American Economic Journal: Applied Economics 2017, 9(2): 93–130
https://doi.org/10.1257/app.20150314

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

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With nearly half of US 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. (*JEL* I31, J12, J13, K36)

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. Forty-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

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† Go to https://doi.org/10.1257/app.20150314 to visit the article page for additional materials and author disclosure statement or to comment in the online discussion forum.

[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 US births.

of a program called in-hospital voluntary paternity establishment (IHVPE), which substantially lowered the cost of legal paternity establishment for unmarried fathers in the United States. 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 (US House of Representatives 2004). IHVPE programs, which have been implemented across all US 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 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. 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 financial, emotional, medical, and social benefits for children (Pearson and Thoennes 1995).

Yet the extent to which IHVPE can be successful in improving child well-being depends on parental choices regarding their relationships with each other and their children. To motivate my empirical work, I present a simple conceptual framework that highlights the trade-offs faced by parents who have children out of wedlock, drawing on existing models of collective decision making and the role of parental rights (Browning, Chiappori, and Weiss 2014; Edlund 2013), 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, and Tartari 2015). In my framework, parents are heterogeneous in match quality and face a trade-off between the benefit of joint child investment and the possible cost of interaction

[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 I provides additional details about IHVPE.

[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.html#background for more information.

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 increase in investments and well-being among switchers out of no relationship and the decrease among switchers out of marriage.

My empirical analysis exploits 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 pretreatment mean).[4] However, IHVPE also affects parental relationships on another margin. Consistent with my conceptual framework, 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.

I then study IHVPE's overall impacts on father involvement and child well-being using data on families with *both* married and unmarried parents. I find that the 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 children of 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 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 father involvement variables available in the data.

Similarly, I find that IHVPE does not lead to any detectable improvements in child well-being. I find no consistent effects of IHVPE on children's physical or mental health except for a small (marginally significant) negative effect on children's access to preventative care, possibly driven by the decline in private health insurance coverage. I also find no impacts on income, poverty status, or welfare benefit receipt in the child's household of residence. The lack of 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 results imply

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

[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, Meyer, and Roff 2007 for more details). The implications of these disregard policies for the empirical results are discussed briefly in Section V.

that policies that change parental relationship options may not fully achieve this goal—any potential 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.

The lack of overall positive effects on father involvement and child well-being suggests that any improvements in outcomes among families with parents who would have never married in the absence of IHVPE (i.e., the "switchers out of no relationship" in my conceptual framework) may be offset by reductions in outcomes in families with parents who would have otherwise been married (i.e., the "switchers out of marriage"). I attempt to identify the effects of IHVPE on the former group by studying outcomes in families with *unmarried parents only*, and using re-weighting and bounding methods to account for selection out of marriage (Imbens 2004, Lee 2009). I find suggestive evidence of improvements in some outcomes for these families: unmarried noncustodial fathers are more likely to make all required child support payments and to cover their children's childcare and medical expenses.

I consider several robustness tests. First, I show that the results are robust across a variety of specifications and different datasets. 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 preexisting 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 2010), 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.

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 2015, and 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

[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 Oliver 2002; and Mincy, Garfinkel, and Nepomnyaschy 2005). Finally, this paper

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 of the trade-off between child investment and partner interaction—long emphasized in family economics theory (e.g., Becker 1993; Edlund 2013; Brien, Lillard, and Stern 2006; Lundberg and Pollak 2012; and Browning, Chiappori, and Weiss 2014)—in understanding the behavior of parents who have children out of wedlock.

The paper proceeds as follows. Section I provides more information on IHVPE, while Section II discusses a conceptual framework that motivates the empirical analysis. Section III describes the data sources and summary statistics, Section IV details the empirical methods, and Section V presents the main results and robustness tests. Finally, Section VI concludes.

I. 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 United States; 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 that facilitate at-birth paternity establishment for children born to unmarried parents. Specifically, all hospitals and birthing centers in each state are required to provide all adult unmarried new mothers 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, and there is no DNA testing involved—paternity is legally established after both parents sign the voluntary paternity acknowledgement form.[8] The OMBRA also mandated that state child support agencies make available materials for educating parents about paternity establishment and for training hospital staff. Additionally, state child support agencies are required to monitor hospital compliance on at least an annual basis. The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) further reinforced OMBRA's IHVPE mandate, and expanded the role

relates to the large literature on how marriage behavior (not just among parents) responds to various policies (e.g., Schoeni and Blank 2000; Bitler et al. 2004; Bitler, Gelbach, and Hoynes 2006; Halla 2013; and Persson 2015).

[7] See twentieth Annual Office of Child Support Enforcement Report to Congress available at: https://www.acf.hhs.gov/css/resource/fy1995-annual-report.

[8] 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.

of hospital staff by requiring them to provide all new unmarried parents with written materials and oral explanations regarding the rights and responsibilities associated with paternity establishment. Policymakers argued that IHVPE programs would be effective as they 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.[9]

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 IV and V, 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 also supplement these data with information collected by Nepomnyaschy and Garfinkel (2007). For most states in my data, the date of implementation refers to the date by which all hospitals needed to have the program in place.[11] However, for some states, I could not find this information, and I instead assign the year of legislation or statute revision as the year of implementation. In Section VE, I show that my results are robust to dropping from the analysis this subset of states with less accurate information on the timing of IHVPE implementation.

Online Appendix Figure A1 shows the variation in the timing of IHVPE program implementation across states, while online Appendix Table A1 presents more details for each of the 44 states in my data.[12] Births in these states account for about 96 percent of all births in the United States over the time period of analysis. Online Appendix Figure A2 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

[9] See the US 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.

[10] See the US 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] Unfortunately, information on the start of implementation (i.e., the date when the first hospital in the state had IHVPE) is not available.

[12] I do not have data for the following states: Iowa, Montana, New Hampshire, New Mexico, Oklahoma, West Virginia, and Wyoming.

between marriage and non-marriage, rather than between marriage, nonmarital cohabitation, and non-marriage/non-cohabitation. For completeness, I also examine parental nonmarital cohabitation and find no statistically significant effects of IHVPE on this outcome (see Section V for more details).

II. Conceptual Framework

To motivate the subsequent empirical analysis, this section outlines a simple conceptual framework that highlights how the implementation of IHVPE—a policy that lowered the cost of a relationship contract granting legal parental rights and obligations to unmarried fathers—can affect family structure and child well-being. More mathematical details are presented in online Appendix B.

Consider a setting where parents derive utility from child quality, private adult consumption, and their match quality. Child quality, in turn, depends positively on both parental investment and match quality, where investments and match quality are complements. Intuitively, the idea is that higher match quality leads to less parental conflict and more productive investments into children. Investments broadly encompass both financial and non-pecuniary (e.g., time) expenditures.

Parents can choose between three relationship contracts: marriage, paternity, or no legal relationship. Fathers have full parental rights in marriage, partial rights in paternity, and no legal rights if they have no legal relationship with the mother. Parents are also heterogeneous in match quality, where some parents experience a benefit from interacting with one another, while others experience a cost. The degree to which match quality can impact parental utility and child quality depends on the level of parental interaction in each relationship contract. I assume that parental interaction is proportional to the amount of parental rights that the father has—thus, parental utility and child quality are more sensitive to match quality in marriage than in paternity and in paternity than in no relationship.

Finally, there are fixed costs associated with entering into the marriage and paternity establishment contracts that are distinct from match quality. The fixed cost of entering into marriage is higher than the fixed cost of establishing paternity. Intuitively, one can think of the marriage cost 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 incorporates the informational, financial, time, administrative, and legal costs related to establishing paternity.

Choosing the Optimal Contract.—To solve for the parents' optimal choices, it is first necessary to characterize the mode of interaction in each relationship contract. In marriage, I assume that the parents cooperate; they have transferable utility and maximize their joint utility subject to a joint income constraint. Outside marriage, following the literature (e.g., Weiss and Willis 1985, Del Boca and Flinn 1995, Willis 1999, and Roff and Lugo-Gil 2012), I assume the parents do *not* bargain cooperatively and instead face a static Stackelberg game. In this game, the father makes a financial transfer (i.e., a child support payment) to the mother, while the mother chooses how to allocate this transfer between child investment and her own private consumption. When choosing his financial transfer, the father takes into account the mother's optimal response function.[13] In the paternity contract, the father faces a constraint of a minimum child support payment (i.e., a child support order), while there is no minimum in the case of no relationship.[14]

Parents choose optimal child investment and private adult consumption levels that maximize their utility functions under each contract. Then, given their match quality and the fixed costs of entering marriage and paternity, each parent prefers the contract that gives him/her the highest indirect utility value. If the parents disagree on their optimal contracts, then we can assume the following decision rules: 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; and paternity establishment is unilateral relative to no relationship, as the mother can essentially force the father to establish paternity by taking him to court.

This framework implies that, for a high enough level of match quality, optimal child investment should be higher in marriage than in paternity, and higher in paternity than in no relationship. Intuitively, the parents face no cooperation problems and thus achieve the first-best child investment level in marriage. Parents invest less outside marriage 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 idea). Moreover, fathers' transfers are higher in paternity than in no relationship as they are only subject to a positive child support order constraint under the former contract.[15]

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, the framework 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.

[13] 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 transferable utility is more realistic for these couples. However, cohabiting unions are quite unstable in the United States—for example, according to data from the Fragile Families and Child Well-Being Study, less than 35 percent of unmarried parents still live together by the time their child is five years old. Thus, a noncooperative Stackelberg setting where the father gains partial rights and responsibilities to his child is likely a more realistic representation of the paternity contract over the longer term.

[14] 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 can predict opposite impacts on formal versus 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.

[15] 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, Grossbard, and Huang 2005).

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 the fixed cost of entering into the paternity establishment contract.

Not surprisingly, as the costs of establishing paternity are lowered, more parents will 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 match quality are monotonically increasing with the optimal relationship level, we can represent the predicted effects graphically in online Appendix Figure A3. Here, θ denotes match quality, which ranges from $\underline{\theta}$ to $\overline{\theta}$. 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.

This framework also highlights why the implications of IHVPE for parental investment and child well-being 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 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 possible 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 noncognitive skills, which may limit them in making highly productive child investments (Cunha and Heckman 2007; Heckman, Humphries, and Kautz 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, mothers who do not marry their children's fathers may be more likely to marry or cohabit with new partners. 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 benefit of joint child investment and the cost of interaction among parents with sufficiently low match quality.[16]

As it is essentially impossible to observe match quality in data, and as parental investments and child quality can only be observed imperfectly, I do not argue that my empirical analysis can conclusively distinguish this model from alternative models with similar predictions. However, it is important to emphasize that it is possible to test for 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, while the framework discussed here unambiguously predicts a decrease.

III. Data and Summary Statistics

A. *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 the 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.[17] 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.[18] 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.[19]

B. *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

[16] 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 of 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, and Edin and Nelson 2013).

[17] 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.

[18] 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.

[19] 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 from the analysis on paternity establishment rates.

Population Survey Child Support Supplements (CPS-CSS) from 1994 to 2008.[20] More details on the sample construction are presented in online Appendix C. 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 noncustodial parent.

My main analysis sample consists of mothers with a youngest child aged 5 years or less in the household, who were aged 18–45 years at the time of childbirth, who resided in the United States in the previous year, and who have non-missing CPS-CSS person weights.[21] These restrictions leave me with 38,445 sample mothers in the CPS-CSS data. Out of them, 7,082 are asked the CSS questions and are not married to their youngest children's fathers.

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.[22] The March CPS analysis sample consists of 212,504 mothers of youngest children aged 5 years or less.

C. *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.[23] 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 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.[24] For confidentiality reasons, actual sample sizes from

[20] 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).

[21] I limit the sample to families with children aged five years or less because I only have information on children's states of residence (and not their states of birth) in the data. Including families who only have older children would introduce more measurement error in assigning treatment. This issue is discussed further below. Additionally, I focus on the age of the *youngest* child because I 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). Finally, the sample drops mothers with missing CPS-CSS supplement weights because most of the regressions are weighted. However, all results are very similar when not weighted.

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

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

[24] The age cutoff 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 six and seven 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 spillover 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

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 7 relatively small states.

D. *Summary Statistics*

Online Appendix Table A2 presents the sample means of key variables in each of the datasets used in my analyses.[25] 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-IHVPE and post-IHVPE: the average among state-year cells without IHVPE is 0.55, while the average among state-year cells with IHVPE is 1.06.[26]

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 1 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 in the CSS sample, child support receipt rates are fairly low: in the year prior to the survey, only 34 percent received any child support payments, while only 21 percent received all of the child support that was due.[27] However, about 69 percent of mothers state that the father has legal visitation rights, and nonresident fathers spend an average of 61 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. Mean after-tax family income (in 2010 dollars) is about \$56,896.[28] The NHIS suggests that cohabitation

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.

[25] Washington was the first state to implement IHVPE in 1989 (see online Appendix Table A1). 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 datasets. 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.

[26] This ratio ranges from 0.12 to 3.9 in the data. It can be larger than 1 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.

[27] One possible reason for the low rate of reported child support receipt may be the fact that in families where mothers receive welfare and/or Medicaid benefits, most child support is kept by the government and not passed through to the mothers themselves. Mothers may be unaware of the amount of child support being paid on their behalf, and may incorrectly report that they are not getting as much child support as they should be getting.

[28] After-tax income is calculated using the TAXSIM program available through the National Bureau of Economic Research (NBER). For each mother in the March CPS sample, it is constructed based on the mother's and her spouse's (if present) wage and salary income, business income, farm income, other income (including interest, rent, alimony, and educational fellowships), pension income, social security benefits, nontaxable transfer

rates are much lower than marriage rates: about 5 percent of mothers report cohabiting with their child's father and 2 percent report cohabiting with someone else.

IV. 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:

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

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 of births by white, black, and Hispanic mothers, the proportion of male births, and the proportion of 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 state time-varying characteristics, including the state unemployment rate, the state minimum wage rate, the state poverty rate, the average Aid to Families with Dependent Children/Temporary Assistance for Needy Families (AFDC/TANF) benefit for a four-person family, the proportion of the population receiving welfare benefits, the proportion of the population receiving Medicaid benefits, the proportion of the population receiving Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), 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 Earned Income Tax Credit (EITC) has been enacted, and indicators for whether an AFDC waiver or the TANF program have been implemented.[29] μ_s is a state fixed effect; α_y is a year fixed effect; $\delta_s \times y$ is a state-specific time trend; and ϵ_{sy} is the error term, which I cluster by state. The key coefficient of

income (welfare, workers compensation, veterans benefits, and child support), unemployment income, and net capital gains/losses. It accounts for the mother's marital status, state of residence, and total number of dependents (including children aged 19 years or less, children who are full-time students aged 24 years or less, and adult disabled individuals).

[29] 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 nonpayment, and the presence of a New Hires directory), and child support enforcement spending come from Nepomnyaschy and Garnkel (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, Gelbach, and Hoynes (2006).

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:

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

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, indicators for the total number of children in the household (one, two, or three or more), and indicators for the child's single years of age.[30] 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 the child's state and year of birth on the outcome of interest.

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. To test for endogenous mobility, I use the 1990 and 2000 US census data (5 percent samples) together with the 2001–2010 American Communities Survey (ACS) waves, which contain information on respondents' states of birth and states of residence. These datasets are not ideal for the main analysis since they are missing observations in years 1991–1999, when much of the IHVPE variation takes place. Nevertheless, I use these data to estimate equation (2) with an indicator for the child residing in a state that is different than the state of his/her birth as the outcome, on a sample of mothers of youngest children aged 5 years or less who were aged 18–45 at childbirth. The results are presented in online Appendix Table A3, showing that there is no statistically significant correlation between IHVPE and mobility.[31]

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

[30] The restricted NHIS data extract available to me does not contain information on the total number of children in the household; these controls are omitted from the NHIS analyses.

[31] I have also used the CPS-CSS data to test whether IHVPE is correlated with 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 -0.0095 with a standard error of 0.0075.

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 V, 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 nonempty).

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 percent 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.[32] The timing of IHVPE initiation is uncorrelated with numerous factors, including fertility (log number 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 online Appendix Table A4, 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, online Appendix Table A5 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).[33]

[32] 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.

[33] I also find no effects on log births in the five years after IHVPE (results available upon request). Additionally, I have 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.

Table 1—IHVPE Programs and Maternal and State Characteristics: 1992–2005

Dependent variable	IHVPE coef. (1)	SE (2)	Dep. var. mean (3)	Coef./mean (4)
log births	−0.0032	(0.0038)	11.8010	−0.0003
Share unmarried births	−0.0121	(0.0108)	0.3334	−0.0363
Share births; mom age < 20	−0.0003	(0.0006)	0.0761	−0.0039
Share births; mom ed: < HS	0.0011	(0.0016)	0.2230	0.0049
Share births; mom ed: college+	−0.0025	(0.0024)	0.2358	−0.0106
Share births; mom non-Hisp. white	−0.0067	(0.0044)	0.5878	−0.0114
Share births; mom black	0.0022	(0.0012)	0.1617	0.0136
Share births; mom Hisp.	0.0025	(0.0024)	0.2016	0.0124
State unemp. rate last year	0.1109	(0.1519)	5.5786	0.0199
State pov. rate last year	0.4218	(0.3373)	13.1277	0.0321
State min. wage last year	−0.0636	(0.0917)	4.7386	−0.0134
Share of pop. on welfare last year	0.0023	(0.0022)	0.0349	0.0659
Welfare ben. (4-p fam) last year	−10.4670	(7.0722)	475.6215	−0.0220
Dem. gov. last year	−0.0575	(0.0793)	0.4030	−0.1427
Share of pop. on Medicaid last year	−0.0038	(0.0040)	0.1329	−0.0286
State EITC implemented	0.0226	(0.0516)	0.2223	0.1017
TANF implemented	−0.0956	(0.0809)	0.5778	−0.1655
AFDC waiver implemented	0.1337	(0.1279)	0.1016	1.3159
Universal wage withholding implemented	0.0357	(0.0974)	0.8959	0.0398
New hires directory implemented	0.0131	(0.0491)	0.5416	0.0242
License revoc. for nonpayment implemented	−0.1181	(0.1222)	0.6688	−0.1766
log tot. child support enforcement spending	0.0339	(0.0286)	18.5804	0.0018

Notes: The number of observations (state-year cells) varies between 574 and 602. Each row contains a different dependent variable. Column 1 contains coefficients from separate regressions for an indicator for IHVPE existing in a state/year. Column 2 contains standard errors; column 3 contains the dependent variable mean; and column 4 contains the effect size calculated by dividing the coefficient by the mean. Units of observation are state-year cells consisting of the 43 sample states (Alabama, Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, District of Columbia, 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 online Appendix Table A1 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.

As discussed further in Section VI, my results do not rule out the possibility that some fertility and marriage-at-birth effects may materialize over a longer time span. However, the results in Table 1 and online Appendix Table A5 imply that fertility and marriage-at-birth responses are not empirically relevant for the time frame in my analysis.

The lack of correlation between IHVPE and the timing of welfare reform is also worth highlighting. As shown by Bitler, Gelbach, and Hoynes (2006), state AFDC waivers were implemented 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, including universal wage withholding, the New Hires Directory, and license revocation for nonpayment. The last row of Table 1 further demonstrates that IHVPE is uncorrelated with total state spending on child support enforcement. Note that many of the child support enforcement laws enacted during the sample time frame were at the federal level 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 I, 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.[34]

V. Results

A. *Effects of IHVPE on Paternity Establishment*

Figure 1 plots the coefficients and 95 percent confidence intervals from estimating an event-study version of equation (1) that includes indicators for five years before, the year of, and five years after IHVPE implementation. I assume that the first year IHVPE is in effect is the same as the year listed in online Appendix Table A1 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. I explore the sensitivity of the main results to alternative ways of assigning IHVPE timing in Section VE.

Thus, at $year - IHVPE\ first\ year = 0$ in states where the month of IHVPE initiation is February–June or unknown, there may only be partial IHVPE treatment. Similarly, at $year - IHVPE\ first\ year = -1$ in states where the month of IHVPE initiation is July or later, there may also be partial IHVPE treatment. In sum, as shown in Figure 1, there are effectively three possible treatment statuses: "pretreatment" $= year - IHVPE\ first\ year \leq -2$, "partial treatment" $= year - IHVPE\ first\ year \in \{-1, 0\}$, and "posttreatment" $= year - IHVPE\ first\ year \geq 1$. The omitted category in Figure 1 is $year - IHVPE\ first\ year = -2$, the first year of sure pretreatment.

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

[34] Of course, it is still possible that unobservable child support enforcement factors are correlated with IHVPE implementation. For instance, IHVPE may lead to better data coordination that enables smoother child support collection processes. However, given that there is no correlation between IHVPE and the establishment of the New Hires Directory (which requires data coordination across agencies), or between IHVPE and total child support enforcement spending, these concerns seem less likely.

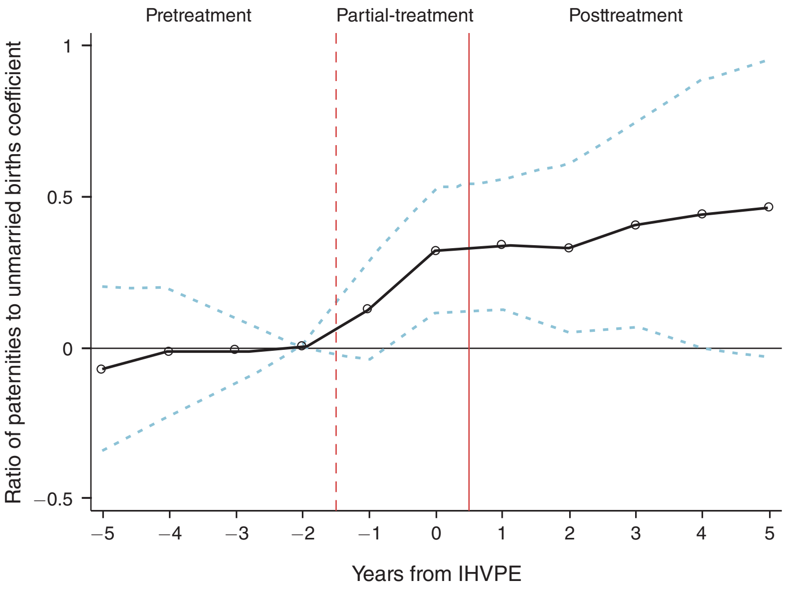

Figure 1. Effects of IHVPE on Paternity Establishment Rates by Year

Notes: This figure plots θ_k coefficients (and 95 percent confidence intervals in dashed lines) from estimating the following equation: $Pat_{sy} = \beta_0 + \sum_{k=-5}^{-3} \theta_k \times IHVPE_{syk} + \sum_{k=-1}^{5} \theta_k \times IHVPE_{syk} + \gamma' X_{sy} + \phi' C_{sy} + \mu_s + \alpha_y + \epsilon_{sy}$, 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 online Appendix Table A1 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: "pretreatment" = *year* − *IHVPE first year* ≤ −2, "partial treatment" = *year* − *IHVPE first year* ∈ {−1, 0}, and "posttreatment" = *year* − *IHVPE first year* ≥ 1. These three groups are depicted using the vertical lines at −2 and 0 in the graph.

the "partial treatment" period. In the sure post-IHVPE period (≥ 1), there is a clear statistically significant increase in the paternity establishment rate.[35] 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

[35] 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 IV above). Additionally, as discussed below, results using log paternities established and the ratio of paternities over total births are similar.

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

	Pat/unmar. births					Pat/ all births	log pat.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IHVPE exists in state/year	0.2412 (0.0606)	0.2064 (0.0597)	0.2009 (0.0660)	0.2027 (0.0594)	0.2100 (0.0601)	0.0674 (0.0196)	0.2791 (0.0958)
Pretreat. mean, dep. var.	0.552	0.553	0.552	0.552	0.552	0.174	9.987
Mother and child controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State time-varying controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Child support laws controls	No	No	Yes	Yes	Yes	Yes	Yes
State EITC implementation	No	No	No	Yes	Yes	Yes	Yes
AFDC/TANF implementation	No	No	No	Yes	Yes	Yes	Yes
State-specific time trends	No	No	No	No	Yes	Yes	Yes
Observations	601	572	544	544	544	544	544
R^2	0.6943	0.7306	0.7338	0.7458	0.8265	0.8400	0.9639

Notes: Each column is a separate regression. In columns 1–5, the dependent variable is the ratio of paternities to unmarried births. In column 6, the dependent variable is the ratio of paternities to all births, and in column 7, the dependent variable is log paternities. The key coefficient of interest is an indicator for IHVPE existing in a state/year. Standard errors are in parentheses below the coefficients. The following row below the standard errors contains the pre-IHVPE mean of the dependent variable. 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 high school, high school, some college, college+), mother's race (non-Hispanic white, non-Hispanic 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 four-person family, the percent of the population on Medicaid, the percent of population receiving WIC, 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 nonpayment, 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.

38 percent increase at the pre-IHVPE mean.[36] 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, lagged child support enforcement spending, state EITC, 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.[37]

[36] The unweighted regressions yield very similar results: the key coefficient of interest is 0.2034 with a standard error of 0.0667.

[37] 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 for some child support laws for Kentucky and South Dakota.

B. *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 datasets is further complicated by the fact that I do not observe children's years or months of birth. Therefore, as discussed in online Appendix C, 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 pretreatment 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: "pretreatment" = *child's birth year* − *IHVPE first year* ≤ -2, "partial treatment" = *child's birth year* − *IHVPE first year* $\in \{-1, 0\}$, and "posttreatment" = *child's birth year* − *IHVPE first year* ≥ 1.

Figure 2 plots the coefficients and 95 percent 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 pretreatment. 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 "partial 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 2.8 percentage points—a 4 percent decrease at the pretreatment mean of 75 percent in the CPS-CSS.[38] However, given that Table 1 shows a lack of correlation 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 22 percent (0.028/0.13).[39] Taken together with the

[38] Unweighted regressions yield very similar results: the key coefficient of interest is -0.0217 with a standard error of 0.0061. Regressions using the larger March CPS sample also yield similar results: the key coefficient of interest is -0.0147 with a standard error of 0.005.

[39] 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 percent. This would imply that the true magnitude of the effect is somewhat lower than

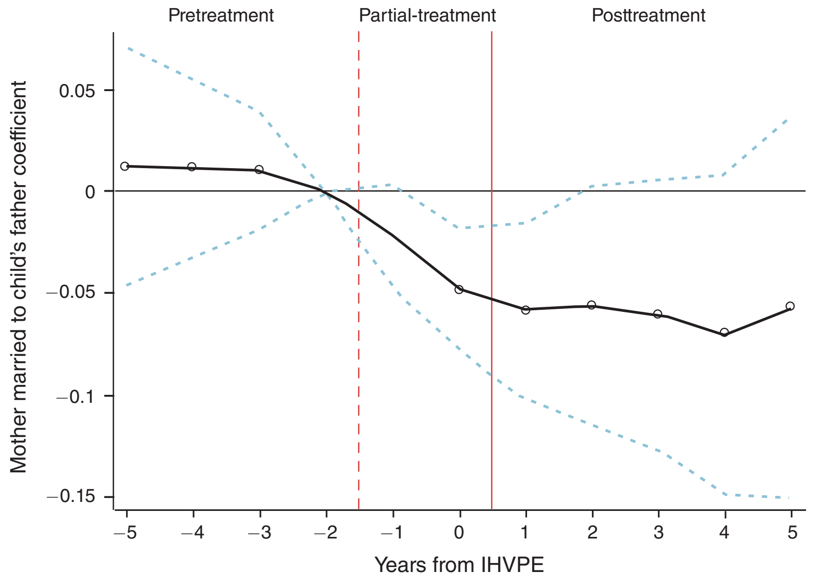

Figure 2. Effects of IHVPE on Parental Marriage by Year

Notes: This figure plots θ_k coefficients (and 95 percent confidence intervals in dashed lines) from estimating the following equation: $Y_{isty} = \beta_0 + \sum_{k=-5}^{-3} \theta_k \times IHVPE_{syk} + \sum_{k=-1}^{5} \theta_k \times IHVPE_{syk} + \gamma' X_{isty} + \phi' C_{st} + \mu_s + \alpha_y + \epsilon_{isty}$, where $IHVPE_{syk}$ is an indicator for k years from IHVPE implementation state s and the child's approximate birth year y. 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 pretreatment 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: "pretreatment" $=$ *child's birth year* $-$ *IHVPE first year* ≤ -2, "partial treatment" $=$ *child's birth year* $-$ *IHVPE first year* $\in \{-1, 0\}$, and "posttreatment" $=$ *child's birth year* $-$ *IHVPE first year* ≥ 1. These three groups are depicted using the vertical lines at -2 and 0 in the graph.

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.[40]

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 I, unmarried cohabiting fathers are by law treated in the same way as unmarried non-cohabiting fathers

22 percent. Unfortunately, CPS-CSS data do not have information on the percentage of parents who marry after childbirth.

[40] This elasticity is calculated by dividing the parental marriage coefficient (0.0281) by the paternity establishment rate coefficient (0.2100).

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

	Dep. var: mother is married to child's biological father				
	(1)	(2)	(3)	(4)	(5)
IHVPE exists in state/year of child's birth	−0.0335 (0.0082)	−0.0271 (0.0075)	−0.0279 (0.0077)	−0.0286 (0.0076)	−0.0281 (0.0088)
Pretreat. mean, dep. var.	0.749	0.750	0.750	0.750	0.750
Mother and child controls	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
State time-varying controls	No	Yes	Yes	Yes	Yes
Child support laws controls	No	No	Yes	Yes	Yes
State EITC implementation	No	No	No	Yes	Yes
AFDC/TANF implementation	No	No	No	Yes	Yes
State-specific time trends	No	No	No	No	Yes
Observations	38,445	37,454	36,241	36,241	36,241
R^2	0.2244	0.2252	0.2249	0.2250	0.2258

Notes: Each column is a separate regression. The dependent variable in all columns is an indicator for the mother being married to child's father. The key coefficient of interest is an indicator for IHVPE existing in the state/year of child's birth. Standard errors are in parentheses below the coefficients. The following row below the standard errors contains the pre-IHVPE mean of the dependent variable. 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 youngest 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 high school, high school, some college; college+ omitted), woman's race (non-Hispanic white, non-Hispanic black, Hispanic; other omitted), child sex, total number of children in the household (1, 2; 3 or more omitted), and indicators for the youngest 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 four-person family, the percent of the population on Medicaid, the percent of the population receiving WIC, 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 nonpayment, 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 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.

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 nonmarital 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 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

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

Dependent variable	Pretreat. mean (1)	IHVPE coef. (2)	SE (3)
Mother is …			
Never married (CPS-CSS, $N = 36{,}241$)	0.134	0.0177	(0.0060)
Married to someone other than bio. father (CPS-CSS, $N = 36{,}341$)	0.010	0.0060	(0.0027)
Divorced (CPS-CSS, $N = 36{,}241$)	0.103	0.0047	(0.0056)
Cohabiting with the bio. father (NHIS)	0.022	−0.0037	(0.0033)
Cohabiting with someone other than bio. 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	0.0100	(0.0047)
Worked any usual hours in the past year	0.967	0.0043	(0.0023)
Is aged 20–24 years	0.050	−0.0079	(0.0032)
Is aged 25–34 years	0.479	0.0101	(0.0078)
Is aged 45+ years	0.064	0.0077	(0.0036)
Has less than high school education	0.118	−0.0014	(0.0045)
Has high school degree	0.332	0.0090	(0.0074)

Notes: Each row contains a different dependent variable. Column 1 contains the pre-IHVPE mean of the dependent variable. Column 2 contains coefficients from separate regressions for an indicator for IHVPE existing in a state/year. Column 3 contains standard errors. Each coefficient is from a separate regression, where the key coefficient of interest is an indicator for IHVPE existing in the state/year of the child's birth. 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.

findings suggest that IHVPE enables some mothers to share a household with new partners who are of higher "quality" than their children's fathers.

Accounting for Welfare Reform.—Online Appendix Table A6 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, Gelbach, and Hoynes 2006; and 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 preferable 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. Online Appendix Table A7 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).

Additional Tests of the Conceptual Model.—Finally, I implement two empirical exercises to shed some more light on the plausibility of the conceptual framework described in Section II. 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 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.[41] I split the sample into deciles of the pre-IHVPE predicted marriage distribution, and estimate separate regressions of equation (2) with parental marriage as the outcome. Online Appendix Figure A4 presents the coefficients.[42] The figure shows that the negative effect of IHVPE on parental marriage is concentrated around the fortieth and fiftieth 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 online Appendix Table A8, I present the relative

[41] Specifically, I use a probit model to estimate a regression of the form: $Married_{isty} = \beta_0 + \gamma' X_{isty} + \phi' C_{st} + \mu_s + \alpha_y + \delta_s \times y + \epsilon_{sy}$, 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.

[42] The 95 percent 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, Gelbach, and Miller 2008) to account for the fact that the sample is split along deciles of a predicted variable.

likelihood ratios for "complier" mothers having different characteristics.[43] 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.

C. *Overall Effects of IHVPE on Parental Investments and Child Well-Being*

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. I next analyze whether the program had any overall impacts on measures of parental investments and child well-being. The average effects on these outcomes depend on the changes in behaviors among two groups of parents—those who would have never been married in the absence of IHVPE, and those who would have otherwise been married. Thus, it is necessary to use data on outcomes that are available for children of *both* married and unmarried parents.

I begin with studying children's private health insurance coverage, which can be seen as a parental investment proxy and is available for all children in the CPS-CSS data. The first row in Table 5 shows 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 online Appendix Table A9.[44] 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 noncustodial fathers provide health insurance coverage for their children through their employers if it is offered.[45]

The next row in Table 5 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 online Appendix Table A10, I document that the labor supply effect is consistent across different

[43] The relative likelihood ratio is calculated by dividing the IHVPE coefficient for the subsample defined by each characteristic by the overall IHVPE coefficient (0.028).

[44] Information on CHIP coverage is only available in the CPS-CSS in 2002, 2004, 2006, and 2008.

[45] See: http://publications.usa.gov/epublications/childenf/obligate.htm.

TABLE 5—OVERALL EFFECTS OF IHVPE ON MEASURES OF FATHER INVOLVEMENT AND CHILD WELL-BEING

Dependent variable	Pretreat. mean (1)	IHVPE coef. (2)	SE (3)
Father involvement and child resources (*March CPS: N* = 184,562*; CPS-CSS: N* = 36,241)			
Child has any private health insurance coverage (CPS-CSS)	0.681	−0.0263	(0.0101)
Mother worked any usual hours last year (March CPS)	0.681	0.0175	(0.0074)
Child resource index (March CPS)	0.006	0.0004	(0.0087)
log after-tax family income (March CPS)	10.555	0.0058	(0.0151)
Above poverty (March CPS)	0.802	0.0033	(0.0045)
Above 1.5 × poverty (March CPS)	0.695	0.0026	(0.0046)
Not receiving welfare (March CPS)	0.888	−0.0054	(0.0045)
Welfare income (March CPS)	634.427	−0.0885	(34.7787)
Child health (*NHIS: N* ≈ 50,000)			
Physical health index	−0.0120	−0.0154	(0.0119)
Mental problems index (higher value = more problems)	1.2620	0.0884	(0.0840)
Any learning disability (age 3+ only)	0.0397	−0.0069	(0.0066)
Any doctor visits (in the past 12 months)	0.8419	−0.0136	(0.0068)

Notes: Each row contains a different dependent variable. Column 1 contains the pre-IHVPE mean of the dependent variable. Column 2 contains coefficients from separate regressions for an indicator for IHVPE existing in a state/year. Column 3 contains standard errors. All income variables are in 2010 dollars. After-tax family income is calculated using the TAXSIM program from the NBER. 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.

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.

Online Appendix Table A11 attempts to study the overall impacts on other father involvement variables that are available in the CSS data. Recall that mothers who are married to their children's biological fathers are not asked CSS questions, and I am therefore missing father involvement variables for these families. To include them, I simply make an assumption that married fathers 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 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; and Carlson, McLanahan, and Brooks-Gunn 2008). The results in online Appendix Table A11 show that when married fathers are included in the analysis in this way, 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 possible increases in involvement among fathers who would have remained unmarried in the absence of IHVPE.[46]

[46] 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

The remainder of Table 5 uses data from the March CPS and the NHIS to study child household resources and child mental and physical health, respectively. As outcomes in the March CPS, I consider log total after-tax family income, indicators for the household living above the poverty line and living above $1.5 \times$ the poverty line, an indicator for the household not receiving welfare income, and the total welfare income received (including zeros). I also use these outcomes to create a "child resource index," following Kling, Liebman, and Katz (2007). 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.

From the NHIS, I consider four outcomes: a "physical health index," a "mental problems index," an indicator for the child having a learning disability (available for children aged 3+ only), and an indicator for the child having at least 1 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 "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.[47]

I find no statistically significant effects of IHVPE on any of the March CPS outcomes. These results suggest that any potential 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 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 in the NHIS. There is, however, a small marginally significant 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.

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 online Appendix Table A12, 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 0.8521, 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.

[47] The "mental problems" index is available for sample children aged two to three 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, and any problems limiting ability to walk/play/run.

D. *Effects on Father Involvement and Child Well-Being in Families with Unmarried Parents*

While the results in the previous subsection suggest that the overall effects of IHVPE on father involvement and child well-being are either zero or negative, they may mask important heterogeneity. A central goal of IHVPE is to improve the welfare of children in families with *unmarried* parents, and it is possible that children whose parents would have not been married in the absence of IHVPE do experience some benefits from the higher paternity establishment rates.

However, 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. Put differently, the composition of unmarried parents is likely different pre-IHVPE and post-IHVPE. To address this issue of selection and to better understand the direct effects of IHVPE on families with parents who would have never been married, I use two approaches. First, I use inverse treatment propensity score weights to create balanced pre-IHVPE and post-IHVPE samples within the unmarried group following procedures described in Imbens (2004) and Garrido et al. (2014). Second, I perform a bounding exercise following Lee (2009).

I begin my analysis by studying father involvement outcomes available in the CSS sample. The first column in panel A of Table 6 shows that IHVPE leads to a 2 percentage point increase in the likelihood of the mother being in the CSS sample, which is consistent with the negative effect on marriage documented above. Panel B of Table 6 presents the results from estimating regression (2) on the entire CSS sample, without accounting for the issue of 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," which consists of all of the involvement outcomes listed in the table and is constructed in the same way as the "child resource index." While the relatively low sample sizes in the CSS imply that many of the coefficients are not statistically significant, the magnitudes are almost all positive and fairly large. There is a marginally significant increase in the father's likelihood of making all child support payments that are due, and significant increases in the likelihoods of fathers covering childcare and medical expenses for their children.[48] Of course, a positive effect on father involvement in panel B 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 father-child relationship for fathers who would have remained unmarried in the absence of IHVPE.

In panel C of Table 6, I reestimate the regressions in panel B without using the CSS sample weights. Since the subsequent analysis re-weights the data without accounting for the sample weights, these results are reassuring as they show that the unweighted results are very similar to those in panel B.

[48] 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 underreport the number of days fathers spend with their children relative to what fathers report themselves.

Table 6—IHVPE and Father Involvement among Families with Unmarried Parents: CPS-CSS 1994–2008

	In CSS (1)	Index (2)	Any CS (3)	All CS (4)	On time (5)	Visits (6)	Days (7)	Gifts (8)	Clothes (9)	Food (10)	Childcare (11)	Med. (12)
Panel A. Whole sample												
IHVPE	0.0206											
	(0.0084)											
Panel B. CSS sample, not accounting for selection, weighted with CSS sample weights												
IHVPE		0.0729	0.0161	0.0435	0.0212	−0.0009	6.7971	0.0339	0.0181	0.0398	0.0358	0.0575
		(0.0468)	(0.0209)	(0.0223)	(0.0356)	(0.0334)	(5.9373)	(0.0357)	(0.0270)	(0.0250)	(0.0175)	(0.0233)
Panel C. CSS sample, not accounting for selection, unweighted												
IHVPE		0.0620	0.0119	0.0278	0.0167	0.0101	4.8638	0.0310	0.0160	0.0394	0.0313	0.0528
		(0.0436)	(0.0215)	(0.0207)	(0.0336)	(0.0328)	(5.6818)	(0.0341)	(0.0238)	(0.0245)	(0.0173)	(0.0222)
Panel D. CSS sample, re-weighted using treatment propensity scores												
IHVPE		0.0703	0.0035	0.0278	0.0207	0.0190	5.8288	0.0410	0.0240	0.0382	0.0308	0.0584
		(0.0418)	(0.0202)	(0.0243)	(0.0348)	(0.0317)	(5.3865)	(0.0336)	(0.0231)	(0.0241)	(0.0181)	(0.0206)
Panel E. CSS sample, Lee lower bound												
IHVPE		0.0294	−0.0017	0.0199	0.0040	−0.0108	−0.7523	0.0177	−0.0047	0.0095	0.0018	0.0262
		(0.0432)	(0.0208)	(0.0223)	(0.0361)	(0.0335)	(5.6790)	(0.0367)	(0.0290)	(0.0263)	(0.0170)	(0.0239)
Panel F. CSS sample, Lee upper bound												
IHVPE		0.0979	0.0227	0.0469	0.0297	0.0248	9.0525	0.0495	0.0279	0.0496	0.0388	0.0632
		(0.0464)	(0.0211)	(0.0227)	(0.0346)	(0.0351)	(5.9669)	(0.0355)	(0.0270)	(0.0253)	(0.0175)	(0.0235)
Mean	0.2195	−0.0051	0.3418	0.2122	0.2912	0.6941	55.4430	0.5007	0.3657	0.2394	0.0921	0.1431
N	36,241	7,082	6,289	6,289	5,108	7,082	6,540	7,082	7,082	7,082	7,082	7,082

Notes: Each column contains a different dependent variable. Each coefficient is from a separate regression, where the key coefficient of interest is an indicator for IHVPE existing in the state/year of the child's birth. Standard errors are in parentheses. The outcomes are: (1) mother is in the CSS sample; (2) father involvement index; (3) father made any CS payments in the last year; (4) father made all CS payments in the last year; (5) father paid on time all or most of the time in the last year; (6) father has court-ordered visitation rights; (7) number of days father spent with the child in the last year; (8) father provided gifts for the child; (9) father provided clothes for the child; (10) father provided food for the child; (11) father covered childcare expenses for the child; (12) father paid for medical expenses for the child. The second to last row reports the pre-IHVPE mean of the dependent variable in each column. 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 and who are not married to their youngest children's biological fathers. Panel A reports results on selection into the CSS sample using the whole CPS-CSS sample. Panel B reports results for the CSS sample, not accounting for selection and weighted with the CPS person weights. Panel C reports results for the CSS sample, not accounting for selection and not weighted. Panel D reports results for the CSS sample, which is re-weighted using inverse propensity scores from a logit model that regresses a dummy for treatment (IHVPE = 1) on all of the maternal and child characteristics described in the notes to Table 3. Panels E and F report results from two trimmed samples, constructed following the Lee (2009) bounding procedure. Please see online Appendix D for more details. 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. Robust standard errors are clustered on the state level.

Next, I estimate regressions that are weighted by inverse treatment propensity scores. Specifically, I first estimate a logit model, regressing a dummy for treatment ($IHVPE = 1$) on all of the maternal and child characteristics contained in X_{isty} in equation (2) in the CSS sample only. The predicted value from this model is the treatment propensity score, p, which is then normalized to add to one. I weight observations with ($IHVPE = 1$) by $1/p$, and observations with ($IHVPE = 0$) by $1/(1-p)$. The idea behind this weighting is that it creates a CSS sample that is more balanced pre-IHVPE and post-IHVPE on observable characteristics. Online Appendix Table A13 confirms that this strategy accomplishes this goal. Columns 1 and 2 in online Appendix Table A13 report the means of maternal and child characteristics in the raw unweighted CSS sample for pre-IHVPE and post-IHVPE observations, respectively. Post-IHVPE mothers are less likely to be aged 25–34 at childbirth (relative to the other age groups), are more educated, and are more likely to be Hispanic and less likely to be non-Hispanic white or black. Column 3 shows that the standardized differences for some of these characteristics are not negligible. Columns 4 and 5 report the means for the two groups, weighted by the inverse treatment propensity scores. After weighting, the standardized differences in these characteristics are much smaller, accounting for at most 4 percent of a standard deviation.

Panel D of Table 6 reports results for the father involvement outcomes. Interestingly, they are quite similar to the results in panels B and C of Table 6 that do not account for selection, suggesting that some of the improvements in these outcomes may be driven by the behavior of fathers who would have never been married rather than by pure selection.

Although the analysis in panel D of Table 6 accounts for nonrandom IHVPE-driven selection out of marriage based on observable characteristics, it cannot control for selection on unobservable variables. To this end, I construct upper and lower bounds on the estimated effect sizes, which account for selection on both observable and unobservable margins. 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 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), while 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). Online Appendix D provides further details. The results from this bounding exercise are presented in panels E and F of Table 6. 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.

To study effects on child resources and child mental and physical health, I turn to the March CPS and NHIS data and limit my analysis to mothers who are not married to their youngest children's fathers. I proceed in the same way as in the analysis of father involvement just described. Table 7 presents the results. Most of the coefficients are not statistically significant, and the bounds in panels D and E of Table 7 contain a wide range of estimates. As such, it is difficult to draw definitive conclusions on whether IHVPE leads to improved resources for children living with single mothers. Similarly, there are no significant changes in these children's physical or mental health, or access to care.

E. *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

Table 7—IHVPE and Child Well-Being among Families with Unmarried Parents

	Child resources (March CPS)						Child health (NHIS)			
	Index (1)	Inc. (2)	> Pov. (3)	> 1.5 × Pov. (4)	No welf. (5)	Welf. inc. (6)	Phys. (7)	Ment. (8)	Learn. (9)	Doc. (10)
Panel A. Unmarried sample, not accounting for selection, weighted with sample weights										
IHVPE	0.0056	0.0360	0.0081	0.0134	−0.0138	1.4954	−0.0232	0.1215	−0.0244	−0.0184
	(0.0223)	(0.0401)	(0.0115)	(0.0074)	(0.0127)	(88.7348)	(0.0248)	(0.1647)	(0.0150)	(0.0157)
Panel B. Unmarried sample, not accounting for selection, unweighted										
IHVPE	0.0055	0.0173	0.0064	0.0106	−0.0081	−19.2759	−0.0011	−0.0577	−0.0159	−0.0076
	(0.0223)	(0.0324)	(0.0117)	(0.0085)	(0.0117)	(89.8508)	(0.0134)	(0.1395)	(0.0095)	(0.0150)
Panel C. Unmarried sample, re-weighted using treatment propensity scores										
IHVPE	0.0160	0.0212	0.0097	0.0134	−0.0024	−54.7077	0.0151	−0.0142	−0.0139	−0.0029
	(0.0239)	(0.0325)	(0.0129)	(0.0091)	(0.0119)	(96.4239)	(0.0125)	(0.1463)	(0.0095)	(0.0181)
Panel D. Unmarried sample, Lee lower bound										
IHVPE	0.0045	0.0328	0.0023	0.0036	−0.0179	−573.0808	−0.0432	−0.0817	−0.0320	−0.0214
	(0.0222)	(0.0402)	(0.0116)	(0.0077)	(0.0127)	(109.7181)	(0.0244)	(0.1382)	(0.0132)	(0.0160)
Panel E. Unmarried sample, Lee upper bound										
IHVPE	0.1503	0.1646	0.0331	0.0254	0.0526	18.7444	0.0380	0.1327	−0.0236	0.0042
	(0.0255)	(0.0416)	(0.0120)	(0.0075)	(0.0138)	(90.1538)	(0.0262)	(0.1679)	(0.0152)	(0.0147)
Mean	−0.678	9.602	0.508	0.363	0.648	2,027.243	−0.0837	1.5086	0.0548	0.8298
N	46,176	44,018	46,176	46,176	46,176	46,176	—	—	—	—

Notes: Each column contains a different dependent variable. Columns 1–6 use data from the March CPS, while columns 7–10 use data from the NHIS. Each coefficient is from a separate regression, where the key coefficient of interest is an indicator for IHVPE existing in the state/year of the child's birth. Standard errors are in parentheses. The outcomes are: (1) child resource index; (2) log after-tax family income; (3) household is above poverty line; (4) household is above 1.5 × poverty line; (5) household is not receiving welfare; (6) total welfare income received (including zeros); (7) physical health index; (8) mental problems index (higher value = more problems); (9) any learning disability (age 3+ only); (10) any doctor visits in the past 12 months. The second to last row reports the pre-IHVPE mean of the dependent variable in each column. The samples are limited to mothers who are not married to their children's fathers. All income variables are in 2010 dollars. After-tax family income is calculated using the TAXSIM program from the NBER. 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. Please see the notes to Table 6 for details on the estimation procedures in each of the panels. Robust standard errors are clustered on the state level.

to her child's father, mother is never married, mother is married to a new partner, mother's spouse has any own wage income, 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, child had any doctor visits in past 12 months, and the father involvement index).[49] 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 "partial treatment" group in which some children who are affected by IHVPE may be erroneously assigned to the pretreatment 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*

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

Table 8—Robustness Tests for Main Outcomes (1)

Dep. var.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Pat./ unmar bir.	0.2100 (0.0601)	0.2922 (0.0762)	0.1737 (0.0781)	0.2100 (0.0676)	0.2136 (0.0782)	0.2183 (0.0665)	0.2227 (0.0692)	—	0.2167 (0.0645)	0.2119 (0.0642)	0.2310 (0.0640)
Observations	544	467	544	544	349	336	544		530	530	530
Mom married to child's dad	−0.0281 (0.0088)	−0.0328 (0.0134)	−0.0252 (0.0083)	−0.0277 (0.0087)	−0.0308 (0.0090)	−0.0349 (0.0092)	−0.0322 (0.0100)	−0.0253 (0.0094)	−0.0270 (0.0090)	−0.0289 (0.0092)	−0.0274 (0.0104)
Observations	36,241	31,809	36,241	36,241	32,968	26,148	36,241	31,905	32,552	33,909	33,830
Mom never married	0.0177 (0.0060)	0.0136 (0.0101)	0.0106 (0.0076)	0.0151 (0.0066)	0.0190 (0.0060)	0.0224 (0.0063)	0.0234 (0.0069)	0.0162 (0.0063)	0.0164 (0.0058)	0.0168 (0.0062)	0.0168 (0.0071)
Observations	36,241	31,809	36,241	36,241	32,968	26,148	36,241	31,905	32,552	33,909	33,830
Mom married to new partner	0.0060 (0.0027)	0.0058 (0.0041)	0.0054 (0.0030)	0.0060 (0.0031)	0.0066 (0.0028)	0.0073 (0.0030)	0.0066 (0.0029)	0.0054 (0.0025)	0.0058 (0.0031)	0.0063 (0.0029)	0.0040 (0.0024)
Observations	36,241	31,809	36,241	36,241	32,968	26,148	36,241	31,905	32,552	33,909	33,830
Mom's spouse has wage inc.	0.0100 (0.0047)	0.0047 (0.0069)	0.0111 (0.0047)	0.0116 (0.0049)	0.0085 (0.0048)	0.0065 (0.0056)	0.0069 (0.0051)	0.0089 (0.0048)	0.0083 (0.0053)	0.0104 (0.0050)	0.0117 (0.0049)
Observations	140,487	128,101	140,487	140,487	113,697	102,689	140,487	132,140	124,098	132,076	130,566
Child has priv. insur.	−0.0263 (0.0101)	−0.0375 (0.0141)	−0.0299 (0.0100)	−0.0290 (0.0108)	−0.0255 (0.0098)	−0.0370 (0.0093)	−0.0273 (0.0086)	−0.0234 (0.0121)	−0.0294 (0.0098)	−0.0250 (0.0100)	−0.0190 (0.0098)
Observations	36,241	31,809	36,241	36,241	32,968	26,148	36,241	31,905	32,552	33,909	33,830

Notes: Each row contains a different dependent variable. Each coefficient is from a separate regression. Standard errors are in parentheses. Column 1 shows the main results presented in the above tables for each of the outcomes. Column 2 omits the "partial treatment" group years ($\textit{child's birth year} - \textit{IHVPE first year} \in \{-1, 0\}$). Column 3 assigns treatment based only on the year listed in online Appendix Table A1 (i.e., ignoring the month). Column 4 uses the fraction of the year that IHVPE existed as the treatment variable based on information on the month of initiation (i.e., if the program was initiated in June 1996, then it existed for $\frac{7}{12}$ of 1996). For states where only the year of IHVPE initiation is known, this variable takes the value of one for all years starting from the year of initiation. Column 5 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 6 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 7 includes quadratic state-specific time trends. Column 8 limits individual-level analyses to samples without black mothers. Columns 9–11 omit observations from California, New York, and Texas, respectively. Please refer to Tables 3, 4, and 6 for details about the samples 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 linear time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.

$\textit{year} - \textit{IHVPE first year} = 0$). I address the concerns about measurement error in assignment of treatment in several ways.

First, column 2 in Tables 8 and 9 omits the "partial 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. Second, in column 3, I assign IHVPE treatment in an alternative way, only using the information on the year listed in online Appendix Table A1 (i.e., ignoring the month). The coefficient for paternity establishment rates is somewhat reduced in magnitude, but is still positive and statistically significant. The coefficients for other outcomes are quite similar to the main results. Third, in column 4, I use the fraction of the year that IHVPE existed as the treatment variable based on information on the

Table 9—Robustness Tests for Main Outcomes (2)

Dep. var.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Mother worked any usual hrs. last yr.	0.0175	0.0274	0.0228	0.0209	0.0176	0.0204	0.0231	0.0167	0.0215	0.0155	0.0116
	(0.0074)	(0.0098)	(0.0069)	(0.0076)	(0.0079)	(0.0084)	(0.0090)	(0.0074)	(0.0077)	(0.0075)	(0.0068)
Observations	184,562	168,186	184,562	184,562	148,974	134,963	184,562	163,580	163,520	172,791	171,609
Child resource index	0.0004	0.0066	0.0021	0.0020	0.0031	−0.0047	−0.0043	−0.0012	0.0067	−0.0020	−0.0042
	(0.0087)	(0.0120)	(0.0089)	(0.0094)	(0.0091)	(0.0106)	(0.0082)	(0.0080)	(0.0073)	(0.0086)	(0.0094)
Observations	184,562	168,186	184,562	184,562	148,974	134,963	184,562	163,580	163,520	172,791	171,609
Physical health index	−0.0154	−0.0281	−0.0327	−0.0251	−0.0127	—	−0.0176	−0.0187	—	—	—
	(0.0119)	(0.0128)	(0.0101)	(0.0118)	(0.0118)	—	(0.0125)	(0.0111)	—	—	—
Observations	—	—	—	—	—	—	—	—	—	—	—
Mental problems index	0.0884	0.0835	0.0114	0.0286	0.1054	—	0.1210	0.0729	—	—	—
	(0.0840)	(0.1488)	(0.0907)	(0.0786)	(0.0924)	—	(0.0997)	(0.0847)	—	—	—
Observations	—	—	—	—	—	—	—	—	—	—	—
Any learn. disability (age 3+ only)	−0.0069	−0.0030	−0.0059	−0.0056	−0.0070	—	−0.0066	−0.0072	—	—	—
	(0.0066)	(0.0086)	(0.0059)	(0.0064)	(0.0066)	—	(0.0076)	(0.0063)	—	—	—
Observations	—	—	—	—	—	—	—	—	—	—	—
Any doctor visits (past 12 months)	−0.0136	−0.0070	−0.0188	−0.0167	−0.0108	—	−0.0116	−0.0092	—	—	—
	(0.0068)	(0.0156)	(0.0082)	(0.0078)	(0.0069)	—	(0.0071)	(0.0068)	—	—	—
Observations	—	—	—	—	—	—	—	—	—	—	—
Father involve. index (unmar. only)	0.0729	0.0410	0.0699	0.0782	0.0823	0.0755	0.0743	0.1058	0.0697	0.0677	0.0507
	(0.0468)	(0.0474)	(0.0456)	(0.0461)	(0.0467)	(0.0592)	(0.0501)	(0.0454)	(0.0505)	(0.0476)	(0.0485)
Observations	7,082	6,161	7,082	7,082	6,525	5,104	7,082	4,799	6,417	6,542	6,623

Notes: Each row contains a different dependent variable. Each coefficient is from a separate regression. Standard errors are in parentheses. Column 1 shows the main results presented in the above tables for each of the outcomes. Column 2 omits the "partial treatment" group years ($\textit{child's birth year} - \textit{IHVPE first year} \in \{-1, 0\}$). Column 3 assigns treatment based only on the year listed in online Appendix Table A1 (i.e., ignoring the month). Column 4 uses the fraction of the year that IHVPE existed as the treatment variable based on information on the month of initiation (i.e., if the program was initiated in June 1996, then it existed for $\frac{7}{12}$ of 1996). For states where only the year of IHVPE initiation is known, this variable takes the value of one for all years starting from the year of initiation. Column 5 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 6 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 7 includes quadratic state-specific time trends. Column 8 limits individual-level analyses to samples without black mothers. Columns 9–11 omit observations from California, New York, and Texas, respectively. 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.

month of initiation (i.e., if the program was initiated in June 1996, then it existed for $\frac{7}{12}$ of 1996). For states where only the year of IHVPE initiation is known, this variable takes the value of one for all years starting from the year of initiation. Again, the results are largely unchanged.

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 posttreatment 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 5 in Tables 8 and 9 limits the samples to narrower windows surrounding IHVPE implementation: the paternities data are limited to years 1992–2000 (one year before and after the 1993–1999 IHVPE rollout), while the individual-level data are limited to mothers of children born in 1988–2004 (five years before and after the 1993–1999 IHVPE rollout). The coefficients from these regressions are similar to the main results.

Column 6 in Tables 8 and 9 addresses concerns about measurement error in treatment status in yet another way—by restricting analysis to the 27 states for which I have the most accurate information on IHVPE implementation.[50] These results also show coefficients that are comparable to those in column 1. In fact, the absolute magnitudes of the coefficients in the "good info" states are mostly larger than those in the baseline results, suggesting that there is less measurement error in this sample. Moreover, the corresponding event-study figures for paternity establishment rates and parental marriage are very similar to the main figures (see online Appendix Figures A5 and A6, respectively).

Column 7 in Tables 8 and 9 presents coefficients from regressions controlling for quadratic state-specific time trends and shows that the results are robust to their inclusion. In column 8, 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 9–11, 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.[51]

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.

VI. Conclusion

As more than one-third of all children in the United States 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

[50] 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 (Arizona, California, Colorado, Connecticut, Delaware, District of Columbia, Florida, Idaho, Illinois, Indiana, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, New Jersey, New York, Oregon, Pennsylvania, Rhode Island, Tennessee, Texas, Washington, and Wisconsin) consist of four groups: states for which I got data from Garfinkel and Nepomnyaschy (2007); states where I was able to speak directly to a representative from the child support office or from the IHVPE office; states where the year and month of program implementation was listed on a website about the program; and 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.

[51] Results from regressions excluding states other than California, New York, and Texas are also similar and available upon request.

important feature of such family 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.

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 United States, 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.

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