Signing Up New Fathers: Do Paternity Establishment Initiatives Increase Marriage, Parental Investment, and Child Well-Being?*

Maya Rossin-Slater†

July 27, 2015

Abstract

With nearly half of U.S. births occurring out of wedlock, understanding how parents navigate their relationship options is important. This paper examines the consequences of a large exogenous change to parental relationship contract options on parental behavior and child well-being. Identification comes from the staggered timing of state reforms that substantially lowered the cost of legal paternity establishment. I show that the resulting increases in paternity establishment are partially driven by reductions in parental marriage. Although unmarried fathers become more involved with their children along some dimensions, the net effects on father involvement and child well-being are negative or zero.

*I am very grateful for the abundance of support, guidance, and helpful comments from Janet Currie, Wojciech Kopczuk, and Ilyana Kuziemko. I thank Doug Almond, Pierre-André Chiappori, Jonathan Dingel, Lena Edlund, Irv Garfinkel, Tal Gross, Peter Kuhn, Jeanne Lafortune, Corinne Low, Shelly Lundberg, Katherine Meckel, Matt Neidell, Dan O'Flaherty, Petra Persson, Kiki Pop-Eleches, Jonah Rockoff, Heather Royer, Lesley Turner, Miguel Urquiola, Eric Verhoogen, Till von Wachter, Jane Waldfogel, Reed Walker, and Elizabeth Ty Wilde for their feedback. I am also thankful to seminar participants at Columbia University, the University of Connecticut, UC Berkeley's Goldman School of Public Policy, UC Santa Barbara, UC Santa Cruz, Stanford Institute for Economic Policy Research, Princeton University, UC Irvine, the University of Maryland, and Mathematica Policy Ressearch, Inc., as well as conference participants at ASSA, APPAM, PAA, and the NBER Children's Program meeting. I am grateful to Jean Roth for help with the CPS-CSS data, and Patricia Barnes for access to and help with the restricted NHIS data. I also thank Irv Garfinkel and Lenna Nepomnyaschy for sharing data on child support laws, and Maria Cancian and Dan Meyer for sharing data on child support disregard policies. All errors are my own.

†University of California at Santa Barbara, Department of Economics. Contact e-mail: maya.rossin-slater@ucsb.edu.

1 Introduction

Over 40 percent of all births in the United States occur out of wedlock, and many of these children grow up in single-mother households, which are disproportionately disadvantaged. Fourty-three percent of children in single-mother households lived below the poverty line in 2010.[1] Learning about parental relationship choices is important both for informing economic models of family behavior and for addressing these families' needs through effective policy design.

This paper makes progress on understanding the behavior and relationship choices of unmarried parents by studying how they react to a large and exogenous change to their legal relationship contract options. The change stems from the introduction of a program called in-hospital voluntary paternity establishment (IHVPE), which substantially lowered the cost of legal paternity establishment for unmarried fathers in the U.S. Legal paternity establishment grants unmarried fathers partial rights and responsibilities to their biological children. Before IHVPE, paternity establishment was a costly and complicated process involving the court system and DNA testing; less than a third of children born out of wedlock had fathers who established paternity in the late 1980s (U.S. House of Representatives, 2004). IHVPE programs, which have been implemented across all U.S. states in the last few decades, enable all unmarried parents to voluntarily establish paternity by filling out a simple one-page form at the hospital at the time of childbirth, and provide information to all new fathers about their rights and obligations.[2] I leverage quasi-exogenous variation provided by the staggered introduction of IHVPE to learn about the relationship choices and behavior of parents who have children out of wedlock.

This paper also provides the first comprehensive causal analysis of IHVPE on a wide range of family outcomes, which is arguably interesting in its own right. IHVPE advocates were very optimistic about the program at the time of its inception, arguing that "the establishment of paternity in unmarried births is one way to address the deprivations associated with out of wedlock parentage," and citing multiple finan-

[1] See http://www.census.gov/population/www/socdemo/hh-fam/cps2010.html for statistics on single-mother households and http://www.cdc.gov/nchs/births.htm for statistics on U.S. births.

[2] Both parents have to be present at the hospital and agree to legally establish paternity by signing the form. There is no DNA testing involved. Section 2 provides additional details about IHVPE.

cial, emotional, medical, and social benefits for families and children (Pearson and Thoennes, 1995). I assess the degree to which IHVPE has been successful in achieving its stated goals, and show that parental relationship choices may complicate the program's intended impacts.

IHVPE is one of several government interventions that seeks to improve child well-being in non-intact families by encouraging father involvement in terms of financial support and parenting time. Some measures, such as the "Healthy Marriage Initiatives," explicitly promote parental marriage as the relationship contract that generates the highest level of father involvement.[3] Some advocates even argue that parental marriage is "America's greatest weapon against child poverty" (Rector, 2010). Other policies such as IHVPE may be viewed as "stepping-stones" to marriage, as they are focused on a more immediate goal of increasing unmarried fathers' involvement with their families by granting them partial legal rights and responsibilities to their children. All such policies share the underlying assumption that father involvement is important for child and family well-being.

Using data from a multitude of sources and variation in the timing of IHVPE initiation across states, I first show that IHVPE substantially increases paternity establishment rates by 21 percentage points (a 38 percent effect at the pre-treatment mean).[4] However, IHVPE also affects another margin of parental behavior. I find a *negative* effect on parental marriage—for each additional paternity established as a result of IHVPE, there are 0.13 fewer parental marriages post-childbirth. Further, I show that mothers, who are less likely to be married to their children's fathers after IHVPE, are instead more likely to marry or cohabit with new partners, who are older and more likely to be employed.

Although, at least for some parents, paternity establishment serves as a substitute

[3]The Deficit Reduction Act of 2005 provided $150 million in funding every year for "healthy marriage promotion and father involvement". Most programs funded by these initiatives provide relationship education and counseling and conduct public advertising campaigns on "the value of healthy marriages". Many of these programs are explicitly targeted at unmarried pregnant women and expectant fathers. See http://www.acf.hhs.gov/healthymarriage/about/mission.htmlbackground for more information.

[4]As detailed in Section 3, my empirical analysis relies on the following main sources of data: 1) self-assembled data on the timing of IHVPE initiation across states from multiple sources, 2) 1992-2005 Office of Child Support Enforcement (OCSE) records, 3) 1994-2008 biannual March/April matched Current Population Survey Child Support Supplements (CPS-CSS), 4) 1989-2010 March CPS annual demographic files, and 5) 1997-2010 annual Sample Child files of the restricted version of the National Health Interviews Survey (NHIS).

(and not a "stepping-stone") to marriage, I find suggestive evidence that IHVPE increases involvement among unmarried non-custodial fathers—bounding methods that attempt to control for selection out of marriage (Lee, 2009) suggest that unmarried fathers are more likely to make all required child support payments, and to cover some of their children's food, childcare, and medical expenses. I also find that children living with unmarried mothers are less likely to be diagnosed with a learning disability, but experience no changes to their physical health or access to care.

I then study IHVPE's average impacts on the *whole* sample (i.e., comprised of families with both married and unmarried parents), which combine effects on parents who would have never married and effects on parents who opt out of marriage as a result of IHVPE. I find that the average effects on observable measures of father involvement are either negative or zero. First, I show a decline in children's private health insurance coverage, driven by fathers who would have been married in the absence of IHVPE. Second, I find a small increase in maternal labor supply that may reflect a net decrease in paternal monetary support, for which mothers must compensate by working. Third, I provide some suggestive evidence of negative consequences for time spent with children and zero effects on other available father involvement variables. In other words, any positive effects on involvement among fathers who would have remained unmarried in the absence of IHVPE seem to be offset by decreases in involvement among fathers who would have otherwise been married to their children's mothers.

Similarly, when accounting for the decline in parental marriage, I find that IHVPE does not lead to any detectable improvements in overall child well-being. I find no net effects of IHVPE on children's physical or mental health except for a small negative effect on children's access to preventative care, possibly driven by the decline in private health insurance coverage. I also find no net impacts on income, poverty status, or welfare benefit receipt in the child's household of residence. The lack of overall effect on welfare receipt has particularly informative public finance implications, as IHVPE and other father involvement measures are designed to shift the financial burden of supporting single-mother households from government programs to fathers.[5] My re-

[5]States vary in how child support payments are treated in establishing welfare eligibility as a result of different disregard policies that delineate the amount of child support income to be ignored in the calculation of welfare benefits (see Cancian *et al.*, 2007 for more details). The implications of these disregard policies for the empirical results are discussed briefly in Section 5.

sults imply that policies that change parental relationship options may not fully achieve this goal—reductions in welfare take-up among unmarried mothers due to higher child support payment rates may be offset by increases in benefit take-up among mothers who would have otherwise been married.

I consider several robustness tests. First, I show that the results are robust across a variety of specifications and different data sets. Second, I test the underlying assumptions of the difference-in-difference approach: I show that the timing of IHVPE implementation is uncorrelated with other state time-varying characteristics and policies, including welfare reform and other child support enforcement policies, and that the estimated effects are *not* driven by any pre-existing trends in the outcomes of interest. Third, consistent with qualitative evidence that most unmarried parents learn about the program at the hospital at the time of childbirth (Martinez, 2011), I show that the effects of IHVPE manifest *post-childbirth*. These tests suggest that the identified relationships between IHVPE and the outcomes of interest are causal and not driven by other factors.

What can we learn about the trade-offs faced by parents who have children out of wedlock from these results? The final part of the paper presents a simple model of parental interaction to rationalize the empirical findings. This framework draws on existing models of collective decision-making and the role of parental rights (Browning *et al.*, 2014; Edlund, 2011), as well as models of parental investment and child support decisions outside marriage (Weiss and Willis, 1985; Flinn, 2000; Brown and Flinn, 2011; Roff and Lugo-Gil, 2012; Tartari, 2014). Parents are heterogeneous in match quality and face a trade-off between the benefit of joint child investment and the possible cost of interaction with each other. Paternity establishment offers an intermediate relationship contract option between the "extremes" of no legal relationship and marriage. When the cost of establishing paternity is lowered, parents who would have previously maintained no legal relationship *and* parents who would have previously been married are more likely to choose the intermediate contract. Further, if child investment is higher in marriage than in paternity and higher in paternity than in no relationship, the net effects on parental investments and child well-being are theoretically ambiguous and depend on the relative magnitudes of the increases among switchers out of no relationship and the decreases among switchers out of marriage.

Since it is difficult to measure match quality in the data, and since I observe parental investments and child well-being imperfectly, I do not argue that this model is the only way to rationalize my empirical results. It is important to emphasize, however, that the empirical evidence does argue against perhaps the leading alternative model—that paternity establishment serves as a "stepping-stone" to marriage by encouraging unmarried fathers to be more involved with their families and be more likely to marry their children's mothers. Such a model would predict that IHVPE causes an increase in the parental marriage rate; not the robust decrease that I find.

This paper contributes to a large literature on parental interaction, especially within non-intact families. One strand of this literature has used a structural model approach to directly estimate parameters of parental utility functions (see, e.g., Del Boca and Flinn, 1995; Flinn, 2000; Del Boca and Ribero, 2003; Brown and Flinn, 2011; Roff and Lugo-Gil, 2012; Tartari, 2014; Beauchamp *et al.*, 2014). While this approach is useful for generating predictions about the impacts of various policy counterfactuals (e.g., perfect institutional enforcement of child support orders versus weak enforcement), functional form assumptions and concerns about endogeneity present some limitations. This paper takes a complementary approach by using quasi-exogenous variation in an existing policy (namely, IHVPE). While my estimates cannot directly speak to parental preferences or overall welfare, my analysis instead focuses on isolating the *causal* effects of a change to the parental relationship contract space.[6] My results suggest that marriage is not the optimal choice for all parents—some parents prefer alternative arrangements that outline partial parental rights and responsibilities for unmarried fathers. Therefore, policies based on the notion that more father involvement is essential to child and family well-being must account for the parents' agency in choosing their partners. More broadly, my findings highlight the empirical relevance

[6]This paper also relates to a literature that uses state-year variation in child support enforcement spending to study the overall impacts of child support enforcement (e.g.: Garfinkel *et al.*, 1998; Nixon, 1997; Freeman and Waldfogel, 2001; Heim, 2003; Aizer and McLanahan, 2006; Nepomnyaschy and Garfinkel, 2007; Takayama and Tanaka, 2011 among others). Many of these studies examine selected outcomes among divorced and unmarried parents, which may be problematic if there are effects on selection in or out of marriage. The handful of existing studies on IHVPE have been limited to reports on individual state programs (for example, Pearson and Thoennes, 1996; Wisconsin Bureau of Child Support, 2010) and cross-sectional analyses of a few states over short periods of time (Turner, 2001; Sorensen and Olivier, 2002; Mincy *et al.*, 2005a). Finally, this paper relates to the large literature on how marriage behavior (not just among parents) responds to various policies (e.g., Schoeni and Blank, 2000; Rosenbaum, 2003; Bitler *et al.*, 2004; Bitler *et al.*, 2006; Halla, 2013; Persson, 2014).

of the trade-off between child investment and partner interaction—long emphasized in family economics theory (e.g., Becker, 1993; Edlund, 2011; Brien *et al.*, 2006; Lundberg and Pollak, 2012; Browning *et al.*, 2014)—in understanding the behavior of parents who have children out of wedlock.

The paper proceeds as follows. Section 2 provides more information on IHVPE, while Section 3 describes the data sources and summary statistics. Section 4 details the empirical methods, and Section 5 presents the main results and robustness tests. Finally, Section 6 presents a theoretical framework for interpreting the empirical results, while Section 7 concludes.

2 Background on In-Hospital Voluntary Paternity Establishment

Without paternity establishment, unmarried fathers have no legal rights or obligations with regard to their children. Paternity establishment grants fathers rights to request partial custody and visitation privileges from the court, to refuse requested adoptions, and to block foster care placements. Importantly, paternity establishment also allows mothers to seek a court order that obligates fathers to make child support payments, which represent significant contributions to family incomes in single-mother households (Garfinkel *et al.*, 1998; Lerman and Sorenson, 2003).

In the 1970s and 1980s, paternity establishment was a relatively uncommon and costly process in the U.S; it involved the court system, and most paternities were only established several years after the child's birth, if ever.[7] To address this issue, the Omnibus Budget Reconciliation Act (OMBRA) of 1993 required all states to implement IHVPE programs, and the 1996 Personal Responsibility and Work Opportunity Act (PRWORA) reinforced this requirement. Policymakers argued that IHVPE programs would be effective as they attempt to reach families during the "happy hour" in the hospital following the birth of the child and encourage the father to stay involved in his family's life.[8] As a result, all states have initiated an IHVPE program, in which all hospitals and birthing centers are required to provide adult unmarried new mothers

[7] See 20th Annual Office of Child Support Enforcement Report to Congress available at: http://www.acf.hhs.gov/programs/cse/pubs/1996/reports/20th_annual_report_congress/.

[8] See the U.S. Department of Health and Human Services report on in-hospital paternity establishment programs for more information: http://oig.hhs.gov/oei/reports/oei-06-95-00163.pdf.

and fathers with an opportunity to sign a voluntary paternity acknowledgement form. Both unmarried parents have to be present at the hospital to participate in IHVPE.[9] State child support agencies are required to make available materials for educating parents, and hospital staff must provide mothers and fathers with both written materials and oral explanations regarding the rights and responsibilities related to paternity establishment. Importantly, IHVPE involves no DNA testing for paternity—paternity is legally established after both parents sign the voluntary paternity acknowledgement form.

Despite the federal mandate, the administration of IHVPE programs was mostly under state discretion. Variation in the timing of IHVPE implementation across states stems largely from the length of time that it took to forge relationships between state child support agencies, vital statistics registries, and hospitals. By 1997, 37 states reported full implementation of IHVPE, while the rest listed reasons such as "too early for the [office of child support] staff to have contacted every state birthing hospital" to explain the delays.[10] Identification of the causal effects of IHVPE requires the assumption that the timing of implementation is uncorrelated with other time-varying determinants of the outcomes of interest. This assumption is explored in detail in Sections 4 and 5, with evidence suggesting that unobserved state time-varying omitted variables should not pose serious issues.

Unfortunately, a unified source of information on the timing of IHVPE program implementation across states does not exist. I obtained information on the year (and month if available) of program implementation from searches of state legal statutes on *LexisNexis Academic*, internet searches of state paternity programs, and direct conversations with officials at state child support agencies and IHVPE programs for most states. I supplement these data with information collected by Nepomnyaschy and Garfinkel (2007) for several states. Appendix Figure 1 shows the variation in the timing of IHVPE program implementation across states, while Appendix Table 1 presents more details for each of the 44 states in my data.[11] Births in these states

[9]According to data from the Fragile Families and Child Well-Being Study, over 1998-2000, 76 percent of unmarried mothers reported that the child's father came to the hospital at the time of the child's birth.

[10]See the U.S. Department of Health and Human Services report on in-hospital paternity establishment programs for more information: http://oig.hhs.gov/oei/reports/oei-06-95-00160.pdf.

[11]I do not have data for the following states: IA, MT, NH, NM, OK, WV, WY.

account for about 96 percent of all births in the United States over the time period of analysis. Appendix Figure 2 plots the trend in the total number of paternities established in these 44 states over 1992-2005, and the substantial increase from about 500,000 to over 1.5 million in the 1990s coincides with the time when most states implemented IHVPE programs.

Finally, it is important to highlight that paternity establishment is applicable to *all* unmarried fathers, including those who cohabit with their children's mothers. As such, the distinction in parental relationships throughout most of this paper is between marriage and non-marriage, rather than between marriage, non-marital cohabitation, and non-marriage/non-cohabitation. For completeness, I also examine parental non-marital cohabitation and find no statistically significant effects of IHVPE on this outcome (see Section 5 for more details).

3 Data and Summary Statistics

3.1 Paternity Establishment Data

Data on the *total* number of paternities established for all children under age 18 in each state and year over 1992-2005 come from Office of Child Support Enforcement reports. There is no consistent information on the number of paternities established *in-hospital* for all years in the time period of analysis.[12] However, since IHVPE programs can only affect paternity establishment rates at the hospital, one can interpret the changes in the total number of paternity establishments following IHVPE implementation as being driven by changes in in-hospital paternity establishments.[13] For the analysis, I use paternity establishment data for the 43 states for which I have information on the year of IHVPE initiation and which initiated their programs in 1993 or later, which results in 601 state-year observations.[14]

[12]The report for years 1992-1996 contains separate tables reporting the number of paternities established in-hospital and the number of paternities established through other methods. Reports for all other years only contain information on the total number of paternities established through all methods.

[13]In fact, in the long run, we should expect paternity establishment rates outside the hospital to decrease as a result of IHVPE programs, as some families that would have established paternity later on instead establish it at the time of the child's birth.

[14]I exclude Washington, which initiated its IHVPE program in 1989. Additionally, Nevada is missing data on paternity establishments in 2000, so I exclude this state-year observation.

3.2 CPS-CSS and March CPS Data

To analyze the effects of IHVPE on marriage behavior and some measures of father involvement, I use data from the biannual March/April matched Current Population Survey Child Support Supplements (CPS-CSS) from 1994 to 2008.[15] More details on the sample construction are presented in Appendix A. These data include households that were surveyed both in the March annual demographic file and in the monthly April CPS. In April, in addition to the standard CPS questions, all household members aged 15 and above who have a child in the household with a parent that lives outside the household are asked detailed questions regarding child support payments and other involvement of the non-custodial parent.

My main analysis sample consists of mothers with a youngest child aged five years or less in the household, who were aged 18-45 years at the time of childbirth and who resided in the U.S. in the previous year.[16] These restrictions leave me with $38,449$ sample mothers in the CPS-CSS data, out of which $8,974$ are asked the CSS questions.

For some analyses, I take advantage of the larger sample sizes in the 1989-2010 March CPS annual demographic supplement files relative to the CPS-CSS.[17] The March CPS analysis sample consists of $212,504$ mothers of youngest children aged five years or less.

3.3 NHIS Data

To examine the effects of IHVPE on child mental and physical health and access to care, I use the restricted version of the 1997-2010 Sample Child files of the National Health Interviews Survey (NHIS) with state identifiers.[18] These data contain detailed information on numerous parent-reported measures of health and access to care together with information on the state of residence and the year and month of birth for

[15]Because of changes to the CPS-CSS in the early 1990s, data collected in or after 1994 are not compatible with those from earlier survey years (Freeman and Waldfogel, 2001).

[16]I focus on the age of the *youngest* child because I aim to examine the relationship between family outcomes observed at the time of the survey (such as marriage, father involvement, etc.) with IHVPE presence at the time of the *most recent* birth. As discussed below, I find no effects on fertility, suggesting that the analysis focused on the youngest child should not be affected by sample selection bias. The sample is further limited to mothers aged 18-45 years at the time of childbirth because there is some variation in how minors are treated in IHVPE programs (Roberts, 2004).

[17]The March CPS data are available through the Integrated Public Use Microdata Series (King *et al.*, 2010).

[18]More information regarding access to restricted NHIS data is available here: http://www.cdc.gov/rdc/.

a randomly picked child within each NHIS sample household. Additionally, NHIS respondents are asked direct questions regarding cohabitation with unmarried partners, which are not asked in the CPS. The NHIS analysis sample includes all mothers aged 18-45 at the time of childbirth, who reside with a sample child aged 7 years or less.[19] For confidentiality reasons, actual sample sizes from these data cannot be released. In the public-use version of the analysis sample that contains all states, the sample size is about 67,100 mothers of sample children aged 7 years or less, a reasonable approximation of the size of the true analysis sample, which omits data from seven relatively small states.

3.4 Summary Statistics

Appendix Table 2 presents the sample means of key variables in each of the data sets used in my analyses.[20] To study paternity establishment, I construct three variables at the state-year level: the ratio of the total number of paternities established for all children under age 18 to the number of unmarried births, the ratio of the total number of paternities established to the number of all births, and the log number of paternities established. The average ratio of paternities to unmarried births is 0.94, which of course substantially overestimates the proportion of unmarried births that have paternity established *in-hospital* as the numerator includes paternities established for all children under age 18 (and not just newborns). Remarkably, however, there is a striking difference in this ratio pre- and post-IHVPE: the average among state-year cells without IHVPE is 0.55, while the average among state-year cells with IHVPE is

[19]The age cut-off is higher than the one used in the CPS analysis because the NHIS data span a later time period. Thus, to retain more children who were born pre-IHVPE initiation, I include slightly older children aged 6 and 7 years. Note that sample children are not necessarily the youngest children in the household. This complicates the analysis, as sample children born prior to IHVPE implementation may have younger siblings who were affected by IHVPE leading to spill-over effects on children that I consider untreated. To address this issue and for comparability with analysis using CPS-CSS and CPS data, I have estimated models limiting the sample to sample children who are the youngest in their households. The results from this analysis are very similar to the results from using all the sample children, although less precise due to sample size reductions.

[20]Washington was the first state to implement IHVPE in 1989 (see Appendix Table 1). Since the state-year data on paternity establishment rates is available for 1992-2005, while the NHIS sample includes children born in 1989 or later, I exclude Washington from analyses with these data sets. In the CPS-CSS and the March CPS, I have observations on children born in 1988 or later, and therefore include mothers from Washington. Thus, the total number of states in the paternity establishment rate and NHIS analyses is 43, while the total number of states in the CPS-CSS and March CPS analyses is 44.

1.10.[21]

The CPS-CSS data show that about 78 percent of mothers with children aged five years or less are married—77 percent are married to their children's fathers, while one percent are married to someone else. Sixty-three percent of mothers are non-Hispanic white, while 14 percent are black and 17 percent are Hispanic. Overall, about 67 percent of children have private health insurance coverage. Among mothers who are asked CSS questions, child support receipt rates are fairly low: In the year prior to the survey, only 36 percent received any child support payments, while only 22 percent received all of the child support that was due. However, about 70 percent of mothers state that the father has legal visitation rights, and non-resident fathers spend an average of 60 days out of the year with their children. The March CPS data show that about 68 percent of mothers reported working in the past year, and over 90 percent of married mothers have a spouse with positive income. The NHIS suggests that cohabitation rates are much lower than marriage rates: About five percent of mothers report cohabiting with their child's father and two percent report cohabiting with someone else.

4 Empirical Methods

My empirical analysis leverages quasi-experimental variation in the timing of IHVPE program initiation across states and years. Using state-year paternity establishment data from 43 states, I estimate a "first-stage" relationship between paternity establishment and IHVPE:

$$Pat_{sy} = \beta_0 + \beta_1 * IHVPE_{sy} + \gamma' X_{sy} + \phi' C_{sy} + \mu_s + \alpha_y + \delta_s * y + \epsilon_{sy} \qquad (1)$$

for each state s and year y. Pat_{sy} is one of the three paternity establishment measures described above, $IHVPE_{sy}$ is an indicator for whether an IHVPE program is operating in state s in year y, X_{sy} is a vector of maternal and child characteristics at the state-year level, including the proportion births by white, black, and Hispanic mothers, the proportion male births, and the proportion births by mothers in different educational (less than high school, high school degree, some college, college or more) and age (less than 20, 20-24, 25-34, 35-44, 45+ years) groups. C_{sy} is a large vector of other

[21] This ratio ranges from 0.12 to 3.9 in the data. It can be larger than one if the total number of paternities established for all children under age 18 exceeds the total number of unmarried births in any given state-year.

state time-varying characteristics, including the state unemployment rate, the state minimum wage rate, the state poverty rate, the average AFDC/TANF benefit for a 4-person family, the proportion of the population receiving welfare benefits, the proportion of the population receiving Medicaid benefits, an indicator for whether the state's governor is Democratic, the fraction of the state house that is Democratic, and log child support enforcement spending in the year before, as well as indicators for whether different child support enforcement and joint custody laws are in effect, indicators for whether a state EITC has been enacted, and indicators for whether an AFDC waiver or the TANF program have been implemented.[22] μ_s is a state fixed effect, α_y is a year fixed effect, $\delta_s * y$ is a state-specific time trend, and ϵ_{sy} is the error term, which I cluster by state. The key coefficient of interest is β_1, which measures the effect of IHVPE on the paternity establishment rate.

The analyses of the CPS-CSS, March CPS, and NHIS data are on the individual level instead of the state-year level. I estimate versions of the following equation:

$$Y_{isty} = \beta_0 + \beta_1 * IHVPE_{sy} + \gamma' X_{isty} + \phi' C_{st} + \mu_s + \alpha_y + \delta_s * y + \epsilon_{isty} \qquad (2)$$

for each mother i, in state s, in survey year t, with a youngest (or sample) child born in year y. Here, Y_{isty} is an outcome of interest, such as an indicator for whether the mother is married to the father of her child. In this specification, X_{isty} contains individual maternal and child characteristics, including indicators for maternal age group at childbirth, indicators for maternal education groups, indicators for maternal race, an indicator for child sex, and indicators for the child's single years of age. I include state and child birth year fixed effects, as well as state-specific time trends, as before. All the state time-varying controls are the same as in equation (1). Again, the key coefficient of interest is β_1, which measures the effect of the existence of IHVPE in

[22] Maternal and child characteristics come from the National Center for Health Statistics (NCHS) Vital Statistics microdata from the universe of birth certificates in the United States over 1992-2005, collapsed into state-year cells. Data on various economic and program transfer variables come from a database maintained by the University of Kentucky Center for Poverty Research. Data on child support laws (automatic wage withholding, license revocation for non-payment, and the presence of a New Hires directory) and child support enforcement spending come from Nepomnyaschy and Garfinkel (2007) for states that established these policies prior to 1994, and from my own searches of OCSE reports and state statutes for the other states and years. Data on joint custody laws comes from Halla (2013). Data on state EITC (tax credits that supplement the federal EITC program) come from the Tax Credits for Working Families organization (see http://www.taxcreditsforworkingfamilies.org/ for more information). Information on AFDC waivers and TANF implementation comes from Bitler *et al.* (2006).

the child's state and year of birth on the outcome of interest.[23]

Identifying assumption The identifying assumption for the estimation of equations (1) and (2) is that the state-year variation in the timing of IHVPE implementation is uncorrelated with other unobserved time-varying determinants of the outcomes of interest. Note that by including state and year fixed effects, I control for all time-invariant state-level variables and overall time trends that might affect the outcomes. Further, the inclusion of state-specific linear time trends allows me to account for differential trends in the outcomes across states over the time period of analysis (although, as I show in Section 5, my results are not sensitive to the inclusion of linear time trends). Moreover, by including a large vector of state time-varying controls, I intend to make the set of unobserved characteristics—and hence the set of characteristics for which a correlation with treatment would be a concern—to be very small (although of course non-empty).

Additionally, while the identifying assumption is inherently untestable, I conduct some indirect tests to evaluate its plausibility. Table 1 presents the estimates of β_1 coefficients from regressions that use various maternal, child, and state time-varying characteristics as dependent variables in the estimation of equation (1) with state and year fixed effects, and state-specific time trends, but without any other controls. Out of 21 coefficients, none is statistically significant at the 5% level, and the majority of the (insignificant) effect magnitudes are relatively small. The only marginally significant coefficient is a positive effect on the likelihood of the mother being black.[24] The timing of IHVPE initiation is uncorrelated with numerous factors, including fertility (log num-

[23]Note that since the CPS-CSS, March CPS, and NHIS data do not contain information on the child's state of birth, I assign the child's state of residence in the year before the survey as the child's state of birth (for non-movers, this variable is also the state of residence in the year of survey). This may be a problematic assumption if IHVPE implementation is correlated with the likelihood of a mother moving out of her child's state of birth. For example, it may be the case that IHVPE has an effect on the likelihood of the mother moving in or out of the state where the father lives. However, I can test this directly in the CPS-CSS, and find no statistically significant effect of IHVPE on the likelihood of the father living in the same state as the mother and child at the time of the survey: The β_1 coefficient from estimating equation (2) with an indicator for the father living in the same state as the child as the outcome is -0.0088 with a standard error of 0.0075.

[24]Minority mothers have higher rates of births out of wedlock—for example, in 2009, while overall, 41 percent of all births were by unmarried mothers, 71 percent of all births by black mothers were by unmarried mothers. Thus, one concern might be that the negative effect on marriage in the CPS-CSS data is spuriously driven by the relative increase in black mothers. However, as shown below, the effects on marriage and other main outcomes are robust to the exclusion of black mothers.

ber of births), the educational and age distributions of mothers, state-level economic, political, and program transfer variables, the timing of AFDC/TANF legislation, the timing of state EITC programs, and the timing of other child support laws. Further, in Appendix Table 3, I show that there is no correlation between IHVPE initiation and pregnancy behaviors or birth outcomes.

Perhaps most importantly, these results address the concern that the effects on parental marriage discussed below could be driven by a correlation between IHVPE initiation and differential growth rates in out of wedlock births across states. However, the fact that I find no relationship between IHVPE and the proportion of births by unmarried mothers, implies that the effects on marriage operate through parental behavior *post-childbirth* rather than differential growth in unmarried births.

One might also imagine that if knowledge about IHVPE spreads, individuals may change their marriage behavior prior to childbirth in anticipation of IHVPE, implying decreases in marriage rates *at childbirth* in years following IHVPE implementation. However, Appendix Table 4 suggests that this is not the case in my data. There are no effects on the fraction of births by married women in the five years following IHVPE initiation (if anything, the only significant coefficient goes in the opposite direction of the main results).[25]

As discussed further in Section 7, my results do not rule out the possibility that some fertility and marriage-at-birth effects may materialize over a longer time span. However, as a longer-term analysis would require a more heroic "parallel trend" assumption in the difference-in-difference design, most of the results described below are based on data sets using around 5-10 years of post-IHVPE observations. The results in Table 1 and Appendix Table 4 imply that fertility and marriage-at-birth responses are not empirically relevant for this shorter-term analysis.

The lack of correlation between IHVPE and the timing of welfare reform is also worth highlighting. As shown by Bitler *et al.* (2006), state AFDC waivers were im-

[25] I have also estimated regressions for maternal marital status at birth separately for first- and higher-parity births. I find no statistically significant effects for either group. While mothers giving birth to higher-parity children are perhaps more likely to know about IHVPE before the time of childbirth, the prediction for their marital status at birth is unclear. On the one hand, they may be less likely to be married if they anticipate the benefits of the "intermediate" IHVPE option and make their marriage decision prior to giving birth. On the other hand, consistent with results discussed below, women giving birth to higher-parity children may be more likely to be married to new partners who are not the fathers of their prior-born children.

plemented in 15 states in 1993, 1994, and 1996, while TANF was implemented in all states in either 1997 or 1998. This variation is thus relatively concentrated in only a few states and indeed turns out to be uncorrelated with IHVPE program implementation over 1993-1999. Additionally, as shown throughout the paper, the inclusion of the welfare reform variables as controls does not meaningfully alter any of the analyses.

Finally, Table 1 shows that the IHVPE rollout is uncorrelated with other child support enforcement laws, such as universal wage withholding, the New Hires directory, and license revocation for non-payment. Note that many of the child support enforcement laws enacted during the sample time frame were at the federal level (Lerman and Sorenson, 2003) and are therefore fully controlled for by year fixed effects in all of my analyses. However, the fact that the major state time-varying child support measures turn out to be uncorrelated with the IHVPE timing is perhaps surprising since states surely viewed IHVPE as part of a package of enforcement tools. Yet as described in Section 2, the variation in IHVPE implementation stems largely from administrative issues—i.e., the length of time required for states to establish networks between child support offices, vital statistics registries, and birthing hospitals. None of the other enforcement policies requires the interaction of these three particular agencies, and this fact may help explain the lack of correlation between the timing of IHVPE initiation and the implementation of other measures.

5 Results

5.1 Effects of IHVPE on Paternity Establishment

Figure 1 plots the coefficients and 95% confidence intervals from estimating an event-study version of equation (1) that includes indicators for 5 years before, the year of, and 5 years after IHVPE implementation. Note that I assume that the first year IHVPE is in effect is the same as the year listed in Appendix Table 1 if only the year is listed or if the month of initiation is June or earlier. If the month of initiation is known and it is July or later, then I assume the first year the program is in effect is the following year. Consequently, at $\textit{year} - \textit{IHVPE first year} = 0$ in states where the month of IHVPE initiation is February-June or unknown, there may only be partial IHVPE treatment. Similarly, at $\textit{year} - \textit{IHVPE first year} = -1$ in states where the month of

IHVPE initiation is July or later, there may also be partial IHVPE treatment. In sum, as shown in the figure, there are effectively three possible treatment statuses: "pre-treatment" = $year - IHVPE\ first\ year \leq -2$, "maybe-treatment" = $year - IHVPE\ first\ year \in \{-1, 0\}$, and "post-treatment" = $year - IHVPE\ first\ year \geq 1$. The omitted category in Figure 1 is $year - IHVPE\ first\ year = -2$, the first year of sure pre-treatment.

Figure 1 shows that in the sure pre-IHVPE period (≤ -2), there are no statistically significant trends in paternity establishment rates. There is a slight increase in paternities at -1 and an even greater increase in paternities at 0, as expected in the "maybe-treatment" period. In the sure post-IHVPE period (≥ 1), there is a clear statistically significant increase in the paternity establishment rate.[26] The lack of statistically significant pre-trends in the pre-IHVPE period of the event-study graph yields support for the identifying assumption that the treatment and comparison states would have had similar trends in paternity establishment rates in the absence of IHVPE introduction.

Table 2 presents regression results, which suggest that IHVPE implementation led to a 21 percentage point increase in the paternity establishment rate—a 38 percent increase at the pre-IHVPE mean.[27] The results are consistent across different specifications and versions of the outcome variable (ratio of paternities over unmarried births, ratio of paternities over all births, and log paternities). Notably, once controls for maternal and child characteristics and state and year fixed effects are included (column 1), the inclusion of state time-varying characteristics, controls for child support laws and lagged child support enforcement spending, state EITC, and AFDC/TANF implementation, and state-specific linear time trends in columns 2-5 does not substantially alter the key coefficient of interest, providing additional support for the validity of the identification strategy.[28]

[26]Note that although using the number of births by unmarried mothers as the denominator would be problematic if IHVPE programs had an effect on the likelihood of marriage *at the time of birth*, my results show that there is no statistically significant association between IHVPE and the proportion of unmarried births (see Table 1 and the discussion in Section 4 above). Additionally, as discussed below, results using log paternities established and the ratio of paternities over total births are similar.

[27]The unweighted regressions yield very similar results: The key coefficient of interest is 0.2136 with a standard error of 0.0659.

[28]The sample size changes once controls for state time-varying characteristics are added as some of the variables are missing for certain state-year cells, and because I am missing data on the year of implementation

5.2 Effects of IHVPE on Marriage

After confirming that IHVPE programs in fact lead to a substantial increase in paternity establishment rates, I turn to the analysis of marriage behavior. Note that the assignment of treatment in the CPS-CSS and March CPS data sets is further complicated by the fact that I do not observe children's years or months of birth. Therefore, as discussed in Appendix A, I assign the child's approximate birth year as *birth year* = *survey year* − *child age* − *1.* Since the surveys are conducted in March, this procedure assigns the correct birth years to children born in April-December, and incorrectly assigns the years immediately prior to their true birth years for children born in January-March. Consequently, some children who are affected by IHVPE may be erroneously assigned to the pre-treatment group. In addition, children born in months prior to the month of program implementation in any given year may be unaffected by IHVPE but erroneously assigned to the treatment group. As a result, as with the paternities data, these data allow for three possible treatment statuses: "pre-treatment" = *child's birth year* − *IHVPE first year* ≤ -2, "maybe-treatment" = *child's birth year* − *IHVPE first year* $\in \{-1, 0\}$, and "post-treatment" = *child's birth year* − *IHVPE first year* ≥ 1.

Figure 2 plots the coefficients and 95% confidence intervals from estimating an event-study version of equation (2) that includes indicators for 5 years before, the year of, and 5 years after IHVPE implementation relative to the child's approximate birth year. The omitted category is *child's birth year* − *IHVPE first year* $= -2$, the first year of sure pre-treatment. The figure shows that in the certain pre-IHVPE period (≤ -2), there are no statistically significant trends in marriage rates. There is a slight (statistically insignificant) decline in marriage at -1 and a larger (statistically significant) decline in marriage at 0, as anticipated in the "maybe-treatment" period. Finally, in the sure post-IHVPE period (≥ 1), there is a further drop in parental marriage rates.

Table 3 presents results from estimating equation (2), which confirm the graphical evidence. The results suggest that IHVPE reduces the likelihood of parental marriage by about 2.7 percentage points—a 4 percent decrease at the pre-treatment mean of 75 percent in the CPS-CSS.[29] However, given that Table 1 shows a lack of correlation

for some child support laws for Kentucky and South Dakota.

[29] Unweighted regressions yield very similar results: The key coefficient of interest is -0.0209 with a

between IHVPE and parental marriage rates pre-childbirth, and since the CPS-CSS captures parental marriages that occur both before and after childbirth, it is necessary to assess the magnitude of this effect relative to the average parental *post-childbirth* marriage rate. Data from the Fragile Families and Child Well-Being Study show that about 13 percent of parents who were unmarried at childbirth will marry by the time their child turns five years old. With this estimate as a baseline, the approximate upper bound on the magnitude of the decrease in marriage post-childbirth is 21 percent (0.027/0.13).[30] Taken together with the results on paternity establishment rates, these magnitudes imply that for every new paternity established as a result of IHVPE, there are 0.13 fewer parental marriages occurring post-childbirth.[31]

Next, in the first five rows of Table 4, I consider the effects of IHVPE on other maternal relationship outcomes. I find that IHVPE, which reduces the likelihood that a mother is married to her child's biological father, increases the likelihood that she either remains never married, is married to someone other than the child's father, or is cohabiting with someone other than the child's father. There is no statistically significant effect on parental cohabitation. As noted in Section 2, unmarried cohabiting fathers are by law treated in the same way as unmarried non-cohabiting fathers in that they must also establish paternity to obtain any legal rights or responsibilities to their children. These results suggest that parents may adjust their behavior on the margin of marriage rather than non-marital cohabitation in response to IHVPE. Alternatively, it is possible that there are offsetting influences on cohabitation that lead to a net zero effect: following IHVPE, some parents who would have previously been married may now be more likely to cohabit, while other parents who would have previously cohabited may now be more likely to live separately. My data do not allow me to distinguish between these possibilities. There is also no statistically significant effect on divorce, suggesting that the "marginal" parental marriages that would have occured

standard error of 0.0062. Regressions using the larger March CPS sample also yield similar results: The key coefficient of interest is -0.0141 with a standard error of 0.0051.

[30] The Fragile Families and Child Well-Being Study follows cohorts of births in 1998-2000. Most states had implemented IHVPE by this time. Consequently, it is likely that the baseline post-childbirth marriage rate prior to IHVPE was larger than 13%. This would imply that the true magnitude of the effect is somewhat lower than 21%. Unfortunately, CPS-CSS data do not have information on the percentage of parents who marry after childbirth.

[31] This elasticity is calculated by dividing the parental marriage coefficient (0.0269) by the paternity establishment rate coefficient (0.2100).

in the absence of IHVPE were no more likely to end in divorce.

The bottom seven rows of Table 4 provide some suggestive evidence that IHVPE leads to an increase in the average “quality” of maternal partners. Following IHVPE, mothers are more likely to be married to men who are older and employed. These findings suggest that IHVPE enables some mothers to share a household with new partners who are of higher “quality” than their children’s fathers.

Appendix Table 5 addresses the important concern that the time frame of the IHVPE rollout overlaps with welfare reform, which has also been shown to affect marriage (Schoeni and Blank, 2000; Bitler *et al.*, 2006; Rosenbaum, 2003; Bitler *et al.*, 2004). Column 1 shows results from a parental marriage regression that does not include controls for the implementation of state AFDC waivers or TANF: The key coefficient on IHVPE is negative, statistically significant, and similar to the one presented in Table 3 (also replicated here in column 4). In contrast, columns 2 and 3 show that welfare reform is actually uncorrelated with parental marriage rates in my sample: The coefficients are insignificant, small, and opposite-signed from the main coefficient on IHVPE. These results are consistent with those in Table 1 showing that the timing of IHVPE implementation is uncorrelated with the implementation of state AFDC waivers or TANF, and suggest that the possible endogeneity of IHVPE with respect to welfare reform should not pose issues for my analysis.

Another important issue related to welfare stems from variation in child support disregard policies across states and years. Among welfare recipient mothers, any child support paid by the father above the disregard amount is fully taxed away by the state. In the case of zero disregard, all child support paid by the father goes to the state, while mothers and children receive no transfers, implying that paternity establishment may not be a preferrable option for these families. Empirically, this means that among mothers receiving welfare benefits, the effects of IHVPE should be concentrated in states and years with higher disregard amounts. Appendix Table 6 suggests that this conjecture is true: The negative effect on parental marriage seems to be driven by mothers with children born in states and years that had a minimum of $50 per month in disregard. However, the effects are not statistically significant likely because of reduced sample sizes (disregard policy data are only available for a limited number of years).

5.3 Effects on Father Involvement and Child Well-Being in Families with Unmarried Parents

The results thus far provide evidence that IHVPE changes parental decisions regarding their relationship contracts on two margins—many parents are more likely to establish paternity, but some are also less likely to get married. In this section, I analyze whether IHVPE achieves its intended goals of increasing father involvement and improving child well-being in families with *unmarried* parents.

Father involvement Studying the behavior and outcomes of families with unmarried parents is complicated by the IHVPE-induced decline in marriage, which leads to selection into the sample of unmarried parents. Recall that father involvement measures are only available in the CSS sample of unmarried mothers, and, consistent with the negative effect on marriage, IHVPE leads to a 1.5 percentage point increase in the likelihood of the mother being a CSS respondent (see the first row in Table 5). The other rows in the left panel of Table 5 present the results from estimating regression (2) on the entire CSS sample, without accounting for the issue of sample selection. Here, in addition to studying a variety of the father involvement variables available in the CSS individually, I also create a "father involvement index," following Kling *et al.* (2007). The index consists of all of the involvement outcomes listed in the table. I orient each outcome such that a higher value represents a better outcome, and then standardize each oriented outcome by subtracting the pre-IHVPE mean and dividing by the pre-IHVPE standard deviation. The index is an equally weighted average of the standardized outcomes.

The results show positive and fairly large effects on unmarried father involvement, driven by increases in fathers making all of their child support payments, providing food for their children, and covering their children's childcare and medical expenses.[32] Note that the increase in father involvement is consistent with both a positive selection effect (as fathers who would have been married to their children's mothers in the absence of IHVPE may be more likely to stay involved with their children and are now more likely to be included in the CSS sample) and with a direct effect of IHVPE on the

[32]As noted by Hanson *et al.* (1996) and others, one reason for a lack of effects on father visits and time involvement may be due to measurement error. In other surveys, mothers tend to under-report the number of days fathers spend with their children relative to what fathers report themselves.

father-child relationship for fathers who would have been unmarried in the absence of IHVPE.

To address the issue of selection and to better understand the direct effects of IHVPE on involvement among unmarried fathers, I perform a bounding exercise with the CSS data following Lee (2009). The idea is to estimate the involvement regressions on a sample of unmarried parents who would have been in the CSS in the absence of IHVPE by trimming the sample by the number of "extra" individuals who are selected in as a result of IHVPE. The upper bound estimate assumes that the "extra" individuals are located at the bottom of the outcome distribution (i.e., parents who would have otherwise been married have the worst outcomes), while the lower bound estimate assumes that the "extra" individuals are located at the top of the outcome distribution (i.e., parents who would have otherwise been married have the best outcomes). Appendix B provides further details. The results from this bounding exercise are presented in the middle and right-hand panels of Table 5. Although the lower-bound coefficients are not statistically significant, nearly all of the coefficients are positive and consistent with IHVPE increasing some measures of involvement among unmarried fathers, as intended by the policy.

Child well-being Do the increases in involvement among unmarried fathers translate into improvements in child well-being? To answer this question, I turn to data from the March CPS and the NHIS on child household resources and child mental and physical health, respectively. I limit the sample to mothers who are not married to their children's fathers, and again perform bounding exercises. As outcomes in the March CPS, I consider log total household income, indicators for the household living above the poverty line and living above 1.5×the poverty line, and an indicator for the household not receiving welfare income.[33] I also create a "child resource index," which consists of these components and is constructed in the same way as the "father involvement index" described above. From the NHIS, I consider four outcomes: a "physical health index," a "mental problems index," an indicator for the child having a learning

[33] Total household income is defined as the sum of total personal pre-tax incomes earned over the previous year of all adult individuals residing in the household. These include incomes from wages, businesses, farms, welfare transfers, SSI, retirement, unemployment transfers, worker's compensation, veterans' transfers, disability, dividends, rent, educational assistance, child support, alimony, financial assistance from friends and family, and other sources.

disability (available for children aged 3+ only), and an indicator for the child having at least one doctor visit in the past 12 months (a measure of access to preventative care). I construct the "physical health index" in the same way as the "father involvement index" and the "child resource index'; the "mental problems index" is provided by the NHIS and records the number of behavioral problems exhibited by the child, where a higher value denotes more problems.[34]

Table 6 presents the results from these specifications. For child resources, the lower bound coefficients are small in magnitude, negative, and insignificant, while the upper bound estimates are large, positive, and significant. Overall, given that the bounds contain such wide ranges of estimates, it is difficult to draw definitive conclusions on whether IHVPE leads to improved resources for children living with single mothers. When considering child health, Table 6 shows a statistically significant reduction in the incidence of learning disabilities among children living with unmarried mothers. However, there are no changes to physical health or access to care.

5.4 Overall Effects of IHVPE on Parental Investments and Child Well-Being

The previous section demonstrated that IHVPE has accomplished some of its intended goals by increasing unmarried father involvement and possibly improving some measures of child well-being in single-mother households. However, the average effects on overall parental investments and child well-being may be complicated by the robust finding that IHVPE induces some parents to opt out of marriage. The net effects thus depend on the changes in behaviors among both the parents who would have never been married in the absence of IHVPE (i.e., those studied in the preceding section) and the parents who would have otherwise been married. In this section, I attempt to quantify the overall average effects of IHVPE on measures of parental investment and child well-being using data on outcomes that are available for *both* married and unmarried parents.

[34]The "mental problems" index is available for sample children aged 2-3 only. The "physical health index" consists of the following outcomes: any asthma, any asthma episodes in the last 12 months, 3+ ear infections in the last 12 months, any skin allergies in the last 12 months, frequent diarrhea in the last 12 months, anemia in the last 12 months, any hearing trouble, any seeing trouble, any problems limiting ability to walk/play/run. The "access to care index" is also constructed in the same way and consists of the following outcomes: any well-child visits in the last 12 months, any doctor visits in the last 12 months.

The first row in Table 7 examines children's private health insurance coverage, which is a parental investment proxy available for individuals in both the CSS and non-CSS samples. I find that IHVPE leads to a 4 percent reduction in the likelihood that a child has private health insurance. I distinguish between private coverage provided by individuals in and outside the household, and coverage through public health insurance programs such as Medicaid and CHIP in Appendix Table 7.[35] I show that the negative effect on private health insurance coverage is driven entirely by a reduction in coverage provided by members of the household and is not compensated by any changes in insurance provision by individuals outside the household. These findings imply that the decline in children's private health insurance coverage is driven by fathers' behavior: Following IHVPE, fathers, who are less likely to be married and in the same household, are also less likely to provide health insurance for their children. Note that this decline in private health insurance coverage is not necessarily a "mechanical" result of the decline in parental marriage as would be the case if unmarried fathers were legally unable to provide insurance for their children. In fact, child support agreements often stipulate that unmarried non-custodial fathers provide health insurance coverage for their children through their employers if it is offered.[36]

The next row in Table 7 shows that mothers are 3 percent more likely to be employed following IHVPE, which may suggest that there is a net decline in fathers' financial support requiring mothers to earn their own incomes. In Appendix Table 8, I document that the labor supply effect is consistent across different definitions and that there is no effect on wages or hours worked on the intensive margin. These results suggest that the effect of IHVPE operates on the extensive margin by inducing more mothers of young children to enter the workforce.

Appendix Table 9 attempts to study the overall impacts on other father involvement variables. While the CSS variables are only limited to unmarried non-custodial fathers, I include married fathers by assuming that they "make all child support payments," "make child support payments on time most or all of the time," have visitation rights and legal custody, spend the whole year with the child, provide food, clothes, and gifts for the child, and cover childcare and medical expenses. While these assumptions

[35]Information on CHIP coverage is only available in the CPS-CSS in 2002, 2004, 2006, and 2008.
[36]See: http://publications.usa.gov/epublications/childenf/obligate.htm.

certainly do not hold true for all married fathers, they are consistent with correlational evidence that married fathers have higher quality parenting skills and greater degree of involvement with their children than unmarried fathers (Cooksey and Craig, 1998; Kalmijn, 1999; Carlson *et al.*, 2008). The results show that when married fathers are included in the analysis, the effects on most involvement variables are either zero or negative. These findings provide some suggestive evidence that declines in various measures of involvement among fathers who opt out of marriage may have actually outweighed any increases in involvement among fathers who would have remained unmarried in the absence of IHVPE.[37]

The remainder of Table 7 looks at overall child household resources and child health outcomes. These results suggest that any increases in single-mother household resources among families with parents who would have never been married in the absence of IHVPE may be offset by some decreases in household resources among families with parents who would have otherwise been married. It seems that the reductions in child resources among "switchers out of marriage" arise despite the fact that some of these mothers are more likely to be employed and more likely to have new partners who are also employed.

Finally, there are no net effects of IHVPE on children's physical or mental health. There is, however, a small decline in children's access to care, which may at least partially be driven by the overall decrease in children's private health insurance coverage.

[37]One potential concern with this analysis is that the negative effects on father involvement are mechanically driven by the decline in marriage—i.e., there are mechanically fewer fathers with a value of "1" for the indicator variables on father involvement due to the negative effect on marriage. To address this, I have estimated regressions treating the only variable available for both married and unmarried parents—children's private health insurance coverage—in the same way by assigning a value of "1" for all married parents. Clearly, this is not an accurate assumption as only 78 percent of children in married households have private health insurance. However, as shown in Appendix Table 10, analysis with this imputed health insurance variable yields results very similar to those from using the true child private health insurance coverage variable. The p-value on the F-test for equality of coefficients across the models is 0.8514, suggesting that the coefficients in the two models are not statistically different from each other. Thus, although not all married fathers provide complete involvement and support for their children, as long as married fathers are more likely than unmarried fathers to do so, the method of assigning values of "1" for measures of involvement for married fathers provides a reasonable upper bound for the magnitude of the overall IHVPE effect when accounting for the decline in marriage.

5.5 Additional Results

Tables 8 and 9 present a series of robustness specifications for some of the main outcomes considered above (the paternity establishment rate, mother is married to her child's father, mother is never married, mother is married to a new partner, mother's spouse has any own wage income, father involvement index, child has any private health insurance coverage, mother worked any usual hours in the previous year, child resource index, physical health index, mental problems index, child has any learning disability, and child had any doctor visits in past 12 months).[38] For ease of comparison, column 1 in both tables lists all the main results for these outcomes.

As noted above, the lack of information about children's exact birth dates in the data together with missing information on IHVPE implementation month for some states create a "maybe-treatment" group in which some children who are affected by IHVPE may be erroneously assigned to the pre-treatment group (i.e., *child's birth year − IHVPE first year* = −1), while some children who are not affected by IHVPE may be erroneously assigned to the treatment group (i.e., *child's birth year − IHVPE first year* = 0). Column 2 addresses these concerns about measurement error in treatment assignment by omitting the "maybe-treatment" group from the analysis. The coefficients from these specifications are similar to those in column 1, although not always statistically significant due to smaller sample sizes and reduced variation.

Another important issue is that most of IHVPE implementation occurs between 1993 and 1999, yet the paternities sample covers years 1992—2005, the CPS-CSS sample covers births in 1988—2008, the March CPS sample covers births in 1983—2010, and the NHIS sample covers births in 1989-2010, suggesting that over 50 percent of observations in these analyses come from the post-treatment periods. While these larger time frames allow for bigger sample sizes and more power, it is important to check whether the estimated effects are concentrated in the sample space where the variation actually occurs. Column 3 in Tables 8 and 9 limits the samples to narrower windows surrounding IHVPE implementation: The paternities data are limited to years 1992-2000 (1 year before and after the 1993-1999 IHVPE rollout), while the individual-level data are limited to mothers of children born in 1988-2004 (5 years before and after the

[38] For confidentiality purposes, robustness checks that are conducted on sub-samples of states in the NHIS data cannot be reported. The results are similar to the main ones presented in the paper.

1993-1999 IHVPE rollout). The coefficients from these regressions are similar to the main results.

Column 4 addresses concerns about measurement error in treatment status in an alternative way—by restricting analysis to the 27 states for which I have the most accurate information on IHVPE implementation.[39] These results also show coefficients that are comparable to those in column 1, suggesting that mis-measurement of the IHVPE implementation date should not pose serious concerns for the main analysis.

Column 5 presents coefficients from regressions controlling for quadratic state-specific time trends and shows that the results are robust to their inclusion. In column 6, I omit black mothers from individual-level analyses since Table 1 suggests a marginally significant positive correlation between IHVPE initiation and births by black mothers. The results are robust to this sample restriction as well. Finally, in columns 7-9, I estimate regressions omitting three large states one at a time; these results are also robust and similar to the main ones in column 1.[40]

On the whole, these robustness tests suggest that the identified effects presented in this paper are not subject to serious biases due to the endogeneity of IHVPE implementation, measurement error, or sample selection.

6 A Theoretical Framework

The empirical results presented above shed light on how an exogenous change to parental relationship contract options affects family structure, parental investments, and child well-being. As intended by the policy, paternity establishment rates increase substantially, and unmarried fathers become more involved with their families along some dimensions—they make child support payments, provide food for their children, and cover some of their children's childcare and medical expenses. This increased fa-

[39]Since I collected data on the timing of IHVPE initiation from multiple sources, there are potential concerns about the accuracy of this information. The 27 "good info" states (AZ, CA, CO, CT, DE, DC, FL, ID, IL, IN, KY, LA, MD, MA, MI, MN, MS, MO, NJ, NY, OR, PA, RI, TN, TX, WA, WI) consist of four groups: 1) states for which I got data from Garfinkel & Nepomnyaschy, 2) states where I was able to speak directly to a representative from the child support office or from the IHVPE office, 3) states where the year and month of program implementation was listed on the program's website, and 4) states where the implementation year and month were clear from the statutes on *LexisNexis Academic*. For all other states not included in the "good info" subset, I assigned the year of implementation as the year of legislation or statute revision, which may not always correspond exactly to program implementation.

[40]Results from regressions excluding states other than CA, NY, and TX are also similar and available upon request.

ther involvement translates into some improvements in the well-being of children in single-mother households. However, as some of the increase in paternity establishment is actually driven by a reduction in parental marriage, the overall effects on parental investment and child well-being depend also on the changes in the outcomes of the families in which the parents would have otherwise been married. When I study outcomes for the entire sample of unmarried and married parents, I find zero or small negative effects on available measures of parental investments and child well-being, such as private health insurance provision, income in the child's household of residence, child mental/physical health, and the child's access to healthcare.

What can we learn from this analysis about the trade-offs that parents face regarding their relationships with each other and their children? In this section, I present a simple model that can illustrate how a policy that lowers the cost of a relationship contract granting legal parental rights and obligations to unmarried fathers can have complex consequences for family structure and child well-being.

Consider parents who derive utility from child quality (Q), private adult consumption (C), and match quality (θ), and who can choose between three relationship contracts: marriage (m), paternity (p), and no legal relationship (n). I denote mothers by subscript x and fathers by subscript y, and represent the parental utility functions as follows:[41]

For each parent $i \in \{x, y\}$,

$$U_{ij} = \beta_i U_Q\Big(Q(K_j, \delta_j \theta)\Big) + (1 - \beta_i) U_C\Big(C_{ij}\Big) + \delta_j \theta - \gamma_{ij} d_j \qquad j \in \{m, p, n\}$$

$U_Q(\cdot)$ represents utility from child quality, $U_C(\cdot)$ represents utility from adult consumption, and β_i, $0 < \beta_i < 1$, represents the weight each parent places on his/her preferences toward children relative to other adult consumption goods. In each state j, child quality, Q, is a positive concave function of total parental investment, K_j. Additionally, as in Tartari (2014), I allow for match quality, θ, to enter the child quality production function, and assume that investments and match quality are complements ($\frac{\partial^2 Q}{\partial K \partial \theta} > 0$). Intuitively, higher match quality leads to less parental conflict and more productive investments. Note investments here can broadly encompass both financial and non-pecuniary (e.g., time) expenditures.

[41] I assume quasi-linear utility functions, which follows Edlund (2011), Flinn (2000), Chiappori and Oreffice (2008), Brown and Flinn (2011), and Roff and Lugo-Gil (2012), among others.

Couples are heterogeneous in θ, which is distributed according to a cumulative distribution function, $F(\theta)$, with support $(\underline{\theta}, \overline{\theta})$. θ can take on both positive and negative values; the negative values imply that some parents experience a *cost* from interacting with each other. The degree to which match quality can impact child quality and parental utility depends on the level of parental interaction and is captured by parameter δ_j. δ_j varies by state j and is proportional to the amount of parental rights that the father has. As fathers have full parental rights in marriage, fewer rights in paternity, and even fewer rights in the state of no relationship, I assume that: $\delta_m = 1$, $\delta_p = \alpha$, and $\delta_n = \rho$, for some $0 \leq \rho < \alpha < 1$. Note that while legally fathers have no rights to their children in the "no relationship" state, I allow for the possibility of some (informal) interaction between parents in this state.

Finally, there are fixed costs associated with entering into the marriage and paternity establishment contracts that are separate from match quality: $d_m > 0$ and $d_p > 0$, while $d_n = 0$. Intuitively, one can think of the marriage cost, d_m, as incorporating the financial and non-pecuniary wedding-related costs, the legal and emotional costs of potential future divorce, as well as the option value costs associated with a limited ability to search for new partners. The paternity cost, d_p, incorporates the informational, financial, time, administrative, and legal costs related to establishing paternity.[42]

Modes of interaction Next, it is necessary to characterize the modes of interaction between parents. In marriage, it is reasonable to assume that parents expect to cooperate. They have transferrable utility, and maximize their joint utility subject to a joint income constraint, which is the sum of their individual incomes, Y_x and Y_y:[43]

$$\max_{K_m, C_{xm}, C_{ym}} (\beta_x + \beta_y) U_Q\Big(Q(K_m, \theta)\Big) + (1 - \beta_x) U_C\Big(C_{xm}\Big) + (1 - \beta_y) U_C\Big(C_{ym}\Big) + 2\theta - d_m$$

$$\text{s.t.} \quad K_m + C_{xm} + C_{ym} = Y_x + Y_y$$

Outside marriage, following the literature (e.g., Weiss and Willis, 1985; Del Boca and Flinn, 1995; Willis, 1999; Roff and Lugo-Gil, 2012), I assume the parents do *not* bargain cooperatively and instead face a static Stackelberg game, where the father

[42]The parameter γ_{ij} ($0 \leq \gamma_{ij} \leq 1$, $\gamma_{xj} + \gamma_{yj} = 1$) depicts how the parents share the fixed costs of marriage and paternity establishment, and is exogenous to the model.

[43]Prices are normalized to one.

chooses his child support payment, s, given the mother's response function, while the mother chooses her spending on the child given the father's payment.[44] In states $j \in \{p, n\}$, the maternal response function is given by $K_j(s_j)^*$, the solution to the following maximization problem:

$$\max_{K_j, C_{xj}} \beta_x U_Q\Big(Q(K_j, \delta_j \theta)\Big) + (1-\beta_x) U_C\Big(C_{xp}\Big) + \delta_j \theta - \gamma_{xj} d_j \qquad \text{s.t.} \quad K_j + C_{xj} = Y_x + s_j$$

The father then maximizes his indirect utility, taking into account the maternal optimal response function for child investment, $K_j(s_j)^*$:

$$\max_{s_j} \beta_y U_Q\Big(Q(K_j(s_j)^*, \delta_j \theta)\Big) + (1-\beta_y) U_C\Big(Y_y - s_j\Big) + \delta_j \theta - \gamma_{yj} d_j \qquad \text{s.t.} \quad s_j \geq \overline{s}_j$$

where $\overline{s}_p = \overline{s}$ (for some $0 < \overline{s} < Y_y$), and $\overline{s}_n = 0$. In other words, in the paternity state, fathers' child support payments must comply with child support orders; by contrast, fathers who do not enter the paternity contract are not subject to any child support order constraints.[45]

Choosing the optimal state Given their match quality, θ, and the fixed costs of entering marriage and paternity (d_m and d_p, respectively), each parent prefers the state that gives him/her the highest indirect utility value. Non-marriage is unilateral relative to marriage; in other words if one parent prefers to be married while the other does not, they will *not* choose marriage. However, the paternity state is unilateral relative to no relationship, as the mother can essentially force the father to establish paternity by taking him to court.

[44] Note that some parents who establish paternity at the time of childbirth may be cohabiting and thus arguably behaving as a married couple. It may seem that an assumption of transferrable utility is more realistic for these couples. However, cohabiting unions are quite unstable in the U.S.—for example, according to data from the Fragile Families and Child Well-Being Study, less than 35% of unmarried parents still live together by the time their child is five years old. Thus, a non-cooperative Stackelberg setting where the father gains partial rights and responsibilities to his child is likely a more realistic representation of the paternity state over the long term.

[45] A more complex framework would distinguish between formal child support payments and informal investments, and allow for them to be substitutes (Lerman and Sorenson, 2003). Such a framework would predict opposite impacts in formal vs. informal investments (e.g., an increase in child support payments and a decrease in time spent with children). However, in my data, I find no evidence of such substitution suggesting that the simplified framework presented here is sufficient for capturing the main considerations in fathers' investment decisions.

This model implies that, for a high enough level of match quality, child investment should be higher in marriage than in paternity, and higher in paternity than in no relationship ($K_m^* > K_p^* > K_n^*$). Intuitively, the parents face no cooperation problems and thus achieve the first-best child investment level in marriage. Parents invest less outside marriage (in the paternity and no relationship states) because the lack of full cooperation means that the father would transfer more if he could be assured that the mother would spend more on the child, and the mother would invest more in the child if she could secure this higher payment (see, e.g., Weiss and Willis, 1985; Roff, 2008 for more discussion of this). Moreover, fathers' transfers are likely lower in no relationship than in paternity as they are not subject to a positive child support order constraint.[46]

However, as parental match quality is complementary to child investments, investments by low-match-quality parents may be less productive in marriage than outside marriage. In general, while parental utility from child investments is increasing with the relationship level, their sensitivity to match quality rises as well: This trade-off between the benefit of joint child investment and the possible cost of parent interaction is key to this framework. Thus, this model implies that average match quality should be higher among parents who choose marriage relative to paternity and higher among parents who choose paternity relative to no relationship. In this sense, paternity can be seen as an "intermediate" state between the "extremes" of marriage and no relationship.

How does IHVPE affect optimal choices? IHVPE introduces an easily accessible and inexpensive way to establish paternity at childbirth. Consequently, one can model the implementation of IHVPE as an exogenous decrease in d_p, the fixed cost of entering into the paternity establishment contract.

Not surprisingly, as the costs of establishing paternity are lowered, more parents choose this option. The switchers into paternity include both parents who would have previously maintained no relationship *and* parents who would have previously been married. In the simplest version of the model, where both child investment and

[46]The increasing relationship between parental rights and fathers' financial transfers to mothers is also highlighted in models where rights to children are treated as private goods and marriage is modeled as a transfer of rights from mothers to fathers (e.g. Edlund and Korn, 2002; Edlund and Pande, 2002; Mincy *et al.*, 2005b).

match quality are monotonically increasing with the optimal relationship level, we can represent the predicted effects graphically in Appendix Figure 3. Here, parents who switch from no relationship to paternity increase their level of child investment, while parents who switch from marriage to paternity decrease their level of child investment.

This model also highlights why the implications of IHVPE for parental investment and child quality are complex. While the switchers from no relationship into paternity are predicted to increase their investment levels, the switchers from marriage into paternity are likely to decrease their child investment levels. Thus, the net effect depends on the relative magnitudes of the two opposing forces. Moreover, the degree to which changes in investments translate into changes in child quality, Q, depends on the child quality production function and its sensitivity to match quality. It is possible that the two types of switchers have different child production functions. In fact, it is likely that the switchers from no relationship to paternity (i.e., parents who would have never gotten married in the absence of IHVPE) are sufficiently disadvantaged and lack both resources as well as important cognitive and non-cognitive skills, which may prevent them from making highly productive child investments (Cunha and Heckman, 2007; Heckman *et al.*, 2014). By contrast, perhaps the switchers from marriage into paternity (i.e., parents who would have been married in the absence of IHVPE) are more advantaged and thus have more efficient child quality production functions. If this is the case, then the absolute value of the negative impact on overall child quality from the decrease in investment among switchers out of marriage may be larger in magnitude relative to the positive impact stemming from the increase in investment among switchers out of no relationship.

Additionally, child quality among switchers out of marriage may be affected through at least two more channels. First, child quality is more sensitive to parental match quality in marriage than in paternity. Thus, it is possible that children whose parents have sufficiently low match quality are actually better off if their parents are not married. Second, I find above that as a result of the decline in parental marriage, mothers are more likely to be married to or cohabiting with new partners (an aspect not explicitly modeled here). Child quality may suffer as a result of interactions with mothers' new partners (Hofferth, 2006); alternatively, child quality may benefit from the higher quality of maternal new partners.

In sum, this framework shows that while IHVPE should increase paternity establishment rates, there may also be some “crowd-out” of parental marriage. The model posits that the reason for this result stems from a trade-off between the benefits of joint child investment and the costs of interaction among parents with sufficiently low match quality.[47] As it is essentially impossible to observe match quality in data, and as parental investments and child quality/well-being can only be observed imperfectly, I want to emphasize that this model is unlikely to be the only way to rationalize the empirical results I find. Nevertheless, this model serves to highlight why some parents might prefer paternity establishment over marriage, and why a policy that seeks to increase involvement among unmarried fathers may not serve as the “stepping-stone” to marriage as some advocates may have hoped.

I implement two additional empirical exercises to shed some more light on the plausibility of this model. First, I examine whether the decline in marriage is actually driven by the marginal parents who are expected to switch out of marriage as a result of IHVPE. The above framework posits an increasing relationship between match quality and the level of parental interaction—“high-quality” matches marry, “medium-quality” matches establish paternity, while “low-quality” matches maintain no relationship. Consequently, parents on the margin of marriage should be located somewhere in the middle of the match quality distribution. While I cannot test this conjecture directly since I do not observe match quality in the data, I construct a proxy for match quality by estimating a predicted probability of marriage based on mothers' observable characterstics using pre-IHVPE data.[48] I split the sample into deciles of the pre-IHVPE predicted marriage distribution, and estimate separate regressions of

[47]Recent sociological work highlights the relevance of the trade-off between children and partner interaction among parents who have children out of wedlock. Edin and Kefalas (2005) argue that many poor women choose to have children outside marriage because of the high value they place on their roles as mothers in light of their disadvantaged circumstances that present them with few other meaningful opportunities. They are cautious about marriage as they do not want to commit to a relationship contract that could jeopardize their well-being and the well-being of their children. This evidence suggests that these women take match quality into serious consideration when determining their relationships with their children's fathers. Similarly, in contrast to the common stereotype of unmarried fathers being “dead-beat dads,” recent surveys and anthropological studies suggest that many unmarried fathers derive substantial utility from their children and view having parental rights as vital to their roles as men (Roy, 1999; Pate, 2002; Edin and Nelson, 2013).

[48]Specifically, I use a probit model to estimate a regression of the form: $Married_{isty} = \beta_0 + \gamma' X_{isty} + \phi' C_{st} + \mu_s + \alpha_y + \delta_s * y + \epsilon_{sy}$, where $Married_{isty}$ is an indicator for the mother being married to the child's father, and the rest of the coefficients and variables are defined as before.

equation (2) with parental marriage as the outcome. Appendix Figure 4 presents the coefficients.[49] The figure shows that the negative effect of IHVPE on parental marriage is concentrated around the 40th and 50th percentiles of the match quality proxy distribution. Consistent with the framework, this figure suggests that parents with very low and very high values for the match quality proxy do not change their marital behavior as a result of lower costs to establishing paternity; the marginal parents induced out of marriage by IHVPE indeed seem to be in the middle of the match quality distribution.

Second, I investigate the "complier characteristics" (Angrist and Pischke, 2009) of the parents induced out of marriage as a result of the introduction of IHVPE. Note that while IHVPE is not a valid instrument for studying the causal effects of marriage because it affects other margins of parental behavior (and thus does not satisfy the exclusion restriction), one can still use the methods described in Angrist and Pischke (2009) to study the characteristics of the parents who "comply" with IHVPE by not marrying. In Appendix Table 11, I present the relative likelihood ratios for "complier" mothers having different characteristics.[50] The results suggest that IHVPE "compliers" induced out of marriage tend to be mothers who are aged 20-24 at childbirth, have a high school degree, and are either black or Hispanic. While the "complier" mothers are relatively disadvantaged, they are not at the bottom of the education or age-at-childbirth distributions, consistent with the fact that they are in the middle of the match quality distribution and on the margin of marriage.

7 Conclusion

As more than one third of all children in the U.S. are born to unmarried women every year, many policymakers seek to address the needs of these children and their families. A number of policies, motivated by the fact that children raised in two-parent households fare better than children in single-mother households, target unmarried fathers by providing them with legal rights and obligations to their children and encouraging them to become more involved with their families. An important feature of such fam-

[49] The 95% confidence intervals (shown as dashed bars) use standard errors clustered on the state level. The listed p-values correspond to estimates from a wild cluster bootstrap (Cameron *et al.*, 2008) to account for the fact that the sample is split along deciles of a predicted variable.

[50] The relative likelihood ratio is calculated by dividing the IHVPE coefficient for the subsample defined by each characteristic by the overall IHVPE coefficient (0.027).

ily policies, however, is that they effectively expand the parental relationship contract space by offering parents an alternative legal contract option to marriage. As a result, their impacts on family and child welfare may be complicated by parental behavioral responses that affect family structure.

In this paper, I provide a detailed analysis of one of the largest expansions in the parental relationship contract space that resulted from the implementation of IHVPE programs nationwide. The programs substantially reduced the cost of paternity establishment for unmarried fathers, which is the only available legal contract that grants them partial rights and responsibilities to their children. Using variation in the timing of IHVPE implementation across states, I show that while IHVPE substantially increases paternity establishment rates, it also reduces parental marriage rates. I find that mothers, who are less likely to be married to their children's fathers, are more likely to remain single or be married to or cohabiting with new "higher-quality" partners in terms of observable characteristics such as age and employment. Although there is some evidence of higher levels of child investment among unmarried fathers and improved well-being among children in single-mother households, I show that the *overall* effects on several available measures of investment are either zero or negative once I account for the selection out of marriage. Finally, I show that IHVPE has little effect on overall child household resources or child mental or physical health.

This overall lack of benefits may be surprising as IHVPE was implemented with the goal of improving the welfare of children born to unmarried parents. However, although the policy achieved some of its goals by raising paternity establishment rates and increasing father involvement and child well-being among families with parents who would have never gotten married, it also enabled some parents to opt out of marriage. In sum, the affected parents responded to the expansion in the relationship contract space in a way that left the average well-being of their children unchanged.

It is important to acknowledge that this paper captures relatively short-term impacts of IHVPE. It is possible that in the longer-run, individuals may adjust their behavior on a variety of other margins including fertility and marriage at birth, and that these behaviors could have additional consequences on child and family welfare. In fact, in a related recent paper, Halla (2013) finds that joint custody reforms that occurred in the U.S. in the 1970s had long-term implications on the incidence of marriage, fertility, and divorce.[51] My paper focuses on a more recent family policy change in the 1990s. Additionally, the study of longer-term effects necessitates a "parallel-trend" assumption (inherent to a difference-in-difference design) for a long time period, which may be harder to satisfy. As such, this paper instead provides a comprehensive examination of relatively immediate parental responses to IHVPE.

The results from my analysis suggest that parents who have children out of wedlock face an important trade-off between having a relationship with one another and being involved with their children. The fact that more parents opt out of marriage when given an alternative relationship contract option implies that there exists demand for contracts that decouple parental obligations to their children from their spousal commitments to one another. Yet the existing alternative contract imparts fairly minimal rights and obligations on fathers and leaves most of the parenting burden to the mothers. As a result, when parents choose paternity establishment over marriage, fathers provide less financial and non-pecuniary support to their children.

While this study provides new evidence on the implications of existing relationship contracts for family behavior and well-being in the U.S., it raises important questions regarding optimal contract design. Perhaps one way to address the unintended consequence of reduced involvement among fathers who would have otherwise been married may be to design a contract that grants equal parental rights and responsibilities to mothers and fathers and does not require any spousal commitments between them. Future research may consider these questions by developing new theories regarding optimal parental relationship contract design, studying the welfare impacts of more recent contracts such as domestic partnerships, and thoroughly examining *why* growing numbers of disadvantaged mothers and fathers prefer to decouple parenting from partnership.

References

Aizer, A. and McLanahan, S. (2006). The impact of child support enforcement on fertility, parental investments, and child well-being. *Journal of Human Resources*, **41** (1), 28–45.

[51] Another related working paper by Takayama and Tanaka (2011) shows that higher state child support enforcement spending is associated with higher marriage rates and lower out of wedlock birth rates.

Angrist, J. and Pischke, J. (2009). *Mostly harmless econometrics: An empiricist's companion*. Princeton Univ Press.

Beauchamp, A., Sanzenbacher, G., Seitz, S. and Skira, M. (2014). Deadbeat dads, Boston College, unpublished manuscript.

Becker, G. (1993). *A treatise on the family*. Harvard Univ Press.

Bitler, M., Gelbach, J. and Hoynes, H. (2006). Welfare reform and children's living arrangements. *Journal of Human Resources*, **41** (1), 1–27.

—, —, — and Zavodny, M. (2004). The impact of welfare reform on marriage and divorce. *Demography*, **41** (2), 213–236.

Brien, M. J., Lillard, L. A. and Stern, S. (2006). Cohabitation, marriage, and divorce in a model of match quality. *International Economic Review*, **47** (2), 451–494.

Brown, M. and Flinn, C. J. (2011). Family law effects on divorce, fertility and child investment, New York University, unpublished manuscript.

Browning, M., Chiappori, P. and Weiss, Y. (2014). *Economics of the Family*. Cambridge Surveys of Economic Literature, Cambridge University Press.

Cameron, A. C., Gelbach, J. B. and Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, **90** (3), 414–427.

Cancian, M., Meyer, D. and Roff, J. (2007). *Testing New Ways to Increase the Economic Well-Being of Single-Parent Families: The Effects of Child Support Policies on Welfare Participants*. Discussion Paper 1330-07, Institute for Research on Poverty.

Carlson, M., McLanahan, S. and Brooks-Gunn, J. (2008). Coparenting and nonresident fathers involvement with young children after a nonmarital birth. *Demography*, **45** (2), 461–488.

Chiappori, P. and Oreffice, S. (2008). Birth control and female empowerment: An equilibrium analysis. *Journal of Political Economy*, **116** (1), 113–140.

Cooksey, E. and Craig, P. (1998). Parenting from a distance: The effects of paternal characteristics on contact between nonresidential fathers and their children. *Demography*, **35** (2), 187–200.

Cunha, F. and Heckman, J. (2007). The technology of skill formation. *The American Economic Review*, **97** (2), 31–47.

Del Boca, D. and Flinn, C. J. (1995). Rationalizing child-support decisions. *The American Economic Review*, pp. 1241–1262.

— and Ribero, R. (2003). Visitations and transfers after divorce. *Review of the Economics of the Household*, **1**, 187–204.

Edin, K. and Kefalas, M. (2005). *Promises I Can Keep: Why Poor Women Put Motherhood Before Marriage*. University of California Press.

— and Nelson, T. J. (2013). *Doing the Best I Can: Fatherhood in the Inner City*. University of California Press.

Edlund, L. (2011). The role of paternity presumption and custodial rights for understanding marriage patterns, Columbia University, unpublished manuscript.

— and Korn, E. (2002). A theory of prostitution. *Journal of Political Economy*, **110** (1), 181–214.

— and Pande, R. (2002). Why have women become left-wing? the political gender gap and the decline in marriage. *The Quarterly Journal of Economics*, **117** (3), 917–961.

Flinn, C. J. (2000). Modes of interaction between divorced parents. *International Economic Review*, **41** (3), 545–578.

Freeman, R. and Waldfogel, J. (2001). Dunning delinquent dads: The effects of child support enforcement policy on child support receipt by never married women. *Journal of Human Resources*, **36** (2), 207–225.

Garfinkel, I., Mclanahan, S., Meyer, D. and Seltzer, J. (1998). *Fathers under fire: The revolution in child support enforcement*. Russell Sage Foundation Publications.

Halla, M. (2013). The effect of joint custody on family outcomes. *Journal of the European Economic Association*, **11** (2), 278–315.

Hanson, T. L., Garfinkel, I., McLanahan, S. S. and Miller, C. K. (1996). Trends in child support outcomes. *Demography*, **33** (4), 483–496.

Heckman, J. J., Humphries, J. E. and Kautz, T. (2014). *The Myth of Achievement Tests: The GED and the Role of Character in American Life*. University of Chicago Press.

Heim, B. T. (2003). Does child support enforcement reduce divorce rates? a reexamination. *Journal of Human Resources*, **38** (4), 773–791.

Hofferth, S. L. (2006). Residential father family type and child well-being: Investment versus selection. *Demography*, **43** (1), 53–77.

Kalmijn, M. (1999). Father involvement in childrearing and the perceived stability of marriage. *Journal of Marriage and the Family*, pp. 409–421.

King, M., Ruggles, S., Aledander, T., Flood, S., Genadek, K., Schroeder, M., Trampe, B. and Vick, R. (2010). Integrated public use microdata series, current population survey: Version 3.0. [machine-readable database]. Minneapolis: University of Minnesota.

Kling, J. R., Liebman, J. B. and Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, **75** (1), 83–119.

Lee, D. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *Review of Economic Studies*, **76** (3), 1071–1102.

Lerman, R. I. and Sorenson, E. (2003). Child support: Interactions between private and public transfers. In *Means-tested transfer programs in the United States*, University of Chicago Press, pp. 587–628.

Lundberg, S. and Pollak, R. A. (2012). The uneven retreat from marriage in the us, 1950-2010. In *Human Capital and History: The American Record*, University of Chicago Press.

Martinez, P. (2011). personal communication, Arizona's Hospital Paternity Program Supervisor.

Mincy, R., Garfinkel, I. and Nepomnyaschy, L. (2005a). In-hospital paternity establishment and father involvement in fragile families. *Journal of Marriage and Family*, **67** (3), 611–626.

—, Grossbard, S. and Huang, C.-C. (2005b). An economic analysis of co-parenting choices: Single parent, visiting father, cohabitation, marriage, Columbia University, unpublished manuscript.

Nepomnyaschy, L. and Garfinkel, I. (2007). Child support, fatherhood, and marriage: Findings from the first 5 years of the fragile families and child wellbeing study. *Asian Social Work and Policy Review*, **1** (1), 1–20.

Nixon, L. A. (1997). The effect of child support enforcement on marital dissolution. *Journal of Human Resources*, **32** (1), 159–181.

Pate, D. J. (2002). *An Ethnographic Inquiry into the Life Experiences of African American Fathers with Children on W-2*. Report, Institute for Research on Poverty, University of Wisconsin-Madison, CSDE Nonexperimental Analyses, Volume II, Chapter 2.

Pearson, J. and Thoennes, N. (1995). *The Child Support Improvement Project: Paternity Establishment*. Tech. rep., Center for Policy Research, `http://www.acf.hhs.gov/programs/cse/pubs/1995/reports/csip/`.

— and — (1996). Acknowledging paternity in hospital settings. *Public Welfare*, **54**, 44–51.

Persson, P. (2014). Social insurance and the marriage market, Stanford University, unpublished manuscript.

Rector, R. (2010). *Marriage: America's Greatest Weapon Against Child Poverty*. Report 2465, The Heritage Foundation.

Roberts, P. (2004). *No minor matter: developing a coherent policy on paternity establishment for children born to underage parents*. Policy Brief 2, Center for Law and Social Policy.

Roff, J. (2008). A stackelberg model of child support and welfare*. *International Economic Review*, **49** (2), 515–546.

— and Lugo-Gil, J. (2012). A model of child support and the underground economy. *Labour Economics*, **19** (5), 668–681.

Rosenbaum, D. (2003). Instrumental variable estimates of the effect of taxes on marriage with endogenous labor supply and fertility, University of North Carolina, Greensboro, unpublished manuscript.

Roy, K. (1999). Low-income single fathers in an african american community and the requirements of welfare reform. *Journal of Family Issues*, **20** (4), 432–457.

Schoeni, R. and Blank, R. (2000). *What has welfare reform accomplished? Impacts on welfare participation, employment, income, poverty, and family structure*. Working Paper 7627, National Bureau of Economic Research.

Sorensen, E. and Olivier, H. (2002). *Child support reforms in PRWORA: Initial impacts*. Discussion Paper 02-02, The Urban Institute.

Takayama, N. and Tanaka, S. (2011). Men's rush to marriage: Implications of the child support enforcement policy for marriage, fertility, and long-term inequality, University of Minnesota, unpublished manuscript.

Tartari, M. (2014). Divorce and the cognitive achievement of children, University of Chicago, unpublished manuscript.

Turner, M. (2001). Child support enforcement and in-hospital paternity establishment in seven cities. *Children and Youth Services Review*, **23** (6-7), 557–575.

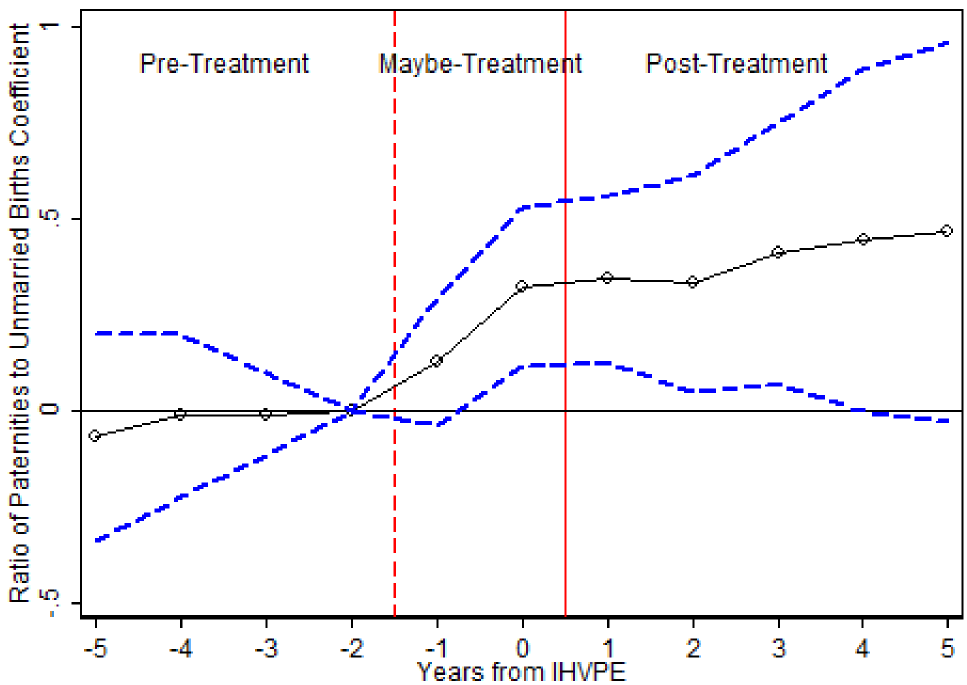
U.S. House of Representatives (2004). *2004 Green Book*. Chapter 8. Available at: `http://frwebgate.access.gpo.gov/cgi-bin/getdoc.cgi?dbname=108_green_book&docid=f:wm006_08.pdf`, (accessed on March 26, 2011).

Weiss, Y. and Willis, R. (1985). Children as collective goods and divorce settlements. *Journal of Labor Economics*, pp. 268–292.

Willis, R. J. (1999). A theory of out-of-wedlock childbearing. *Journal of Political Economy*, **107** (S6), S33–S64.

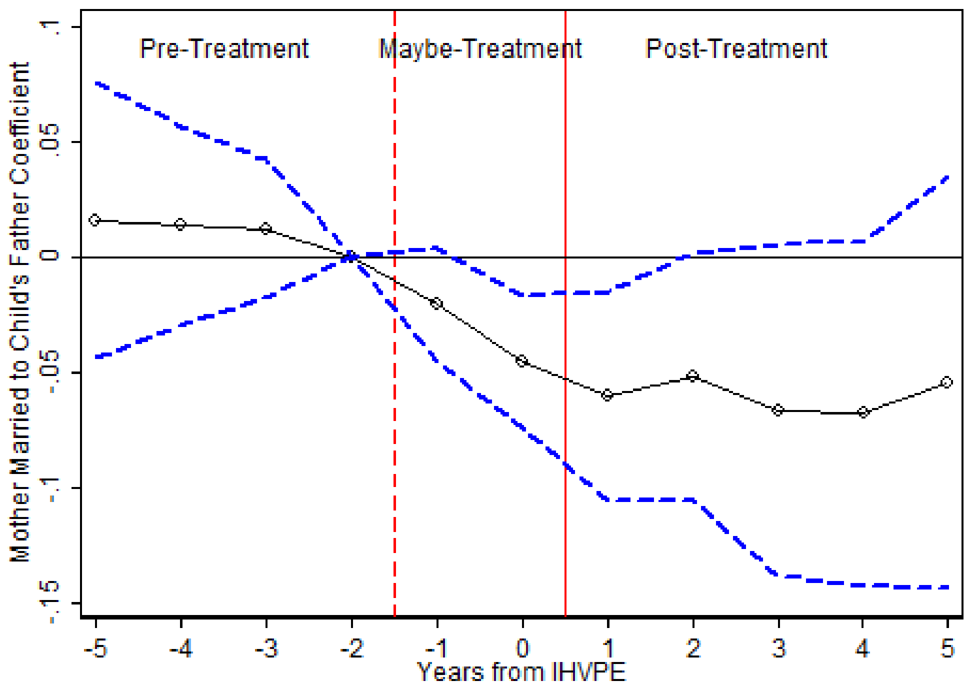
Wisconsin Bureau of Child Support (2010). *Voluntary Paternity Acknowledgement*. Tech. rep., Department of Children and Families.

Figure 1: Effects of IHVPE on Paternity Establishment Rates by Year



Notes: This figure plots θ_k coefficients (and 95% confidence intervals in dashed blue lines) from estimating the following equation: $Pat_{sy} = \beta_0 + \sum_{k=-5}^{-3} \theta_k * IHVPE_{syk} + \sum_{k=-1}^{5} \theta_k * IHVPE_{syk} + \gamma' X_{sy} + \phi' C_{sy} + \mu_s + \alpha_y + \epsilon_{sy}$, where $IHVPE_{syk}$ is an indicator for k years between IHVPE implementation and year y in state s. The omitted category is -2. I assume that the first year IHVPE is in effect is the same as the year listed in Appendix Table 1 if only the year is listed or if the month of initiation is June or earlier. If the month of initiation is known and it is July or later, then I assume the first year the program is in effect is the following year. Note that at *year* $-$ *IHVPE first year* $= 0$, for states where the month of IHVPE initiation is February-June or unknown, we may only expect a partial increase in paternity establishment rates as a result of this timing assignment. Similarly, at *year* $-$ *IHVPE first year* $= -1$, for states where the month of IHVPE initiation is July or later, we should expect a partial increase in paternity establishment rates as well. As a result, these data allow for three possible treatment statuses: "pre-treatment" = *year* $-$ *IHVPE first year* ≤ -2, "maybe-treatment" = *year* $-$ *IHVPE first year* $\in \{-1, 0\}$, and "treatment" = *year* $-$ *IHVPE first year* ≥ 1. These three groups are depicted using the vertical lines at -2 and 0 in the graph.

Figure 2: Effects of IHVPE on Parental Marriage by Year



Notes: This figure plots θ_k coefficients (and 95% confidence intervals in dashed blue lines) from estimating the following equation: $Y_{isty} = \beta_0 + \sum_{k=-5}^{-3} \theta_k * IHVPE_{syk} + \sum_{k=-1}^{5} \theta_k * IHVPE_{syk} + \gamma' X_{isty} + \phi' C_{st} + \mu_s + \alpha_y + \epsilon_{isty}$, where $IHVPE_{syk}$ is an indicator for k years from IHVPE implementation in state s and the child's approximate birth year. The omitted category is -2. I assign the child's approximate birth year as *birth year = survey year − child age − 1.* Since the surveys are conducted in March, this procedure assigns the correct birth years to children born in April-December, and incorrectly assigns the years immediately prior to their true birth years for children born in January-March. Consequently, some children who are affected by IHVPE may be erroneously assigned to the pre-treatment group. In addition, children born in months prior to the month of program implementation in any given year may be unaffected by IHVPE but erroneously assigned to the treatment group. As a result, these data allow for three possible treatment statuses: "pre-treatment" = *child's birth year − IHVPE first year* ≤ -2, "maybe-treatment" = *child's birth year − IHVPE first year* $\in \{-1, 0\}$, and "treatment" = *child's birth year − IHVPE first year* ≥ 1. These three groups are depicted using the vertical lines at -2 and 0 in the graph.

Table 1: IHVPE Programs and Maternal and State Characteristics: 1992-2005

Dependent Variable	IHVPE Coefficient	SE	Mean of Dep. Var	Coef/Mean
Log Number Births	-0.0032	(0.0038)	11.8010	-0.0003
Proportion Births by Unmarried Mothers	-0.0121	(0.0108)	0.3334	-0.0364
Proportion Births with Mother's Age <20	-0.0003	(0.0006)	0.0761	-0.0038
Proportion Births with Mother's Ed: <HS	0.0011	(0.0016)	0.2230	0.0048
Proportion Births with Mother's Ed: College+	-0.0025	(0.0024)	0.2358	-0.0108
Proportion Births by Non-Hispanic White Mothers	-0.0067	(0.0044)	0.5878	-0.0113
Proportion Births by Black Mothers	0.0022+	(0.0012)	0.1617	0.0138
Proportion Births by Hispanic Mothers	0.0025	(0.0024)	0.2016	0.0123
State Unemployment Rate in Previous Year	0.1109	(0.1519)	5.5786	0.0199
State Poverty Rate in Previous Year	0.4218	(0.3373)	13.1277	0.0321
State Minimum Wage in Previous Year	-0.0636	(0.0917)	4.7386	-0.0134
Proportion of Population Receiving Welfare Benefits in Previous Year	0.0023	(0.0022)	0.0349	0.0661
Welfare Benefit for 4-Person Family in Previous Year	-10.4670	(7.0722)	475.6215	-0.0220
Governor is Democratic in Previous Year	-0.0575	(0.0793)	0.4030	-0.1428
Proportion of Population on Medicaid in Previous Year	-0.0038	(0.0040)	0.1329	-0.0288
State EITC Implemented	0.0226	(0.0516)	0.2223	0.1016
TANF Implemented	-0.0956	(0.0809)	0.5778	-0.1655
AFDC Waiver Implemented	0.1337	(0.1279)	0.1016	1.3163
Universal Wage Withholding Implemented	0.0357	(0.0974)	0.8959	0.0399
New Hires Directory Implemented	0.0131	(0.0491)	0.5416	0.0242
License Revocation for Non-Payment Implemented	-0.1181	(0.1222)	0.6688	-0.1766

Notes: N = 601 state-year cells. Each coefficient is from a separate regression. Units of observation are state-year cells consisting of the 43 sample states (Alabama, Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, DC, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Kansas, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nebraska, Nevada, New Jersey, New York, North Carolina, North Dakota, Ohio, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, and Wisconsin). I assume that the first year the program is in effect (year of initiation) is the same as the year listed in Appendix Table 1 if only the year is listed or if the month of initiation is June or earlier. If the month of initiation is known and it is July or later, then I assume the first year the program is in effect is the following year. All regressions include state and year fixed effects, and state-specific time trends. Regressions with maternal characteristics as outcomes are weighted by the number of births in each state-year cell, while all other regressions are weighted by state-year populations. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 2: Effects of IHVPE on Paternity Establishment Rates: 1992-2005

	Paternities Over Unmarried Births					Paternities Over All Births	Log Paternities
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre-Treat. Mean of Dep. Var.	0.552	0.553	0.552	0.552	0.552	0.174	9.987
IHVPE Program Exists in State and Year of Observation	0.2412***	0.2064**	0.2009**	0.2027**	0.2100**	0.0674**	0.2791**
	(0.0606)	(0.0597)	(0.0660)	(0.0594)	(0.0601)	(0.0196)	(0.0958)
Mother and Child Controls	√	√	√	√	√	√	√
Year FEs	√	√	√	√	√	√	√
State FEs	√	√	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√	√	√
Child Support Laws Controls			√	√	√	√	√
State EITC Implementation				√	√	√	√
AFDC/TANF Implementation				√	√	√	√
State-Specific Time Trends					√	√	√
N	601	572	544	544	544	544	544
R-squared	0.6943	0.7306	0.7338	0.7458	0.8265	0.8400	0.9639

Notes: Each column is a separate regression. Please refer to Table 1 for details about the sample. The maternal and child controls include controls for the proportion of births with the following characteristics: mother's age (<20, 20-24, 25-34, 35-44, 45+), mother's education (less than HS, HS, some college, college+), mother's race (white, black, Hispanic, other), and child sex. The controls for state characteristics in the year before include the unemployment rate, the poverty rate, the state minimum wage, the percent of the population that receives AFDC/TANF benefits, the AFDC/TANF benefit for a 4-person family, the percent of the population on Medicaid, total spending on child support enforcement, an indicator for a Democratic governor, and the fraction of the state House that is Democratic. The child support laws controls are indicators for whether the following laws are in place in the state and year of observation: universal wage withholding, New Hires directory, license revocation for non-payment, and joint custody. The state EITC implementation controls are indicators for whether a state EITC has been implemented in the state and year of observation. The AFDC/TANF implementation controls are indicators for whether an AFDC waiver or TANF has been implemented in the state and year of observation. All regressions are weighted by the state-year populations. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 3: Effects of IHVPE on Parental Marriage: CPS-CSS 1994-2008

	Dependent Variable: Mother is Married to Child's Biological Father				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Pre-Treat. Mean of Dep. Var.	0.749	0.750	0.750	0.750	0.750
IHVPE Program Exists in State and Year of Child's Birth	-0.0324***	-0.0261***	-0.0267**	-0.0274***	-0.0269**
	(0.0080)	(0.0074)	(0.0075)	(0.0074)	(0.0087)
Mother and Child Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	38,449	37,457	36,243	36,243	36,243
R-squared	0.2232	0.2239	0.2236	0.2237	0.2245

Notes: Each column is a separate regression. The CPS-CSS sample of analysis includes all women with a youngest child aged 5 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 44 sample states (the states listed in the notes to Table 1 and Washington) in 1994, 1996, 1998, 2000, 2002, 2004, 2006 and 2008. The sample omits all individuals who moved from abroad last year and assigns the state of last year's residence as the state of child's birth. Mothers are coded as married to the biological father if they are married and their child is coded as living with both parents in the household. The mother and child controls include controls for the woman's age at childbirth (<20, 20-24, 25-34; 35-44 omitted), woman's education (less than HS, HS, some college; college+ omitted), woman's race (white, black, Hispanic; other omitted), child sex, and indicators for child's age in years. The controls for state characteristics in the year before include the unemployment rate, the poverty rate, the state minimum wage, the percent of the population that receives AFDC/TANF benefits, the AFDC/TANF benefit for a 4-person family, the percent of the population on Medicaid, total spending on child support enforcement, an indicator for a Democratic governor, and the fraction of the state House that is Democratic. The child support laws controls are indicators for whether the following laws are in place in the state and year of observation: universal wage withholding, New Hires directory, license revocation for non-payment, and joint custody. The state EITC implementation controls are indicators for whether a state EITC has been implemented in the state and year of observation. The AFDC/TANF implemenation controls are indicators for whether the AFDC waiver or the TANF program is implemented by the state and year of observation. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 4: Effects of IHVPE on Maternal Relationships and the Characteristics of Mothers' Spouses

Dependent Variable	Pre-Treat Mean of Dep. Var.	Coefficient	SE
Mother is…			
Never Married (CPS-CSS, N=36,243)	0.134	0.0169**	(0.0060)
Married to Someone Other than Biological Father (CPS-CSS, N=36,343)	0.010	0.0060**	(0.0027)
Divorced (CPS-CSS, N=36,243)	0.103	0.0046	(0.0056)
Cohabiting with the Biological Father (NHIS)	0.022	-0.0037	(0.0033)
Cohabiting with Someone Other than Biological Father (NHIS)	0.022	0.0086**	(0.0024)
Mother's Spouse… (March CPS, N=140,487)			
Had Any Own Wage Income in the Past Year	0.899	0.0109**	(0.0045)
Worked Any Usual Hours in the Past Year	0.967	0.0044+	(0.0024)
Is Aged 20-24 Years	0.050	-0.0083**	(0.0031)
Is Aged 25-34 Years	0.478	0.0129+	(0.0073)
Is Aged 45+ Years	0.064	0.0076**	(0.0036)
Has Less than High School Education	0.118	-0.0012	(0.0043)
Has High School Degree	0.332	0.0077	(0.0074)

Notes: Each coefficient is from a separate regression. Please refer to Table 3 for details about the CPS-CSS sample and controls. The March CPS sample is further limited to mothers who can be linked to their spouses in the household. The NHIS regressions use data from the Sample Child Files over 1997-2010 on all women with a sample child aged 7 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 43 sample states. Sample sizes in the NHIS data cannot be released due to confidentiality concerns. Please see the text for more information about approximate sample sizes. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, and state-specific time trends. The regressions using CPS-CSS and March CPS are weighted by the CPS person weights, while the regressions using NHIS are weighted by the sample child weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 5: IHVPE and Father Involvement among Families with Unmarried Parents: CPS-CSS 1994-2008

Dependent Variable	**Pre-Treat. Mean of Dep. Var.**	**N**	**Coefficient**	**SE**	**N**	**Coefficient**	**SE**	**N**	**Coefficient**	**SE**
Mother Eligible to be Asked CS Supplement Questions	0.255	36,243	0.0148+	(0.0087)						
			Whole CSS Sample			**Trimmed CSS Sample - Lower Bound**			**Trimmed CSS Sample - Upper Bound**	
Father Involvement Index	-0.0071	8,362	0.0831+	(0.0434)	7,366	0.0301	(0.0411)	8,219	0.1064**	(0.0408)
Father Made Any CS Payments in Last Year	0.355	7,507	0.0146	(0.0190)	7,364	0.0005	(0.0190)	7,375	0.0233	(0.0188)
Father Made All CS Payments in Last Year	0.221	7,507	0.0476**	(0.0204)	7,363	0.0237	(0.0207)	7,377	0.0520**	(0.0205)
Father Paid On Time All or Most of the Time in Last Year	0.296	6,141	0.0293	(0.0297)	6,020	0.0035	(0.0305)	6,031	0.0345	(0.0291)
Father Has Court-Ordered Visitation Rights	0.702	8,361	0.0064	(0.0311)	8,210	-0.0001	(0.0311)	8,215	0.0268	(0.0333)
Number Days Father Spent with Child	55.076	7,733	7.5007	(5.2555)	7,587	1.3690	(4.6793)	7,599	9.2954+	(5.2460)
Father Provided Gifts for Child	0.509	8,361	0.0493	(0.0358)	8,203	0.0354	(0.0364)	8,214	0.0654+	(0.0338)
Father Provided Clothes for Child	0.363	8,361	0.0300	(0.0246)	8,204	0.0107	(0.0248)	8,220	0.0462+	(0.0241)
Father Provided Food for Child	0.237	8,361	0.0430+	(0.0221)	8,200	0.0256	(0.0227)	8,223	0.0518**	(0.0221)
Father Covered Childcare Expenses for Child	0.092	8,361	0.0329**	(0.0152)	8,213	0.0000	(0.0143)	8,214	0.0359**	(0.0155)
Father Paid for Medical Expenses for Child	0.146	8,361	0.0554**	(0.0201)	8,208	0.0280	(0.0208)	8,214	0.0585**	(0.0202)

Notes: Each coefficient is from a separate regression. The sample of analysis includes all women with a youngest child aged 5 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 44 sample states (the states listed in the notes to Table 1 and Washington) in the biannual Child Support Supplement (CSS) over 1994-2008. The sample omits all women who moved from abroad last year and assigns the state of last year's residence as the state of child's birth. Mothers are eligible to be asked CSS questions if they have a biological child in the household whose father lives outside the household.

The trimmed samples are constructed as follows. I estimate separate regressions of equation (2) with an indicator for being in the CSS sample as the outcome for 16 mutually exclusive groups of mothers defined by interactions between maternal education (less than high school, high school degree, some college, college or more) and race (non-Hispanic white, non-Hispanic black, Hispanic, and other) categories. For each group, g, I obtain the coefficient on the IHVPE indicator, β_g, and then trim the group by $(\beta_g * 100)$ percent of the post-IHVPE sample. For each outcome, the lower-bound sample drops post-IHVPE observations that are in the top $(\beta_g * 100)$ percent of the post-IHVPE outcome distribution, while the upper-bound sample drops post-IHVPE observations that are in the bottom $(\beta_g * 100)$ percent of the post-IHVPE outcome distribution.

All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.

Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 6: IHVPE and Child Well-Being Among Families with Unmarried Parents

Dependent Variable	Pre-Treat. Mean of Dep. Var.	N	Coefficient	SE	N	Coefficient	SE	N	Coefficient	SE
		Whole "Unmarried Sample"			Trimmed "Unmarried Sample" - Lower Bound			Trimmed "Unmarried Sample" - Upper Bound		
Child Resources (March CPS)										
Child Resource Index	-0.740	46,176	-0.0069	(0.0196)	45,658	-0.0293	(0.0206)	44,542	0.1311***	(0.0244)
Log Household Income	10.030	45,296	0.0005	(0.0218)	44,780	-0.0363+	(0.0209)	43,797	0.1315***	(0.0215)
Above Poverty	0.508	46,176	0.0012	(0.0104)	45,653	-0.0061	(0.0105)	44,860	0.0272**	(0.0101)
Above 1.5xPoverty	0.363	46,176	0.0077	(0.0076)	45,694	-0.0029	(0.0075)	45,035	0.0250**	(0.0076)
Not Receiving Welfare	0.648	46,176	-0.0154	(0.0130)	45,537	-0.0210	(0.0130)	44,610	0.0589***	(0.0145)
Child Health (NHIS)										
Physical Health Index	-0.080	--	-0.0159	(0.0230)	--	-0.0330	(0.0227)	--	0.0435+	(0.0252)
Mental Problems Index (higher val. = more prob.)	1.538	--	-0.0575	(0.1586)	--	-0.1945	(0.1385)	--	-0.0071	(0.1652)
Any Learning Disability (Age 3+ only)	0.062	--	-0.0274**	(0.0115)	--	-0.0426***	(0.0102)	--	-0.0259**	(0.0116)
Any Doctor Visits, past 12 months	0.826	--	-0.0223	(0.0149)	--	-0.0255+	(0.0149)	--	-0.0004	(0.0150)

Notes: Each coefficient is from a separate regression. The samples are further limited to mothers who are not married to their children's fathers. Please refer to Tables 3 and 4 for details about the samples and controls. Sample sizes in the NHIS data cannot be released due to confidentiality concerns. Please see the text for more information about approximate sample sizes. The regressions using March CPS data are weighted by the CPS person weights, while the regressions using NHIS are weighted by the sample child weights. Robust standard errors are clustered on the state level. The trimmed samples are constructed as follows. I estimate separate regressions of equation (2) with an indicator for the mother being not married to the child's father as the outcome for 16 mutually exclusive groups of mothers defined by interactions between maternal education (less than high school, high school degree, some college, college or more) and race (non-Hispanic white, non-Hispanic black, Hispanic, and other) categories. For each group, g, I obtain the coefficient on the IHVPE indicator, β_g, and then trim the group by ($\beta_g * 100$) percent of the post-IHVPE sample. For each outcome, the lower-bound sample drops post-IHVPE observations that are in the top ($\beta_g * 100$) percent of the post-IHVPE outcome distribution, while the upper-bound sample drops post-IHVPE observations that are in the bottom ($\beta_g * 100$) percent of the post-IHVPE outcome distribution. Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 7: Overall Effects of IHVPE on Measures of Father Involvement and Child Well-Being

Dependent Variable	Pre-Treat. Mean of Dep. Var.	Coefficient	SE
Father Involvement and Child Resources (March CPS: N=183,283; CPS-CSS: N=36,243)			
Child Has Any Private Health Insurance Coverage (CPS-CSS)	0.681	-0.0265**	(0.0010)
Mother Worked Any Usual Hours Last Year (March CPS)	0.681	0.0181**	(0.0076)
Child Resource Index (March CPS)	0.005	0.0011	(0.0074)
Log Household Income (March CPS)	10.749	0.0082	(0.0102)
Above Poverty (March CPS)	0.802	0.0025	(0.0040)
Above 1.5xPoverty (March CPS)	0.695	0.0027	(0.0045)
Not Receiving Welfare (March CPS)	0.888	-0.0056	(0.0045)
Child Health (NHIS: N~50,000)			
Physical Health Index	-0.009	-0.0081	(0.0120)
Mental Problems Index (higher value = more problems)	1.264	0.0156	(0.0730)
Any Learning Disability (Age 3+ only)	0.041	-0.0084	(0.0053)
Any Doctor Visits (in the past 12 months)	0.832	-0.0138**	(0.0066)

Notes: Each coefficient is from a separate regression. Please refer to Tables 3 and 4 for details about the samples and controls. Sample sizes in the NHIS data cannot be released due to confidentiality concerns. Please see the text for more information about approximate sample sizes. The regressions using March CPS data are weighted by the CPS person weights, while the regressions using NHIS are weighted by the sample child weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 8: Robustness Tests for Main Outcomes (1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome:	**Main Results**	**No "Maybe-Treatment"**	**Limited Window**	**"Good Info" States**	**Quadtratic Trends**	**No Black Mothers**	**Omit CA**	**Omit NY**	**Omit TX**
Ratio of Paternities	0.2100**	0.2922***	0.2136**	0.2183**	0.2227**		0.2033**	0.2119**	0.2310***
to Unmarried Births	(0.0601)	(0.0762)	(0.0782)	(0.0665)	(0.0692)	---	(0.0592)	(0.0642)	(0.0640)
N	544	467	349	336	544		530	530	530
Mother is Married to	-0.0269**	-0.0301**	-0.0295**	-0.0336**	-0.0309**	-0.0241**	-0.0256**	-0.0275**	-0.0259**
Child's Father	(0.0087)	(0.0127)	(0.0089)	(0.0091)	(0.0098)	(0.0093)	(0.0088)	(0.0090)	(0.0102)
N	36,243	31,811	32,970	26,148	36,243	31,907	35,720	33,910	33,831
Mother is Never	0.0169**	0.0143	0.0181**	0.0212**	0.0225**	0.0154**	0.0158**	0.0158**	0.0158**
Married	(0.0060)	(0.0102)	(0.0061)	(0.0064)	(0.0069)	(0.0062)	(0.0062)	(0.0062)	(0.0070)
N	36,243	31,811	32,970	26,148	36,243	31,907	35,720	33,910	33,831
Mother is Married to	0.0060**	0.0059	0.0066**	0.0072**	0.0067**	0.0054**	0.0061**	0.0063**	0.0040
Someone Else	(0.0027)	(0.0042)	(0.0028)	(0.0030)	(0.0029)	(0.0025)	(0.0027)	(0.0029)	(0.0024)
N	36,243	31,811	32,970	26,148	36,243	31,907	35,720	33,910	33,831
Mother's Spouse Has	0.0109**	0.0061	0.0097**	0.0077	0.0081+	0.0099**	0.0112**	0.0114**	0.0128**
Any Own Wage Inc.	(0.0045)	(0.0065)	(0.0045)	(0.0052)	(0.0047)	(0.0045)	(0.0045)	(0.0047)	(0.0044)
N	140,487	128,108	113,697	102,689	140,487	132,140	138,758	132,076	130,566
Father Involvement	0.0831+	0.0590	0.0951**	0.0816	0.0886+	0.1236**	0.0860+	0.0820+	0.0675
Index (unmar. only)	(0.0434)	(0.0459)	(0.0440)	(0.0545)	(0.0471)	(0.0426)	(0.0437)	(0.0448)	(0.0484)
N	8,362	7,298	7,710	5,991	8,362	5,936	8,214	7,766	7,788

Notes: Each coefficient is from a separate regression. Please refer to Tables 3, 4, and 5 for details about the samples and controls. Column 1 shows the main results presented in the above tables for each of the outcomes. Column 2 omits the "maybe-treatment" group years (*child's birth year* − *IHVPE first year* $\in \{-1, 0\}$). Column 3 limits the samples to narrower windows surrounding IHVPE implementation: The paternities data are limited to years 1992-2000, while the individual-level data are limited to mothers of children born in 1988-2004. Column 4 restricts the sample to the 27 "good info" states for which I have the most accurate information on the timing of IHVPE implementation (please refer to the text for more details). Column 5 includes quadratic state-specific time trends. Column 6 limits individual-level analyses to samples without black mothers. Columns 7-9 omit observations from CA, NY, and TX, respectively. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, and state-specific linear time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level. Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 9: Robustness Tests for Main Outcomes (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome:	**Main Results**	**No "Maybe-Treatment"**	**Limited Window**	**"Good Info" States**	**Quadtratic Trends**	**No Black Mothers**	**Omit CA**	**Omit NY**	**Omit TX**
Child Has Any	-0.0265**	-0.0395**	-0.0257**	-0.0372***	-0.0274**	-0.0236+	-0.0272**	-0.0253**	-0.0196+
Private Health Insur.	(0.0010)	(0.0143)	(0.0095)	(0.0091)	(0.0083)	(0.0120)	(0.0101)	(0.0099)	(0.0098)
N	36,243	31,811	32,970	26,148	36,243	31,907	35,720	33,910	33,831
Mother Worked Any	0.0181**	0.0296**	0.0181**	0.0209**	0.0245**	0.0176**	0.0174**	0.0161**	0.0121+
Usual Hours Last Yr.	(0.0076)	(0.0105)	(0.0081)	(0.0088)	(0.0091)	(0.0074)	(0.0075)	(0.0076)	(0.0070)
N	183,283	168,202	148,974	134,963	184,562	163,580	182,222	172,791	171,609
Child Resource	0.0011	0.0100	0.0039	-0.0045	-0.0012	0.0003	0.0007	0.0009	-0.0015
Index	(0.0074)	(0.0116)	(0.0080)	(0.0087)	(0.0077)	(0.0067)	(0.0075)	(0.0078)	(0.0087)
N	183,283	168,202	148,974	134,963	184,562	184,565	182,222	172,791	171,609
Physical Health	-0.0081	-0.0201	-0.0057	--	-0.0095	-0.0121	--	--	--
Index	(0.0120)	(0.0136)	(0.0118)	--	(0.0127)	(0.0105)	--	--	--
N	--	--	--	--	--	--	--	--	--
Mental Problems	0.0156	0.0100	0.0376	--	0.0229	0.0080	--	--	--
Index (higher = more probs)	(0.0730)	(0.1299)	(0.0790)	--	(0.0813)	(0.0772)	--	--	--
N	--	--	--	--	--	--	--	--	--
Any Learning Disability	-0.0084	-0.0058	-0.0085	--	-0.0065	-0.0098+	--	--	--
(Age 3+ only)	(0.0053)	(0.0066)	(0.0053)	--	(0.0062)	(0.0050)	--	--	--
N	--	--	--	--	--	--	--	--	--
Any Doctor Visits	-0.0138**	-0.0063	-0.0110	--	-0.0123+	-0.0112	--	--	--
(past 12 months)	(0.0066)	(0.0144)	(0.0067)	--	(0.0066)	(0.0067)	--	--	--
N	--	--	--	--	--	--	--	--	--

Notes: Each coefficient is from a separate regression. See Table 8 for more information. For confidentiality purposes, results from NHIS regressions that limit the sample to subsets of states cannot be reported. Please see text for more details. Robust standard errors are clustered on the state level. Significance levels: + p<0.10 ** p<0.05 *** p<0.01

A CPS-CSS Sample Construction

The CPS-CSS analysis sample is constructed as follows. I first create a "youngest child" data set by considering all individuals who are the youngest within their household and who are aged 5 years or less.[52] I drop all children who have been adopted, who have a parent that died, or who live with either no biological parent or only a father. All children who live with at least one parent have information on the line number of his/her parent in the household (which can be a mother or a father). Thus, I am able to merge children who list their mothers' line numbers directly to their mothers. I merge children who list their fathers' line numbers to their fathers and merge the fathers to their spouses in the household to obtain information on the mothers. I drop all father-child pairs in which the father cannot be merged to a spouse in the household.[53] This results in a data set of mother-child pairs, and I use the mother as the unit of observation in all analyses.

Next, using the child's age at the time of the survey, I calculate the child's approximate birth year: *birth year* $=$ *survey year* $-$ *child age* $-$ *1*.[54] Since there is some variation in how minors are treated in IHVPE programs, I limit my analysis to mothers aged 18-45 at the time of childbirth. Finally, I drop all mothers who moved from outside the U.S. in the last year.

In this sample, a mother is categorized as married to the biological father if she is married and her child is coded as living with both parents in the household. A mother is categorized as married to someone other than the biological father if she is married,

[52] I randomly pick one child if there are multiple children that satisfy this condition (i.e., non-singleton children).

[53] I do this because I want to use the mother as the unit of observation and data limitations prevent me from observing information on the child's mother when the father is listed as the child's parent and the parents are not married. As a result, all mother-child pairs in which the unmarried parents are cohabiting and the child's parent is listed as the father are dropped. This results in only about 1% of the sample being dropped. This may still be problematic if there is an effect of IHVPE programs on the likelihood that unmarried parents cohabit. However, I can check this given that I do observe mother-child pairs in which the unmarried parents are cohabiting and the child's parent is listed as the mother. There is no statistically significant effect of IHVPE on cohabitation for these mothers — the coefficient of interest is -0.000082 with a standard error of 0.0005. Additionally, results from the NHIS data where respondents are explicitly asked about cohabitation suggest that there are no effects of IHVPE on parental cohabitation; instead, the likelihood that a mother cohabits with someone other than the father increases. Thus, I can conclude that this omission is likely negligible.

[54] I chose this specification because the data are collected in March; therefore, only individuals born in the first three months of the year will have had their birthday by the time of the survey.

but the child is coded as living with only a mother in the household. Mothers who are married to the biological father are by construct ineligible to be asked CSS questions.

B Addressing Selection in the Unmarried Sample

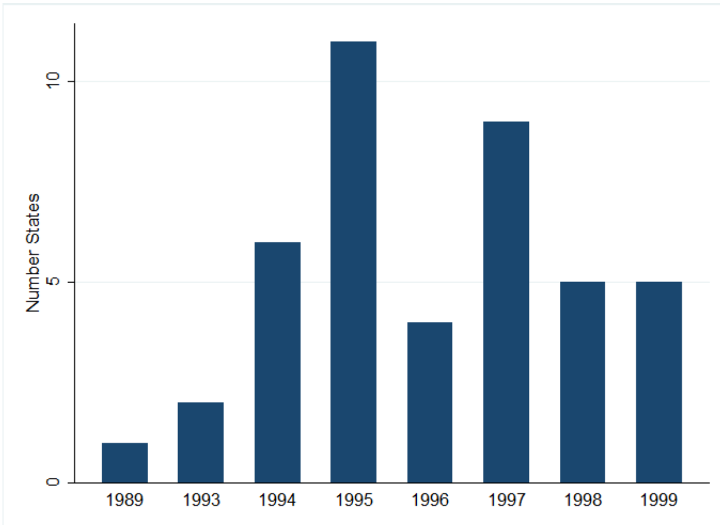
As discussed in Section 5, results on father involvement and child well-being in families with unmarried parents are complicated by the IHVPE-driven decline in parental marriage. To address this issue of selection, I calculate upper and lower bound estimates on the effect sizes following Lee (2009). The idea is to trim the unmarried sample by the number of "extra" individuals who are there post-IHVPE. The upper bound estimate assumes that the "extra" individuals are located at the bottom of the outcome distribution (i.e., parents who would have otherwise been married have the worst outcomes), while the lower bound estimate assumes that the "extra" individuals are located at the top of the outcome distribution (i.e., parents who would have otherwise been married have the best outcomes).

I implement this method by estimating separate regressions of equation (2) with an indicator for being in the unmarried (or CSS) sample as the outcome for 16 mutually exclusive groups of mothers defined by interactions between maternal education (less than high school, high school degree, some college, college or more) and race (non-Hispanic white, non-Hispanic black, Hispanic, and other) categories. For each group, g, I obtain the coefficient on the IHVPE indicator, β_g, and then trim the group by $(\beta_g * 100)$ percent of the post-IHVPE sample. To calculate the lower bound of the effect on each outcome, I drop post-IHVPE observations that are in the top $(\beta_g * 100)$ percent of the post-IHVPE outcome distribution; for the upper bound, I drop post-IHVPE observations that are in the bottom $(\beta_g * 100)$ percent of the post-IHVPE outcome distribution. For binary outcomes, the lower bound trim drops $(\beta_g * 100)$ percent of post-IHVPE observations that all have a value of "1", while the upper bound trim drops $(\beta_g * 100)$ percent of the post-IHVPE observations that all have a value of "0".[55]

[55] Results that simply trim the top and bottom 2% of each outcome distribution yield similar, but wider bounds. Lee (2009) shows that bounds calculated by conditioning on covariates (such as education and race above) are narrower than those calculated without controlling for any covariates.

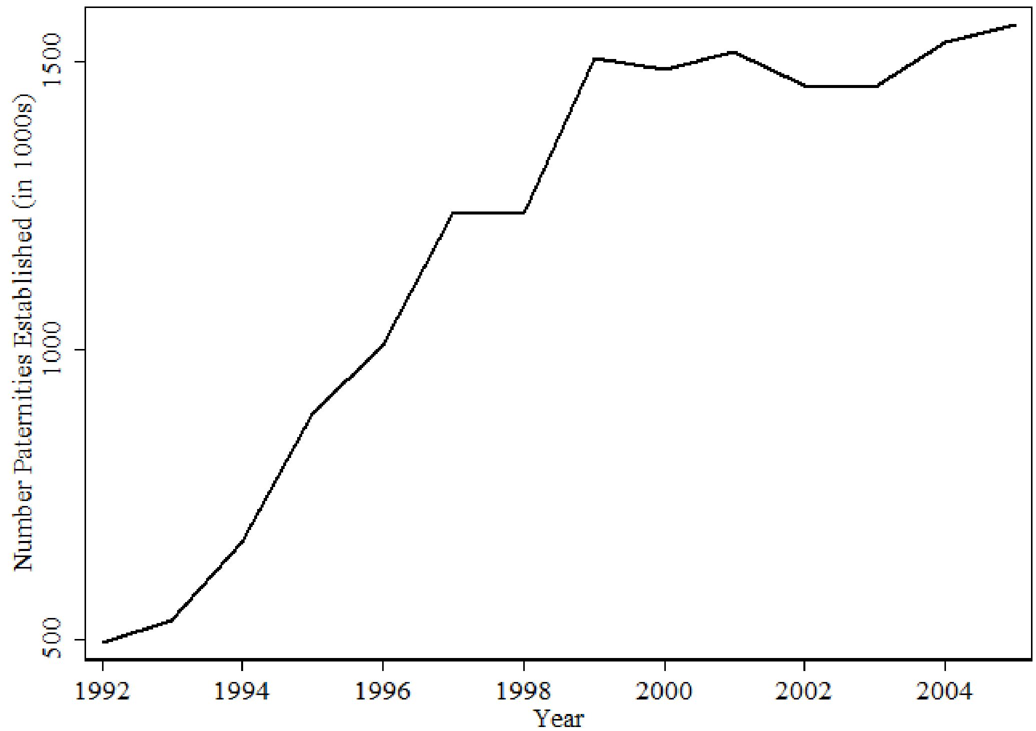
C Appendix Figures and Tables

Appendix Figure 1: Variation in IHVPE Program Initiation Across States

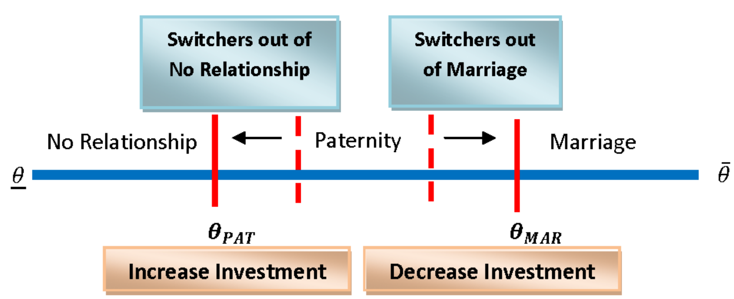


Notes: This figure plots the number of states that initiated IHVPE in each year. Forty-four states are included in the figure.

Appendix Figure 2: Number Paternities Established in 44 Analysis States: 1992-2005

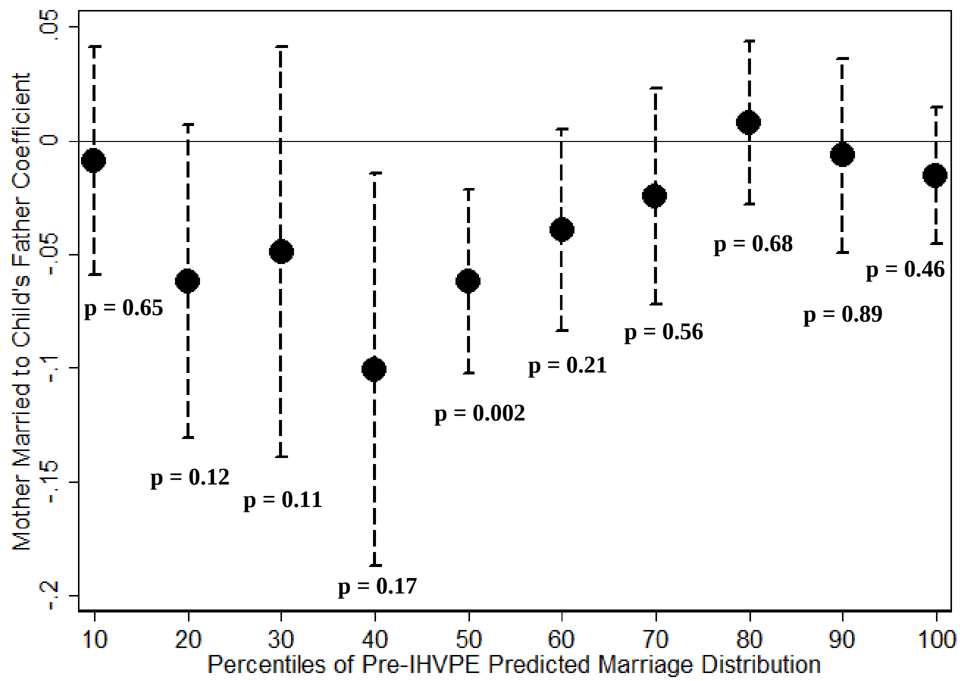


Appendix Figure 3: The Effects of Lowering the Cost of Paternity on Optimal Parental Relationships



Notes: This figure shows the relationship between match quality, θ, and the optimal parental relationship choice. In a simple version of the model, parental match quality, θ, is monotonically increasing with the parental relationship state. Parents with match quality above θ_{MAR} choose marriage; parents with match quality between θ_{PAT} and θ_{MAR} choose paternity; while parents with match quality below θ_{PAT} choose no relationship. When the costs of establishing paternity are lowered, more parents choose this option: θ_{PAT} will fall while θ_{MAR} will rise.

Appendix Figure 4: Effects of IHVPE on Parental Marriage by Deciles of the Pre-IHVPE Predicted Marriage Distribution



Notes: This figure plots the coefficients from estimating equation (2) for parental marriage as an outcome separately by deciles of the pre-IHVPE predicted marriage distribution. To obtain the pre-IHVPE predicted marriage distribution, I use a probit model to estimate a regression of the form: $Married_{isty} = \beta_0 + \gamma' X_{isty} + \phi' C_{st} + \mu_s + \alpha_y + \delta_s * y + \epsilon_{sy}$, where $Married_{isty}$ is an indicator for the mother being married to the child's father, and the rest of the coefficients and variables are defined as in equation (2). The 95% confidence intervals (shown as dashed bars) use standard errors clustered on the state level. The listed p-values correspond to estimates from a wild cluster bootstrap (Cameron *et al.*, 2008) to account for the fact that the sample is split along deciles of a predicted variable.

Appendix Table 1: Timing of IHVPE Program Initiation

State	Year (and Month) of Initiation	Source
Alabama	1994	Alabama Code Section 26-17-22, part c)
Alaska	1997	Alaska Statutes 18.50.165
Arizona	July, 1996	Marjorie A. Cook, Arizona Department of Economic Security Division of Child Support Enforcement. Personal communication: 12/27/2010
Arkansas	1994	Arkansas Code 9-10-120
California	January, 1995	California Family Code 7571
Colorado	June, 1996	C.R.S. 25-2-112, Sec. 3.5
Connecticut	July, 1994	Conn. Gen. Stat. Sec. 17b-27
Delaware	January, 1995	http://www.paternitynet.com/art04.html
District of Columbia	2/27/1998	D.C. Code Sec. 16-909.03
Florida	August, 1997	Fla. Stat. Sec. 742.10
Georgia	1999	OCGA 19-7-27
Hawaii	1999	HRS 584-3.5
Idaho	May, 1998	http://www.healthandwelfare.idaho.gov/portals/_rainbow/manuals/cs/chapter_3/3.8_voluntary.htm
Illinois	1997	Garfinkel & Nepomnyaschy (2007)
Indiana	1997	Angelica Carter, Attorney with the Indiana State Child Support Bureau. Personal communication: 4/13/2011
Kansas	1997	KSA 38-1137
Kentucky	7/15/1996	KRS 406.025
Louisiana	July, 1998	La.R.S. 40:46.1
Maine	1996	22 M.R.S. Sec. 2761-B
Maryland	1997	Garfinkel & Nepomnyaschy (2007)
Massachusetts	1994	Garfinkel & Nepomnyaschy (2007)
Michigan	1/21/1993	Public Health Code - Act 368 of 1978
Minnesota	6/15/1995	Molly Mulcahy Crawford; Paternity Program Administrator, Minnesota Department of Human Services, Child Support Enforcement Division. Personal communication: 4/20/2011
Mississippi	1995	www.acf.hhs.gov/programs/cse/pubs/1998/best_practices/bppat98.htm
Missouri	July, 1994	R.S. Mo 193-087
Nebraska	1995	R.R.S. 43-1408.01
Nevada	1995	Nev. Rev. Stat. Ann. 449.246
New Jersey	July, 1996	Paternity Opportunity Program: http://pop.njchildsupport.org/
New York	March, 1995	www.lawny.org/index.php/advocate-page-attorney-resources-119/38-public-advocate-information/171-paternity-for-advocates
North Carolina	1997	GS 110-132
North Dakota	1996	N.D. Cent. Code 14-19-06
Ohio	1999	ORC Ann. 3111.71
Oregon	November, 1995	Or. Admin. R. 333-011-0048
Pennsylvania	January, 1998	23 PA Cons. Stat. Sec. 5103
Rhode Island	January, 1995	R.I. Gen. Laws § 40-6-21.1
South Carolina	1994	S.C. Code Ann. § 44-7-77
South Dakota	1994	S.D. Codified Laws § 25-8-50
Tennessee	1994	Garfinkel & Nepomnyaschy (2007)
Texas	1999	Kevin O'Keefe, Texas Office of the Attorney General Child Support Division. Personal communication: 10/8/2010
Utah	1995	Utah Code Ann. 26-2-5
Vermont	1997	Vermont Statutes Title 15, Ch. 5, § 307
Virginia	1995	VA Code 63.2-1914
Washington	July, 1989	Paternity Affidavit Program: www.dshs.wa.gov/dcs/services/providers.asp
Wisconsin	1999	Wisconsin Bureau of Child Support (2010), Department of Children and Families Report, "Voluntary Paternity Acknowledgement"

Notes: Searches of state statutes were conducted using *LexisNexis Academic*.

Appendix Table 2: Sample Means

	Mean		Mean
State-Year Data		Child Has Private Health Insurance Coverage	0.673
Ratio of Paternities to Unmarried Births	0.935	**CS Supplement**	
Ratio of Paternities to All Births	0.313	Father Paid Any Child Support in the Past Year	0.358
Log Paternities Established	10.506	Father Paid All Child Support in the Past Year	0.221
CPS-CSS		Father Has Legal Visitation Rights	0.702
Mother is Married	0.775	Number Days Father Spent with Child in the Past Year	60.311
Mother is Never Married	0.137	Father Provided Any Gifts, Clothes, Food, Childcare or Medical Help	0.558
Mother is Married to Child's Father	0.765	**March CPS (1)**	
Mother is Married to Someone Else	0.010	Mother Worked Any Usual Hours in the Past Year	0.678
Mother's Age at Birth: <20 years	0.045	Real Household Income in the Past Year (2010 Dollars)	71,373.30
Mother's Age at Birth: 35-45 years	0.193	**March CPS (2)**	
Mother's Education: <HS	0.141	Mother's Spouse Had Any Own Income in Past Year	0.905
Mother's Education: HS	0.296	Mother's Spouse's Age	34.438
Mother's Education: Some College	0.292	Mother's Spouse Has Less than High School Education	0.116
Mother is Non-Hispanic White	0.632	Mother's Spouse Has High School Degree	0.309
Mother is Black	0.144	**NHIS**	
Mother is Hispanic	0.173	Mother is Cohabiting with Child's Father	0.047
Child is Male	0.510	Mother is Cohabiting with Someone Else	0.019
Child's Age	2.180	Child Had Any Doctor Visits in Past 12 Months	0.852

Notes: The state-year data (N=601) on paternity establishment rates is available for the following 43 states over 1992-2005: Alabama, Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, DC, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Kansas, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nebraska, Nevada, New Jersey, New York, North Carolina, North Dakota, Ohio, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, and Wisconsin. The CPS-CSS sample (N=38,449) includes all women with a youngest child aged 5 years or less in the household who were between the ages of 18 and 45 at the time of childbirth in the above 43 states and Washington (44 sample states) in 1994, 1996, 1998, 2000, 2002, 2004, 2006 and 2008. The CS Supplement sample (N=8,974) limits the CPS-CSS sample to mothers who are eligible to be asked the CS Supplement questions. The March CPS (1) sample (N=212,504) includes all women with a youngest child aged 5 years or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 44 sample states over 1989-2010. The March CPS (2) sample (N=162,691) limits the March CPS (1) sample to married mothers who are matched to their spouses. The NHIS sample comes from the Sample Child Files over 1997-2010 on all women with a sample child aged 7 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 43 sample states. Sample sizes in the NHIS data cannot be released due to confidentiality concerns. Please see the text for more information about approximate sample sizes. All individual-level samples omit individuals who moved from abroad last year and assign the state of last year's residence as the state of child's birth. Mothers are coded as married to the biological father if they are married and their child is coded as living with both parents in the household. Mothers are coded as married to someone other than the biological father if they are married, but their child is coded as living with one parent in the household. The state-year means are weighted by state-year populations, while individual-level data means are weighted by the provided sample weights.

Appendix Table 3: IHVPE and Pregnancy Behaviors and Birth Outcomes

Dependent Variable	Pre-Treat. Mean of Dep. Var.	Coefficient	SE
1st Tri. Prenatal Care Initiation	0.799	-0.0040	(0.0030)
Child is Male	0.512	0.0005	(0.0003)
Maternal Weight Gain (lbs.)	30.644	-0.1387	(0.0890)
Birth Weight (g)	3324.488	-0.7896	(0.8771)
Low Birth Weight (<2500g)	0.074	0.0003	(0.0003)
Very LBW (<1500g)	0.014	0.0000	(0.0001)
Gestation (weeks)	38.956	-0.0127+	(0.0068)
Any Complications	0.322	0.0043	(0.0072)
Any Abnormal Cond. of Newborn	0.069	0.0010	(0.0040)

Notes: Each coefficient is from a separate regression. Please refer to Tables 1 and 2 for details about the sample and controls. All regressions are weighted by the number of births in each cell. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Appendix Table 4: Effects of IHVPE on Marriage Rates *At Childbirth* In Years After IHVPE

	Dependent Variable: Proportion Married Births				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Pre-Treat. Mean of Dep. Var.	0.681	0.682	0.681	0.681	0.681
Year of IHVPE Initiation	0.0060	0.0082	0.0088	0.0082	0.0124**
	(0.0054)	(0.0057)	(0.0060)	(0.0057)	(0.0060)
One Year Post IHVPE	0.0023	0.0039	0.0036	0.0027	0.0072
	(0.0061)	(0.0068)	(0.0070)	(0.0060)	(0.0068)
Two Years Post IHVPE	0.0025	0.0046	0.0038	0.0031	0.0067
	(0.0039)	(0.0046)	(0.0048)	(0.0042)	(0.0054)
Three Years Post IHVPE	0.0018	0.0036	0.0034	0.0024	0.0052
	(0.0040)	(0.0046)	(0.0047)	(0.0040)	(0.0043)
Four Years Post IHVPE	0.0005	0.0021	0.0016	0.0015	0.0040
	(0.0027)	(0.0029)	(0.0028)	(0.0027)	(0.0036)
Five Years Post IHVPE	-0.0001	0.0001	-0.0001	-0.0002	0.0031
	(0.0022)	(0.0020)	(0.0022)	(0.0022)	(0.0031)
Mother and Child Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	602	573	545	545	545

Notes: Each column is a separate regression. Please refer to Tables 1 and 2 for details about the sample and controls. All regressions are weighted by the number of births in each cell. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Appendix Table 5: Welfare Reform, IHVPE, and Parental Marriage: CPS-CSS 1994-2008

	Dependent Variable: Mother is Married to Child's Biological Father			
	(1)	**(2)**	**(3)**	**(4)**
IHVPE Program Exists in State and Year of Child's Birth	-0.0294**			-0.0269**
	(0.0088)			(0.0087)
AFDC Waiver or TANF Implemented		0.0049		
		(0.0149)		
AFDC Waiver Implemented			0.0042	0.0040
			(0.0217)	(0.0224)
TANF Implemented			0.0053	-0.0040
			(0.0168)	(0.0167)
N	36,243	36,243	36,243	36,243

Notes: Each column is a separate regression. Please refer to Table 3 for details about the sample and controls. Information on AFDC waiver and TANF implementation is available from Bitler *et al.* (2006). All regressions also include mother and child controls, controls for state time-varying characteristics (excluding controls for the percent of the population that receives welfare benefit and the welfare benefit for a 4-person family), controls for child support laws, and controls for state EITC implementation. All regressions include state and child birth year fixed effects, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Appendix Table 6: Effects of IHVPE on Marriage by State Child Support Disregard Policies

	Dependent Variable: Mother is Married to Child's Biological Father			
	All Sample Mothers (Child Birth Years 1990-2003)		Welfare Recipients (Child Birth Years 1990-2003)	
	Disregard: $50/month +	Disregard: <$50/month	Disregard: $50/month +	Disregard: <$50/month
Pre-Treat. Mean of Dep. Var.	0.741	0.711	0.184	0.133
IHVPE Program Exists in State and Year of Child's Birth	-0.0129+	0.0146	-0.0342	0.0232
	(0.0068)	(0.0174)	(0.0222)	(0.0823)
N	97,340	29,280	8,110	1,137

Notes: Each column is a separate regression using data from the March CPS. Data on child support disregard policies come from Cancian *et al.* (2007) for years 1990-2003. The sample is split according to the disregard amount in each child's state and year of birth. Please refer to Tables 3 and 4 for details about the sample and controls. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Appendix Table 7: Effects of IHVPE on Child Health Insurance Provision: CPS-CSS 1994-2008

	Any Private Health Insurance Coverage	Coverage by Member of Household	Coverage by Person Outside Household	Coverage by Medicaid	Coverage by CHIP	Any Health Insurance Coverage
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treat. Mean of Dep. Var.	0.681	0.635	0.046	0.236	0.050	0.866
IHVPE Program Exists in State and Year of Child's Birth	-0.0265**	-0.0284**	0.0019	0.0065	0.0192	-0.0072
	(0.0100)	(0.0111)	(0.0039)	(0.0107)	(0.0185)	(0.0107)
Mother and Child Controls	√	√	√	√	√	√
Year FEs	√	√	√	√	√	√
State FEs	√	√	√	√	√	√
State Time-Varying Characteristics Controls	√	√	√	√	√	√
Child Support Laws Controls	√	√	√	√	√	√
State EITC Implementation	√	√	√	√	√	√
AFDC/TANF Implementation	√	√	√	√	√	√
State-Specific Time Trends	√	√	√	√	√	√
N	36,243	36,243	36,243	36,243	15,178	36,243
R-squared	0.2579	0.2474	0.0180	0.2057	0.0739	0.0493

Notes: Each column is a separate regression. Please refer to Table 3 for more details about the sample and controls. All regressions are weighted by the CPS person weights. Information on CHIP coverage is only available in 2002, 2004, 2006, and 2008 in the CPS-CSS. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Appendix Table 8: Effects of IHVPE on Mothers' Labor Supply, Different Definitions: March CPS 1989-2010

	Any Hours Worked	Mother is Employed	Mother is in Labor Force	Any Wage Income	Log Wage	Usual Hours Worked
Pre-Treat. Mean of Dep.Var.	0.681	0.582	0.630	0.643	9.228	23.616
IHVPE Program Exists in State and Year of Child's Birth	0.0181**	0.0141+	0.0115	0.0199**	0.0191	0.4772
	(0.0076)	(0.0081)	(0.0071)	(0.0077)	(0.0152)	(0.3096)
N	184,562	184,562	184,562	184,562	118,581	184,562

Notes: Each coefficient is from a separate regression. Please refer to Table 3 for details about the sample and controls. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Appendix Table 9: Net Effects of IHVPE on Father Involvement: CPS-CSS 1994-2008, Accounting for Selection Out of Marriage

Dependent Variable	**Pre-Treat. Mean of Dep. Var.**	**Coefficient**	**SE**
Father Made Any CS Payments in Last Year	0.863	-0.0122	(0.0077)
Father Made All CS Payments in Last Year	0.836	-0.0100	(0.0083)
Father Paid On Time All or Most of the Time in Last Year	0.878	-0.0122	(0.0097)
Father Has Court-Ordered Visitation Rights	0.931	-0.0066	(0.0086)
Father Has Joint Legal Custody	0.801	-0.0219**	(0.0083)
Number Days Father Spent with Child	299.489	-5.7399+	(2.9124)
Father Provided Gifts for Child	0.887	-0.0031	(0.0093)
Father Provided Clothes for Child	0.856	-0.0090	(0.0076)
Father Provided Food for Child	0.828	-0.0079	(0.0090)
Father Covered Childcare Expenses for Child	0.794	-0.0133	(0.0085)
Father Paid for Medical Expenses for Child	0.806	-0.0058	(0.0101)

Notes: Sample sizes range from N=33,328 to N=35,302. Each coefficient is from a separate regression. Married fathers are included in the regressions and are assumed to have made all their "child support payments" and to have made them on time in the previous year. They are assumed to have "visitation rights" and "joint legal custody", and are assumed to have spent 365 days with the child in the past year. They are assumed to have provided gifts, food, clothes, childcare, and medical help for the child. Please refer to Table 3 for details about the sample and controls. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Appendix Table 10: Effects of IHVPE on Imputed Private Child Health Insurance Provision: CPS-CSS 1994-2008

	Dependent Variable: Child Has Private Health Insurance (=1 if married parents)				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Pre-Treat Mean of Dep. Var.	0.846	0.846	0.847	0.847	0.847
IHVPE Program Exists in State and Year of Child's Birth	-0.0285***	-0.0231***	-0.0230***	-0.0234***	-0.0245***
	(0.0060)	(0.0054)	(0.0056)	(0.0056)	(0.0061)
Mother and Child Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	38,449	37,457	36,243	36,243	36,243
R-squared	0.1959	0.1970	0.1970	0.1970	0.1978

Notes: Each column is a separate regression. Children of married parents are coded as having private health insurance. Please refer to Table 3 for details about the sample and controls. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Appendix Table 11: Complier Characteristics of Parents Induced Out of Marriage by IHVPE: CPS-CSS 1994-2008

Maternal Characteristic	**Fraction of Sample (Weighted)**	**Relative Likelihood "Complier" Has Characteristic**
Mother's Age at Birth: <20	0.045	0.966
Mother's Age at Birth: 20-24	**0.203**	**3.050**
Mother's Age at Birth: 25-34	0.558	0.554
Mother's Age at Birth: 35+	0.195	0.086
Mother's Education: <HS	0.141	0.880
Mother's Education: HS degree	**0.296**	**1.712**
Mother's Education: Some College	0.292	0.811
Mother's Education: College+	0.270	0.356
Mother is Non-Hispanic White	0.632	0.696
Mother is Black	**0.144**	**1.864**
Mother is Hispanic	**0.173**	**1.884**
Child is Male	0.510	0.900

Notes: The table reports an analysis of the "complier characteristics" of parents who are induced out of marriage as a result of IHVPE. The ratios in column 2 give the relative likelihood that "compliers" have the characteristic indicated on the left. The relative likelihood ratio is calculated by dividing the IHVPE coefficient for the subsample defined by each characteristic by the overall IHVPE coefficient (0.027). Please refer to Table 3 for details about the sample and controls.