

# Did the legalization of abortion increase women's household bargaining power? Evidence from labor supply

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Received: 31 July 2006 / Accepted: 17 January 2007 / Published online: 20 March 2007  
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**Abstract** I estimate the impact of abortion legalization on spouses' labor supplies to test whether legalization increased women's household bargaining power, in a collective household behavior framework. Based on CPS data, I find that wives' labor supply decreased and their husbands' increased, which is consistent with the bargaining hypothesis. This contrasts with most studies of abortion and birth control technologies, which predict a labor supply effect only for women, and of opposite sign. Also consistent with the bargaining interpretation, I estimate no significant impact on anti-abortion religious couples or on those who regularly used contraceptives. PSID data yield supportive evidence.

**Keywords** Legalization of abortion · Household bargaining power · Labor supply

**JEL Classifications** D1 · J22

## 1 Introduction

This paper examines the effect of abortion legalization on household labor supply. Most abortion and birth control technology studies have focused on substitution effects through decreased fertility (e.g., Levitt & Donohue, 2001; Levine, Staiger, Kane, & Zimmerman, 1999), including increased human capital accumulation, wages, and labor supply of women (Angrist & Evans, 1999; Goldin & Katz, 2002). However, such a shift in spouses' fertility decision rights may also have generated an income effect, by affecting the balance of power within the couple, thus altering

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the allocation of resources within households<sup>1</sup> in which the wife is fertile. In particular, according to models of collective household behavior, if abortion legalization increased women's bargaining power within the household, one would expect a reduction in wives' labor supply and an increase in husbands' labor supply. The fact that the predicted effect on female labor supply differs starkly from the one shown in the literature, and the presence of an additional impact on husbands, offer an ideal opportunity to examine the bargaining power effect of abortion legalization.

The effects of abortion legalization are identified using state law changes in California, New York, Washington, Alaska, and Hawaii in 1970 and the change in federal law in 1973. Although 12 other states legalized abortion between 1967 and 1970, abortion was permitted only under restrictive circumstances such as danger to the life of the mother and rape (Merz, Jackson, & Klerman, 1996). I follow convention and ignore legalization in these states.

My identification strategy consists of estimating the effects of legalization on husbands' and wives' labor supplies for households that were married prior to the onset of legalization. Prior to *Roe v. Wade*, the effects of legalization are identified by comparing changes in the labor supply behavior of households in states that legalized abortion with the changes in labor supply of households in other states. Additional identification occurs with the passage of *Roe v. Wade*, which affected only households in states that had not yet legalized. The bulk of the empirical work in this paper examines changes in labor supplies of married couples using data from the March and June Supplements of the Current Population Survey (CPS) over the period 1968–1979. I also use data from the Panel Study of Income Dynamics (PSID) as a robustness check.

In principle, it is possible to identify relative changes in intrahousehold bargaining power by comparing labor supplies in households married prior to and households married after the onset of legalization. However, such a comparison runs into tricky selection issues. Selection issues, while not entirely avoidable, are considerably mitigated when investigating changes in behavior in households that were formed prior to legalization.

To foreshadow the results, abortion legalization significantly decreased the labor supply of married women in their fertile age by 83 annual hours and significantly increased their husbands' labor supply by 34 annual hours. In addition, the theory provides a number of other predictions. First, abortion legalization should have no effect on couples with strongly held religious beliefs against abortion. Second, abortion legalization should have little impact on couples who regularly use contraceptives. Third, to the extent that households from the upper part of the income distribution would have been able to obtain abortions more readily even when they were illegal, the effects of legalization should be less evident. The empirical evidence is consistent with these predictions.

A number of alternative explanations are considered. There may have been a reduction in the demand for women's labor in states and at times during which abortion was legal. There could have been an increase in the availability of

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<sup>1</sup> Neither I, the CPS, nor the PSID in 1970s differentiate between legally married and cohabitant households.

contraception, in female long-term wages and labor market opportunities relative to men. Divorce laws may also have increased women's bargaining power or divorce rates may have been affected by abortion legalization. Increased generosity in welfare programs would also have increased the bargaining power of lower income married women, by enhancing the value of single motherhood, or decreased their labor supply by creating disincentives to work. I argue that these phenomena cannot consistently explain my results, given their time and state patterns, my intra-household bargaining predictions, and empirical evidence.

The paper is organized as follows. Section 2 describes the theoretical framework. Section 3 describes the empirical specification and data. Section 4 presents the empirical results. Section 5 considers alternative explanations for the findings. Section 6 concludes the paper.

## 2 Theoretical framework

### 2.1 Background

Abortion legalization clearly enhanced women's labor market options by enhancing their opportunities to control childbearing. For example, Goldin and Katz (2002) presented evidence that the widespread availability of oral contraception led to significant increases in female human capital accumulation. Heer and Grossbard-Shechtman (1981) considered the contraceptive revolution to be a factor leading to a rise in the proportion of married women at work. Using Census microdata, Angrist and Evans (1999) found that state-level abortion legalizations led to increased schooling and employment rates among young black women. The results of these studies suggest that abortion legalization should have had, if anything, positive effects on women's labor supply.

However, recent innovations in the theory of collective household behavior suggest another channel through which abortion legalization could have operated. In particular, the model of Chiappori, Fortin, and Lacroix (2002), outlined below, suggests that if abortion legalization increased women's bargaining power in the household, it would have exerted a negative impact, through an income effect, on wives' labor supplies and a positive impact on husbands' labor supplies.

It is beyond the scope of this paper to demonstrate theoretically that abortion legalization enhanced married women's bargaining power. However, Demand and Supply marriage models developed by Grossbard-Shechtman (1984, 1993) and Grossbard (2005) show that marriage market factors external to the couple and to the spouses' own characteristics, such as laws, customs, and government programs, influence men's and women's relative welfares and also affect their labor market decisions. Grossbard-Shechtman (1993) states that laws can affect the "compensations for spousal labor," and "the higher (a woman's) total compensation for spousal labor, the more a married woman is likely to have power in household decision-making." In particular, Grossbard (2005), in her analysis of welfare dependency, women's labor supply, and marriage, states that reproductive

technology is a factor that influences women's "marital reservation wage" and thus, their bargaining power and relative gains from marriage, given that "the production of children is a major aspect of marital production."<sup>2</sup>

Recently, Chiappori and Oreffice (2006) developed an equilibrium analysis of the effects of birth control technologies on female empowerment. They model a frictionless marriage market in which men are identical while women have heterogeneous tastes for children, who reduce the mother's ability to earn income. The availability of a new birth control technology raises the utility of being single. If women are not in excessively large supply, this will lead to an increase in the utility of the marginal woman (who is indifferent between getting married and remaining single), and therefore, in the utility of all women 'above' the marginal one, also of those who want children.<sup>3</sup> Because married women have the option of seeking a divorce, abortion legalization should enhance the bargaining power of women already married—the focus of the empirical work in this paper—to the extent that it enhances their opportunities outside the marriage. It is widely acknowledged in the literature that married people are responsive to changing outside factors, which can lead to income and bargaining power redistribution between the spouses and new marital labor relations (Chiappori et al., 2002; Grossbard-Shechtman, 1984; Lundberg & Pollak, 1996).

Additionally, sociological and gender studies argue that abortion rights reduced the degree of male domination within families (e.g., Héritier, 2002). The developmental and sociology literature asserts that women would have fewer children if they could better control their fertility. The Allan Guttmacher Institute reports that about 20% of women undergoing abortions are legally married, an additional 8% cohabit, and the total annual number of abortions reached one million in 1975, even though the contraceptive pill and other female contraceptives were readily available by the time of abortion legalization. There is also evidence that a substantial proportion of women having abortions may not have used a contraceptive method in the month they conceived mainly because of a perceived low risk of pregnancy (Jones, Darroch, & Henshaw, 2002), which shows that abortion can represent an opportunity for women not using contraception. Although there is no way to determine whether these abortions are favored by the husbands or not, the data suggest that since legalization abortion has been a viable option that can represent a credible threat to the husband.

## 2.2 A simple model

I extend the collective household bargaining model of Chiappori et al. (2002) to a multiperiod setting (see also Mazzocco, 2007). A household is composed of two decision makers, husband and wife, each having a distinct utility function on consumption and leisure. I assume that households make Pareto-efficient decisions

<sup>2</sup> In the same Demand and Supply marriage market framework, Choo and Siow (2006) also state that abortion legalization affects gains to marriage.

<sup>3</sup> Birth control technology innovations may benefit some women, and none are made worse off, even when men are in excess supply.

about leisure and consumption of husband and wife, who is assumed to be fertile. Preferences are egoistic in that the welfare of each spouse does not depend on consumption or leisure of the other household member, although the model can be extended to caring preferences. Let  $h_t^i$  and  $C_t^i$  for  $i = f, m$ , denote member  $i$ 's labor supply and consumption of a private composite good (whose price is normalized to unity) in each period  $t \in \{0, \dots, T\}$ . The utility function of member  $i$  is  $U^i(1 - h_t^i, C_t^i)$ , where  $U$  is strictly quasi-concave, increasing, and continuously differentiable for  $i = f, m$ . For simplicity, I abstract from the non-pecuniary benefits of companionship, an inessential detail provided that the probability distribution of these benefits in the population is not affected by abortion legalization.<sup>4</sup>

Let  $y_t$  denote household non-labor income and  $w_t^i$  the wage rate of spouse  $i$ . To reduce complexity, households are assumed to be unable to either lend or borrow, and there is no uncertainty. Denote the discount factors of the female and male to be constant and equal to  $\beta^f$  and  $\beta^m$ , respectively. The optimal allocation functions,  $C^i(\cdot)$  and  $h^i(\cdot)$ , are assumed to be stationary and do not depend on past realizations of non-labor income or wages.

The key result can be conveniently demonstrated solving the model for the optimal sharing rule,  $\varphi_t(s)$ , which is Pareto-optimally chosen by the spouses at decision time  $s \in \{0, \dots, T\}$  and depends on the balance of bargaining power at that specific time. In particular,  $\varphi_t(s)$  represents the wife's share of non-labor income  $y_t$  (the husband receives  $y_t - \varphi_t(s)$ ) over the life of the marriage  $t \in \{s, \dots, T\}$ : the stronger her bargaining power, the higher her share of non-labor income and the lower her husband's (Chiappori, 1992). The sharing rule,  $\varphi_t(s)$ , is in general a function of prices (here normalized to unity), spouses' wages, non-labor incomes, and so-called distribution factors that affect spousal opportunities, and thus, bargaining power, but do not influence individual preferences or consumption possibilities. Abortion legality is the distribution factor at stake in this analysis. The sex ratio, divorce, alimony, child benefits laws, and spousal share of non-labor income are examples of distribution factors that have been studied in the literature (see Vermeulen, 2002 for a survey). For notational simplicity and to highlight the effects of abortion legalization, I do not include all those explanatory variables in the functional form of the sharing rule.

The optimal allocations of labor supply of each spouse are determined by the following program. At decision time  $s$ , the woman faces the following problem:

$$\begin{aligned} & \max_{\{h_t^f, C_t^f\}_{t=s, \dots, T}} \sum_{t=s}^T \beta^f U^f(1 - h_t^f, C_t^f) \\ & \text{subject to} \\ & C_t^f \leq \varphi_t(s) + w_t^f h_t^f \quad \forall s, t. \end{aligned}$$

<sup>4</sup> Similarly, it is assumed that abortion legalization does not affect the length of marriage  $T$ . There is no evidence of a correlation between abortion legalization and increases in divorce rates (see Subsect. 5.3).

The male of the household faces a symmetric problem. Solving these maximization problems yields the following equilibrium labor supply functions for the spouses:<sup>5</sup>

$$\begin{aligned} h_t^f &= h^f[w_t^f, \varphi_t(s)] \\ h_t^m &= h^m[w_t^m, y_t - \varphi_t(s)]. \end{aligned}$$

The derivatives of each labor supply function with respect to the second arguments are unambiguously negative, reflecting a pure income effect. Hence, factors that strengthen women's bargaining power reduce the labor supplied by the wife and increase the labor supplied by the husband, *ceteris paribus*. I investigate whether abortion legalization is such a factor, by testing its impact on spouses' labor supplies. The theoretical and anecdotal evidence on abortion discussed in the previous sub-section is suggestive of a bargaining power effect of legalization.

### 2.2.1 Effect of changes in abortion laws on labor supply

Spouses decide on the sharing rule and optimal allocations of consumption and leisure at the start of the marriage at time  $s = 0$ . Unanticipated changes in outside opportunities that affect the balance of bargaining power will lead to renegotiation in a world in which spouses cannot fully commit to a given course of action over the entire marriage. Admittedly, casual observation and introspection suggests that this assumption is not unrealistic (see also Mazzocco, 2007), but in any case, failure of this assumption works against any positive findings.

If, then, abortion is legalized unexpectedly at some time  $s = l$  (with  $l$  corresponding to the period in which abortion is legalized), the spouses will renegotiate a new sharing rule,  $\{\varphi_t(l)\}$ , where  $\{\varphi_t(l)\} > \{\varphi_t(0)\}$ ,  $\forall t \geq l$ . The wife's increased bargaining power leads to her receiving a greater share of the household's non-labor income, which leads to a reduction in her labor supply and an increase in her husband's labor supply, due to the income effect. The following conditions summarize those effects:

$$\begin{aligned} \{\varphi_t(l)\} > \{\varphi_t(0)\} &\Rightarrow h_{l+k}^f < h_{l-j}^f, \quad \forall k \geq 0, \forall j > 0 \\ \{\varphi_t(l)\} > \{\varphi_t(0)\} &\Rightarrow h_{l+k}^m > h_{l-j}^m, \quad \forall k \geq 0, \forall j > 0. \end{aligned}$$

In principle, abortion legalization could exert a substitution effect and an income effect through higher female wages, which could result from greater levels of formal schooling or more continuous labor force participation. Provided that the effects of legalization are captured in the current wage—included in the model above and the empirical work below—the bargaining power effects of legalization conditioned on

<sup>5</sup> Chiappori et al. (2002) and Vermeulen (2002) report that in a collective model with egoistic preferences those labor supply functions allow identification of the collective model provided that the sharing rule depends on at least the spouses' wages and one distribution factor. My collective labor supply model outlined above fulfills those characteristics and is identifiable.

the current wage should be entirely through the income effect. To the extent that they are not, it is even less likely that one should find a negative effect of legalization on wives' labor supply.

*Effect on single individuals.* Young single women may experience an expected gain, and single men an expected loss, if they plan to marry in the future. Thus, they may be exposed to the change in intra-household bargaining power. Demand and Supply marriage models provide a useful framework to analyze young singles' decisions about labor and marriage. Grossbard-Shechtman (1984, 1993) and Grossbard (2005) report that expected "marital reservation wages" (i.e., their future intra-household bargaining power) are found to affect the labor supply of single women. In particular, she argues that marriage market factors, such as reproductive technologies, affect single women's decisions about marriage and their marital reservation wage. If abortion legalization allows women to avoid unwanted pregnancies and relationships with undesirable men, they can obtain a higher marital reservation wage by marrying better quality mates and bargaining for a more favorable intra-household allocation of resources. Young single women, many of whom plan to get married, would respond to this expected future bargaining power shift by decreasing their labor supply.<sup>6</sup> In the long run, it is true that those young single women have been largely affected by legalization through the positive substitution effect of increased human capital accumulation, labor market opportunities and future wages, which should increase female labor supply, contrary to the income effect I am estimating (Angrist & Evans, 1999; Goldin & Katz, 2002). However, the drop in expected marital gains for men and rise for women immediately following legalization could decrease their labor supply, if such a bargaining effect is strong enough.

### 2.2.2 Other predictions

The effect of abortion legalization will depend on the extent that it affects the bargaining power of the wife, which will not necessarily be uniform across households.

*Role of religion.* Some religious denominations strongly oppose abortion. For example, Catholic doctrine holds that abortion is mortal sin, that it "directly violates the divine commandment 'you shall not kill'... [and is] an attack against human life". To take another example, the Baptist Church "oppose[s] abortion as a means to avoid responsibility for conception." Other denominations have very different views. For instance, Methodist doctrine emphasizes "the sacredness of the life and well-being of the mother, for whom devastating damage may result from an unacceptable pregnancy" (United Methodists Social Justice Stands). Catholic and Baptist women, particularly those living in households in which the husband is of the same faith, are unlikely to even consider, much less credibly threaten their

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<sup>6</sup> Grossbard-Shechtman (1984) states that, "single persons looking for marriage partners respond to market forces and decide accordingly whether to marry and how much work to invest and employ in the framework of marriage."

spouse with obtaining an abortion. Hence, one should expect to find much smaller, if any, estimated effects of abortion legalization on the labor supply of religious households.<sup>7</sup>

*Use of contraceptives.* In households that regularly use contraceptives, abortion legalization does not represent an actual option to control fertility or a change in outside opportunities for wives. Therefore, one does not expect any effect of legalization in such households.

*Role of availability of abortion.* The estimated impact of legalization should be particularly strong for women with greater access to lower cost abortion services after legalization and weak for those with better access prior to legalization. Unfortunately, it is difficult to find good measures of availability, particularly prior to legalization. Wealthier households should have had better access to abortion services prior to legalization; therefore, legalization should have had a smaller impact on labor supply in wealthier households. I measured the availability of abortion services after legalization using information on annual abortion rates in a state.

These predictions will be tested in the data. The negative effect on female labor supply predicted by the bargaining model is opposed to the traditionally emphasized effect. Moreover, traditional analyses make no prediction regarding spouses.

### 2.3 Abortion laws: expectations and endogeneity

Changes in abortion laws should have had no effect on the sharing rule to the extent that they were anticipated at the time of marriage. It seems unlikely that the eventual legalization could have affected women's bargaining power during the late 1960s or early 1970s. First, bargaining power is influenced by whether abortion is legal and accessible, not by the fact that in those years abortion started to be debated in society. Second, the liberalization of abortion was hardly uniform across states or over time. *Roe v. Wade*, filed in March 1970 in Texas, argued in the U.S. Supreme Court in 1971, re-argued in 1972, was “suddenly extended to the entire United States” in January 1973 (Levitt & Donohue, 2001, p. 384, emphasis added). A good case can be made that *Roe v. Wade* even reaching the Supreme Court in 1971 was unexpected. Although challenges to abortion laws were mounted since the mid 1960s, in the early 1970s, a number of state supreme courts upheld the existing restrictive abortion laws, and most such cases were not heard in higher courts.<sup>8</sup> A similar case can be made that state legalizations in 1970 were unexpected as well.<sup>9</sup>

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<sup>7</sup> I am implicitly assuming that there is assortative matching along the religious dimension (i.e., those religious couples are likely to belong to a separate marriage market). The predicted effects would be more complex if people in the same marriage market differed on their religious views on abortion (Chiappori & Oreffice, 2006).

<sup>8</sup> Examples include *Steinberg v. Brown* (Ohio 1970), *Babbitz v. McCann* (Wisconsin 1970), *Doe v. Rampton* (Utah 1971), *Cheney v. State* (Indiana 1972). For example, the Indiana Supreme Court upheld Indiana's restrictive pre-Roe abortion law, which allowed abortion only to protect the life of the mother, just months before national legalization.

<sup>9</sup> For instance, Glendon (1987) reports that in New York, “both opponents and supporters were surprised...when the repeal bill passed.”



Concern arises that some factor outside the model could have changed, affecting both the relative bargaining power of women and the likelihood that abortion was legalized. It is almost inconceivable that national legalization, enacted not by the Congress, but by the Supreme Court, was anything but exogenous. All but two of the justices had been appointed before 1970, some as early as the 1950s. Moreover, of the two justices appointed in 1971 by Nixon, one dissented. A similar argument holds for legalization in California, which was decided by the state's Supreme Court.

Endogeneity is arguably of more concern in the cases of New York, Alaska, Hawaii, and Washington, all of whom legalized abortion legislatively in 1970.<sup>10</sup> In particular, an exogenous increase in the bargaining power of women living in these states could also have led politicians in those states to legalize abortion. However, a general increase in women's bargaining power should have affected the behavior not only of married women of fertile age, but of women in general, including those with strong religious beliefs, women who used contraception, or women who had high levels of wealth. The contrasts between these groups are therefore crucial.

### 3 Empirical specification and data

#### 3.1 Identification strategy

I focus on couples married prior to legalization.<sup>11</sup> According to theory, if abortion legality enhances women's bargaining power in the household, the labor supply of fertile wives should decline, and the labor supply of their husbands should rise. Older couples should experience a negligible impact on their labor supplies. I also consider singles. Young single women may experience an expected gain, and single men an expected loss, if they plan to marry in the future and be exposed to the change in intra-household bargaining power. In particular, young single women, many of whom plan to get married, would respond to legalization by decreasing their labor supply if the increase in expected marital gains is strong enough.

I exclude widowed and separated couples to keep a clear distinction between multiple and one decision maker households. I also exclude the few older singles above 40 years old, as they are not relevant to this analysis. Finally, I do not include households that have other income earners besides the husband and wife, the focus being on spousal bargaining power. I include intact couples who spend at least 2 consecutive years in the sample as married, but only if the wife is actually present.

The following equations for labor supply were estimated separately for wives and husbands:<sup>12</sup>

<sup>10</sup> Washington legalized abortion in a referendum (with 56.6% of the vote).

<sup>11</sup> Cohabitants are classified as married in both datasets in the period under consideration.

<sup>12</sup> I also used joint estimation to account for a possible correlation in the spouses' error terms, which yielded the same results at a similar significance level.

**Table 1** Year of abortion legalization across states

California, New York, Washington, Alaska, Hawaii	1970
All other states	1973

Although 12 other states legalized abortion between 1967 and 1970, abortion was permitted only under restrictive circumstances such as danger to the life of the mother and rape. I follow convention and ignore legalization in these states

$$h_t^f = h_t^f(\ln w_t^f, \ln w_t^m, y_t, X_t, LEGAL_t) + \varepsilon_t^f$$

$$h_t^m = h_t^m(\ln w_t^f, \ln w_t^m, y_t, X_t, LEGAL_t) + \varepsilon_t^m$$

I have also estimated a corresponding labor supply equation for unmarried women and men, using the same specification (without spousal variables):

$$h_t^u = h_t^u(\ln w_t^u, y_t, X_t^u, LEGAL_t) + \varepsilon_t^u.$$

LEGAL is a dummy variable for whether abortion is legal. LEGAL takes a value of unity in 1970 and on for households located in any of the five states that legalized abortion in that year and for all households after January 1973 following the Supreme Court's ruling on *Roe v. Wade*, and it is zero otherwise (Table 1 reports the state legalization years). My identification strategy of the bargaining power effect consists of estimating the coefficient of such a dummy variable. The other regressors are the wage rate  $w_t^i$  (of spouse  $i$  or of unmarried individual  $u$ ), household non-labor income  $y_t$ , and  $X_t$ .  $X_t$  includes age, age squared and education of each spouse (measured as number of grades completed), race, number of family members, number of children in the family, and presence of infants or number of young children.  $X_t$  also includes the logarithm of state total income and year and state fixed effects, to control for the level of economic activity in states and the overall US economy and for changing social conditions, such as fluctuations in the sex ratio. The dependent variable in my labor supply regressions is annual hours worked, defined as total annual hours worked on all jobs in a given year. Households in which the wife or the husband does not work are also included in my samples,<sup>13</sup> and I account for a possible selection bias toward working men and women by correcting for sample selection with Heckman MLE. As source of identification, I use distributional assumptions on the first step residuals alone or exclusion restrictions.<sup>14</sup> Both procedures yield similar robust results. All female and male labor supply regressions exhibit the same results when run without selection correction. I use predicted wages to measure the non-working spouses' wages and to address the possible endogeneity of individuals' observed wages. To predict individuals' wages, I take a standard human capital approach, also implemented in the collective labor supply literature (e.g., Donni, 2005), and consider a wage

<sup>13</sup> I only exclude household observations where neither spouse works, given that this analysis measures bargaining power changes through labor supply.

<sup>14</sup> The latter are presence of young children or number of family members only affecting the participation decision but not labor supply. Tables report estimation with identification from statistical distribution assumptions.

equation where wage depends on the individual's age, race, education, education squared, and cubed, but does not depend on his/her spouse's characteristics. This equation is then estimated separately for participating wives, husbands, single men, and single women, with a correction for selection bias.<sup>15</sup> The generated fitted values then replace the wage observations of the corresponding individuals in my samples.<sup>16</sup> Finally, Wald tests of overall statistical significance performed on the above labor supply regressions do not reject the validity of the framework I use.

I run my labor supply regressions using robust standard errors clustered by state, which allows for correlation of households' observations within states. My specifications do not use a differences-in-differences estimator: husbands' and wives' regressions, as well as older couples' and singles', are run separately from one another. As such, they should not suffer from the understated standard errors highlighted by Bertrand, Duflo, and Mullainathan (2004). At any rate, clustering by state should rectify such an underestimation, if it is present.

### 3.2 Data

Estimation is carried out on the March and June Supplements of the CPS, in the period 1968–1979. The CPS is a series of monthly cross sections, with a short longitudinal component. Individuals in the sample are interviewed eight times—four times, followed by a break of eight months, and then interviewed for the same four months the following year. I am able to match individuals in consecutive years to form a series of biannual panels with size of at most one fourth of the original sample size.<sup>17</sup> Data on labor force activity are taken from the demographic (March) Supplements, to which I merge data on age at marriage from the June Supplements from 1973, to determine which couples were married before legalization (before 1973, age at marriage is recorded in the March Supplements). I use the short longitudinal dimension to match labor market and income variables to the years when they actually take place.

I additionally examine data from the PSID between 1970 and 1979,<sup>18</sup> to replicate the results and perform the robustness checks on religion, contraceptive use, and abortion availability. Considering the different data source and the reduction in sample size with respect to CPS, it would not be surprising to find noisier estimates of the effects. The PSID is a panel of households that collects annual observations by following the same families and their split-offs over time. The panel dimension allows one to determine which households were married prior to legalization as well as to match labor market and income variables to appropriate years. The PSID also allows me to control for differences in divorce laws across states because it identifies each specific state of residence (CPS identifies only major states

<sup>15</sup> The participation decision depends on the number of children, dummies for age brackets, education, race and year and state fixed effects, and measures of local economy.

<sup>16</sup> Tables report estimation with the predicted spouse's and own wages.

<sup>17</sup> Miscoding and absence of proper identification variables make matching between consecutive years impossible for 1972 and 1976; merging the June Supplement drops year 1978 and half of the observations after the March merging.

<sup>18</sup> Due to miscoding of some variables in the PSID, I cannot use years prior to 1970.

**Table 2** Summary statistics CPS

Variable	Fertile couples		Non-fertile couples	
	Mean	SD	Mean	SD
Hours worked by wife <sup>a</sup>	1537.55	656.12	1655.08	609.70
Hours worked by husband <sup>a</sup>	2250.79	594.30	2172.26	592.40
Log of wage of wife <sup>a</sup>	0.84	0.62	0.84	0.61
Log of wage of husband <sup>a</sup>	1.31	0.55	1.31	0.63
Age of husband	34.21	7.32	53.24	8.08
Age of wife	30.83	5.82	50.61	7.07
Education of husband	12.06	3.02	10.92	3.51
Education of wife	11.83	2.30	10.99	2.86
Household non-labor income	285.06	1431.40	1087.28	2547.10
Number of children	2.33	1.60	1.31	1.54
Number of family members	4.38	1.64	3.39	1.59
Dummy for black	0.08	0.28	0.07	0.25
Number of observations	17,941		11,625	
	Young single women		Young single men	
	Mean	SD	Mean	SD
Hours worked <sup>a</sup>	1017.69	988.28	1189.73	1046.55
Log of wage <sup>a</sup>	0.89	0.62	1.00	0.76
Age	25.09	6.22	25.06	6.24
Education	12.26	2.82	12.09	3.04
Household non-labor income	1636.44	2408.50	1715.43	2574.46
Number of children	1.60	1.63	1.46	1.62
Number of family members	2.96	1.91	2.81	1.80
Dummy for black	0.35	0.48	0.22	0.41
Number of observations	8,921		10,988	

The sample contains data from the March supplement years 1968–1979 matched to the June supplement years 1973–1979

<sup>a</sup> For women and men with positive hours of work

Fertile couples are defined as those with wives 40 years old or younger; young as those 40 years old or younger

separately until 1977). Many states enacted the unilateral divorce provision between 1970 and 1975 and changes in divorce law that were favorable to women could have enhanced their bargaining power in their existing relationships. Following Chiappori et al. (2002), Gray (1998), and Friedberg (1998), I construct three

**Table 3** Summary statistics PSID

Variable	Fertile couples		Non-fertile couples	
	Mean	SD	Mean	SD
Hours worked by wife <sup>a</sup>	1247.31	687.63	1345.90	666.25
Hours worked by husband <sup>a</sup>	2212.98	521.42	1964.91	712.94
Log of wage of wife <sup>a</sup>	0.87	0.54	0.87	0.52
Log of wage of husband <sup>a</sup>	1.36	0.43	1.31	0.56
Age of husband	32.40	6.28	59.38	7.68
Age of wife	29.58	5.05	56.91	6.78
Education of husband	12.71	2.71	10.92	3.09
Education of wife	12.07	2.79	11.16	2.62
Household non-labor income	734.18	1848.33	2400.19	3045.56
Number of children	1.95	1.27	0.21	0.65
Number of family members	3.97	1.29	2.27	0.75
Dummy for black	0.08	0.27	0.06	0.23
Number of observations	7,566		2,824	
	Young single women		Young single men	
	Mean	SD	Mean	SD
Hours worked <sup>a</sup>	1095.38	660.22	1102.07	679.98
Log of wage <sup>a</sup>	0.51	0.58	0.68	0.60
Age	24.62	6.20	21.20	4.04
Education	11.61	2.02	11.11	1.76
Household non-labor income	909.59	2429.13	948.91	2771.86
Number of children	1.64	1.55	1.08	1.54
Number of family members	3.61	2.04	3.70	2.30
Dummy for black	0.32	0.47	0.20	0.40
Number of observations	9,348		8,865	

The sample contains data from survey years 1970–1979

<sup>a</sup> For women and men with positive hours of work

Fertile couples are defined as those with wives 40 years old or younger; young as those 40 years old or younger

dummy variables to be used in my regressions: a dummy for the presence of unilateral divorce laws, one for community property laws, and one for the joint presence of these two provisions.<sup>19</sup>

<sup>19</sup> A state in a given year is defined to have a unilateral divorce law if spousal consent is not required to divorce and if fault grounds are not allowed, including property settlements and alimony.

**Table 4** CPS data. Estimation of the labor supply regressions of fertile wives and husbands

	Wives (1)	Husbands (2)
Dummy for legalized abortion (legal = 1)	-83.05*** (15.83)	33.97** (15.57)
log (wage of wife)	214.27*** (77.14)	-35.33 (65.8)
log (wage of husband)	118.64*** (19.53)	-472.13*** (19.05)
Age of husband	5.86 (8.87)	39.77*** (5.38)
Age of husband squared	-0.1554 (.10)	-0.51*** (.065)
Age of wife	-23.05 (15.80)	27.71*** (6.45)
Age of wife squared	0.41* (0.23)	-0.37*** (0.085)
Education of husband	-99.40*** (14.61)	38.57*** (3.42)
Education of wife	-16.86 (15.20)	25.84*** (9.46)
Household non-labor income	-0.02*** (0.01)	0.0002 (0.007)
Number of children aged $\leq 3$	-153.11*** (34.08)	-15.98** (7.13)
Number of children aged $\leq 25$	-69.541*** (7.43)	12.3278*** (4.66)
Number of household members	54.45** (21.41)	-15.846 (20.88)
Dummy for black	247.33*** (69.19)	-226.28*** (55.55)
Constant	906.2*** (258.1)	1021.26*** (222.7)
Inverse Mill's ratio	35.791 (13.94)	83.2348 (357.28)
Year fixed effects	yes	yes
State fixed effects	yes	yes
Sample size	17,941	17,941

\* Significant at 10%; \*\* Significant at 5%; \*\*\* Significant at 1%. Estimated coefficients, and standard errors (in parenthesis) are clustered by state

All tables report regressions with the same set of covariates described in Sect. 3. Regressions are corrected for sample selection with Heckman MLE

Fertile women are defined as those 40 years old or younger

**Table 5** CPS data. Effect of abortion legalization on annual hours worked, by demographic group

	Fertile couples (1)	Non-fertile couples (2)		Fertile singles (3)
Wives	-83.052*** (15.83)	20.25 (47.68)	Women	-86.95 (65.44)
	17,941	11,625		8,921
Husbands	33.97** (15.57)	-8.76 (16.25)	Men	-22.55 (59.97)
	17,941	11,625		10,988

\* Significant at 10%; \*\* Significant at 5%; \*\*\* Significant at 1%. Estimated coefficients, standard errors (in parenthesis) are clustered by state, and sample size

All tables report regressions with the same set of covariates described in Sect. 3. Regressions are corrected for sample selection with Heckman MLE

Fertile women are defined as those forty years old or younger

No specific question is asked about the couple's biological ability to have children, neither in the CPS nor in the PSID. Therefore, I define households in which the woman is 40 years old or younger to be fertile ("young"), as age 40 is considered a standard upper bound for the age of fertility. I have also used 41- to 45-year-old thresholds for robustness checks.

Tables 2 and 3 present the descriptive statistics for the main demographic groups and variables. In both samples, men on average work more annual hours than women and earn a higher hourly wage, while they have similar levels of education. Husbands are on average 3 years older than wives. Non-labor income is higher for older married couples than for younger couples.

## 4 Results

### 4.1 Main evidence

The main results are shown in Tables 4 and 5. As predicted by the theory, the estimated effect of abortion legalization on labor supply is positive for husbands and negative for wives. The point estimates of the dummy for abortion legality indicate that annual hours worked by fertile wives fell by about 83 h ( $P$ -value = .001), while their husbands' increased by 34 h per year ( $P$ -value = .03), and the coefficients are statistically different from each other (column (1) of Table 5). Those changes correspond to a 5.4% reduction of the average annual hours worked by fertile married women<sup>20</sup> and a 1.5% increase for their husbands'. These effects are sizable, given the acknowledged rigidities in the husbands' labor supply (e.g., Donni, 2005) and the frequency of the reported labor supply peaking around 40 h of work per week. In particular, the impact on husbands is remarkable since traditional analyses make no prediction regarding them as being affected by abortion legalization, let alone about their labor supply increasing with legalization. The direction of those effects is also the same as in the labor supply impact of divorce laws found by Chiappori et al. (2002). As to the other covariates in the spouses' labor supply equations, most parameter estimates are comparable to the literature. In particular, those associated to the wage rates exhibit signs and significance levels found specifically in the family labor supply literature. The wives' own wage response is positive significant (column (1) of Table 4), as it is common in the female labor supply literature, while the husbands' own wage coefficient is negative significant (column (2) of Table 4), and the effects are sizable. The husband estimate is in accord with previous empirical findings in the family labor supply literature as well. In fact, Chiappori et al. (2002) run similar spouses' labor supply equations and show negative own wage estimates for husbands in all their regressions, even after instrumenting for the wage. Ransom (1987) and Kooreman and Kapteyn (1986) also

<sup>20</sup> This decline in fertile wives' labor supply does not appear to be driven by decreased participation or women less attached to the labor force entering the labor market and working fewer hours. Female participation in the labor market does not exhibit any significant bargaining power effect of abortion legalization (it would be too drastic to expect this bargaining power effect to lead young married women to withdraw from the labor market).

**Table 6** CPS data. Effect of abortion legalization on annual hours worked of married fertile women, by income class

	High income (1)	Low income (2)	Neither categories (3)
Fertile wives	31.74 (37.86)	-332.76*** (94.75)	-119.51*** (30.65)
	2,536	3,338	12,067

High (Low) income households are the top (bottom) 20% of the national per-capita household income distribution. Neither categories households are those not in the top or bottom 20% of the per-capita income distribution

\* Significant at 10%; \*\* Significant at 5%; \*\*\* Significant at 1%. Estimated coefficients, standard errors (in parenthesis) are clustered by state, and sample size

All tables report regressions with the same set of covariates described in Sect. 3. Regressions are corrected for sample selection with Heckman MLE

Fertile women are defined as those 40 years old or younger

show negative own wage responses for husbands. The negative coefficient should be the result of the standard income effect reinforced by the income effect of the increase in the husbands' intra-household bargaining power due to their own wage. Furthermore, I also find a positive significant cross-wage effect of husbands' wages on wives' labor supply, as documented in Chiappori et al. (2002) and Blundell, Chiappori, Magnac, and Meghir (2002). Standard income effects would predict a negative coefficient, but the loss in female bargaining power due to increases in male wages may induce the positive sign.

The bargaining power effect is also estimated on older couples and young singles. The labor supply regressions on older couples show no significant impact, as theory would predict: both husbands and wives exhibit economically negligible and statistically insignificant coefficients on the abortion legality dummy variable, the wives' coefficient being even positive (column (2) of Table 5). As to singles, for young single women the estimated coefficient on the legalization dummy is negative and sizable, although not really significant ( $P$ -value = .18, column (3) of Table 5). The Demand and Supply marriage models provide an interesting interpretation of the results in terms of young singles' decisions about marriage and labor. Abortion legalization increased the expected marital gains of young single women, leading to a decrease in their labor supply. As some of those women plan to marry in the future, they experience an expected positive bargaining power effect. However, this impact is significant only at the 18% level and is not paired with an opposite effect on young single men, most likely because it is an effect expected only in the future and marriage plans may not be relevant for young single men. In fact, for young single men the coefficient on the abortion legality dummy variable is negative and statistically insignificant. It is also different from the young married men's, which emphasizes the bargaining power effect on husbands.

#### 4.1.1 Availability of abortion prior to legalization

Wealthier households should have had better access to safe abortion services prior to legalization. Hence, legalization should have had a smaller impact on the bargaining power of wives in wealthier households. I estimated the labor supply



**Table 7** PSID data. Effect of abortion legalization on annual hours worked by demographic group, religion, birth control use, abortion access

	Fertile couples (1)		Non-fertile couples (2)	Fertile singles (3)
Wives	-105.45** (55.29)		-99.64 (162.3)	Women -92.93 (120.66)
	7,566		2,824	9,348
Husbands	51.20** (22.78)		8.91 (69.4)	Men 55.77 (53.75)
	7,566		2,824	8,865
Fertile Couples	Catholic (4)	Baptist (5)	Other (6)	Birth control users (7)
Wives	15.57 (253.6)	-58.15 (232.2)	-167.95*** (52.91)	-61.10 (127.7)
	1,010	1,862	2,894	1,259
Husbands	-51.39 (71.76)	-15.91 (109.1)	64.74* (36.92)	-70.01 (66.3)
	1,010	1,862	2,894	1,259
Fertile Couples	High access (8)		Non-high access (9)	
Wives	-246.95*** (61.94)		-150.85*** (39.6)	
Husbands	88.91* (52.07)		59.94** (28.72)	
	7,566			

(4); (5); (6). The three subsamples do not sum up to the entire sample because only same faith couples are considered in those religious groups

(8); (9). The estimated coefficients of the dummy variables for high (non-high) abortion access are reported, which replace the usual LEGAL dummy. A state is defined as high access if the state ranks among the top 10% states in terms of annual abortion rates and abortion is legal

\* Significant at 10%; \*\* Significant at 5%; \*\*\* Significant at 1%. Estimated coefficients, standard errors (in parenthesis) are clustered by state, and sample size

All tables report regressions with the same set of covariates described in Sect. 3. Regressions are corrected for sample selection with Heckman MLE

Fertile women are defined as those 40 years old or younger

regressions separately for wealthy and non-wealthy households.<sup>21</sup> As seen in Table 6 (columns (1) and (3)), the estimated effect of legalization was not significant among wealthy fertile wives, while for non-wealthy households it remains statistically significant.

This evidence represents a first empirical support of the bargaining power effect of abortion legalization. Further evidence presented below, together with the discussion of various alternative explanations, should help making this claim convincing.

<sup>21</sup> Wealthy households are defined as the top 20% of the national per-capita household income distribution. Non-wealthy households refer to neither wealthy nor poor households, or households that are not in the top or bottom 20% of the per-capita income distribution (“neither categories” in Table 6). The results are robust to other wealth thresholds, e.g., 10% and 15% of the national per-capita household income distribution.

## 4.2 Evidence from PSID data

Results in the PSID estimations are quite similar to those in the CPS (Table 7). The PSID point estimates of the abortion legality dummy show that married fertile women's labor supply decreased by 105 h per year ( $P$ -value = .05), while their husbands' increased by 51 h per year ( $P$ -value = .03), respectively a 8.4% decrease and a 2.3% increase (column (1) of Table 7). In particular, such a significant impact on husbands' labor supply highlights the remarkable intra-household bargaining power effect of abortion legalization. As to the other covariates in the spouses' labor supply equations, and to wages in particular, they exhibit signs and significance levels of the CPS estimates, to which discussion I refer.

Older couples exhibit a non-significant legalization effect, but wives' estimated coefficient is negative and relatively sizable (column (2) of Table 7). The smaller sample size of older couples in the PSID may explain their estimates being noisier than in the CPS.<sup>22</sup> Young single women also exhibit a negative and relatively sizable point estimate, which is not significant but in any case emphasizes the favorable changes in women's reservation values for marriage due to legalization (column (3) of Table 7).

### 4.2.1 Religious beliefs

Bargaining theory predicts that abortion legalization should have had no effect on couples with strongly held religious beliefs against abortion, particularly if both spouses are of the same faith. I test these predictions on the PSID data, which include information on religious beliefs.

I divided the PSID sample into two groups: a so-called anti-abortion sample and "others." Anti-abortion households are defined as those in which the spouses are either Catholic or Baptist, and of the same faith; "others" is defined as those whose religion does not ban abortion.<sup>23</sup> Then, I ran my main labor supply regressions on those two subsamples, noting that the results are biased against my prediction to the extent that individual households' views on abortion may diverge from the official positions of their churches'. The results are shown in Table 7 (columns (4), (5), and (6)). Consistent with the predictions of the theory, the estimated coefficients on the legalization dummy for Catholics or Baptists are not statistically significant, and the estimated coefficients in the young husbands' hours regression are mildly negative. By contrast, the estimated effect on the "other" fertile households is sizable, statistically significant and wives' and husbands' respective coefficients of the

<sup>22</sup> Standard errors in my PSID estimates and the estimates that are not corrected for selection are generally larger than standard errors in my CPS estimates. This pattern seems reasonable since PSID has a panel structure while CPS has a cross-sectional structure. The correlation within clusters (states) will be positively higher in PSID since it is more likely that the same people are within the same cluster over time.

<sup>23</sup> The religious denominations that do not ban abortion include Methodists, Unitarians, Episcopalians, and some Presbyterians, which were divided on the issue during my sample period. Sample sizes were too small to carry out the analysis for these groups separately. In line with Lehrer (1995), I consider couples to be of the same faith provided that each spouse belongs to any of those above religious groups.

legality dummy are statistically different from each other. Indeed, the estimated effects are larger in magnitude than for the sample as a whole, with legalization estimated to reduce wives' labor supply by 168 h and increase husbands' labor supply by 65 h. It is interesting to interpret the above wives' estimates in light of Lehrer (1995)'s findings that wives' labor supply is higher in same-faith couples with egalitarian attitudes toward gender roles than in same-faith couples with unequal gender roles (Baptists being unequal, "others" egalitarian and Catholics somewhere in between). My evidence of a significant decline in wives' labor supply for "others," and no decline for Baptists due to abortion legalization, clearly reflects a strong difference in the role of abortion and its bargaining power effect across religions, given that female labor supply patterns across religions in general show higher labor supply for "others" and lower for Baptists.

#### 4.2.2 *Birth control use*

Legalization should not have much impact on couples who regularly used contraceptives prior to legalization, since abortion legalization would not represent a new option to control fertility.<sup>24</sup> I test this prediction on the PSID data, which include information on birth control use. I estimated the labor supply regressions for such households, focusing on the portion of birth control-using households that do not strongly oppose abortion. As seen in Table 7 (column (7)), the estimated effects of legalization are in fact not statistically significant.<sup>25</sup>

#### 4.2.3 *Availability of abortion after legalization*

I measured the availability of abortion services using information on annual abortion rates in individual states for given years (data from the Allan Guttmacher Institute, with the annual abortion rate defined as the number of abortions per thousand live births). The PSID data allow me to identify the state of residence for each household. For each year, states were ranked from 1 to 51 according to their state abortion rate. I defined a state to have high access to abortion in a given year if abortion is legal and the state ranks among the top 10% of states in terms of annual abortion rates. I then allowed the estimated effects of legalization to vary between high-access and non high-access states.<sup>26</sup> The results are reported in Table 7 (columns (8) and (9)). As can be seen, the bargaining power effect is confirmed: legalization is associated with significantly lower female labor supply and higher

<sup>24</sup> The theory makes no clear cut prediction regarding households that do not use contraceptives. Lack of contraceptive use may reflect conservative views regarding fertility control, rendering legalization irrelevant.

<sup>25</sup> The PSID question on birth control was asked only in 1972, prior to national legalization but after the 1970 legalizations of California, New York, Washington, Alaska, and Hawaii. I am assuming that use of birth control in 1972 is a good indication of whether that household used birth control prior to the passage of state legalization. To the extent that this is not true, my sample of birth control users will include non-users as well, thus biasing the results against finding no effect of legalization for that group.

<sup>26</sup> There is no particular reason to expect bargaining power to vary continuously with abortion rates. Specifications in which the measures of availability were entered directly yielded an estimated coefficient of the right sign, but was not significant.

male labor supply among fertile couple households in both high-access and non high-access states (and coefficients of wives and husbands are statistically different from each other for both low availability and high availability states). Also, the point estimates are stronger in high-access states, though not statistically different from non-high access states.<sup>27</sup>

### 4.3 Mobility

In the early 1970s, when only some states had legalized abortion, women were known to travel to neighboring states to obtain abortion services. To the extent that such women already had greater bargaining power in their state of residence prior to national legalization, the estimated effects of legalization are biased toward zero. Such a bias is most likely for households living within driving distance of states in which abortion was legal. Omitting states that border New York and California left the results intact. In addition, there is information on the state of origin for recipients of abortion services in New York City. Most out-of-state recipients originated from New Jersey, Illinois, Michigan, Ohio, and Pennsylvania;<sup>28</sup> the results were robust to the deletion of these states. Excluding residents of Washington, D.C., in which more than half of all abortions were performed on women living outside the District, also left the results intact. Finally, the results were robust to the exclusion of households living in the two states with the lowest abortion rates: West Virginia and Mississippi.

### 4.4 Fertility effects and endogeneity of children

The bargaining power effect analyzed here need not work through reduced fertility or actual use of abortion services, neither of which is modeled in this paper. However, I controlled for children as extensively as the data sets allow, and I conducted two sensitivity tests to ensure that my findings are not affected by the possible endogeneity of children. The results were not affected when I dropped the children variable from the labor supply regressions, nor did instrumenting for the number of children reverse the findings.<sup>29</sup> At any rate, Mroz (1987) uses the same PSID data on married women's labor supply in the 1970s and fails to reject the exogeneity of children. Finally, although abortion legality was accompanied by delayed and decreased fertility, those phenomena tend to increase female labor supply, not to decrease it as my framework predicts.

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<sup>27</sup> Similar results were obtained by modifying the ranking threshold for high access to the top 20%, 25%, and 33% of annual state abortion rates or measuring accessibility using the number of abortion providers (number of clinics providing abortion in a state, data from the Allan Guttmacher Institute), either by itself or scaled by state size or population. Data on providers are available only from 1973, so I used 1973 values for prior years in states that legalized early.

<sup>28</sup> Non-residents account for about one half of the total abortions performed in New York City between 1970 and 1972 (Pakter, O'Hare, Nelson, & Svigir, 1973).

<sup>29</sup> The instruments included husband's and wife's education levels squared and age at marriage. For the PSID sample, I also included education of the husband's father and a dummy variable for Catholic. The IV regression for female labor supply was run without selection correction.

My exclusion of couples married after legalization makes my sample of fertile couples older over the 1970s, so that the decrease in the labor supply of fertile married women may be driven by those women getting older and choosing to have children as fertility procrastination is no longer feasible. I believe that this “binding fertility” effect does not bias the fertile wives’ results for the following reasons. In all my labor supply regressions, I specifically control for the number of infants (presence of children aged 0–3 in the PSID and number of children aged 0–1 and aged 0–3 in the CPS), which should clearly capture the reduction in female labor supply due to fertility. Moreover, restricting my sample to couples where the wife is about 32–35 years old or younger still yields a significant decrease in female labor supply due to abortion legalization (those couples, for whom fertility is not yet binding, actually represent the vast majority of my fertile samples). Also, as pointed out above, abortion legality was accompanied by decreased fertility, which tends to increase female labor supply, and fertility rates decreased during the 1970s. Actually, excluding newly married couples could make my prediction less likely to be verified: young and newly married couples were definitely more likely to have children and decrease female labor supply in the 1970s. Finally, there is no reason why this “fertility binding” effect should be accompanied by an increase in the labor supply of married men in the face of abortion legalization.

#### 4.5 Changes in non-market time

The results thus far indicate that abortion legalization caused married women in their fertile years to reduce their supply of market labor, which raises the question of what they did with their extra time. Although not formally modeled, bargaining theory seems to predict that time spent doing household chores should have also declined with legalization. The PSID does record annual hours spent doing housework, but it is unfortunately contaminated by time spent with children, which is likely to *increase* with female bargaining power (Thomas, 1990), and by the fact that PSID changed the coding of the housework variable in 1976. Given these problems, it is not entirely surprising that I found no evidence of any legalization effect on annual hours of housework (results not reported for brevity).

## 5 Alternative explanations

### 5.1 Labor market downturns, decreasing trends or temporal concavity in female labor supply

It may be possible that the labor supply of fertile married women fell not as a result of the bargaining power effect of abortion legalization, but due to local or national economic downturns. In fact, national abortion legalization in 1973 nearly coincided with the first oil shock. There are at least three reasons to believe that economic downturns do not provide a plausible alternative explanation for my findings. First, my labor supply regressions include individuals’ wages, state total income, and year and state fixed effects, all of which should help pick up the effects of variation in

labor market opportunities. Second, it is difficult to understand why the labor supply of men married to fertile women should increase in the face of a downturn, while the labor supply of other men did not. Third, I find that older married women did not experience the same impact of legalization as married fertile women.

The decline in female labor supply captured by the legalization dummy may be due to a decreasing trend in the growth of female labor supply in the 1970s or to a temporal concavity. However, female labor supply was actually growing in the 1970s, with a trend increase (Blundell & MaCurdy, 1999; Juhn & Kim, 1999). Moreover, Shapiro and Shaw (1983) argue that the mean hours worked by married women aged 30–34 increased rapidly between 1967 and 1978. I control for year fixed effects and state fixed effects, which capture changes across time and space in female labor supply, and the dummy LEGAL varies across states between 1970 and 1972. Its variation across states should not capture a temporal concavity while year fixed effects are present. Including nationwide time trends (linear or quadratic) does not modify the estimated coefficient of the dummy LEGAL, which remains negative and significant.<sup>30</sup> I also control for age squared of both spouses in my main regressions. I have tried to include age cubed, or dummy variables for age brackets, and/or education squared and experience. All those variables should capture a temporal concavity, if at all present; however, they do not change the negative significant effect of abortion legalization on female labor supply. Finally, there does not seem to be any reason (or evidence in the literature) that such a potential female labor supply pattern/concavity should be accompanied by an increase in the labor supply of married men in the face of abortion legalization.

## 5.2 Increased female long-term wages

It has been acknowledged that abortion legalization affected women through enhanced long-term labor market opportunities and wages. I argue that such a pattern cannot explain the results presented in this paper because it does not match the predictions of the bargaining power effect. Increased female future wages exerted a positive substitution effect, likely to increase future female labor supply without influencing men's hours worked. The bargaining power effect instead created an income effect on both men and women, leading to a decline in wives' labor supply and an increase in their husbands'. Moreover, I control for current wages, although my framework does not account for future wages. The effects of legalization conditioned on the current wage should be entirely through the income effect. To the extent that the intertemporal effect is not captured in the current wage, it is even less likely that one should find a negative effect of legalization on wives'

<sup>30</sup> Including state specific time trends yields the same result in the CPS sample. However, in the PSID sample the dummy LEGAL changes sign and lacks significance. I attribute this result to the inclusion of the very large number of state specific time trends into a specification with already a large number of dummies correlated with states (state fixed effects, dummy LEGAL, dummies for divorce). The state specific trends can correlate with the fixed effects, the dummies LEGAL and divorce and create spurious estimates. In the PSID specification, there are more than twice as many state dummies with less than half the number of observations than in the CPS. In fact, including state specific time trends while dropping year and state fixed effects keeps the estimated coefficient of the dummy LEGAL negative and significant in the PSID as well.

labor supply. Finally, women's wages relative to men changed little during the 1970s, as most of the decrease in the gender wage gap happened after 1979 (Blau, 1998). Then, those improvements in female labor market opportunities should not have led to an increase in wives' bargaining power in the 1970s.

### 5.3 Divorce

On the one hand abortion legalization may have increased couples' likelihood to divorce. On the other hand, changes in divorce laws may have increased women's bargaining power. Both those phenomena could have potentially made my samples of intact couples exhibit a lower female labor supply than prior to legalization. However, I argue that they do not represent alternative explanations for my findings.

#### 5.3.1 *Legalization of abortion and divorce*

There is no evidence in the literature that abortion legalization increases the likelihood of divorce or the credibility of either spouse's (the husband's in particular) threat to divorce. At any rate, my results should not be jeopardized by such a connection for the following reasons. Johnson and Skinner (1986), among others, show that married women increase their labor supply in the years prior to separation, while my empirical evidence exhibits a significant decrease in their labor supply as a consequence of abortion legalization and an effect on husbands as well. Moreover, my samples include intact couples, married for at least 2 years. The more stable couples, with a better conflict-resolution technology and possibly more commitment to their marriage, should exhibit a milder, if any, bargaining power effect than the entire population of couples, biasing the bargaining power effect against my findings. Also, data on divorce rates trends do not match with the timing of abortion laws. Regressing the probability of a couple getting divorced in my PSID sample on the same controls as in my main specification shows a negligible impact of legalization.<sup>31</sup> Finally, in my data divorce laws seem not to have interacted with the legalization of abortion. Adding an interaction dummy variable "LEGAL x UNILATERAL x COMMUNITY PROPERTY" to my main labor supply specification does not affect the estimated coefficient of the legalization of abortion, while the interaction term is statistically insignificant.

#### 5.3.2 *Divorce laws*

I control for divorce laws in the labor supply regressions run on the PSID sample. Thus, the labor supply effect of the abortion legality dummy variable, conditioned on those controls, should be generated by abortion legalization. However, the dummy variables for the presence of the unilateral divorce provision and of the

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<sup>31</sup> This lack of significance holds also when the dummy variable for legality of abortion is interacted with wives' labor supply. Thus, it can be excluded that only "career" women were induced to divorce as abortion became legal, making my sample of married fertile women experience a decrease in labor supply. If anything, female contraceptive methods were already available and widespread (i.e., the pill), so that those women were able to work many hours also before the legalization.

community property division are not significant. Estimating the main labor supply regressions without those controls yields the same results both in terms of significance and magnitude of the abortion legalization coefficient. This lack of significance appears to contrast with the significant impact of divorce laws on labor supply reported in Chiappori et al. (2002). However, there are reasons why it does not. The sample of Chiappori et al. (2002) is a cross section of households from the 1989 PSID interview wave with both spouses being at least 30 years old and below age 60, while my main sample is restricted to women younger than 40. Younger households, who are possibly less stable and had not accumulated much wealth or assets yet, may be less affected by divorce laws. Also, in my samples cohabitants are classified as “married.” This can generate spurious results for the effect of divorce laws because the bargaining power balance of cohabiting couples should not be affected by divorce laws, at least not as much as legally married couples.

#### 5.4 Availability of female contraceptives

Major female contraceptives such as IUD, diaphragm, and, above all, the contraceptive pill, whose usage may have increased in the 1970s, could yield the same bargaining power effect on labor supply as abortion legalization. They give women the same independence of choice as legal abortion, possibly at a lower economic and emotional cost. However, the pill became available to married women in 1960, and its diffusion was very rapid.<sup>32</sup> As for the other two contraceptives, they were already available and widely adopted in the 1960s and major changes in their usage happened in the decade preceding the time period I analyze. Furthermore, women do not seem to have substituted away from female contraceptives to abortion: US data on married women and contraceptive use show, if anything, a mild increase (rather than a decrease) in the percentage of married women using contraception in general including the pill, IUDs, and diaphragms during the 1970s.

#### 5.5 Welfare programs for women

Welfare programs favorable to women may discourage female labor supply or increase the bargaining power of married women by enhancing the value of single motherhood. However, by definition, welfare programs benefit only low-income households, while my results hold for all levels of income. In particular, when low income households are removed from my samples, there is still a significant

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<sup>32</sup> “Its use was approved by FDA in 1960 “and its diffusion was so rapid that, by 1965, 40% of young married women, using some form of contraception, were “on the pill”” (Goldin & Katz, 2000).



decrease in married fertile women's labor supply<sup>33</sup> (column (3) of Table 6). Also, the timing and pattern of the main welfare benefits such as AFDC, EITC and mandated benefits are different from abortion legalization.<sup>34</sup>

## 6 Conclusions

This paper tests whether abortion legalization increased women's bargaining power in households formed prior to legalization. Consistent with this hypothesis, empirical analysis of CPS data reveals that married women of fertile age significantly reduced their supply of market labor, while their husbands increased theirs, as a consequence of legalization. Evidence from PSID data yields similar findings. It also strengthens the bargaining power interpretation with a number of additional results: the lack of an effect for couples with strongly held religious beliefs against abortion, for those who regularly use contraceptives, or for households with better financial resources. Alternative explanations such as labor market downturns, increased female long-term wages, divorce patterns, increased availability of female contraceptives, and welfare programs are rejected.

The findings presented here represent a first empirical support of the claim that abortion legalization influenced intra-household bargaining power. Additionally, they add to a growing body of evidence against unitary models of the household, which do not allow extramarital spousal opportunities to affect intra-household decisions.

**Acknowledgements** I thank my advisors Gary Becker, Marianne Bertrand, Pierre-André Chiappori, Erik Hurst, and Steve Levitt for their advice and comments. I thank Gino Cateau, Shoshana Grossbard (the editor), and Curtis Simon for their valuable help and suggestions. Errors are mine.

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<sup>33</sup> Low income households are defined as the bottom 20% of the national per-capita household income distribution. Non-poor households refer to neither wealthy nor poor households, or households that are not in the top or bottom 20% of the per-capita income distribution (“neither categories” in Table 6). The results are robust to other wealth thresholds, e.g., 10% and 15% of the national per-capita household income distribution.

<sup>34</sup> For instance, EITC begins in 1975 as a modest program and then starts to expand in the 1980s (Eissa & Liebman, 1996; Moffitt, 1992). AFDC real benefit per capita decreases in the 1970s and is generally not available to two-parent families (Moffitt, 1992). Mandated benefits were affected by the federal legalization in 1978 (Gruber, 1994).

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