were in place. They classify these policies as “a pre-*Roe* abortion ban, a law requiring parental consent for a minor’s abortion and a restriction on abortion at 20

weeks’ gestation.” In actuality, as outlined in Appendix Table 1, we do not claim that California had a parental consent law in place in 1995. The three restrictions to which we refer are an unconstitutional pre-*Roe* abortion ban, an unconstitutional ban on abortions prior to fetal viability and a requirement that abortions be performed by physicians—all of which are included in the source material.³ In our base cases analysis, we included these in our hostility variable because, as the source material states, these bans were unenforceable but had not yet been repealed. We feel that if the authors revisit the source material in light of the criteria outlined on page 73 of our article, these inconsistencies will be explained.

We note that even if including unconstitutional measures in our hostility index is a misclassification, as Jones and Nash suggest, this would not have impacted our results. First, as we point out on page 73, “we combined the less hostile and moderately hostile categories” in our base case multivariate analyses. This implies that we are picking up variation only between that combined group and more hostile states. Second, acknowledging that the inclusion of unenforceable restrictions is a potential limitation of our hostility index, we conducted numerous sensitivity analyses. As noted on page 79, in our Multivariate Findings section, we altered the definition of abortion hostility categories and the number of categories in the index. These specifications included groupings where low hostility was defined as one, two or three restrictions; where high hostility was defined as five or more restrictions; and where the number of categories ranged from two to four. Our results were robust to these alternate specifications.

In our article, we draw on a rational choice framework.⁴ Jones and Nash contend that one of the limitations of this theoretical framework is that it requires the subjects in the model to be perfectly informed. Rational choice models are simplifications of reality, and perfect information is a simplifying assumption, not a description of actual circumstances. An absence of perfect information in the real world does not nullify how predictive these models may be. Furthermore, we used multiple abortion context variables (i.e., provider access, specific abortion policies and overall policy hostility). Although women may not have perfect information about all abortion policies, they may very well have sufficient and correct information regarding some of these variables. We also controlled

for education and age (admittedly, imperfect proxies for knowledge) and conducted sub-analyses among women with different levels of education and income, and in various age-groups, some of which we discuss on page 79. Further, the use of a multinomial outcome, in which hormonal methods are compared with other and no methods, does not imply the need for knowledge about absolute levels of contraceptive effectiveness. Instead, women in our framework would be required only to be aware of the relative effectiveness of different hormonal methods as compared with other methods or no method.

Finally, Jones and Nash bring up the point that contraceptive access is an important determinant of contraceptive choice. We note that we considered access differences by income and conducted subanalyses among low-income and uninsured women, although these groups were often too small for the types of analyses we were conducting. We strongly agree that access to contraceptives is central to contraceptive choice. Indeed, the concluding sentence of our article states this quite clearly.

We disagree, however, that it is “more appropriate” to consider state policies that would be “intended to directly affect” contraceptive behavior. Levine makes a strong case that “changes in the availability and accessibility of abortion have the potential to change sexual activity and/or contraceptive behavior.”^{4(p. 39)} Downstream effects of many types of reproductive health policies may impact contraceptive decisions, and to overlook these potential effects would imply an incomplete and possibly erroneous evaluation of the potential costs of these policies.

The letter from Rachel Jones and Elizabeth Nash encouraged us to revisit our analyses and results. We hope that this response provides some clarification about our study. The discussion about the role of policy in women’s reproductive health is important, and more research is needed, not only in the United States, but also from a global perspective. Finding the appropriate data and trying to evaluate the impacts of policy measures is challenging, and various approaches and perspectives are needed to fully reflect the

complexity of this research issue. Our article is one contribution to a much wider literature that we hope will help to inform the debate.

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