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Same-Sex Marriage and Negative Externalities: A Critique, Replication, and Correction of Langbein and Yost

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LINK TO ABSTRACT

> If no statistically significant adverse effect can be found, then the argument that same-sex marriage poses a negative externality on society cannot be rationally held.
>
> —Laura Langbein and Mark Yost (2009, 292–293)

> It follows that there can be no rational argument against these laws based on the alleged negative consequences of gay marriage for "family values."
>
> —Langbein and Yost (2009, 293)

> The argument that same-sex marriage poses a negative externality on society cannot be rationally held.
>
> —Langbein and Yost (2009, 292)

The remarkable quotations above come from "Same-Sex Marriage and Negative Externalities," an article published in *Social Science Quarterly*. As of December

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2014, Langbein and Yost's article had garnered thirty-three Google Scholar citations, and it was cited favorably in a legal decision overturning California's Proposition 8 on same-sex marriage (*Perry v. Schwarzenegger* 2010, 47, 92).

An important debate in family law rages over same-sex marriage. As of August 2014, nineteen states plus the District of Columbia have instituted same-sex marriage. Other states have had bans overturned, but the decisions have been stayed during appeal.[3] Most of these developments have happened after 2012. Such a radical change in an ancient institution prompts the question: What consequences will follow? Supporters have claimed that extending a right of equality, freedom, and liberty to gays and lesbians is costless and will have no adverse consequences. Opponents have argued that there could be dire negative outcomes.

The first attempt to empirically investigate the potential harm of same-sex marriage on traditional family outcomes in the U.S. was the 2009 article by Langbein and Yost.[4] They were interested in whether or not same-sex marriage laws or bans on same-sex marriage imposed any "negative externalities" on society. Their underlying framework was a presumption of liberty. As they put it:

> A basic understanding of economic theory regarding externalities and personal choices implies that in the absence of negative externalities, there is no reasonable rationale for government to regulate or ban those choices. (Langbein and Yost 2009, 292)[5]

To investigate the consequences of changes in marriage law, Langbein and Yost used a series of reduced form regressions to see whether state same-sex marriage laws had a negative correlation with various family outcomes. In particular, they looked for an effect on marriage rates, divorce rates, abortion rates, births out of wedlock, and the percent of households headed by women. They found either no effect or positive effects, and drew the conclusion that a concern over the externalities of same-sex marriage cannot be rationally held.

But the Langbein and Yost study is methodologically seriously flawed in two ways. First, it fails to spell out an actual externality mechanism to test. We articulate two broad categories of possible externality channels found in the literature, and we show that the empirical strategy of Langbein and Yost is suitable for testing

3. In November 2014, the U.S. Court of Appeals for the Sixth Circuit reversed lower-court decisions that had overturned same-sex marriage bans in Kentucky, Ohio, Michigan, and Tennessee. Until the U.S. Supreme Court rules on the matter of same-sex marriage directly, the flux will continue.

4. Recently, other studies have attempted the same thing. Trandafir (2014) finds heterogeneous effects on marriage rates in the Netherlands when examining individual-level data, and no effect using aggregate data. Dinno and Whitney (2013) find no effect on marriage rates across the United States.

5. Although we believe the presumption-of-liberty framework is inapt and debatable, we leave this criticism for others to make. There are other reasons for having state involvement in marriage (see Allen 2010).

neither. Second, the test they conducted has low power because all but one of their observations are prior to any legal implementation of same-sex marriage. That is, there is essentially no period *after* implementation in which to find an effect.

In addition to these problems with Langbein and Yost's method, there are coding issues in their data, misreported procedures, and unreported sensitivity to estimation methods. Exploring these empirical issues demonstrates that their results lack precision and power. The results are simply not robust enough to draw any conclusion regarding the effect of same-sex marriage over the outcomes examined, let alone a general conclusion that laws concerning same-sex marriage have no effects. Their claim "that there can be no rational argument" is therefore wrong.

What is the externality channel?

In order to test for an externality, it is necessary to identify the channel through which such an effect would be manifest. In the literature debating same-sex marriage, two such channels are mentioned, and here we label these as "general" and "specific" externalities:

1. A *general* externality influences social norms and is beyond, or transcends, the law.
2. A legally *specific* externality works through state-specific legal details.

A general externality results from the fact that marriage is a social institution that depends only in part on family law. Marriage is the larger set of social constraints imposed by social norms, religious organizations, family members, and the individual couple. These larger social constraints often are functions of the legal institutions, and so changes in legal rules about what is denominated and certified as marriage can have a direct impact on the social rules regulating marriage—and any impact on social rules is very unlikely to remain bound within the given jurisdiction where the law changed.

A specific externality results from reactions to changes in family law by individuals living within the jurisdiction that made the changes. It is critical that the legal changes be binding on everyone in order for a specific externality to exist. Many different specific externalities are possible, and they depend on the legal context.

Here we go through a specific example of each potential type of externality, and argue that the Langbein and Yost empirical strategy fails to adequately address either category.

General externality

Langbein and Yost would appear to have a general externality notion in mind. Their presentation on the nature of an externality is based solely on an obscure journal article written by Aristides Hatzis (2006), a proponent of same-sex marriage.[6] Hatzis's claim is that "moral" people do not like the "immoral" behavior of other people, and this dislike can lead to "detrimental effects to the social order." Here is the quotation on externality from Hatzis as reproduced by Langbein and Yost:

> The externality argument against same-sex marriage (and against any "immoral" activity for that matter) goes like this: A part of the cost of the voluntary but "immoral" activity spills over onto "moral" people, who are annoyed by the way of life of "immoral" people. … Then, the way of life or the acts of some people can be said to offend the majority. Their acts or transactions have negative external effects of such magnitude that they can have detrimental effects to the social order itself. (Hatzis 2006, 58; quoted in Langbein and Yost 2009, 294)

Langbein and Yost write in response:

> The problem with this line of argument is that there is hardly any type of behavior or action that could not seem to cause "harm" to others because harm is being defined to include disliking or despising the behavior or actions. This is not an economically acceptable view of harm. (Langbein and Yost 2009, 294)

The Langbein and Yost response is weak. All values are the result of preferences and so utility can fall for reasons other than physical harm. Therefore, it is by no means obviously illegitimate to take the moral objection seriously, as in the cases of laws against cruelty to animals and many other issues. Even those who favor the liberalization of prostitution, for example, will usually support local restrictions on where a brothel may operate, simply because those who would otherwise live next door to the brothel would dislike what goes on there. As for marriage, many in various faith traditions consider it to be a sacred sacrament and covenant that is literally defined as being between one man and one woman. Official recognition of

6. Hatzis (2006) was published in *Skepsis*, a Greek philosophy journal that is not indexed by Web of Science. Leaving Langbein and Yost (2009) and self-citations by Hatzis aside, Google Scholar lists only one journal article as citing Hatzis (2006), such article appearing in the Peruvian journal *Revista de Economía y Derecho* in 2007.

same-sex "marriage" could be an abomination to these groups. Such an anticipated harm could warrant officialdom's refraining from certifying same-sex marriage. Others may diminish these sentiments, and pay little regard to the fact that they relate to cultural traditions spanning hundreds, even thousands of years. But in economic terms such sentiments correspond to a reduction in utility that presumably should be considered in a cost-benefit analysis.

Langbein and Yost thus raise the "immoral" externality, but they find it hard to make a causal connection between annoyed "moral" people and any decline of secular marriage. For example, in the context of same-sex marriage laws interfering with marriage incentives to have children within traditional marriage, they say "this seems like a somewhat stilted and strange claim" and "we do not pretend that we can construct a convincing causal story" (ibid., 296).

Had they looked further, they could have found examples in the literature of a general externality with a reasonable causal connection to marriage outcomes. The institution of marriage, though a function of the law, is larger than the law regarding the terms of entering and exiting marriage. A legal change that recognizes same-sex couples as "married" could change the cultural and social meaning of marriage for everyone, and therefore change both well-being and behavior.[7] It has been argued, for example, that same-sex marriage accentuates the view that marriage is based on love, not children and commitment. When such a view is generally adopted it can have effects on marital behavior in general. Persons in loving relationships might be quicker to marry, and married persons who come to consider their relationship to be unloving might be more willing to divorce. Hence, marriage and divorce rates might change through this general change in social norms which could result from same-sex marriage.

Regardless of what the actual general mechanism might be, our methodological point is that the Langbein and Yost reduced-form equation procedure fails to test for *any* type of general externality. Langbein and Yost examine the effect of a given state ban or permission to marry on the behaviors of individuals *within the same state*. But the concept of general externalities concerns itself with effects occurring within and beyond state boundaries. To use Hatzis's terminology, "moral" Americans who happen to live in Idaho may be disturbed by "immoral" activity in Washington, and may change their behavior accordingly. Indeed, surveys of public acceptance for same-sex marriage show an increase in acceptance across the country as more states adopt such laws (see McCarthy 2014).

If same-sex marriage changes the cultural meaning of marriage for everyone, then a change to the law in one state may have wide-ranging effects beyond state

7. Such matter of social or moral consequences is raised by Badgett (2009, 7); Blankenhorn (2007, 152); Gallagher (2004, 69); Glenn (2004, 25); Kurtz (2004); and Stewart (2008, 323).

borders. But Langbein and Yost look for effects only within the state that legally changed. This measurement error leads to a bias in their coefficients towards zero, since only the state legally certifying same-sex marriage would be coded as having changed, when *de facto* changes occurred in other states as well. Thus, their empirical strategy is an improper test for a general externality.

Specific externality

The specific externality channel arises because of the nature of family law—everyone within a jurisdiction is bound by the same family laws. Same-sex marriage is logistically more complicated than a simple decree. Often many legislative acts are required to be updated, but more importantly, many definitions must be changed as well. These changes can be complicated, counterintuitive, and unanticipated (Hunter 2012). These changes, made to accommodate the biological similarity of same-sex couples become automatically binding on opposite-sex couples as well—hence the specific externality.[8]

An example may help illustrate the specific externality effect. Historically, marriage law bound the act of sex and procreation together through a legal doctrine usually called 'presumption of paternity.' If a man was married to a woman and she gave birth to a child, he was presumed the father with all of the rights and responsibilities that went with it. Such a presumption of paternity makes no sense for a same-sex couple. That is, the act of sex and the outcome of children *must* be legally separated for same-sex married couples, and such separation may then be generally applied in marriage law.

Specific externalities thus can result from a strange characteristic of family law: within a given jurisdiction, everyone is under the same marriage regulations.[9] Therefore, legal changes made to accommodate one type of marriage, say same-sex marriages, are necessarily binding on all marriages. Escaping this impact is difficult. One escape might be a large overhaul to 'custom marriages' for each different type of family; however, this may open the door to 'contract marriage,' issues of polygamy, and so on, and would create a different set of specific externality consequences.

Now, let us return to Langbein and Yost. In creating their "gay marriage permitted" variable, they lump together all states that allow domestic partnerships and civil unions with the one state in their sample, Massachusetts, that has legal same-sex marriage. This amounts to a coding error under the specific externality

8. Allen (2006) lays out in detail the complexity of the jurisdictional-specific mechanism. Hunter (2012) spells out some specific changes that have taken place in the United States.
9. The only exceptions are the few states that have introduced 'covenant marriage.'

channel. Such an externality can only result if the legal reform alters opposite-sex marriage within the state. However, changes to civil union laws do not create a specific externality linkage because civil union laws are not binding on opposite-sexed couples. Since such an externality works through the specific changes brought about by altering legal marriage, there is a serious problem with the empirical specification. Because domestic partnerships and civil unions have no legal connection with opposite-sex marriages, Langbein and Yost have essentially miscoded their legal variable. When a civil union law is enacted, theoretically, there is no specific externality to test for. By combining the states together into their "gay marriage permitted" variable, the estimated coefficients are again biased.

Along the same lines, Langbein and Yost also presume that "prohibitions" or "bans" on same-sex marriage would have *positive* externality effects on traditional family outcomes. But if the externality mechanism works through the specific externality legal channel, then prohibitions should have no direct effect on family outcomes. Such prohibitions merely formalize the status quo, and do not change the legal constraints facing existing or future opposite-sex marriages, and therefore should have no effect.

So what is being tested? It is hard to say, because of Langbein and Yost's failure to take the externality argument seriously and to spell out exactly how any such mechanism might work. They note that "many might believe that it is a waste of time to test this claim" (2009, 293), and that they are doing it only because others have brought it forward. However, a more serious consideration of what the externality might entail would have led to more serious and better testing. As it stands, the results provide no meaningful test for external effects, and their results have no clear interpretation. In our replication below we test explicitly for a specific externality effect by properly coding Massachusetts as the only state with same-sex marriage in their panel.

Empirical matters

The Langbein and Yost paper is also fraught with serious empirical problems.

Questionable coding

Langbein and Yost code their data for three legal variables: (1) Same-sex marriage is either permitted or not; (2) same-sex marriage is either prohibited or not; and (3) some equivalent of same-sex marriage is either permitted or not. In their paper, Langbein and Yost are vague in terms of the source for their legal coding and only state that "data on the legal recognition and forbiddance of marriage rights [are] provided from research by the Human Rights Campaign." They provide a table of counts for each legal classification, but no other sources or details. By examining the actual data used by Langbein and Yost, we are able to determine their actual coding by state, but not the source of the coding.

In contrast, our coding comes from several sources. We began with online sources, such as the Human Rights Campaign (**link**), Wikipedia (**link**), and a report by the law firm Fennemore Craig (**link**).[10] We then supplemented this and cross checked with an American Bar Association white paper (ABA Section of Family Law 2005), and then tracked down the statutory or constitutional acts. These various acts and state codings are listed in the notes to Table 1, and in the Appendix Tables 1A and 2A.

There are coding discrepancies over each legal variable. Table 1 shows some discrepancies in coding over the legal variables defining whether or not same-sex marriage is allowed or whether some type of equivalent exists. Columns (1) and (2) show the coding used by Langbein and Yost, while columns (3) and (4) show our coding.

One type of coding discrepancy is found in the first two rows. Connecticut adopted civil unions for same-sex couples in 2005, but Langbein and Yost code them as present in 2004. Similarly, Maine adopted domestic partnerships in 2004, but they took effect on January 2005, not 2004 as coded by Langbein and Yost.

An opposite issue occurs with the coding for the District of Columbia and Hawaii. The District of Columbia recognized domestic partnerships in 1992, and so the equivalence variable should be coded 1 in 2000, but instead is coded 0 by Langbein and Yost. Hawaii adopted reciprocal beneficiary relationship laws in 1997, which is a form of same-sex recognition. As such it should be coded as equivalent in both 2000 and 2004. If Langbein and Yost do not consider the reciprocal beneficiary relationship as a form of same-sex recognition, then Hawaii should be coded 0 in both years. Langbein and Yost code Hawaii as 0 for 2000, but 1 for 2004.

Finally, Vermont was the first state to adopt civil unions in 2000, and adopted same-sex marriage in 2009. In their data set, Langbein and Yost code Vermont as never having civil unions, and as a same-sex marriage state in 2004.

Discrepancies also occur with respect to Langbein and Yost's variable for defining state bans on same-sex marriage. Bans on same-sex marriage come in many forms, and are more complicated to identify than civil unions or marriage adoptions, and the Langbein and Yost paper is not clear on how such bans are defined.[11] In their paper they state a prohibition of same-sex marriage as "either

10. We viewed all these sources during November 2014.

11. We are unable to construct a list of state bans using only the Human Rights Campaign website.

through a direct ban or a legally exclusive definition of marriage. For the purposes of this study, we include both constitutional and statutory bans in the same category" (2009, 297).[12] The first part of their condition would suggest that legislation delimiting marriage as a union between one man and one woman would count as a ban.

The Langbein and Yost classifications for bans are laid out in column (1) of Table 2. In columns (2) and (3) we list our classification of states with statutory and constitutional bans, based on the sources listed above and in the Appendix. According to their data there were three states with bans in 1990, 33 in 2000, and 45 in 2004. By our count there should be eight, 37, and 41. There is a considerable difference in the classification of whether a state had a ban in place or not.

Estimation procedures

In their paper, Langbein and Yost claim that:

> We use robust estimates of standard errors, clustering by state to recognize that, because within-state observations may share similar determinants and may not be independent, the within-state variance of stochastic terms is less than the variance between states. (Langbein and Yost 2009, 297)

Such a procedure is appropriate whenever there is an explanatory variable that varies only at the group level. In this context the same-sex marriage laws and bans vary only at the state level, and so clustering is appropriate. However, in replicating the Langbein and Yost results, we have discovered they actually report results from a feasible generalized least square (GLS) estimation. If the assumed correlation caused by clustering is correct, then GLS should provide a more efficient estimate. If it is not, then the estimates will not be consistent, and an ordinary least squares (OLS) estimation with clustering will provide more robust estimates.[13]

12. We do object to their using the term "prohibition" expansively, to include what is chiefly a matter of how government runs its own operations in certifying and denominating "marriage" (again, the presumption-of-liberty framework seems inapt), but we leave this aside.

13. Generally speaking, given the small efficiency gains of GLS it is more common to use OLS with correct standard errors (see Cameron and Trivedi 2005, 838).

Replication

Robustness to estimation and coding

Here we show the effect of differences in coding, demonstrate the sensitivity of the various Langbein and Yost results to different GLS and OLS estimation procedures, and conduct the specific externality test that should have been done. The data used in our estimations come from Laura Langbein, and we are able to replicate the results of the original paper, both in terms of descriptive statistics and estimated parameters. Furthermore, we retain the same variable and table names for ease of reference; that is, Tables 3 through 7 repeat the various Langbein and Yost regressions for the different left-hand side variables mentioned above. Hence, Table 3 shows various estimates of same-sex marriage laws on marriage rates, Table 4 shows estimates for divorce rates, and so on. To keep the tables simple, we present the parameter estimates for only the three legal variables; however, the regressions contain the exact set of variables used by Langbein and Yost.[14]

Within each table we present six regressions. The first three regressions in each table use feasible GLS. The last three regressions use OLS, but with clustered standard errors. Hence, columns (1)–(3) use the estimation procedure actually used by Langbein and Yost, while equations (4)–(6) use the procedure that the original paper (wrongly) said was used.[15] As will be clear from the tables and our discussion, the main findings are three: correcting the legal coding has a minimal impact on the reported results when simply replicating the Langbein and Yost exercise; changing only the estimation procedure reduces the precision of many of the results; and running a specific externality test shows there is no statistical power to the test. Taken together, the generally inconsistent and imprecise results reasonably lead to the conclusion that any particular result is likely spurious, the artifact of a data quirk.

The first column within each of the GLS/OLS groupings, that is, columns (1) and (4), show the coefficients from a straightforward replication of Langbein and Yost that includes their coding. The second column within each group (that is, (2) and (5)) is the Langbein and Yost regressions run with our new legal coding,

14. Langbein and Yost use two dummy variables to indicate whether a state has a ban or not, based on the duration the ban was in place.

15. We performed several other sensitivity tests in addition to the ones reported. We dropped the marriage and divorce rate controls and found results similar to equations (1)–(3). We also excluded observations from Hawaii and Nevada, with results similar to the ones presented in the tables here. We also ran the regressions with Missouri bans classed in different ways, with little impact. The regression data we present has Missouri coded as having no bans in 2000.

but retaining the incorrect assumption that civil unions are equivalent to same-sex marriages. Finally, the last two columns in each group (that is, (3) and (6)) are the Langbein and Yost regressions but with civil unions now not treated as equivalent to same-sex marriage, and thus with Massachusetts correctly identified as the only state allowing same-sex marriage in 2004. This last regression (columns (3) and (6)) tests for a specific externality.

Let us examine Table 3 in some detail. Table 3 column (1) replicates the Langbein and Yost regression on marriage rates. Langbein and Yost found a positive, statistically significant effect of same-sex marriage 'equivalent' laws on marriage rates (the coefficient being 0.719), and they found smaller, statistically insignificant effects of same-sex marriage bans. Moving from column (1) to (2) of Table 3 corrects only the legal coding discrepancies found in Tables 1 and 2, and we find very little change in the regression results. This surprisingly suggests that correcting the legal coding does not matter. However, comparing the results in columns (1) and (2) with those in columns (4) and (5), we find large changes in precision when the estimation procedure moves from GLS to OLS. Hence, the finding of statistical significance is not robust to the estimation procedure. Finally, columns (3) and (6) show that when the same-sex marriage variable is properly coded to include only Massachusetts, there is a lack of precision for all variables. If we consider column (6), the 95% confidence interval on same-sex marriage is −0.77 to 1.93. A clear conclusion is that the Langbein and Yost exercise has little to no precision and therefore cannot identify if an externality exists or not. These general findings hold for all of the tables.

Table 4 column (1) replicates the Langbein and Yost regression on divorce rates, and columns (2) through (6) offer our new results. Langbein and Yost found no statistically significant relationship in their paper for pro-same-sex marriage laws, and that is confirmed here. They found a reduction in divorce for one definition of ban, and that holds up under the new coding. With one or two exceptions, the results in Table 4 are not sensitive to the estimation procedure; that is, the coefficients in columns (1), (2), and (3) are similar to those in (4), (5), and (6), respectively. However, the Table 4 results are not robust to the changes in coding: Comparing column (1) against (2) and (4) against (5), we find different results.

Table 5 shows that the Langbein and Yost results on abortion rates are strengthened by our new coding, but they lose precision under the different estimation procedure and then disappear entirely when only Massachusetts is counted as a same-sex marriage state. Table 6 shows that, like Langbein and Yost, we find no significant effects on out-of-wedlock births. Table 7, which examines the relationship between female-headed households and same-sex marriage laws, is the only case where a statistically significant effect found by Langbein and Yost has some robustness to the new coding, estimation procedure, and proper treatment of civil unions.

Considering all of our regressions together, the bottom line is that there is too much imprecision in the estimates to draw any reasonable conclusion. But such a general finding of sensitivity and statistical insignificance is *not* the same as a finding "that laws permitting same-sex marriage or civil unions have no adverse effect on marriage, divorce, and abortion rates, the percent of children born out of wedlock, or the percent of households with children under 18 headed by women," as asserted by Langbein and Yost (2009, 305–306). The sensitivity and insignificant results rather stem from the low power of their regressions based on the test design. The correct conclusion to draw from the results is that we cannot tell whether an effect exists or not. Which prompts the question: Why not?

Why power is so lacking

Langbein and Yost conduct a simple before-and-after test across a panel of states. In order for such a test to have power, it is necessary to have data both before *and after*. Yet the experimental design used by Langbein and Yost uses only data from 1990, 2000, and 2004. During these three years, only *one* state in the *last* year had same-sex marriage: Massachusetts, passed in May 2004. This is the likely reason why Langbein and Yost classified states with civil unions as having same-sex marriage. But even this only increases the number of state-year observations to seven, and it causes measurement error, which reduces the power of the regression.

Other factors contribute to the low power of the regressions. First, the overall sample size used in the regressions is small. In contrast to a long panel, Langbein and Yost only use data from three years. With a single observation for each state and the District of Columbia, this only amounts to 153 observations. Small sample sizes naturally lead to low levels of statistical significance, especially in the context of family decisions where there is so much variation in circumstances between families.[16] Despite the small sample size, Langbein and Yost (appropriately) use large numbers of independent control variables. However, increasing the number of independent variables naturally increases standard errors and reduces the level of significance. Langbein and Yost include a dummy variable for each state (50 variables), time dummies (two variables), and a host of other controls (eight variables). With so few observations, there is little power left. The states that changed their laws are simply too few and too recent for any measurable effect

16. Outliers also play a large role in small-sample regressions. Langbein and Yost include Nevada in their regressions. In the context of marriage rates, Nevada is an enormous outlier. Whereas the average marriage rate in the other states is below 9, Nevada has marriage rates of 99, 77, and 63 for 1990, 2000, and 2004.

to take place. When using aggregate state statistics to test for the effect of legal changes on family behavior, it is critical to have data many years before and after the actual change.[17]

Conclusion

Despite the lack of an explicit theoretical externality mechanism, a poorly defined test, a weak design, and low-power regressions, Langbein and Yost (2009) said repeatedly, as shown at the head of this paper, that no rational argument against same-sex marriage can be held. But there may be other externalities that were not examined, there may be other types of costs beyond externalities that could justify an opposition to the reform, and their tests may just be wrong.

If we take the externality arguments seriously and use the Langbein and Yost data, then only testing for a specific externality in Massachusetts is appropriate. Our column (3) and (6) regression results in Tables 3 through 7 show that all such a test finds is noise—as it should, since it is a before-and-after test that has only one observation in the 'after' period and only covers a short period of time.

Same-sex relationships will be very difficult to investigate given their small numbers and complexity. If there is an externality effect, it is likely to be compounded by many factors. Rushing into empirical work before the data are ready, or before an appropriate empirical strategy can be identified, is likely to cause more harm than good. As other studies come forward and the inevitable 'counting up' of studies takes place, the Langbein and Yost paper should not be counted. It found nothing.

17. With this simple design and the brief period since the introduction of same-sex marriage it is unlikely an update to 2009 (the latest year data is readily available) matters since only Connecticut, Iowa, and Massachusetts would have same-sex marriage. New Hampshire came into effect in 2010, and Vermont changed late in the year. In perhaps the most important and definitive paper on using aggregate data to understand the effect of legal parameters on marriage behavior, Wolfers (2006) shows how important it is to have a panel series long before and after a legal change. In his study he uses a panel 30 years prior to changes in no-fault divorce laws, and 20 years afterwards.

TABLE 1. Coding discrepancies for legalized same-sex marriage

	Langbein and Yost (2009) coding		Our coding	
	Permitted (1)	Some equivalent (2)	Permitted (3)	Some equivalent (4)
CT 2004	0	1	0	0
ME 2004	0	1	0	0
DC 2000	0	0	0	1
HI 2000	0	0	0	1
VT 2000	0	0	0	1
VT 2004	1	0	0	1

Notes. (a) Connecticut created civil unions in 2005: An Act Concerning Civil Unions (Public Act No. 05-10). (b) Langbein and Yost code Maine as having civil unions in 2004. The law came into effect on July 30, 2004: Chapter 672, LD 1579, HP 1152. (c) The District of Columbia legalized domestic partnerships in 1992: The Health Benefits Expansion Act of 1992. (d) Hawaii introduced same-sex benefits in 1997: Reciprocal Beneficiaries Law (Act 383). (e) Vermont created civil unions in 2000: Vermont House Bill 847. Vermont did not create same-sex marriage until September 1, 2009: the Marriage Equality Act.

TABLE 2. Coding discrepancies for bans on same-sex marriage

		Our coding	
	Langbein and Yost (2009) coding	Statutory bans	Constitutional bans
1990	CA MD WY	CA FL MD NH PA UT VA WY	
2000	AL AK AZ CO DE FL GA HI ID IL IN IA KS KY LA ME MI MN MS MT NE NC ND OK PA SC SD TN UT VT VA WA WV	AL AK AZ AR CA CO DE FL GA HI ID IL IN KS KY LA ME MD MI MN MS MT NE NH ND OK PA SC SD TN TX UT VA WA WV WY	AK HI NE
2004	All states, *except: DC NJ NM NY RI*	All states, *except: CT DC IA MA NJ NY NM RI VT WI*	AR GA KY LA MI MS MO MT NV ND OH OK OR UT

TABLE 3. Marriage rates on same-sex marriage laws

	GLS			OLS (Cluster)		
Variable	Langbein and Yost's coding (1)	Our coding (2)	Our coding, and civil unions not treated as marriage (3)	Langbein and Yost's coding (4)	Our coding (5)	Our coding, and civil unions not treated as marriage (6)
gaymarriage ok	0.719*** (0.248)	0.889** (0.369)	0.521 (0.329)	1.102 (0.759)	1.595 (1.587)	0.578 (0.690)
gaymarriage ban duration 1	−0.100 (0.141)	−0.065 (0.209)	−0.147 (0.185)	−0.065 (0.533)	−0.093 (0.857)	−0.208 (0.847)
gaymarriage ban duration 2	0.430 (0.330)	0.470 (0.381)	0.233 (0.351)	1.322 (1.301)	1.652 (1.339)	1.457 (1.308)

Note. Column (1) is our successful replication of the regression that Langbein and Yost report in their Table 3 (2009, 301). *** p<0.01; ** p<0.05; * p<0.1.

TABLE 4. Divorce rates on same-sex marriage laws

	GLS			OLS (Cluster)		
Variable	Langbein and Yost's coding (1)	Our coding (2)	Our coding, and civil unions not treated as marriage (3)	Langbein and Yost's coding (4)	Our coding (5)	Our coding, and civil unions not treated as marriage (6)
marriagerate	0.107*** (0.015)	0.106*** (0.011)	0.106*** (0.012)	0.127*** (0.043)	0.112** (0.048)	0.115*** (0.043)
gaymarriage ok	−0.174 (0.210)	0.034 (0.203)	−0.308 (0.248)	−0.446 (0.542)	0.164 (0.958)	−0.271 (0.318)
gaymarriage ban duration 1	−0.122** (0.051)	−0.469*** (0.068)	−0.481*** (0.069)	−0.115 (0.069)	−0.552** (0.219)	−0.565** (0.231)
gaymarriage ban duration 2	−0.041 (0.093)	−0.350*** (0.099)	−0.397*** (0.101)	−0.066 (0.322)	−0.329 (0.340)	−0.372 (0.258)

Note. Column (1) is our successful replication of the regression that Langbein and Yost report in their Table 4 (2009, 302). *** p<0.01; ** p<0.05; * p<0.1.

TABLE 5. Abortion rates on same-sex marriage

	GLS			OLS (Cluster)		
Variable	Langbein and Yost's coding (1)	Our coding (2)	Our coding, and civil unions not treated as marriage (3)	Langbein and Yost's coding (4)	Our coding (5)	Our coding, and civil unions not treated as marriage (6)
gaymarriage ok	−3.529*** (0.765)	−8.307*** (2.176)	−0.585 (1.730)	−3.252 (2.563)	−13.036* (7.049)	3.194 (3.992)
gaymarriage ban duration 1	−0.441 (0.344)	−1.267*** (0.360)	0.160 (0.470)	−0.337 (1.774)	−2.353 (1.752)	−1.286 (1.927)
gaymarriage ban duration 2	−0.711** (0.332)	−1.441*** (0.513)	0.983 (0.694)	−1.763 (3.019)	−2.971 (2.693)	−0.767 (2.578)

Note. Column (1) is our successful replication of the regression that Langbein and Yost report in their Table 5 (2009, 303). *** p<0.01; ** p<0.05; * p<0.1.

TABLE 6. Out-of-wedlock birth rates on same-sex marriage laws

	GLS			OLS (Cluster)		
Variable	Langbein and Yost's coding (1)	Our coding (2)	Our coding, and civil unions not treated as marriage (3)	Langbein and Yost's coding (4)	Our coding (5)	Our coding, and civil unions not treated as marriage (6)
gaymarriage ok	0.399 (0.620)	0.525 (0.720)	0.786 (1.257)	−0.632 (1.255)	−0.733 (1.918)	0.856 (1.126)
gaymarriage ban duration 1	−0.006 (0.232)	0.064 (0.302)	0.141 (0.295)	0.026 (0.740)	−0.184 (1.123)	−0.110 (1.127)
gaymarriage ban duration 2	0.314 (0.434)	0.422 (0.470)	0.533 (0.452)	−0.095 (1.422)	−0.144 (1.628)	0.013 (1.583)

Note. Column (1) is our successful replication of the regression that Langbein and Yost report in their Table 6 (2009, 304). *** p<0.01; ** p<0.05; * p<0.1.

TABLE 7. Percent female-headed households on same-sex marriage laws

	GLS			OLS (Cluster)		
Variable	Langbein and Yost's coding (1)	Our coding (2)	Our coding, and civil unions not treated as marriage (3)	Langbein and Yost's coding (4)	Our coding (5)	Our coding, and civil unions not treated as marriage (6)
gaymarriage ok	−1.587*** (0.439)	−1.701*** (0.401)	−2.246*** (0.347)	−0.876 (1.344)	−1.866* (0.928)	−2.242** (0.968)
gaymarriage ban duration 1	0.083 (0.188)	0.166 (0.302)	−0.038 (0.304)	−0.195 (0.627)	−0.414 (0.811)	−0.387 (0.835)
gaymarriage ban duration 2	−1.695*** (0.359)	−0.087 (0.458)	−0.289 (0.442)	−2.313** (1.119)	−1.245 (1.342)	−1.005 (1.435)

Note. Column (1) is our successful replication of the regression that Langbein and Yost report in their Table 7 (2009, 305). *** p<0.01; ** p<0.05; * p<0.1.

Appendix

Our data and code, in Stata formats, can be downloaded **here**.

TABLE 1A. Statutory ban legislation

State	Legislation	Year
Maryland	Maryland Code, Family Law §2-201	1973
Virginia	Virginia Code § 20-45.2	1975
California	Assembly Bill 607	1977
Florida	Florida Statutes § 741.04	1977
Wyoming	Wyoming Statutes §20-1-101	1977
Utah	Utah Code, 30-1-2	1977
New Hampshire	NH Statutes § 457:1-2	1987
Louisiana	Louisiana Civil Code, Article 89 3520(B)	1988
North Carolina	North Carolina General Statues § 51-1.2	1995
Tennessee	Tennessee Code § 36-3-113	1996
South Dakota	South Dakota Codified Laws § 25-1-1	1996
Arizona	Arizona Revised Statues § 25-101 and 25-112	1996
Oklahoma	Oklahoma Statutes 43 §3.1	1996
Kansas	Kansas Statutes § 23-115	1996
Pennsylvania	Pennsylvania Consolidated Statutes 23 § 1704	1996
Alaska	Alaska Statutes § 25.05.013	1996
Michigan	Michigan Compiled Laws § 551.1, 551.271, 551.272, 551.3, 551.4	1996
South Carolina	South Carolina Code § 20-1-10, 20-1-15	1996

TABLE 1A (cont'd). Statutory ban legislation

State	Legislation	Year
Delaware	Delaware H.B. 503	1996
Georgia	Georgia Code § 19-3-30 19-3-3.1	1996
Idaho	Idaho Code § 32-209	1996
Illinois	Illinois Marriage and Dissolution of Marriage Act 750 ILCS 5/212	1996
Indiana	Indiana Code § 31-11-1-1	1997
Arkansas	Arkansas Code § 9-11-208, 9-11-107, 9-11-109	1997
Maine	Maine Revised Statutes § 611	1997
Minnesota	Minnesota Statutes 517.01, 517.03	1997
Mississippi	Mississippi Code § 93-1-1	1997
Montana	Montana Code § 40-1-103, 40-1-401	1997
North Dakota	North Dakota Century Code § 14-03-01, 14-03-08	1997
Texas	Texas Family Code § 2.001	1997
Kentucky	Kentucky Revised Statutes § 402.005, 402.020, 402.040	1998
Alabama	Alabama Code § 30-1-19	1998
Washington	Defense of Marriage Act	1998
West Virginia	West Virginia Code § 48-2-104, 48-2-603	2000
Colorado	Colorado Revised Statutes § 14-2-104	2000
Missouri	Revised Statutes § 451.022, 451.012	2001
Ohio	Ohio Revised Code § 3101.01	2004

TABLE 2A. Constitutional bans

State	Amendment	Year
Alaska	Ballot Measure 2	1998
Hawaii	Amendment 2	1998
Nebraska	Initiative 416	2000
Nevada	Question 2	2002
Arkansas	Amendment 3	2004
Georgia	Amendment 1	2004
Kentucky	Amendment 1	2004
Louisiana	Amendment 1	2004
Michigan	State Proposal 04-2	2004
Mississippi	Amendment 1	2004
Missouri	Amendment 2	2004
Montana	Initiative 96	2004
North Dakota	Measure 1	2004
Ohio	State Issue 1	2004
Oklahoma	State Question 711	2004
Oregon	Measure 36	2004
Utah	Amendment 3	2004

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