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Does Custody Law Affect Family Behavior In and Out of Marriage?*

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Abstract

We examine the effect of joint custody on marriage, divorce, fertility and female employment in Austria using individual-level administrative data, covering the entire population. We also use unique data obtained from court records to analyze the effect on post-divorce outcomes. Our estimates show that joint custody significantly reduces divorce and female employment rates, significantly increases marriage and marital birth rates, and leads to a substantial increase in the total money transfer received by mothers after divorce. We interpret these results as evidence against Becker-Coase bargains and in support of a mechanism driven by a resource redistribution that favors men giving them greater incentives to invest in marriage specific capital.

JEL Classification: J12; J13; J18; K36; N32; R2
Keywords: Divorce; Fertility; Bargaining; Intrahousehold Allocations; Austria

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1 Introduction

With a substantial proportion of children in many industrialized countries expected to live apart from one of their parents before reaching adulthood, child custody after divorce is of considerable concern not only for the parties directly involved (i.e., children, mothers, fathers, lawyers, and judges) but also for society at large.

Most of what we know about child custody comes from studies by legal scholars (Maidment 1984; Mason 1994; Maccoby and Mnookin 1997). This literature argues that the 'best interest of the child' — the key legal principle that underpins custody allocations in most countries around the world — is indeterminate, in the sense that it gives excessive discretion to judges to impose their own value judgements about which parent might better serve the child's interests. The same literature argues also that this indeterminacy exacerbates the inefficiencies of parents' allocative decisions at the time of divorce. Such studies, however, do not substantiate this conjecture with reliable quantitative evidence.

Economists, instead, have paid scant attention to child custody issues. Weiss and Willis (1985) were the first to formulate a model where divorce settlements, which specify alimony transfers and the allocation of custody rights, are determined within the marriage, when parents and children still live together. Divorce settlements have efficiency consequences after divorce, because the non-custodial parent cannot costlessly monitor the allocative decisions of the custodian. They have, however, no efficiency consequences within marriage because — in line with Coase theorem (1960) — spouses are assumed to be able to write down complete contracts on intrahousehold allocations of resources, whereby they can commit to a binding system of side payments well before the marriage eventually breaks down. Rasul (2006a) and Francesconi and Muthoo (2011) question this assumption and develop incomplete-contracting models that allow custody rights to have efficiency implications even within marriage.

The early empirical economics literature on divorce has also neglected the assignment of custody rights and its effects on household behavior (e.g., Becker, Landes, and Micheal 1977). Weiss and Willis (1993), Del Boca and Flinn (1995), and Garfinkel et al. (1998) analyze divorce settlements among couples in which the mother has sole (physical and legal) custody, explicitly assuming that all the salient allocative decisions concerning children must be made or approved by her. This assumption is justified on the basis that sole maternal custody has been the dominant custody arrangement in the 1970s and 1980s, the period over which the data used in such studies were collected. Although this assumption may not be tenable in more recent years, three findings from this research are relevant to our work. First, alimony and child support transfers are found to increase with husband's income and to decline with wife's income, although their overall size and sensitivity to income are small. Second, child support orders are institutionally set at low levels both because higher orders might lead to noncompliance and because courts are likely to place a high weight on non-custodians' (fathers') welfare. Third, divorce transfers are sufficiently low to reduce child expenditures and welfare substantially.[1]

Only recently have we witnessed increased interest in the effects of child custody on family behavior. The evidence, however, is still scant and it is primarily for the United States and takes advantage of the time difference in the introduction of joint custody reforms across states. For instance, Del Boca and Ribero (1998) provide correlational evidence according to which nonresidential parents with joint custody transfer more resources to children in addition to those ordered by courts. More recently, Halla (2013) shows that the introduction of joint custody led to an increase in marriage rates, overall fertility, and divorce rates as well as to a decrease in female labor market participation, male suicide rates, and domestic violence. Another example is the work by Allen, Nunley, and Seals (2011), which finds evidence that joint custody reforms increased the probability of receiving child support payment among divorced mothers.[2]

This paper, for the first time, examines the effect of joint custody on marriage, divorce, fertility and female employment in Austria. It has at least two original data features. First, it primarily relies on large individual-level administrative data, covering the entire population. Second, it uses unique data obtained from court records, which have never been utilized before to investigate child custody issues and which allow us to analyze the effect of custody reform on the bargaining arrangements within divorcing couples. Unlike previous studies, our identification strategy does not rely on time and space variation in the introduction of the custody reform. Rather, it is based on a difference-in-differences design, which takes advantage of the individual level information of the data. For instance, to analyze the effect of joint custody on marriage formation, we rely on the possibility that couples with women who have passed childbearing age and have no dependent children are not affected by the reform. This allows us to identify the effect of the reform on treated couples, i.e., couples with women in childbearing age or couples with dependent children. We shall perform several sensitivity exercises to check whether our results are robust to alternative definitions of childbearing age.

[1] The research on the effect of unilateral divorce laws also disregards custody allocations, and focuses instead on how and to what extent the adoption of such laws have affected divorce rates (e.g., Friedberg 1998; Wolfers 2006; Alesina and Giuliano 2007), family violence, suicide and spousal homicide (Stevenson and Wolfers 2006), or education, family income, marriage and labor force participation of individuals exposed to unilateral divorce regulations as children (Gray 1998; Gruber 2004).

[2] In addition, contrary to Halla's (2013) results, Nunley and Seals (2011) document that joint custody leads to higher rates of labor force participation among married mothers.

Another important feature of our paper is that we rely on economic theory to interpret our empirical results. On this we follow Becker (1993) who suggested that the Coase theorem (1960) provides the natural framework to evaluate the effect of changes in the legal environment on family behavior. (See also Stevenson and Wolfers 2006; Rasul 2006b.) We develop a simple conceptual framework based on the influential works by Clark (1999), Fella, Mariotti, and Manzini (2004) and Chiappori, Iyigun, and Weiss (2007), which examine the impact of changes in the law on divorce rates and allocations within marriage. In particular, Chiappori and colleagues emphasize the prominence of the Becker-Coase argument, according to which changes in divorce laws should not affect divorce rates. Our study is also related to the recent analysis by Lafortune et al. (2011), which examines the effect of a reform granting alimony rights to cohabiting couples in Canada. Their empirical work is embedded on a collective household model in a matching framework that predicts that changes in alimony laws affect existing couples differently from couples-to-be. In particular, they find that for couples formed before the alimony reform, obtaining the right to petition for alimony led women to reduce their labor force participation, but for newly formed cohabiting couples, this result does not hold.

We extend such earlier contributions to analyze the case of the effect of changes in the legal custody of children after divorce. In line with the Becker-Coase argument, our framework allows us to show that, if changes in the custody law which determine divisions of property rights conditional on marital status can be undone by individual recontracting, then such legislative changes should not affect the incidence of divorce. We can also show that the change in custody laws should not affect the joint expected utility from marriage and therefore the entry into marriage and the likelihood of having children.

If Coasean bargains are not attainable, however, divorce, marriage, fertility, and female labor force participation rates can be influenced by changes in the child custody legislation. This is because custody allocations are expected to affect the share of the marital surplus that each partner gets in marital bargaining, and this in turn affects each partner's investment incentives both in the marriage state and in the divorce state. We illustrate two mechanisms that might underpin such influences. The first hinges on a relatively greater investment made by parents (especially fathers) during marriage. The argument is that joint custody arrangements give fathers greater incentives to invest within marriage than the sole custody rule, because they expect they might receive a larger share of control rights over their children. If this is the case, joint custody redistributes control rights in a way that is favorable to men. A result of this redistribution of control rights is that men may be willing to transfer resources to their former partners even after divorce, e.g., in the form of greater alimony payments. The second mechanism mirrors the first, in the sense that fathers are less likely to make marriage specific investments, since they expect to receive some share of their children's time anyway, as a result of the joint custody that the reform entitles them to. Our empirical results along all margins (i.e., divorce rates, marriage rates, marital and nonmarital fertility, female labor force participation, and post-divorce outcomes) provide evidence of non-Coasean bargains and are broadly consistent with the first mechanism driven by greater parental investments and a redistribution of control rights deemed favorable to men.

2 The 2001 Joint Custody Law Reform in Austria

Austria long admitted the termination of marriage by mutual consent on the basis of irretrievable breakdown, irreconcilable differences and incompatibility. Unilateral divorce was introduced in 1978 (Boele-Woelki et al. 2003; González and Viitanen 2009), allowing either partner to force a dissolution of the marriage without the consent of the other. Despite this, mutual consent was (and still is) necessary to find an agreement on a number of important issues, with courts playing an active role in proposing and formulating a settlement. One of such issues is child custody arrangements.

Before July 2001, parents with a dependent child (aged 18 or less) had to agree on a sole custodian upon divorce. If no agreement was reached, the court assigned sole custody to one of the parents. This decision was (at least partly) based on the principle of the best interest of the child, which emphasized the importance of the continuity of the parent-child relationship and aimed at minimizing disruptions in the child's life, such as reducing the chances of a change in school, neighborhood and social networks, and of a decline in living standards (Mnookin 1975). This legal environment, therefore, permitted only two alternative custody arrangements, maternal or paternal sole custody.[3] About 90 percent of the children whose parents divorced in the decade before the reform lived with their mothers after divorce and the remaining 10 percent lived with their fathers.

On July 1, 2001, the Austrian government introduced a custody law reform, which changed the standard focal arrangement from sole custody to joint custody of dependent children. Courts, however, grant joint custody only if parents agree on it as well as on the child's primary residence (physical custody). If parental agreement on physical and joint custody is not reached, then the judge, who is randomly matched to couples, assigns sole

[3] Exceptionally, custody could have been assigned to a party other than parents (e.g., grandparents or close relatives). Cases of nonparental custody, however, were and continue to be rare, and therefore we do not consider them in the analysis below.

custody on the ground of the best interest of the child.

With the 2001 reform, therefore, there exist four different settlements, the two sole custody allocations as in the pre-reform period and two joint custody allocations, one in which the mother has physical custody and the other one in which the child co-resides with the father. Since the introduction of the reform, the distribution of physical custody has been virtually identical to the allocations observed before the reform, with 91 percent of dependent children living with their mothers and about 9 percent living with their fathers. But now nearly 44 percent of all children have parents who agreed on joint custody and more than 10 percent of these children co-reside with their fathers.

The Austrian Parliament started the public discussion of the reform on September 27, 2000. The bill was discussed and approved almost two months later by the lower house and then confirmed by the upper house on December 14, 2000. It was officially published on December 29, 2000, and became law on July 1, 2001. In this context, therefore, there seems to be little room for announcement effects (Blundell, Francesconi, and van der Klaauw 2011).[4] It is possible, however, that Austrian parents had already considerable information about joint custody at their disposal at the time of the reform, since Austria was a late adopter and several public debates on child custody took place before, especially when Germany introduced joint custody in 1997.

In addition, parents who wanted to end their marriage at the time of the parliamentary debates but were unhappy with sole custody arrangements might have had an incentive to prolong their union for about 6–9 months before filing for divorce and obtaining custody under the new regime. This may lead to spuriously longer pre-reform marriage durations and a spike in the divorce rate immediately after the reform. Empirically, we shall see if this is borne out by the data. We will also take this possibility into account in our sensitivity analysis by pre-dating the reform to September 2000 (and not July 2001), when the bill was first introduced in Parliament.

[4] A content analysis of the major Austrian newspapers reveals that media coverage of child custody issues was 4 to 5 times greater between September and December 2000 — that is, 6 to 9 months prior to the implementation of the reform — as compared to the coverage during the preceding eight months of 2000. Exposure to relevant information therefore was greater, but parents who preferred the pre-reform settlements could still opt for sole custody even after the reform and, thus, they did not have to hasten a decision before July 2001. Similarly, couples who were childless and unmarried at the time of the public discussions had no incentive to move their family decisions forward because the reform expanded their choice set and did not abolish pre-reform settlements.

3 Child Custody Rules and Household Behavior

Divorce

A child custody reform that shifts the standard custody allocation from sole custody to joint custody transfers a well-defined control right — the right to care for own dependent children[5] — from the spouse who would have received custody under the sole custody arrangement (usually, the mother) to the other partner. Our analysis draws from earlier works by Clark (1999), Fella, Mariotti, and Manzini (2004) and Chiappori, Iyigun, and Weiss (2007), which have focused on the allocative consequences of the introduction of unilateral divorce.

When marriage is formed, spouses are uncertain about the quality of the match and their future productivities in the market and nonmarket sectors. When these random variables are realized, spouses re-evaluate their decision to marry and decide whether or not to stay married. Even if the marriage ends, they can still transfer resources to each other. For instance, among couples with children, child expenditures are collective goods (Weiss and Willis 1985) and transfers from the non-custodial parent to the custodian can be made with the intention of increasing child welfare.

Let M be the utility possibility set which is defined by the combined resources of the couple when married. If the union dissolves, the couple's total resources determine the utility possibility set that can be achieved under divorce. As in Mnookin and Kornhauser (1979) as well as in the models above, couples bargain in the shadow of the law, with the law defining — conditional on the marital state — what each partner is entitled to. But unlike the studies mentioned above, the legal background is that of unilateral divorce, both before and after the 2001 reform (see Section 2). Clearly, the introduction of joint child custody rights may affect the collection of possibilities faced by each divorcing partner. Thus, the position of the utility possibility set after divorce depends on the custody rights over children. We label D_S and D_J the sets determined under sole custody and joint custody, respectively.

The boundaries of M, D_S, and D_J determine their Pareto frontiers. These are denoted by B_M if partners are married, B_S if they divorce and children are allocated under the sole custody rule, and B_J if, when the union dissolves, children are allocated according to the joint custody law. Figure 1 illustrates an example of such Pareto frontiers, where u_h and u_w are the utility levels of male and female partners respectively. If married, the

[5] In our setup, child care comprises a broad range of parental behaviors. It includes the provision and satisfaction of basic child needs (such as food and health care) as well as decisions over important aspects of the child's life (such as school choice, cultural upbringing, and moral values).

couple will reach an efficient agreement on the allocation of resources along B_M giving a utility pair denoted by m^*.

Consider the case in which the divorce law allows only for sole custody (as in the pre-reform period in Austria) and assume that the custody legislation gives preference to the mother.[6] If in the divorce state the couple's resource allocation is given by s^*, then $s^* \notin M$, and with unilateral divorce, the marriage cannot be saved and the outcome is s^* (Clark 1999).

Now consider a world where only joint custody is available for divorcing parents.[7] The move from sole custody to joint custody essentially transfers some of the control rights over children from the default custodian (i.e., the mother) under the sole custody regime to the non-custodial parent (i.e., the father). Assume j^* is the efficient agreement attained by the spouses along B_J if they divorce. In this case, $j^* \in M$. Therefore, even if the father might benefit by a unilateral divorce, there will generally exist a range of marital allocations that make both partners better off compared to j^*. The couple will then renegotiate the initial allocation within marriage, m^*, and the renegotiation will have to provide the husband with a payoff within marriage that exceeds what he would get if they divorce. The final outcome will be located along B_M to the northeast of j^*.

In this example, the introduction of joint custody reduces the likelihood of divorce. But the availability of joint custody does not always guarantee this outcome. In Figure 1, in fact, the opposite result occurs if the intrahousehold equilibrium allocations in divorce are s' when children are assigned under sole maternal custody and j' when they are allocated according to joint custody law. In this case, therefore, the switch from sole to joint custody leads to an increase in the risk of divorce.

Figure 1 illustrates also the case implied by the Becker-Coase argument in our environment. Suppose the divorce allocations are either (s', j^*) or (s^*, j'). In both cases, whether divorce occurs or not depends not on the custody law, but on the relative size of the gains and losses from divorce. Moving from sole to joint custody rights, therefore, affects neither the allocation of resources nor the incidence of divorce.

Determining which of these three results emerges is an empirical issue that will be addressed in the subsequent analysis.[8] To guide the interpretation of our empirical find-

[6]The position of B_S in Figure 1 reflects the pre-reform situation according to which the overwhelming majority of divorced children lived with their mothers.

[7]As noted in Section 2, if spouses cannot agree on joint custody, judges will allocate sole custody to one of the parents (typically the mother). Thus in reality and in the data, as in most other institutional environments, joint custody is likely to coexist with sole custody.

[8]Of course, the likelihood of divorce could remain unchanged if investments and shocks offset each other. In that case, the resulting outcome is observationally equivalent to what predicted by the Becker-Coase theorem, even if this is actually violated. This is why we submit our statistical analyses to a range

ings, however, it is important to have an insight into some of the economic mechanisms that might be at work when joint custody arrangements replace sole custody rules. If Becker-Coase bargains are attainable, spouses will renegotiate the marriage contract and commit to a binding system of side payments (possibly from the mother to the father, as the divorce legislation becomes more favorable to fathers) irrespective of the initial distribution of property rights.

If the shift from sole to joint custody implies a move from s^* to j^*, the divorce agreements under sole custody become inefficient under the new joint custody rule and therefore, as in the Becker-Coase framework, are renegotiated. A shift to j^* may be driven by a relatively greater investment made by the father during marriage: ceteris paribus, joint custody arrangements give fathers greater incentives to invest within marriage than the sole custody rule, because they know they will receive a larger share of control rights over their children (Halla 2013). The same shift can occur with the realization of marriage specific shocks that make marriage more valuable to the wife, as in the Chiappori-Iyigun-Weiss (2007) setup. It is the wife who, in the marriage contract renegotiation, must be willing to accept a redistribution of resources to dissuade her partner from seeking divorce, moving from m^* to somewhere to the northeast of j^* along B_M.

Conversely, if the joint custody reform leads to an increase of the divorce rate as implied by a move from s' to j', the shift might be driven by a combination of a redistribution of resources and the realization of marital shocks that give the father enough bargaining power that the mother cannot compensate within marriage despite a relatively lower marriage specific paternal investment.

Marriage, Fertility, and Mother's Labor Supply

The shift from sole to joint custody may influence other behaviors that the Becker-Coase theorem would predict to remain unaffected. The argument based on redistribution favorable to fathers and greater paternal investment during marriage, which is associated with a reduction in the divorce rate, may lead to an increase in the likelihood of marriage. If joint custody gives fathers a greater share of the marital surplus and thus greater incentives to invest in marriage specific capital, this expands the marital surplus and, all else equal, more perspective partners will find marriage a desirable option.[9] By the same argument, if children are one of the results of such marital investments, the reform may

of sensitivity checks where we aim to uncover such offsetting forces. We also investigate the existence of heterogeneous responses among specific subgroups of the population.

[9]For ease of exposition, here and in what follows we assume that the realization of match specific shocks does not offset the investment decisions made by parents.

also induce a positive effect on the probability of having children among married couples relative to the probability of out-of-wedlock births.[10]

Conversely, if the move to joint custody is accompanied by reduced incentives for paternal investment, then the reform may discourage the formation of marriages characterized by a lower marital surplus. Likewise, the probability of marital births is expected to decline relatively to the probability of out-of-wedlock births.[11]

Finally, the custody reform might affect specialization within the family. Custody laws that make divorce less likely, as it might be the case of the joint custody legislation if it triggers greater father's investments and a redistribution of resources that reflect his changed bargaining power more closely, may lead to more specialization as evidenced by lower maternal employment.[12] This argument is similar to Gray's (1998) and Stevenson's (2007) insights on the effect of the introduction of unilateral divorce laws.[13] By contrast, less specialization (i.e., higher maternal employment) is consistent with custody laws that heighten the risk of divorce, such as joint custody if it is associated with lower paternal investment or sole maternal custody and with a resource redistribution that is favorable to women (Chiappori, Fortin and Lacroix 2002). Specialization, in fact, which means that one spouse specializes in the market sector and the other specializes in the nonmarket sector, leads to skills that are complementary within a marriage, but are likely to be less productive when single or imperfectly transferable to another union. Specialization, therefore, may be a costly strategy if partners live in a world with a greater risk of marital dissolution.

Divorce Transfers and Legal Cost of Divorce

When going through a divorce, spouses must settle on child custody and the division of joint property. As mentioned in Section 2, all divorces in Austria are arranged through a court settlement with the active involvement of a judge. This is costly. The Becker-Coase argument, which hinges on costless bargaining, is unlikely to be useful to characterize post-

[10]To the extent that parents face a quantity-quality tradeoff, the reform may not induce a shift in relative birth rates, but could lead to greater investments in children who live with legally married parents. Since we do not have data on child quality, however, we cannot investigate this implication.

[11]This mechanism echoes what Alesina and Giuliano (2007) call 'dilution effect' in their analysis of the effect of unilateral divorce laws on fertility, according to which the value of marriage goes down, because it is cheaper to dissolve it, and thus people are less likely to marry and marital fertility decreases because an easier divorce law lowers the propensity to invest in children. Since fewer people marry, they may also choose to have children out of wedlock.

[12]To the extent that fathers' labor supply is more inelastic than mothers', the larger paternal investment in marriage specific capital does not necessarily coincide with lower employment or reduced hours worked by the father.

[13]For similar insights on the impact of innovations in birth control technologies on intrahousehold allocation of resources, see also Oreffice (2007) and Chiappori and Oreffice (2008).

divorce interactions in such circumstances. But, conditional on divorce, the mechanism based on differential marriage specific investments could provide us with different testable predictions on divorce settlements.

Suppose the introduction of joint custody is accompanied by more marriage specific investments, which lead to a lower risk of divorce. If nonetheless divorce occurs, since for example marriage specific shocks make the union relatively less favorable to the husband, fathers are expected to transfer more resources to their children and ex-wives. This is because the marginal value of such transfers is greater as they have invested more during marriage. Of course, there are different types of transfers (e.g., alimony and child support payments), and husbands — over and above the awards prescribed by the judge — may not want to increase all of them.[14] But the total value of the transfers is expected to go up. If instead shared access to children through joint custody is associated with a greater chance of divorce and lower marital investment, we expect to observe a reduction in total divorce transfers from husbands to wives.

Another component of the settlement is the legal cost of divorce. Besides the actual monetary expenses incurred during the divorce, other aspects that contribute to the cost are the length of the divorce process and the number of post-divorce trials. If joint custody implies increased marital investments, the legal cost of divorce is expected to go down (i.e., shorter length of the process and fewer trials), since a relatively larger marriage specific capital makes each parent more likely to continue investing in children. If instead joint custody sets off lower marital investments, ex-spouses are expected to go through lengthier divorce processes and to face more post-divorce trials.

Caveats on the Empirical Analysis

Four points are worth keeping in mind in the empirical analysis that follows. First, the two non-Coasean effects we just described (higher and lower parental investment effects) could operate differently in different groups of the divorcing population or the reform may have a different salience for different couples. The net aggregate effect then might be zero, but not because the reform did not have an impact on couples' lives. This is why we perform several robustness checks and try to uncover the presence of heterogeneous responses to the reform in different subsets of couples.

Second, in 2000/2001 Austria went through a severe, albeit short, economic crisis. Although there is evidence that adverse family-level economic shocks, such as the job

[14]Other forms of transfers, including property transfers, gifts and informal cash transfers, are only imperfectly recorded in our data.

loss of a husband and negative financial surprises, increase the probability of divorce (Weiss and Willis 1997; Böheim and Ermisch 2001; Charles and Stephens 2004), we have no reason to expect that the 2000/2001 downturn affected the likelihoods of divorce among families with children differently than among families without.[15] But to the extent that children imply greater constraints on mothers, mothers' labor market and fertility decisions might have responded differently to that crisis than other women's. We therefore shall perform sensitivity analyses that are based on different comparator groups of women.

Third, our results must be seen within the broader context of other family policies in Austria that might have influenced family decisions. Lalive and Zweimüller (2009) and Lalive et al. (2011) show that maternity leave reforms introduced in the 1990s and in 2000 had substantial effects on fertility decisions, in the sense that they increased the likelihood that mothers who give birth to their first child immediately after the reform had more second children than pre-reform mothers. Interestingly, however, Lalive and colleagues show no effect on mothers' labor market outcomes in the medium run, despite an impact on the time on leave. As the next section makes clear, our identification strategy assumes that, other than the introduction of the custody reform, there are no contemporaneous shocks that affect the *relative* outcomes of the treatment and control groups. The shorter spacing between first and second born induced by the maternity leave reforms implies an effect on the intensive margin of fertility but not its extensive margin suggesting our groups have not been differentially affected, and the lack of an employment response in the medium term is reassuring in relation to our labor market analysis.

Fourth, if the reform had an effect on marriage and divorce, then selection into marriage and divorce may change as a result. In the case of the marriage selection, which is relevant for post-marriage outcomes, we shall compare our baseline results with those found from a reduced sample that includes only marriages formed *before* the reform. Anticipating our results, we find that our estimates are not sensitive to this change. We thus conclude that compositional effects are likely to be of minor importance in this case. For the selection into divorce, instead, there is no straightforward way to separate out treatment from compositional effects. This means that some of our estimates on post-divorce outcomes (see Section 6) may also reflect a compositional variation in the group of divorced couples, although other estimates based on instrumental variables models are less likely to be affected by compositional effects.

[15] Recent studies find less clear results (e.g., Hankins and Hoekstra 2011) or provide evidence of procyclical divorce rates (e.g., Hellerstein and Morrill 2011).

4 Data and Methods

Our empirical analysis uses unique, high-quality administrative data drawn from three national registers, the Austrian Social Security Database (ASSD), and official civil courts records.

Register data

The three registers are the Austrian Marriage Register (AMR), the Austrian Divorce Register (ADR), and the Austrian Birth Register (ABR). The AMR collects data on the universe of marriages in the country.[16] Besides basic socio-demographic data, it contains information on the date of the marriage, the location of the registry office where the marriage is recorded, the district in which each spouse resides, and spouses' age at marriage.

Compiled by district courts, the ADR has records of all divorces. This register collects data on spouses' age, number and age of children, the location of the registry office where the marriage was recorded, the location of the court that grants the divorce, and the legal grounds on which the divorce is conferred.

The ABR contains all birth records in Austria. Each record has data on child and parents' birth dates, mother's residence and nationality, her marital status at the time of the child's birth, and the date and location of the marriage if the mother is married at the time of birth, allowing us to establish whether a child is born out-of-wedlock or not.

The three registers give us the universe of births, official marriages and divorces occurred in Austria from January 1995 to December 2005 for an annual average of approximately 39,000 marriages, 19,000 divorces and 77,000 births over this period.[17] The unit of observation in our empirical analysis will be at the district level.[18] We therefore construct monthly series by treatment status of divorces, marriages, and births for 120 districts.[19] This gives us a sample of 31,680 observations (=120 districts $\times$ 12 calendar months $\times$ 11 years $\times$ 2 groups). In the case of births we have a sample size of almost 17,000, owing to

[16] The data are gathered by about 1,400 local registry offices. In Austria, a marital union gains legal status only when it is recorded by a registry office. Private religious ceremonies that are not collected in the marriage register do not confer legal status.

[17] The birth data are over a shorter time period, from July 1998 to December 2005. The reason, which is imposed for identification purposes, will be discussed below.

[18] An individual level analysis of marriage and divorce decisions in fact will require full knowledge of the stock of the married/unmarried population in Austria, which we do not have from the registers.

[19] During the sample period, Austria encompassed 98 districts with Vienna, its largest district, having a population of about 1.6 million in 2005 accounting for almost 20 percent of the entire population. For analytical purposes, then we divided Vienna into its 23 municipal districts and treated these as separate districts. This leads to the 120 districts used in the analysis.

the shorter time period over which fertility decisions are examined.

With such data, we analyze the effect of the reform on each outcome estimating the following difference-in-differences (DiD) specification

$$y_{dt} = \alpha_0 + \alpha_1 T_{dt} + \alpha_2 I(t \geq s) + (\alpha_{31} + \alpha_{32} T_{dt})\delta_t + \alpha_4 \gamma_d + \beta I(t \geq s) T_{dt} + \varepsilon_{dt}, \tag{1}$$

where d refers to district and t to time measured in months from January 1995 (or July 1998) to December 2005. The term $I(z)$ is a function indicating that the event z occurs, δ_t is a vector of time (year) dummies, γ_d is a vector of district dummies, and ε_{dt} is an i.i.d. error shock with $\mathrm{E}(\varepsilon_{dt}|T_{dt}, t) = 0$ and where $\mathrm{E}(\cdot)$ is the mathematical expectation operator. Equation (1) allows for different intercepts (when $\alpha_1 \neq 0$) and differential time trends (when $\alpha_{32} \neq 0$) for treatment and control groups.

The dependent variable is the monthly number of group-specific divorces, marriages, and births per district expressed as a fraction of the corresponding overall cell mean. The term T_{dt} is an outcome-specific treatment group. In the case of divorce, the treatment group is defined by couples with at least one dependent child and the control group by divorcing couples without dependent children. Thus, T_{dt} is equal to 1 for the observations from the series on monthly number of divorces by treated couples, and is equal to 0 for the observations from the series on the monthly number of divorces by control couples. To identify the effect of joint custody on divorce without confounding it with the impact of the changing composition of the married population (an effect driven by the potential selection into marriage), we shall also consider a variant of (1) in which we only consider divorces among married couples formed *before* the introduction of the reform.

When marriage is the outcome of interest, the treatment group is defined by couples with at least one dependent child or by childless couples in which the wife is aged 45 or less at the time of marriage. The control group comprises couples without dependent children and with women aged more than 45. The rationale behind these definitions is that only women of childbearing age or women with dependent children were potentially affected by the introduction of the custody reform, while childless older women were not. The term T_{dt} therefore is equal to 1 for the observations from the monthly series on marriages by couples with treated women, and is equal to 0 for the observations from the monthly marriage series in which women are from the control group.

In the case of births, there is no obvious comparison group. Our identification strategy then relies on comparisons with neighboring Germany as a control. In particular, we use births in the 96 Bavarian districts as control for the treatment that characterized births which occurred in the 120 Austrian districts. It is worth stressing that, besides sharing the language, the main religion, and a border of about 500 miles, Austria and Bavaria have historically strong political and economic links, which lasted for centuries until the end of WWI and shaped highly comparable legal institutions and social norms on nuptiality and illegitimacy (Knodel 1967; Shorter 1978). For robustness, we will also limit the analysis to the 64 bordering districts only, 45 for Bavaria and 19 for Austria. We restrict our analysis to the period following July 1998, since Germany (including Bavaria) introduced joint custody at that point in time. Our results, however, do not change if the analysis is performed over a longer period.

As mentioned earlier, we will experiment with alternative definitions of treatment and control groups for all outcomes, in the attempt of detecting heterogenous responses by different groups of the population and more broadly assessing the sensitivity of the estimates to such changes.

Our treatment effect is measured in (1) by β. In line with the arguments developed in Section 3, a value of β different from zero implies a departure from Becker-Coase bargains. In the case of marriages and births, a positive value of β is consistent with a mechanism that operates through greater marriage specific investment; so too if β is negative in the case of divorce. Evidence of lower marital investments instead will emerge if we find the opposite values of β for the two sets of outcomes respectively.

The three outcome variables described so far are shown in Figure 2 by treatment status. For both treatment and control groups, divorces increased from the mid 1990s to the time of the reform (top left panel). Contrary to the discussion of Section 3 on the possible strategic effect in response to the reform announcement, we do not observe any sharp increase in the number of divorces immediately after the introduction of the reform. In fact, from the introduction of joint custody onwards, divorces decreased amongst treated-group couples, while they continued to increase amongst control-group couples.

In the case of marriage (top right panel), we observe an increasing trend for the control group over the whole sample period, whereas the declining secular trend among treated couples was reversed after the introduction of the reform. The bottom left panel displays the trends in marital and nonmarital births starting from July 1998, because that was the time when Germany introduced joint child custody as the default custody arrangement. Births (whether marital or nonmarital) have declined over time, especially in Bavaria. But the reduction in marital births in Austria has leveled out since the introduction of the 2001 reform.

Social security data

To examine the effect of the reform on female employment, we use individual quarterly data from the ASSD between January 1995 and December 2005. For each quarter we have labor force status information on more than 2 million women aged between 16 and 55, for a total of about 54 million observations.

We estimate a variant of model (1) in which the new outcome variable, y_{it}, is equal to 1 if woman i is employed in quarter t, and is equal to 0 otherwise, while the new treatment variable, T_{it} is equal to 1 for all women with dependent children or women aged 45 or less, and is equal to 0 for women aged more than 45 without dependent children. Following the discussion of Section 3, a negative value of the treatment effect β is consistent with the notion that the reform led families to undertake greater marriage specific investments.

The bottom right panel of Figure 2 shows that, irrespective of treatment status, female employment rates rose from the beginning of the sample period up to 80–82 percent in the middle of 2000, declined sharply down to 72–74 percent over the next two years, and remained fairly flat thereafter. Although the employment rate fell just before the introduction of the reform as a result of the 2000/2001 recession, both treatment and control groups seemed to have been similarly affected.

Court Data

We have another unique data source that permits us to examine parents' interactions at the time of (and after) divorce. This is given by court records on approximately 7,000 divorces, which were initiated between 1997 and 2003 and completed by May 2004. Such data come from official divorce records compiled by 5 district courts in Austria that have full jurisdiction over divorce proceedings.[20]

This data set contains information on partner-specific age, education, citizenship, income, and number of previous marriages. For each dissolving marriage we also know the length of the marriage, the duration of the divorce proceedings, the type of other support obligations for each divorcing partner (both towards former spouses and towards children born in earlier unions), whether any party hired a lawyer, a judge identifier and the sex of the judge (to whom couples are randomly matched).[21] Court outcomes include

[20]The courts are Hall, Kitzbühel, Kufstein, Linz, and the Vienna district of Favoriten. Over the sample period, we have information on all divorces in Hall and Kitzbühel (which comprise more rural areas), 90 percent of all divorces in Kufstein, and 80 percent of all divorces in Linz and Favoriten in Vienna (which refer to more populated urban areas).

[21]It is worth stressing that, irrespective of the 2001 reform, over 90 percent of divorces are by mutual consent and the information on which partner initiated the divorce case is thus not collected.

alimony and child support payments, the overall length of the divorce process (in days), and the likelihood of post-divorce trials.[22]

Treatment effects and their identification differ across outcomes. For alimony payments (incidence and amount) and the length of the divorce process we estimate the intention-to-treat (ITT) effect of the introduction of the reform as well as the local average treatment effect (LATE) of the actual assignment of joint custody. Instead, for child support payments (incidence and amount), we can only identify the latter effect.

Since alimony payments and length of the divorce process are observed for all divorcing couples, regardless of whether they had children or not, we can estimate a DiD specification of the form

$$y_{it} = \alpha_0 + \alpha_1 T_{it} + \alpha_2 I(t \geq s) + (\alpha_{31} + \alpha_{32} T_{it})\delta_t + \beta_1 I(t \geq s) T_{it} + \mathbf{X}'\gamma_1 + \varepsilon_{it}, \tag{2}$$

where the subscript i denotes a dissolving couple (or, interchangeably, the father and mother in the couple) at time t. As in (1), equation (2) allows for group-specific time trends. The new treatment variable, T_{it}, is equal to 1 if the divorcing couple has at least one dependent child, and is equal to 0 otherwise. More precisely, we perform separate analyses with two different control groups. One is defined by all households that do not have dependent children (control group 1), either because they are childless or because their children are older. The other is defined only by households in which the youngest child is 18 years old or older (control group 2).

The effect of interest is captured by β_1, which measures the effect of the 2001 reform that is shared by *all* divorcing couples, irrespective of their custody arrangements, and thus identifies an intention-to-treat (ITT) effect. Estimates of $\beta_1 > 0$ in the case of alimony payments and estimates of $\beta_1 < 0$ in the case of the length of the divorce process will support the notion of greater marriage specific investments. Opposite estimates will be taken as evidence in support of a lower investment. Values of β_1 equal to zero, instead, will be consistent with Becker-Coase bargains.

Unlike the previous outcomes, child support payments (incidence and amount) are observed only for couples with children. Thus, for this outcome — and for completeness also for the case of alimony payments and divorce process length — we estimate models

[22]Weiss and Willis (1993) discuss important issues related to the data on alimony and child support payments, such as the conversion of flows to stocks, incomplete information on payment flows beyond the sample period and the time variation in legal obligations and actual payments. In our empirical work we abstract from these issues.

of the form

$$y_{it} = a_0 + a_1\delta_t + \beta_2 J_{it} + \mathbf{X}'\gamma_2 + \epsilon_{it}, \tag{3}$$

where J_{it} is an indicator function that equals 1 if the divorcing parents settle on joint custody, and equals 0 otherwise. The effect of interest in (3) is β_2, which measures the effect of the actual joint custody allocation. Its identification is made possible, with data covering only the post-reform period by those families that opt for joint custody upon divorce.

Although the reform has made joint custody the dominant arrangement, settling on joint custody is endogenous to the extent that it requires divorcing spouses to agree on the main aspects of the divorce arrangement. Put differently, JC_{it} might share some common process with the unobservables that underpin y_{it}. Because of this, we estimate (3) by two-stage least squares (2SLS) instrumenting J_{it} with judge indicators. Judges, in fact, might have different views on joint custody and different inclinations to dispense it.[23] We take advantage of the fact that the assignment of a divorcing couple to a judge is random, based on the first letter of the husband's surname. This then will allow us to identify the local average treatment effect (LATE) of joint custody for 'compliers', i.e., those couples with dependent children who were exposed to the reform and, following the judge's recommendation, opted for joint custody.

A potential problem with this approach is that judges might have an effect not only on the likelihood of a joint custody arrangement but also on the outcome itself, e.g., the amount of child support and alimony payments. To account for this possibility we consider an additional specification in which we also include judge specific effects, which are obtained from outcome specific regressions on judge dummy variables.[24]

As shown, among others, by Angrist et al. (1996) and Angrist (2006), the ITT estimate, captured by β_1 in (2), and the β_2 LATE estimate in (3) are straightforwardly linked. In particular, the former is re-scaled by the sample of compliers in the treatment group, implicitly assuming that those who did not comply received a zero impact from the reform. That is, letting $\Pi(y)$ be the compliance rate in the original treatment group for outcome y, then $\beta_2(y) = \beta_1(y)/\Pi(y)$. Thus, in spite of their different nature and the different structure they impose on the data, the two estimates capture meaningful aspects

[23]There is no evidence (whether hard or anectodal) that judges systematically changed their views on child custody as a result of the reform.

[24]In fact, to gain further variation, we performed four different of such regressions stratified by wife's age, so that we have four different coefficients per judge. Our substantive results, in any case, are insensitive to whether we use four or fewer coefficients.

of the causal impact of the custody reform on couples' behavior. We shall see — for alimony payments and the length of the divorce process — whether such a relationship is borne out by our data.

A positive estimate of β_2 in the case of alimony and child support payments, and a negative estimate in the case of the length of the divorce process and the likelihood of post-divorce trials will, in general, support the story of greater marriage specific investments. More specifically, our interpretation of a positive β_2, which according to (3) can be estimated only after marriages have been dissolved, is that fathers make their post-divorce investments in a way that they are in line with those made within marriage. An opposite (negative) sign of β_2 will support the notion of joint custody leading to lower marital investments, while estimates of β_2 equal to zero will be in line with Becker-Coase bargains.

But the story behind β_2 could be more complex for at least three reasons. First, alimony and child support payments may be perceived (both by parents and by judges) as close substitutes. In this case, it is possible that nonresidential parents increase one type of expenditure and decrease the other. Second, after the reform, nonresidential parents are likely to spend more resources on their children as a result of joint custody. Judges could then internalize this possibility and deliberately set lower child support orders.[25] Third, as in Weiss and Willis (1985), nonresidential parents may suffer a partial loss of control over allocative decisions of residential parents. As a result, they might prefer to reduce child support transfers, given that joint custody allows them to share time and resources with their children and thus helps reduce the Pareto loss induced by divorce. The results for β_2 therefore will have to be interpreted looking at all the outcomes together. This is an additional compelling reason as to why we estimate (3) also for alimony payments and the length of the divorce process.

Table 1 reports summary statistics on the court data outcomes and of the variables contained in the vector $\mathbf{X}$ in (2) and (3) and used in the analysis, before and after the reform and by treatment status.[26] Before and after the reform, the incidence of alimony

[25]Indeed, the Austrian divorce law provides flexible guidelines to judges on how much the nonresidential parent should pay the other parent for child support.

[26]The table shows we have a total of 3,533 couples with dependent children at the time of divorce in the treatment group and 2,929 couples in control group 1 and 570 in control group 2. Despite the smaller sample size, control group 2 is likely to be comprised of couples whose wives are more comparable to the those included in the treatment group in terms of the court's appraisals of alimony and child support payments. Notice that, from the whole sample, we exclude divorces occurred among couples who married after the reform (115 couples), divorces that ended with non-parental custody arrangements (25 couples) as well as cases where the wife pays alimony to the husband (6 observations). We also exclude 384 observations due to missing information on at least one of the outcomes or explanatory variables. After such selection criteria, we have a sample of 6,462 couples for our analysis on the treatment group and control group 1.

transfers remained at about 19 percent among treatment households but declined slightly among households without children (from 12 to 9 percent) and sharply among households with older children (from 41 to 27 percent), while the monetary amount of alimony payments went up, but proportionally more so for households in the treatment group. After the reform, the length in the divorce process rose across all groups, with families with older children facing the steepest increase, whereas the fraction of treatment households experiencing further trials after divorce declined. The incidence of child support payments also declined among divorcing couples with dependent children, while the amount of child support payments increased by almost 15 percent to €267 per month. Unsurprisingly, households with older children have also older mothers and fathers, whose marriages have lasted substantially longer than those of the households in the other two groups. Husbands in such older households have higher incomes, but wife's incomes are fairly similar across groups. The low fraction of men and women with university degrees or higher qualifications is well known for older cohorts in Austria and it has been documented in other studies (e.g., Fersterer and Winter-Ebmer, 1999; Ichino and Winter-Ebmer 2004).

5 Register and Social Security Data Results

Benchmark Estimates

The main estimates obtained from the three registers and the ASSD data are summarized in Table 2. In the first column, the treatment effect estimate for the divorce outcome reveals that the reform significantly reduced the number of divorces by 8.3 percent (SE=2.4 percent), which implies around 3,400 fewer divorces (95% CI=[–5,300; –1500]) over the post-reform sample period.

The second column indicates that the reform led to a significant increase in the number of marriages of 3.8 percent (SE=1.7 percent). This means that the reform induced approximately 5,500 additional marriages (with a 95% confidence interval of about 650–10,900 extra marriages) from its introduction until the end of 2005.

The birth outcome results are reported in the next two columns. We find evidence of a significant increase of 2.8 percent (SE=0.9 percent) in the number of births within marriage (third column), representing an increment of about 6,300 (95% CI=[2,300; 10,300]) babies born within marital unions as a result of the reform between July 2001 and the end of 2005. The treatment effect estimate on the number of out-of-wedlock births (fourth column) is instead lower and statistically insignificant (β=1.3 percent, SE=1.3 percent).

Finally, the reform led to a significant reduction in female employment by 2.3 percentage points (fourth column), which corresponds to almost 38,000 fewer jobs held by women during the post-reform period.

Taken together, therefore, these results offer evidence of non-Coasean bargains and are consistent with a mechanism driven by a redistribution of resources that favors men. By giving fathers a joint custody option, the reform provided men (and women) with a propensity for marriage specific investments greater incentives to marry and have children while married. Such marriages, in turn, have become more stable and fewer divorces are observed. One of the results of the lower divorce risk was greater intrahousehold specialization, as documented by lower female employment.

Sensitivity Analysis

We test the robustness of our benchmark treatment effect estimates to a number of alternative specifications. The results are summarized in Figure 3, which, for convenience, also reports the benchmark estimates for each outcome (in the left-hand side column of each panel).

In the case of divorce (top left panel), we examine the sensitivity of our benchmark regressions to either a shorter or a longer post-reform periods (second and third bar, respectively). The results vary slightly, but retain both the sign and significance of our earlier estimates. While the benchmark least squares estimates implicitly give equal weight to each of the districts, we also found a similar, marginally larger, effect using population-weighted least squares regressions (fourth bar). Pre-dating the reform to September 2000 also leads to a treatment effect estimate that is virtually identical to the corresponding benchmark estimate (not shown for convenience), suggesting little scope for announcement effects. Finally, to separate the effect of the reform on divorce from differential selection into marriage, we re-estimated the model using only marriages formed before the reform. The treatment effect on this subsample, reported in the last bar of this panel, is statistically indistinguishable from the one reported in Table 2, indicating that the changing composition of married couples due to selection into marriage could not explain the divorce results.

For marriage, the top right panel of the figure shows that changing the length of the sample by either omitting or including one year in the post-reform period does not alter our results (second and third bar, respectively). This holds true even if the window is ± 2 years. Weighted regressions (in which the weights are defined by the size of the population in each district) also yield similar results. Further, we checked our results against different definitions of the treatment group. For instance, we redefined the treatment group as married couples comprising women aged 40 or less or married couples in which women had children before marriage. We always found estimates (not shown) that are close to those presented in Table 2.

When we reduce the sample to only bordering districts (45 districts in Bavaria and 19 in Austria), the estimated effect of joint custody on marital births is still positive and of the same size as the benchmark estimate (see the second bar of the bottom left panel), but it is no longer statistically significant. Using population-weighted regressions and pre-dating the introduction of the reform to September 2000 when the Austrian Parliament started the first public discussions on the custody reform deliver larger treatment effect estimates on marital births (third and fourth bar), while changing the definition of the post-reform period does not affect the results (not shown).

Finally, lengthening or shortening the post-reform period does not alter the treatment effect results on employment (second and third bar of the bottom right panel). Our baseline estimate might reflect differences in responses to labor market changes that do not depend on the introduction of the custody reform. To account for such differential responses, we use group-specific district level unemployment rates as additional controls. As shown in the fourth bar, the estimated treatment effect estimate is essentially unchanged. This is also the case if the pre-reform period is pre-dated to September 2000 (not shown). We also checked robustness by lowering the age cutoff for the definition of treatment and control groups from to 40 years of age. This is to account for the observation discussed in Section 3, according to which women of different age or with different family obligations might have responded differently to the 2000/2001 recession. The estimate of –1.9 percent (SE=0.04 percent) are economically identical to those found with the baseline specification.

In sum, therefore, we retain the same interpretation proposed earlier. The overall evidence found with the sensitivity analysis is that the joint custody reform led to intra-household bargaining which favored men giving them greater incentives to invest within marriage.

6 Evidence on Post-Divorce Outcomes

Table 3 reports the estimates obtained from the civil courts data. Panel A shows the results from two versions of model (2), one in which the control group is given by divorcing couples without dependent children (specification (i)), the other in which the control group is given by couples with children aged 18 or more (specification (ii)).[27] Panel B instead presents the 2SLS estimates from (3). For this panel, the estimates presented under specification (iv) do account for outcome specific judge effects, while the estimates in specification (iii) do not.[28]

Looking first at panel A, we find evidence that the reform increased the likelihood of the wife receiving alimony payments from her ex-partner by almost 13 percentage points (specification (ii)). This represents a quantitatively large impact, boosting the baseline probability of alimony payments by almost 60 percent. In specification (i), instead, where the control group consists of all couples without dependent children, the same effect is again positive but not statistically significant. The greater significance level found with specification (ii) is not surprising: as mentioned in Section 4 the control group used in specification (ii) is comprised of couples whose wives are more likely to be comparable to those included in the treatment group in terms of alimony salient characteristics from the courts' viewpoint. The reform had also a strong positive impact on the amount of alimony transfers from fathers to mothers, ranging from €207 to €346 extra per month (specifications (i) and (ii) respectively), implying a twofold increase at least over the average baseline transfer. We also find a negative impact on the length of the overall divorce process. The estimated effect of 6 days reported in specification (ii) is substantial, representing a 10 percent reduction from the sample mean, but it is not statistically significant.

This evidence, therefore, partly confirms the interpretation we have given to our earlier findings. The results on alimony payments uphold the notion that the shift to joint custody was associated with a relatively greater marriage specific paternal investment and, possibly, with the realization of marital shocks that favored the husband. Instead, the lack of statistical significance for the estimates on the length of the divorce process and for the likelihood of post-divorce trials (not shown) suggests that Becker-Coase bargains may not be too far off the mark for such outcomes. The direction of these effects, however, is still consistent with intrahousehold bargaining driven by a greater paternal investment, and their magnitude indicates the potential for a substantial impact.

We repeated the whole analysis after stratifying the sample by wife's age into two groups (couples with the woman aged 35 or less and couples with the woman aged more than 35) and found no difference in the treatment effect estimates between the two groups.

[27] As discussed at the end of Section 3, we cannot rule out that these estimates partly reflect compositional changes.

[28] All the estimates shown in Table 3 are obtained from regressions that control for all the variables in **X** in (2) and (3) and listed in Table 1, except for husband's and wife's incomes. Our results, however, are not sensitive to their inclusion and also are robust to the exclusion of all the other controls.

Similarly, we performed again the analysis after dropping from the sample the couples in which the wife has at least a university qualification. We detected no significant estimate heterogeneity along any of the outcomes under study.

The LATE estimates in panel B show that accounting for the endogeneity of the joint custody decision provides us with evidence which is similar to that found in panel A for alimony payments, length of the divorce process and the likelihood of post-divorce trials. In each case, the F test on the instrument is about ten and tends to be higher when we control for judge-specific effects that vary across outcome variables and wife's age groups. Again, the probability that the ex-wife receives alimony transfers is positive, large, and significant, irrespective of whether judge specific effects are accounted for or not. For instance, the 2SLS estimate of specification (iv) implies a large impact, increasing the baseline probability of 14 percent by more than 50 percent. Interestingly, recalling our discussion in Section 4 and noting that the compliance rate for alimony is $\Pi(\text{alimony}) = 0.44$, the LATE estimate β_2 for alimony in specification (iv) yields a value of 9.5 when rescaled by $\Pi(y)$, for which we cannot statistically reject the hypothesis of equality with the 12.5 estimate of the ITT effect reported in panel A. We take this as robust evidence that increases confidence in our estimates.

Similarly, alimony transfers more than doubled from €94 to about €206–€209 per month. Although larger in absolute value than before, the effect on the length of the divorce process is again negative and statistically insignificant. In line with our previous discussions, therefore, these results broadly support the notion of non-Coasean bargains accompanied with greater parental investments.[29]

This, however, is not the case when we consider child support payments. For this outcome, in fact, we find that the reform led to a reduction in child support payments by €41–€46 per month, which correspond to a 20 percent decline with respect to the average monthly transfer. These effects, however, are not statistically significant at conventional levels.[30] This reduction is not in line with the presence of Becker-Coase bargains. But it is also inconsistent with a mechanism driven by greater parental investment, according to which we would have observed an increase in the contribution made by the father. When we look at the incidence of child support payments we also find a negative effect, but the magnitude of this effect is smaller and never statistically significant.

As anticipated in Section 4, however, the interpretation of β_2 is more complex and

[29]As before, we find no evidence of treatment effect estimate heterogeneity by wife's age and education.

[30]Notice that the evidence from least squares estimates (not reported for simplicity) is of a *positive*, albeit statistically insignificant, effect. The negative impact measured by the estimates shown in Table 3 therefore underlines the possible correlation between the unobservables that generate child custody payments and the parents' decision to settle on joint custody.

needs to be assessed looking at all outcomes together. As mentioned, alimony and child support payments might be perceived as substitutable with each other; and judges might deliberately reduce child support orders given that fathers are likely to face greater child expenditures as a result of the joint custody award and they are also likely to face greater alimony payments. To get an insight into this possible substitution, we then consider the total amount of transfers which include both alimony payments *and* child support payments together (A+CS column). The evidence is that the reform led to a substantial increase in this total transfer regardless of the definition of control group and of the type of estimation (Del Boca and Ribero 1998). The 2SLS estimates of additional €163 and €191 per month represent respectively an increase of nearly 50 and 60 percent in total transfers received by the mother, who is the parent responsible for the physical custody. As a whole, therefore, these estimates are in line with non-Coasean bargains based on a redistributive mechanism that favored fathers and gave them greater incentives to invest in marriage specific capital.

7 Conclusions

This paper examines the effects of the introduction of joint custody of children after divorce in Austria on family behavior within and outside marriage. Our results rest, for the first time, on the evidence from a number of extraordinarily rich data sources. We use divorce, marriage, and birth registers as well as administrative social security data covering the whole population to look at divorce, marriage, births, and female labor market participation. We also use unique family court records to study the effect of the reform on several post-divorce outcomes. The results are interpreted within a simple framework that allows us to tests whether Becker-Coase bargains characterize family behavior or not. If the ex-post custody allocations affect that share of marital surplus that each spouse can appropriate, then this in turn determines each spouse's investment incentive. Alternatives to Becker-Coase bargains, therefore, can be underpinned either by a world in which fathers make more marital investments as a result of the custody reform or by an environment characterized by a combination of relatively lower marriage specific paternal investments and the realization of marital shocks that favor the mother.

Along all margins of behavior, we find evidence against Becker-Coase bargains and in support of the mechanism driven by greater parental investments. In particular, our estimates show that the reform significantly reduced divorce and female employment rates and significantly increased marriage and marital birth rates. Similarly, the reform led to a substantial and statistically significant increase in the total private transfers received by mothers after divorce, with the (legally justifiable and statistically insignificant) reduction in child support payments being more than offset by the huge increase in alimony payments from their former partners.

At a policy level, the implications of our results are potentially far-reaching. The fact that family decisions respond to the introduction of joint custody arrangements both within and outside marital unions suggests the possibility of an important interaction between the legal environment surrounding the family and socially relevant family behaviors. The effects on marriages, divorces, births, and female paid work are strong indications of a substantial adjustment of individual family members to the reform. Likewise, the changes in post-divorce money transfers from fathers to mothers provide evidence of how the amount of resources available to children might vary as a result of the reform. Since many public programs are designed to protect families in general and, especially, children within them, it is important to know whether and how individuals within households change their behavior in response to family law reforms.

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Figure 1: Pareto Frontiers in Marriage and under Sole and Joint Custody

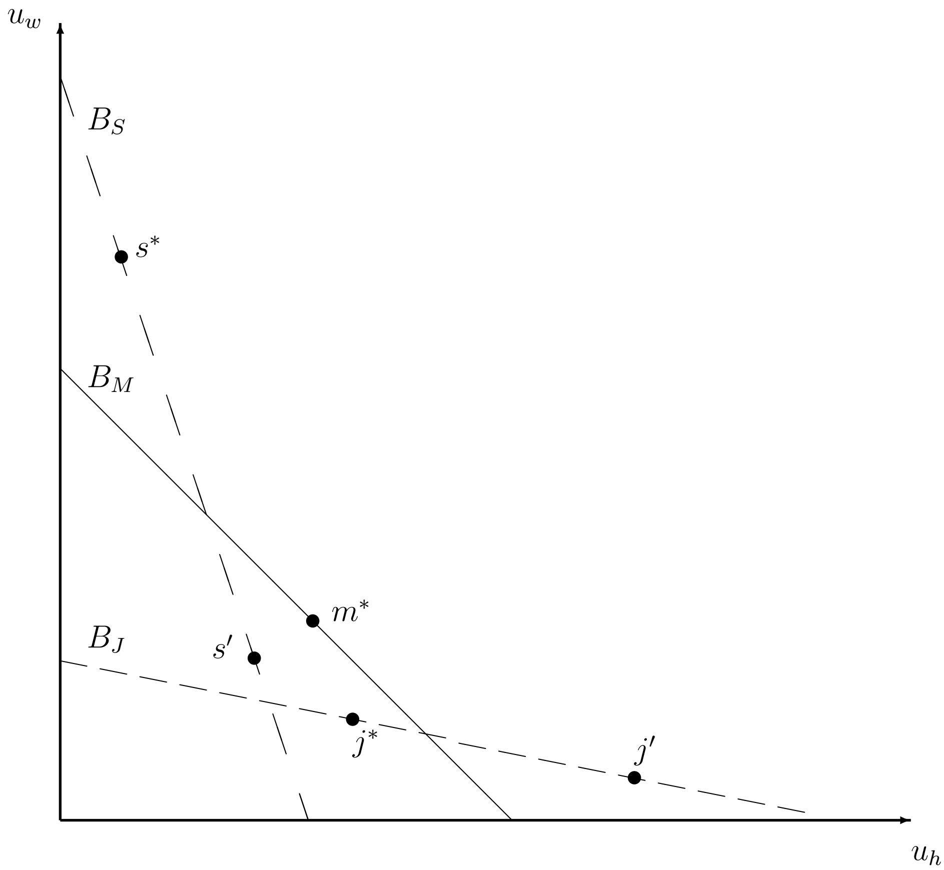
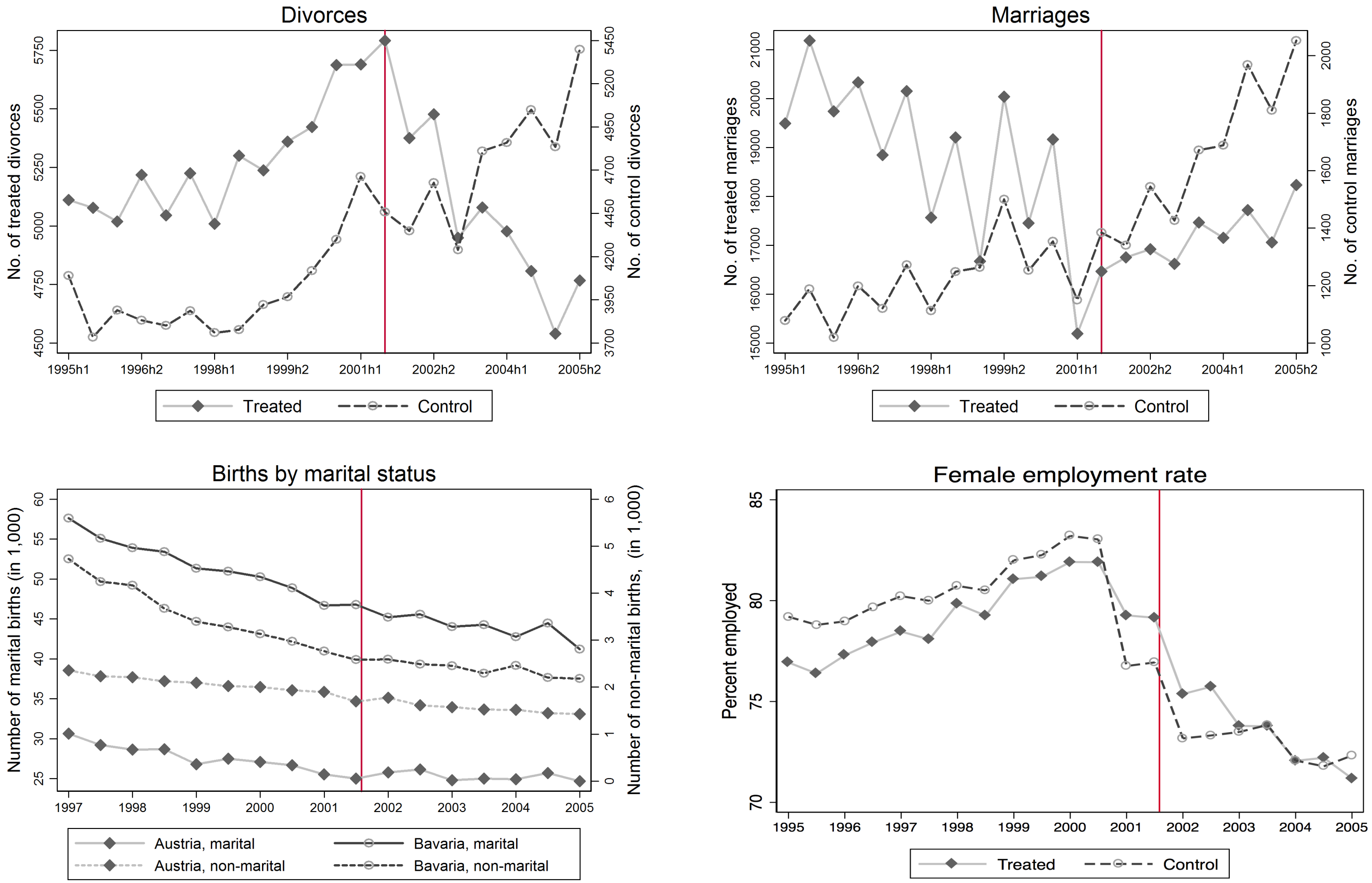


Table 1: **Summary Statistics of Variables in the Court Data**

	Control group 1		Control group 2		Treated	
	Before	After	Before	After	Before	After
Outcome variables						
Share with alimony award (%)	11.84	9.23	40.68	27.02	18.89	18.69
Alimony (€ per month)	827.64	872.63	857.35	978.00	630.85	742.91
Length (days)	71.35	77.30	66.67	82.55	63.62	71.95
Share with child support award (%)					90.45	86.73
Child support (€ per month)					235.16	266.91
Share with post-divorce trial (%)					28.44	16.02
Wife's characteristics						
Age	39.02	40.43	50.54	49.91	35.00	36.34
	(11.08)	(10.47)	(7.07)	(6.40)	(6.30)	(6.18)
Has no university degree	0.98	0.97	0.99	0.98	0.98	0.97
Monthly income (in € 100)	6.72	7.00	6.30	7.08	6.32	6.59
	(2.75)	(2.37)	(3.17)	(2.62)	(3.53)	(3.21)
Born in Austria	0.80	0.75	0.91	0.90	0.88	0.83
Number of marriages	1.36	1.35	1.11	1.08	1.12	1.10
	(0.69)	(0.63)	(0.35)	(0.27)	(0.35)	(0.32)
Husband's characteristics						
Age	40.93	42.52	52.65	52.84	37.97	38.99
	(11.31)	(11.06)	(6.96)	(6.79)	(7.39)	(7.02)
Has no university degree	0.96	0.95	0.94	0.95	0.96	0.95
Monthly income (in € 100)	14.15	13.97	16.01	15.47	14.82	15.98
	(4.37)	(4.04)	(6.27)	(4.01)	(6.27)	(6.78)
Born in Austria	0.79	0.76	0.92	0.89	0.88	0.82
Number of marriages	1.33	1.33	1.11	1.12	1.14	1.13
	(0.64)	(0.63)	(0.33)	(0.37)	(0.40)	(0.37)
Marriage duration (years)	10.21	11.62	26.98	26.25	10.65	11.23
	(10.36)	(10.48)	(7.87)	(7.86)	(6.18)	(5.94)
Number of children	0.30	0.39	1.71	1.73	0.07	0.12
aged 18 or more	(0.73)	(0.81)	(0.80)	(0.79)	(0.32)	(0.38)
Child's age					9.96	10.38
					(5.09)	(4.77)
Observations	1,824	1,105	322	248	2,388	1,145
Sum	2,929		570		3,533	

Notes: Figures are means and standard deviations (in parentheses, for continuous variables only). 'Control group 1' comprises all divorcing couples without dependent children at the time of divorce. 'Control group 2' is given by the sub-group of couples in control group 1 that have children aged 18 or more. 'Treatment group' is given by all divorcing couples with dependent children at the time of divorce.

Figure 2: Trends in Number of Divorces, Marriages, and Births, and in Female Employment Rate by Treatment Status



Notes: Top left panel shows group-specific trends in the number of divorces from 1995 to 2005. Treated divorces include all divorces with dependent children at the time of divorce. Control divorces are given by divorces without dependent children. Top right panel shows group-specific trends in the number of marriages from 1995 to 2005. Treated marriages include all marriages with wives aged less than 45 and marriages with wives of any age and with dependent children. Control marriages are given by marriages with wives aged 45 or more without dependent children. Bottom left panel shows group-specific trends in the numbers of marital and non-marital births from 1997 to 2005 for Austria and Bavaria. Bottom right panel shows group-specific trends in female employment rates from 1995 to 2005. Treated group includes all women aged less than 45 and women of any age with dependent children. Control group is given by women aged more than 45 without dependent children.

Table 2: Effect of Joint Custody on Divorce, Marriage, Fertility, and Female Employment

	Divorce Rate[a]		Marriage Rate[b]		Marital Birth Rate[c]		Non-marital Birth Rate[d]		Female Employment[e]	
β (treatment effect)	−8.3*	(2.3)	3.8*	(1.7)	2.8**	(0.9)	1.3	(1.3)	−2.3**	(0.1)
α_1	71.5**	(5.7)	198.2**	(10.4)	−50.6**	(4.8)	54.6**	(5.1)	1.8**	(0.1)
α_2	−0.4	(2.2)	−1.8	(1.1)	−2.8**	(0.9)	0.6	(1.2)	−9.9**	(0.1)
α_{31}	2.7**	(0.3)	1.4**	(0.2)	−5.2**	(0.3)	2.2**	(0.3)	−1.0**	(0.1)
α_{32}	−3.0**	(0.5)	−4.7**	(0.4)	3.3**	(0.4)	0.1	(0.4)	1.0**	(0.1)
District fixed effects	Yes		Yes		No[f]		No[f]		Yes	
Year fixed effects	Yes		Yes		Yes		Yes		Yes	
Month fixed effects	Yes		Yes		Yes		Yes		Yes	
Population size	Yes		Yes		Yes		Yes		No	
Observations	31,680		31,680		16,668		16,668		53,989,633	
R-squared	0.702		0.656		0.967		0.944		0.023	
Mean of outcome var.	5.07		13.58		56.35		18.54		0.864	

Notes: Estimates are from OLS regressions. They represent the percent change in the number of divorces, marriages, marital and non-marital births, and the percentage point change in the employment probability as a result of the joint custody reform. Robust standard errors (allowing for heteroskedasticity of unknown form) are in parentheses.

* and ** indicate that the estimates are statistically significant at the 5% and 1% level, respectively.

[a] Estimation uses data from the Austrian Divorce Register (ADR). Dependent variable is equal to the absolute number of divorces per district, month, and group (divided by the overall cell mean) from January 1995 to December 2005. Control group is made up of divorces among couples without dependent children, and divorces among couples with dependent children define the treatment group.

[b] Estimation uses data from the Austrian Marriage Register (AMR). Dependent variable is equal to the absolute number of marriages per district, month, and group (divided by the overall cell mean) from January 1995 to December 2005. Control group is made up of marriages in which the wife is aged 45 or more *and* have no dependent children. Treatment group is defined by marriages in which the wife is aged less than 45 or, regardless of the wife's age, in which there are premarital children.

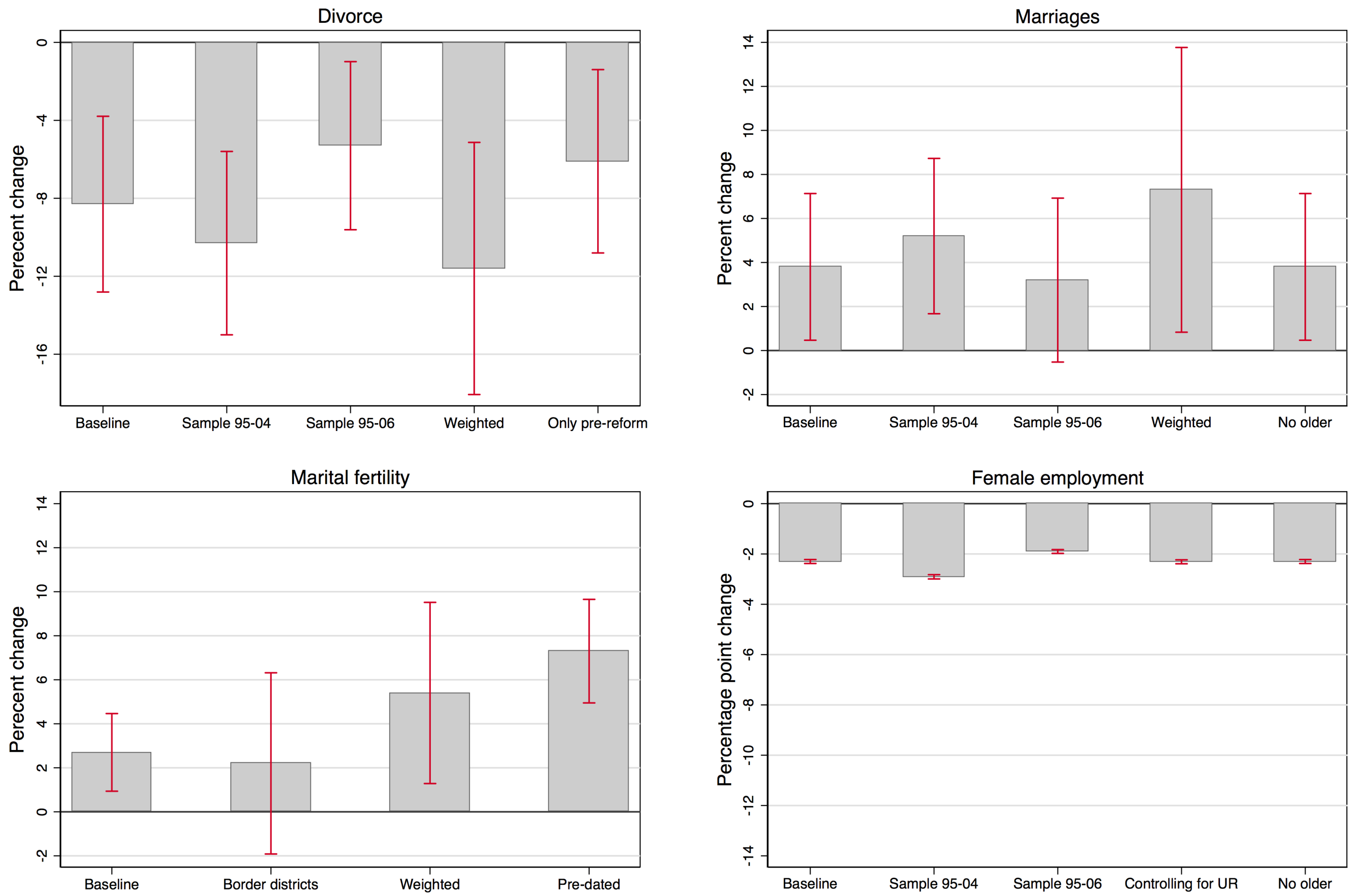
[c] Estimation uses data from the Austrian Birth Register (ABR). Dependent variable is equal to the absolute number of marital births in 121 Austrian and 96 Bavarian districts per month and group (divided by the overall cell mean) from July 1998 to December 2004. Austrian districts define the treatment group, while Bavarian districts define the control group.

[d] Estimation uses data from the ABR. Dependent variable is equal to the absolute number of non-marital births in 121 Austrian and 96 Bavarian districts per month and group (divided by the overall cell mean) from July 1998 to December 2004. Austrian districts define the treatment group, while Bavarian districts define the control group.

[e] Estimation uses quarterly unbalanced panel data on the employment status of women aged between 16 and 55 from the Austrian Social Security Database (ASSD) from January 1995 to December 2005. Dependent variable is equal to one if the individual is employed (on the first day of the quarter), and zero otherwise. Control group is made up of women aged 45 or more without dependent children. Treatment group is defined by women aged less than 45 or by women (of any age) with dependent children. Additional controls are woman's age and age squared, her past labor market experience (measured by total months in employment), and individual-level fixed effects. We include quarter fixed effects rather than month fixed effects.

[f] District fixed-effects are collinear with the treatment group indicator.

Figure 3: Robustness Checks for the Register and Social Security Data Results



Notes: Estimates obtained from OLS regressions are reported with the bars. They represent the percent change in the number of divorces, marriages, marital and non-marital births, and the percentage point change in the employment probability as a result of the joint custody reform. The whiskers in each bar show 95 percent confidence intervals based on robust standard errors (allowing for heteroskedasticity of unknown form. The left hand side of each panel reports the corresponding baseline estimate shown in Table 2. Top left panel (divorces) reports treatment effect estimates found with a shorter sample (second bar), a longer sample (third bar), population weighted least squares estimation (fourth bar), and a sample comprising only pre-reform marriages (fifth bar). Top right panel (marriages) reports treatment effect estimates found with a shorter sample (second bar), a longer sample (third bar), a population weighted least squares estimation (fourth bar), by excluding marriages with dependent children in which the wife is aged 45 or more (fifth bar). Bottom left panel (marital births) reports treatment effect estimates found with sub-sample comprising only 19 Austrian and 45 Bavarian bordering districts (second bar), population weighted least squares estimation (third bar), and pre-dating the reform by 9 months (fourth bar). Bottom right panel (female employment) reports treatment effect estimates found with a shorter sample (second bar), a longer sample (third bar), including group-specific district level unemployment rate as an additional control (fourth bar), and by excluding women with dependent children who are aged 45 or more (fifth bar).

Table 3: Effect of Joint Custody on Alimony, Child Support, and Divorce Process Length

	Alimony (A)				Child Support (CS)				Length		A+CS	
	Incidence[a]		Amount[b]		Incidence[c]		Amount[d]		Days[e]		Amount[f]	
Panel A	(i)	(ii)	(i)	(ii)					(i)	(ii)		
β_1 (treatment effect)	2.7	12.5**	206.8	345.8*					0.7	−6.2		
	(1.7)	(4.0)	(122.8)	(162.4)					(10.3)	(12.8)		
Observations	6,462	4,103	6,462	4,103					6,462	4,103		
Mean of dep. var.	0.15	0.21	110.1	151.4					69.62	67.33		
Panel B	(iii)	(iv)	(iii)	(iv)	(iii)	(iv)	(iii)	(iv)	(iii)	(iv)	(iii)	(iv)
β_2 (treatment effect)	24.6**	21.6**	206.2**	209.0**	−5.6	−5.4	−41.3	−45.5	−22.0	−14.4	163.1*	190.8*
	(7.9)	(8.1)	(75.5)	(85.6)	(4.3)	(4.2)	(29.3)	(29.5)	(29.6)	(29.6)	(80.5)	(87.1)
Judge-specific component		30.8		17.1		2.5		−0.1		0.2**		−0.2
		(20.6)		(194.0)		(3.4)		(0.1)		(0.1)		(0.2)
Mean of dep. var.	0.14	0.14	93.84	93.84	0.88	0.88	228.6	228.6	73.57	73.57	322.4	322.4
F-test (weak IV)	9.29	9.29	9.29	9.29	9.25	11.34	9.35	11.82	8.18	10.87	9.29	9.58

Notes : Estimates in panel A are obtained from single-equation regressions on observations before and after the reform (see equation (2)). Control group in specification (i) is defined by divorcing couples without dependent children at the time of divorce. Control group in specification (ii) is defined by the group of divorcing couples without dependent children but with children aged 18 or more. In both cases, treatment group is given by divorcing couples with dependent children. Estimates in panel B are obtained from 2SLS regressions on 894 couples who have dependent children and divorced after the reform (see equation (3)). The endogenous variable indicating whether the spouses agreed on joint custody arrangement or not (binary variable) is instrumented by section-judge dummies. In contrast to specification (iii), specification (iv) controls also for judge-specific effects that vary across outcome variables and brides' age-groups. Judge-specific effects are derived as follows: Based on all (control and treated) couples from the period before the reform we estimate each judge's specific effect for each outcome by brides' age (i.e., four equal sized groups). In particular, for each each group, we regress each outcome on all judge dummies and use the estimated outcome-age-specific judge effects as an additional control variable in the 2SLS estimation. In both panels, each estimation controls for a set of further covariates comprising wife's and husband's age, education, country of birth, number of marriages, their marriage duration, their number of non-minors, and the husband's further support obligations. Robust standard errors allowing for heteroskedasticity of unknown form in parentheses.

[a] Dependent variable is equal to one if the wife receives a positive award and zero otherwise. Figures give the estimated percentage point change from a linear probability model (panel A) and 2SLS models (panel B).

[b] Dependent variable is the monthly alimony payment. Figures give the estimated change in Euros from Tobit regression models (panel A) and 2SLS models (panel B).

[c] Dependent variable is equal to one if the child receives a positive award and zero otherwise. Figures give the estimated percentage point change from 2SLS models. Further controls included in estimation are child's age, child's income, and an indicator of whether the child is (predominantly) living with the mother or with the father.

[d] Dependent variable is equal to the monthly child support payment. Figures give the estimated change in Euros from 2SLS models. For further controls, see the previous note.

[e] Dependent variable is equal to the length of the divorce process (measured in days). Figures give the estimated change in days from OLS regressions (panel A) and 2SLS models (panel B). Further controls included in estimation are quarter dummy variables.

[f] Dependent variable is equal to the sum of the monthly alimony (A) and child support (CS) payment. Figures give the estimated change in Euros from 2SLS models.