The effect of marriage tax penalties and subsidies on marital status*

Hayley Fisher
University of Sydney

June 2013

Abstract

The United States personal income tax system treats married and unmarried couples differently, creating both penalties and subsidies for marriage. This paper examines the effect of these penalties and subsidies on the choice of marital status. Endogeneity between the marriage penalty a couple faces and their marital status is dealt with using a simulated instrument capturing variation in the tax code over time and between states. I find that a $1000 change in the financial incentive for marriage has a 1.7 percentage point (1.9%) effect on the probability of marriage. This effect is symmetric for subsidies and penalties and, whilst modest, is four times larger than previously estimated. Lower education groups, younger couples and couples without children are the most responsive. These results suggest that a financial incentive in the income tax system at the level of the proposed £750 transferable tax allowance in the UK is likely to cause at most a 0.25% increase in the rate of marriage amongst existing couples.

JEL classification: H31, J12, J18

Keywords: marriage, cohabitation, marriage penalty

- Evidence from marriage non-neutrality in the United States personal income tax system shows that a $1000 subsidy to marriage increases the probability of marriage (over unmarried cohabitation) by 1.9%
- Estimated effects of financial incentives are largest amongst lower education groups and younger couples with no children
- Based on these estimates, the £750 transferable tax allowance proposed by the Conservative party in 2010 would lead to a maximum 0.25% increase in the proportion of existing couples choosing to marry

*I thank Thomas Crossley and Hamish Low for their support and guidance. I am grateful to Reagan Baughman, Richard Blundell, Pramila Krishnan, Guy Lacroix and participants at various seminars and conferences for their comments and suggestions. Correspondence: Hayley Fisher, School of Economics, The University of Sydney, NSW 2006, Australia; hayley.fisher@sydney.edu.au

1 Introduction

Most developed societies have experienced a dramatic increase in the incidence of non-marital cohabitation and concurrent decline in formal marriage since the late 1960s (Kennedy and Bumpass 2008). This is reflected by an increasing proportion of births to unmarried women (Popenoe 2009). Some societies have embraced this change, creating new institutions and legal protections for those who do not wish to subscribe to the tradition of marriage (for example registered partnerships in the Netherlands and the pacte civil de solidarité in France). However, this transition has raised concerns elsewhere since there is a substantial body of research relating marriage to better health, more wealth and fewer social problems, and it is argued that formal marriage can result in better child outcomes (Waite and Gallagher 2000). It is, however, unclear to what extent these observed correlations in outcomes are in fact causal effects of marriage (Ribar 2004).

As a result, in the United States substantial federal funds have been devoted to marriage promotion, often specifically targeted at unmarried cohabiting parents. The Healthy Marriage Initiative (HMI) devoted $750 million to marriage promotion. One key programme supported, Building Stronger Families, was specifically directed at unmarried parents in romantic relationships. The HMI programmes have been accused of promoting legal marriage over all other relationship types, regardless of the consequences (Furstenberg 2007).

It is well documented that the US personal income tax system is not neutral to marriage: there are both marriage penalties and subsidies (Alm, Dickert-Conlin and Whittington 1999).[1] The extent of these penalties and subsidies has changed significantly with changes in federal and state income taxes and the Earned Income Tax Credit (EITC), and they can be a large proportion of income, especially for low income couples (Eissa and Hoynes 2000a). The Bush tax cuts[2] contained three provisions aimed at reducing and eliminating marriage penalties in the personal

[1] This phenomenon is popularly referred to as the marriage penalty, despite involving both penalties and subsidies. I follow the popular terminology of the marriage penalty in this paper.

[2] 2001 Economic Growth and Tax Relief Reconciliation Act and 2004 Working Families Tax Relief Act

income tax system (Burman, Maag and Rohaly 2002).

Concerns about declining marriage rates have also been the focus of much discussion in the United Kingdom. In their 2010 election manifesto, the Conservative party proposed that they would 'recognise marriage and civil partnerships in the tax system in the next parliament' (Conservative Party 2010), with the intention of sending a signal that relationship commitment is valued. Whilst budgetary constraints have so far prevented the introduction of a transferable tax allowance, the failure to do so is the subject of controversy within the Conservative Party (The Centre for Social Justice 2012), and the policy aim of encouraging marriage remains central to government policy (see chapter 1 of the Department for Work and Pensions strategy document 'Social Justice: transforming lives' (Department for Work and Pensions 2012)).

The proposal to privilege marriage in the tax system has been justified not through its potential incentive effects on the choice to get married, but through its symbolic value. During the 2010 general election campaign, future Prime Minister David Cameron stated:

> I don't think people are going to rush out and get married because there's a certain amount of money on offer every week. I just think that we as a country should recognise the importance of committed relationships. (Daily Telegraph 18 January 2010)

This paper presents results suggesting that, far from just being a symbolic gesture, financial benefits from marriage do provide incentives inducing some couples to choose formal marriage as opposed to non-marital cohabitation. Exploiting variation in marriage tax penalties and subsidies that exist in the United States personal income tax system, I show that a 1% increase in household income just for being married increases the probability of being married by 1.1%, and for men without college education the effect is as high as 1.3%.

Whilst these estimates are four times larger than found in the previous literature, they imply only a modest effect of the Conservative Party's proposed £750

transferable tax allowance.

Marriage taxes have been linked to various choices, including decisions about partnering and partnership type.[3] Individuals facing a marriage penalty are less likely to be married (Alm and Whittington 1995, Alm and Whittington 1999), more likely to delay marriage (Sjoquist and Walker 1995, Alm and Whittington 1996a), and more likely to divorce (Whittington and Alm 1997, Dickert-Conlin 1999), although the effects found are modest. Much of this work has focused on the margin between being single and being partnered. However, with substantial concern about the higher instability and poorer outcomes of unmarried couples, the margin between cohabitation and legal marriage is of interest. Two papers have directly addressed this margin. Alm and Whittington (1999) find that a higher marriage penalty reduces the probability of a cohabiting couple making the transition to marriage, whilst Eissa and Hoynes (2000b) report that a $1000 increase in the marriage penalty is associated with a 0.4 percentage point reduction in the probability of marriage relative to cohabitation. In addition, Ellwood (2000) considers the effect of the EITC on marital status, finding a modest, lagged response in marital status to the introduction of the EITC, with those gaining from marriage increasing their propensity to be married.

However, these estimates treat a couple's marriage penalty as exogenous. If the marriage penalty experienced by a couple is correlated with unobserved characteristics which also affect the marriage decision, these estimates may be biased. For example, if women in high marriage penalty states are less likely to work, married couples in these states will experience a low marriage penalty relative to the tax regime they face and so a simple regression will underestimate the negative effect of marriage penalties on the propensity to marry. The results presented in this paper revisit the results of Eissa and Hoynes (2000b) and directly address this issue. I exploit variation in marriage penalties between states and over time resulting from

[3]Marriage penalties in the tax system have also been linked to female labour supply (LaLumia 2008, Crossley and Jeon 2007), the allocation of unearned income between spouses (Stephens and Ward-Batts 2004), fertility (Whittington, Alm and Peters 1990) and timing of births (Dickert-Conlin and Chandra 1999).

changes to federal and state income taxes, including the EITC, to identify the effect of the potential marriage penalty on marital status.

Using data from the Current Population Survey (CPS) from 1984 to 2008, I find that a $1000 increase in the marriage penalty causes a 1.7 percentage point (1.9%) fall in the probability of marriage. Although modest, this response is symmetric, and more than four times greater than previous estimates. This masks heterogeneity: for those in the lowest educational groups the fall is as much as 2.7 percentage points, with the response declining as education increases. Young couples without children respond most to the tax incentives they face, and couples with children under five respond more than those with older children. This suggests that a financial incentive for marriage might be more successful at increasing marriage rates than previously thought. However, this paper does not consider the wider causal consequences of these additional marriages. Given the lack of convincing evidence of causal effects of marriage on, for example, children's outcomes (Crawford, Goodman, Greaves and Joyce 2012), these results do not imply that incentivising marriage through the tax system will lead to the better outcomes popularly associated with marriage, despite any resulting increase in the propensity to marry.

The paper proceeds as follows. Section 2 describes the interaction of marital status with the income tax system and explains how this might affect marital status decisions. Section 3 describes my data and sets out my empirical strategy, including construction of the simulated instrument. Results, including a number of sensitivity checks, are presented in section 4, and section 5 concludes.

2 Marriage and the US tax system

The US federal personal income tax system is not neutral towards marriage as it combines a progressive income tax schedule and taxation on the basis of family income. This means that, when married, a couple's tax liability is based on its combined income. Where only one partner works, this might lead to a subsidy to marriage since the worker's income falls into a lower tax bracket than if he were

single. However, the federal tax code also leads to marriage penalties for some couples, since the tax brackets in the married schedule are less than twice as large as those in the single schedule: if a couple earn equal incomes, their combined income will be in a higher married tax bracket than their tax brackets on the single schedule. The more equal a couple's incomes, the more likely they will experience a marriage penalty in the federal income tax system.

This phenomenon is popularly known as the marriage penalty. This term is perhaps misleading, since the system also creates subsidies – indeed, as will be seen below, only around half of all couples face a marriage penalty with the remainder earning a subsidy, and the average couple over the majority of the time period I consider receives a subsidy. Therefore, it would be more accurate to refer to this phenomenon as marriage tax non-neutrality. However, I follow the more common terminology of the marriage penalty within this article.

Crucially for this study, it is the *legal* marital status which determines whether a couple files their tax return as married or single.[4] This means that a cohabiting but unmarried couple can avoid a marriage penalty by remaining unmarried, whilst still gaining the emotional and financial benefits of living in one household. This directly corresponds to the proposed transferable marriage tax allowance in the UK: it would be legally married couples who are entitled to the benefit, and not those who merely live together.

The magnitude of marriage penalties and subsidies in the federal income tax system has varied substantially over time. This can be seen in figure 1, which shows the average marriage penalties faced by couples in a sample from the CPS (described below) from 1984 to 2008. On average, a marriage subsidy existed in the mid 1980s, which steadily decreased through the 1990s. The impact of the Bush tax cuts of 2001 and 2003, which eliminated marriage penalties resulting from differences in the lowest two tax brackets, are clearly reflected in the large

[4]A married couple may choose to file jointly or separately. The rate schedule for married filing separately is exactly half that for married filing jointly, so filing separately does not reverse the marriage penalty. In addition there are tax credits and deductions, including the Hope and American Opportunity tax credits for education, that are unavailable to couples with married filing separately status: this is not a close substitute for being married filing jointly.

fall in the average penalty faced at this time. However, this discussion obscures substantial heterogeneity, which is illustrated in figure 2 which shows the 10th, 25th, 50th, 75th and 90th percentiles of marriage penalties and subsidies over time. Whilst the median couple faced a reasonably marriage-neutral tax code, for those with marriage subsidies these could be large, and increased over the time period substantially. In contrast, those facing penalties saw those penalties increase up to the Bush tax cuts of 2001 and 2003 (partially targeted at providing marriage penalty relief).

There are two main factors driving changes in the marriage penalty over time. First, there are changes to the tax code. These include the Tax Reform Act of 1986 (TRA86), which reduced the progressivity of the income tax system and so reduced average marriage penalties, and the tax acts of 1990 and 1993 (OBRA90 and OBRA93) which extended the EITC and increased the progressivity of the tax system, especially amongst those with low incomes. This drove the increase in average experienced penalties through the 1990s. In addition to changes in the tax code, demographic change, namely the increasing labour market participation of women, contributes to the upward trend in average marriage penalties through the 1980s and 1990s. Eissa and Hoynes (2000a) estimate that from 1984 to 1997, changes to the tax code explain 55-60% of the change in the average marriage penalty, with the remainder explained by female labour force participation. It is the changes in penalties caused directly by changes to the tax code that I exploit in this paper.

In addition to the marriage penalties and subsidies created by the federal income tax system, there is further variation in the marriage penalties faced by couples generated by state income tax systems. Whilst some states have no income tax or a flat rate income tax at the individual level and so do not create any additional marriage tax or penalty, other states have a code which either amplifies or mitigates the federal marriage penalty. For example, California has a state income tax system in which the tax brackets for married couples are twice those for singles, and so only marriage subsidies are possible. These can mitigate the federal penalty. In

Figure 1: Average marriage penalties over time

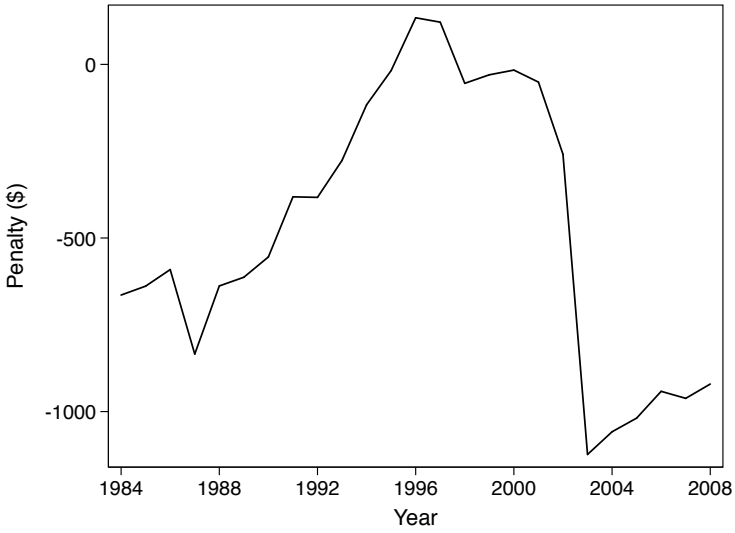
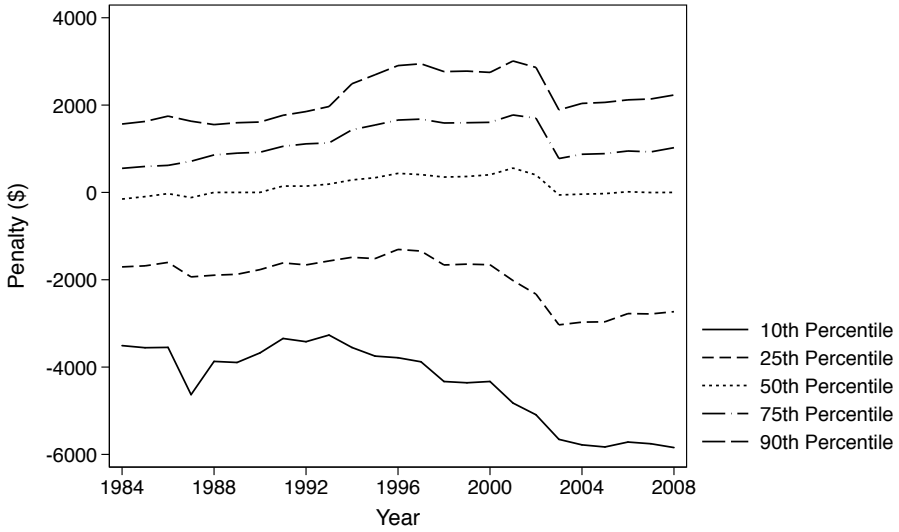


Figure 2: Distribution of marriage penalties over time



Marriage penalties calculated as difference between tax liability when married and when cohabiting, assuming children are assigned to female when cohabiting. Graph 1 shows average total (state and federal) marriage penalties by year for the CPS sample of couples used in this paper. The average penalty increases due to the expansion of the EITC. The Bush tax cuts in 2003 dramatically reduce the marriage penalties. Graph 2 illustrates percentiles of the sample of penalties and subsidies: there is substantial variance in penalties experienced.

contrast, other states have tax brackets when filing as married which are less than twice those for singles, and so can add to the federal marriage penalty.[5]

Furthermore, some states also apply their own Earned Income Tax Credit, typically as a proportion of the federal EITC. These are at varying levels and have been introduced and changed at different times in different states (see Leigh (2010) for more information). Since filing as single or married can change tax liabilities by the full amount of EITC, this adds to the variation in the marriage penalty between states in a given time period, and to the asymmetry of changes in the penalty between states and over time.

2.1 Marriage penalties and marital status

Whilst couples with higher income generally experience higher absolute marriage penalties (conditional on experiencing a marriage penalty), penalties as a percentage of income are higher for low income couples: on average 7.6% of income for couples with less than $20,000 per annum income (Eissa and Hoynes 2000a). The lower income couples are also those who have been most affected by expansions of the EITC (Holtzblatt and Rebelein 2000). The decision to marry is more commonly associated with factors other than taxes, but with potentially large tax implications of the choice of marital status, the marriage penalty (or subsidy) may well feature in people's decisions about the legal status of their relationship.[6] There are several mechanisms through which this might work.

First, the prospect of a marriage penalty may discourage an individual from entering into a cohabiting relationship regardless of its legal status. Various studies have suggested that this is the case, although the effect is modest (Alm and Whittington 1995, Alm and Whittington 1999). In contrast, the prospect of a subsidy may encourage partnering.

Second, a couple may decide to forego legal marriage in order to minimise tax li-

[5] See appendix B of Congressional Budget Office (1997) for more information.

[6] Similar penalties exist in the benefits system. There is an extensive literature considering the impact of such penalties on the prevalence of single parent families. See Dickert-Conlin (1999) and Moffitt (1998).

abilities, yet still live together in unmarried cohabitation. Alternatively, it might be the case that a couple's transition from cohabitation to legal marriage is influenced by the marriage penalty they face: the higher the penalty, the longer they remain unmarried and conversely the higher the subsidy the quicker the transition. This may simply affect the choice of whether or not to delay a marriage until the following tax year: this effect is thought to be modest (Sjoquist and Walker 1995, Alm and Whittington 1996a). Initial relationship choice and time to transition are addressed by Alm and Whittington (2003) using the Panel Study of Income Dynamics (PSID). They find that whilst the initial choice between marriage and cohabitation is not affected by the magnitude of the marriage penalty (or subsidy) faced, the choice to make the transition from cohabitation to marriage (given a coresidential relationship has been formed) is modestly influenced by the marriage penalty faced. Eissa and Hoynes (2000b) use CPS data and regress a couple's marital status on their estimated marriage penalty. They find that a $1000 higher marriage penalty is associated with a 0.4 percentage point lower probability of marriage relative to cohabitation.

Finally, the presence of a marriage penalty may influence relationship breakdown: facing a large marriage penalty might make a married couple more likely to divorce and the couple either become single, or repartner without remarrying. On the other hand, a cohabiting couple who would face a marriage subsidy if married might be more likely to separate and repartner with someon who is more willing to marry.

With the exception of the choice to remain single or enter a coresidential relationship, all of these mechanisms suggest that when a couple faces a larger marriage penalty or smaller subsidy, we are more likely to observe them as unmarried cohabitants than as a married couple: they may simply not marry, spend more years cohabiting than they otherwise would, or may divorce. So, in states and time periods where marriage penalties are higher, we should observe a higher proportion of all couples cohabiting than in states and time periods where marriage penalties are lower.

3 Data and Empirical Strategy

I follow Eissa and Hoynes (2000b) and use the March supplement of the Current Population Survey (CPS), extending the sample period to 1984 through to 2008. The CPS interviews around 50,000 households each month, principally collecting survey data on employment status in the previous week in order to produce monthly unemployment statistics. The March supplement additionally collects rich data on annual incomes, work experience and other demographic characteristics. This allows me to construct estimates of a couple's marriage penalty. Using the CPS March supplement gives a large repeated cross section. This large sample size gives sufficient observations in each year and state to implement my identification strategy.

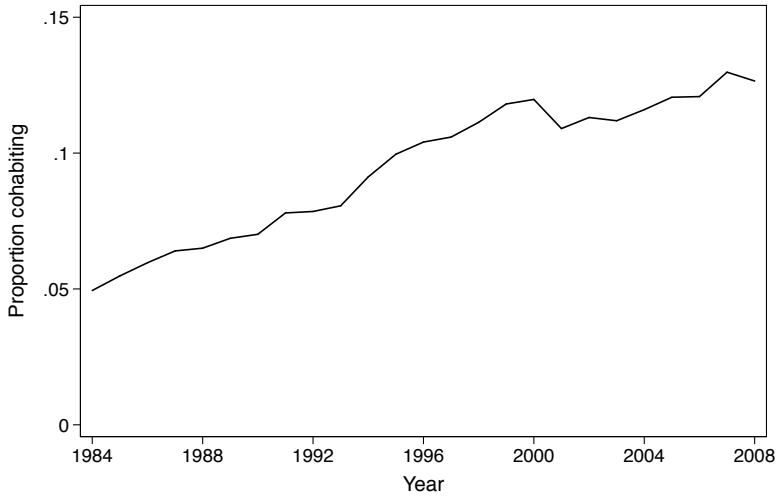
Whilst the CPS March supplement does not allow me to track transitions between cohabitation and marriage, or to observe relationship length, as argued above the mechanisms by which marriage penalties discourage marriage will affect the stocks of married and cohabiting couples in a consistent manner. The results presented below also show clearly the effect of accounting for the potential endogeneity of marital status and the marriage penalty faced in comparison to earlier results.

My sample consists of individuals in coresidential relationships where both partners are between 18 and 50 years old. A couple is married if both partners report being married and residing with a spouse. Other individuals are classified as unmarried cohabitants.[7] Prior to 1993 the CPS does not record which couples are unmarried cohabitees. Before this, cohabitation is inferred where there are two opposite sex adults who are reported as being non-relatives or roommates. Once it is possible for respondents to declare themselves to be unmarried partners, the number of cohabitors inferred in this way falls dramatically. So overall my data suggest a smooth upward trend in the proportion of my sample cohabiting, as shown in figure 3, reflecting previous research (Ellwood 2000).

In addition to individual level data, I use census data and projections from

[7]Those not residing with a partner are discarded, including those reporting being married with spouse not present.

Figure 3: Proportion of CPS sample cohabiting



Proportion of selected sample of couples who are cohabiting. In line with reported trends, the proportion of my CPS sample cohabiting rises steadily over the period. There is no spike in this proportion in 1993 when it became possible for couples to report being unmarried cohabitants.

the Bureau of Labor Statistics to calculate sex ratios. These are grouped by year, state, age and race, and are used in my analysis to control for local marriage market conditions.[8] A higher value of this sex ratio indicates better odds on the relationship market. It has been shown that being on the scarce side of the relationship market increases the probability of being partnered (Angrist 2002). However, it is not clear how, conditional on being partnered, the sex ratio will affect marital status choice. Whether an individual exploits their scarcity in the marriage market to choose to cohabit or to marry will depend on their preferences over marriage and cohabitation. To the extent that marriage provides additional stability and security through increasing the costs of separating, it might be the case that women prefer marriage due to the higher likelihood of having a resident father for any children they have. Similarly, with average male wages exceeding female wages over this time period, marriage and its associated stability may be preferred by women more than by men due to the increased household resources offered.

[8]Prior to 1990 my data is grouped into five-year age groups; thereafter they are calculated by year.

Some measure of social norms for marriage should be included as a control variable in my estimation. Unfortunately the CPS does not provide a rich set of potential controls (for example, religion is not recorded). I proxy social norms using a couple's location: living in a metropolitan statistical area (MSA), or a city within an MSA. It is expected that more traditional values will hold outside of large cities (Carter and Borch 2005).

After eliminating observations with missing data, I am left with a sample of 570,751 couples, of whom 54,758 are unmarried. Average characteristics by sex and marital status are given in table 1. Cohabitees are on average younger, slightly less educated, more likely to be nonwhite and have fewer children than those who are married. Whilst married men have higher income than their cohabiting counterparts, the opposite is true for women.

3.1 Calculation of marriage penalties

I calculate marriage tax penalties for all couples in my sample using the NBER TAXSIM program.[9] A couple's marriage penalty is the difference between their tax liabilities when married and when cohabiting. It is not immediately obvious how to calculate these tax liabilities: a married couple's liability is only observed when they are married, and so assumptions must be made about how their income and dependents would be split between them if they are unmarried.

I assume that if a married couple were unmarried, the wife would have custody of any children, and so would file as head of household, whilst the husband would file as a single. Any unearned income is split equally. This method follows Eissa and Hoynes (2000b) and Alm et al. (1999), allowing for direct comparison with previous results. This is the most commonly reported arrangement amongst cohabiting couples in my CPS sample.[10]

[9] See http://www.nber.org/~taxsim/ and Feenberg and Coutts (1993) for more details.

[10] Tax filing status is recorded from 1991-2007. In this period there are 25556 cohabiting couples with at least one dependent, of which 20790 report their filing status. In 47% of these cases the female files as head of household and male as single; this is reversed in 23% of cases. In 3% of couples both file as Head of Household (ie. more than one dependent). In all other cases at least one partner does not file a tax return.

Table 1: Descriptive statistics

	Married		Cohabiting	
Men				
Age	37.54	*(7.45)*	32.22	*(8.05)*
Education *(years)*	13.23	*(2.57)*	12.70	*(2.30)*
Non white	0.12	*(0.32)*	0.18	*(0.39)*
Earnings *($000)*	41.14	*(40.08)*	25.61	*(26.78)*
Sex ratio *(women/men)*	1.00	*(0.07)*	0.99	*(0.08)*
Women				
Age	35.62	*(7.48)*	30.57	*(8.18)*
Education *(years)*	13.23	*(2.47)*	12.91	*(2.25)*
Non white	0.12	*(0.32)*	0.17	*(0.38)*
Earnings *($000)*	16.75	*(21.98)*	17.03	*(19.17)*
Sex ratio *(men/women)*	1.01	*(0.07)*	1.02	*(0.09)*
Household				
Household earnings *($000)*	61.04	*(52.85)*	44.57	*(40.12)*
Dependent children	1.46	*(1.20)*	0.79	*(1.09)*
Marriage Penalty *($)*				
Total	-567.8	*(2977)*	-64.3	*(1845)*
Low education	-728.4	*(2472)*	-252.6	*(1874)*
Medium education	-528.7	*(2841)*	52.8	*(1721)*
High education	-343.5	*(3737)*	356.2	*(1862)*
Proportion of couples facing a marriage penalty				
Total	0.508	*(0.50)*	0.548	*(0.50)*
Low education	0.456	*(0.50)*	0.504	*(0.50)*
Medium education	0.539	*(0.50)*	0.589	*(0.49)*
High education	0.565	*(0.50)*	0.625	*(0.48)*
Observations	516003		54748	

1. Calculations from 1984-2008 CPS
2. Standard deviations in parentheses
3. Dollar amounts in 1997$
4. Low education - no college; medium education - some college; high education - advanced degree
5. Marriage penalties: negative values represent subsidies

Other approaches taken in calculating the marriage penalty are to assume that the couple arranges their affairs so as to minimise their tax liability (Feenberg and Rosen 1995), and to assume that the higher earner files as head of household. My choice reflects the empirical method, as discussed by Holtzblatt and Rebelein (2000): since women generally take primary custody of children on relationship breakdown, I assume that it is the female who will claim the child exemption. However, as has been shown (Alm and Whittington 1996b), different calculation methods can produce very different estimates of the marriage penalty, and this is shown clearly in figure 5 in the appendix which shows average penalties for my CPS sample estimated making different assumptions about the allocation of children. It is clear that assigning children to the female partner in the case of cohabitation results in a more favourable view of the marriage penalty, with an estimated average subsidy through much of the period. Quite differently, when the overall tax burden is minimised, estimated marriage penalties are much higher. This is because on average female incomes are lower than male incomes. The assignment of children, and so the child exemption, to the female partner results in a higher combined tax burden when cohabiting since the higher earner is facing a higher marginal tax rate. In contrast, when children are assigned to the higher earner, less tax is paid when cohabiting and so the calculated marriage penalty is higher. Section 4.1 shows that whilst different assumptions lead to different estimates of levels of marriage penalties (or subsidies), my results – which rely on changes in penalties over time and across states – are robust to the different calculation methods.

I calculate marriage penalties using actual earnings and numbers of children reported in the CPS: this assumes that a couple's behaviour does not change with its marital status. Whilst this is a strong assumption, it seems reasonable to assume that cohabiting individuals change their behaviour less on getting married than those who were previously single and living alone. TAXSIM calculates federal and state tax liabilities, including the EITC. All transfers are assumed to be zero.[11]

[11]This will affect the results of those on low incomes. See Eissa and Hoynes (2000b) for some suggestive results when transfers are included.

Table 1 reports the average marriage penalties by marital status, in addition to the proportion of couples facing an estimated penalty. For the marriage penalty figures, a negative number indicates a marriage subsidy. Married couples face an average marriage tax subsidy of $568, with the lowest educational group having a subsidy of $728 and the highest educational group at $344. In contrast, cohabiting couples face an average subsidy of just $64 per year. Average subsidies here are driven by the inclusion of years after the Bush tax cuts: before 2002 married couples had an average subsidy of $373, whilst cohabiting couples faced an average penalty of $170. This disguises substantial variation between educational groups: whilst the lowest education couples have an average subsidy of $253, the highest educational group suffer an average penalty of $356. Large standard deviations for all of these estimates reflect substantial variation in the subsidies and penalties faced, and this is reinforced by the fact that only around half of couples face an estimated marriage penalty. The proportion of couples facing a penalty shows a similar pattern, with a higher proportion of cohabiting than married couples facing a penalty, and the proportion facing a penalty increasing with education.

3.2 Empirical strategy

A couple chooses to marry, rather than to cohabit, if the expected joint utility from marriage exceeds that from cohabitation. Since joint utility is assumed to be increasing in income, a couple is less likely to be married as their marriage tax penalty increases. I model the expected excess joint utility from marriage over cohabitation as an index function M^*_{ist}, indexed by individual (i), state of residence (s) and year (t):

$$M^*_{ist} = \alpha Penalty_{ist} + \beta X_{ist} + \varepsilon_{ist} \tag{1}$$

where $Penalty_{ist}$ is the marriage tax penalty faced by the couple, X_{ist} is a vector of covariates affecting marriage probability, and ε_{ist} is an iid error term. M_{ist} is an indicator variable representing whether a couple is married or not:

$$M_{ist} = \begin{cases} 1 & \text{if } M^*_{ist} > 0 \\ 0 & \text{if } M^*_{ist} \leq 0 \end{cases} \tag{2}$$

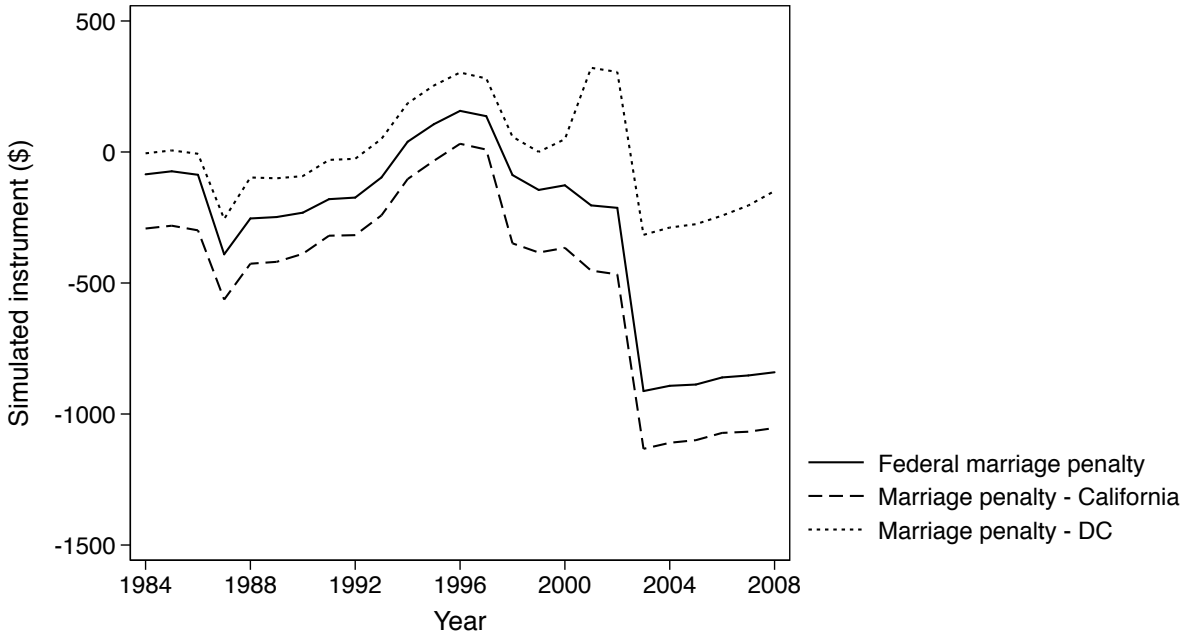
I begin by assuming that the marriage penalty a couple faces is exogenous from their marital status, and estimate using a probit model. However, these probit estimates are potentially subject to bias. The marriage penalty a couple experiences might be correlated with unobserved characteristics which influence the marital status choice. For example, couples from higher socioeconomic status backgrounds may have a stronger preference for marriage, and also be more likely to have access to higher status education (which is not reflected in recorded years of education) and so be more likely to both be employed in careers with similar earnings. This might lead to an underestimation of the discouraging effect of marriage penalties on the probability of marriage. Alternatively, couples with traditional attitudes may be more likely to marry and prefer that one partner stays at home after having children, leading to lower marriage penalties (and higher marriage subsidies): this would lead to an overestimation of the causal relationship. A further cause for concern is the uncertainty over the correct measure of the marriage penalty, with measurement error potentially leading to attenuation bias.

To address this endogeneity of marital status and the marriage penalty faced and measurement error of the marriage penalty, I also present instrumental variables estimates. I use simulated instruments: I simulate the marriage penalty in every state and year for a random sample of couples, and so calculate an average marriage penalty for each state and year which reflects the tax code and not the economic or demographic characteristics of the state and year.

I randomly select 1000 couples from my 1998 sample.[12] I then find the average penalty for this selected group in each state and for each year. I take the average penalty in each state and year, giving a set of 1275 state-year averages which

[12]Each additional observation increases the number of TAXSIM runs required by 3,825 (each state, year and for marriage, cohabitation with children and cohabitation without children), so increasing this sample size is very computationally intensive.

Figure 4: Instrument – penalties based on policy



Graph shows average marriage penalty from federal income taxes for same random sample of 1000 couples in each year. Also shown are the average marriage penalties for these couples in California (smallest average penalty) or DC (largest average penalty).

capture the policy variation in marriage penalties over the period. The results and conclusions presented in this paper are not changed if the base sample for the instrument is drawn from 1988 or 2008, or the three samples are combined, as demonstrated in section 4.1. I therefore exploit the variation in marriage penalties resulting from tax code changes and between-state variation in income taxes and the EITC.[13]

Figure 4 illustrates the instrument. The middle line shows the federal marriage penalty for a constant sample. The policy changes can be seen, but the pattern is less pronounced than in figure 1, since there the policy changes are reinforced by demographic change (see Eissa and Hoynes (2000a)). I also include the instrument for California and DC, with these two regions having the highest and lowest average marriage penalty over the period respectively. This shows the variation between states.[14] Whilst states' total marriage penalties mostly move in line with the federal penalty, there is some variation in the extent of the changes. Once again, in contrast to the 'marriage penalty' terminology, only 21% of the state year averages calculated show penalties, with the majority of state year averages being subsidies.

In the full sample, the majority of the variation in my instrument results from changes in the federal tax code over time: 91% of the variation in the instrument can be explained by a set of year dummies, versus just 8% by a set of state dummies. However, the additional variation contributed by interstate differences is valuable: introducing the state dummies when time variation has been accounted for explains 73% of the remaining variation. Moreover, the dominance of time variation is strongly driven by the Bush tax cuts. In the period before 2003, time variation accounts for 62% of variation in the instrument versus 26% for state variation, and in the period from 2003-2008 accounts for just 5% of the instrument variation compared to the 92% driven by state differences.

Whilst this strategy deals with the endogeneity problem described above, and

[13]This is a similar strategy to that used by Currie and Gruber (1996) who capture differences in children's Medicaid eligibility between states using just one sample of children.

[14]Figures 6 and 7 in the appendix show, respectively, that using different instrument calculation methods and base years changes the level of the instrument estimated, but not the pattern of changes.

with the problem of individual-level omitted variable bias, there is still concern over omitted variables which are correlated with changes in state and federal marriage penalties and differences in the probability of being married. I include state fixed effects and state specific time trends in order to capture difference levels and trends in marriage between states.

In my instrumental variables estimates the marriage penalty faced is modeled as a function of the simulated instrument $\overline{Penalty}_{st}$:

$$Penalty_{ist} = \gamma \overline{Penalty}_{st} + \delta X_{ist} + \eta_{ist} \tag{3}$$

I estimate the model by assuming that ε_{ist} and η_{ist} are jointly normally distributed with covariance ρ and using full information maximum likelihood. This effectively instruments the marriage penalty faced with the average penalty in the relevant state and time period.[15] X_{ist} is a vector of individual and local characteristics which influence the choice between marriage and cohabitation including demographic characteristics, income measures, the sex ratio, size of town or city and state fixed effects and time trends.

4 Marriage penalties and the choice to marry

Estimates of the effect of marriage penalties on marital status, split by sex, are presented in table 2. The dependent variable is equal to one if the individual is married or zero if cohabiting. The regressor of interest is the marriage penalty faced – the difference in household income for the couple between marriage and cohabitation. The marginal effects reported are for the impact of a $1000 increase in this difference.

Column 1 shows the association between the marriage penalty faced and marital status without controlling for observable characteristics.[16] A $1,000 increase in the

[15] If instead I ignore the dichotomous nature of the marital status variable and estimate using two stage least squares, the conclusions reached in this paper are unchanged.

[16] Note that this regression coefficient applies to both men and women, since the model simply regresses the marriage dummy on the estimated marriage penalty, both of which are defined for a couple rather

penalty faced is associated with a 0.6 percentage point fall in the probability of being married. Columns 2 and 4 introduce a set of controls including state fixed effects and state time trends. This reduces the correlation between the marriage penalty experienced and marital status: the estimated fall in the propensity to marry for a $1000 increase in the penalty faced is reduced from 0.6 to 0.2 percentage points for both men and women.

In columns 3 and 5, I present estimates which control for the potential endogeneity between marital status and the experienced marriage penalty. When I instrument for the penalty experienced with my simulated instrument, I find a much larger effect: a 1.7 percentage point reduction in the probability of marriage for a $1000 increase in the penalty. With average total household income of $59,462, this represents an elasticity of around -1.1. This suggests that the marriage penalty and marital status are indeed endogenous. There is selection into marriage on unobservable characteristics which also influence the marriage penalty. The correlations shown in columns 2 and 4 underestimate the magnitude of the true effect. This is in contrast to selection on observable characteristics (seen by the change in coefficients moving from column 1 to columns 2 and 4): for example, older people are more likely to be married and to have a smaller marriage penalty, so merely controlling for age reduces the estimated effect of the marriage penalty.

To be confident that the identification achieved is due to variation in the simulated instrument, it is important to consider the strength of the instrument via its significance in the penalty equation (equation 3 above). Estimation results from the penalty equation are presented in table 3. The simulated instrument is strongly positively correlated with the actual penalty faced, with a coefficient of 1.11 in both the male and female estimations. In addition, table 2 reports the chi-squared statistics from the tests of the significance of the coefficient on the instrument. Both are far in excess of 10,[17] and so I conclude that the average marriage penalty is a strong

than an individual. Differences in characteristics between men and women explain the differences between columns (2) and (4), and (3) and (5).

[17]Stock, Wright and Yogo (2002) suggest an F-statistic in excess of 10 is sufficient to be confident of a strong instrument.

Table 2: Effect of the marriage penalty on the probability of being married

		Men		Women	
	Raw	Probit	Probit (IV)	Probit	Probit (IV)
Marital status	(1)	(2)	(3)	(4)	(5)
Penalty ($000)	**-0.0058**	**-0.0023**	**-0.0167**	**-0.0023**	**-0.0170**
	(0.0003)	*(0.0003)*	*(0.0013)*	*(0.0003)*	*(0.0014)*
Education		**-0.0157**	**-0.0143**	**-0.0170**	**-0.0151**
		(0.0009)	*(0.0009)*	*(0.0013)*	*(0.0010)*
Education^2		**0.0009**	**0.0008**	**0.0009**	**0.0008**
		(0.0001)	*(0.0001)*	*(0.0001)*	*(0.0001)*
Age		**0.0095**	**0.0099**	**0.0113**	**0.0118**
		(0.0007)	*(0.0007)*	*(0.0005)*	*(0.0006)*
Age^2		**-0.0001**	**-0.0001**	**-0.0001**	**-0.0001**
		(0.0000)	*(0.0000)*	*(0.0000)*	*(0.0000)*
Black		**-0.0514**	**-0.0490**	**-0.0376**	**-0.0347**
		(0.0040)	*(0.0040)*	*(0.0040)*	*(0.0040)*
Other race*		-0.0111	-0.0094	-0.0077	-0.0063
		(0.0092)	*(0.0096)*	*(0.0073)*	*(0.0077)*
Children		**-0.0027**	**-0.0080**	-0.0011	**-0.0065**
		(0.0013)	*(0.0015)*	*(0.0012)*	*(0.0014)*
Children under 17		**0.0355**	**0.0379**	**0.0337**	**0.0361**
		(0.0013)	*(0.0015)*	*(0.0013)*	*(0.0014)*
Sex ratio		-0.0205	-0.0153	**0.0383**	**0.0324**
		(0.0117)	*(0.0105)*	*(0.0098)*	*(0.0097)*
Own wages		**0.0012**	**0.0007**	**-0.0005**	**0.0010**
		(0.0000)	*(0.0001)*	*(0.0000)*	*(0.0001)*
Partner's wages		**-0.0001**	**0.0013**	**0.0014**	**0.0009**
		(0.0001)	*(0.0002)*	*(0.0000)*	*(0.0001)*
State fixed effects	×	✓	✓	✓	✓
State time trends	×	✓	✓	✓	✓
Joint estimation	×	×	✓	×	✓
Test of instrument			**2795**		**2819**
Estimated ρ			**0.2199**		**0.2248**
Mean of married			0.90		0.90

1. Table reports marginal effects (at mean characteristics) from probit estimation. Standard errors clustered by state in parentheses
2. Other controls: quadratic and interaction terms in wages, dividend, property and unemployment income, size of city (3 dummies), state dummies, state time trends
3. Other race: not white or black
4. Bold indicates significance at 5% level
5. Joint estimation denotes joint estimation of marriage equation and penalty equation. Estimation from penalty equation presented in table 3
6. Test of instrument gives F statistic from test that the instrument's coefficient is equal to zero in the marriage penalty equation
7. 570751 observations

Table 3: Estimated penalty regressed on simulated instrument: from joint estimation

	Men	Women
Marriage penalty ($000)	(1)	(2)
Simulated instrument ($000)	**1.1122**	**1.1107**
	(0.0233)	*(0.0231)*
Education	**0.1368**	**0.1713**
	(0.0256)	*(0.0305)*
Education^2	**-0.0082**	**-0.0098**
	(0.0010)	*(0.0012)*
Age	-0.0047	0.0015
	(0.0111)	*(0.0103)*
Age^2	-0.0000	-0.0001
	(0.0002)	*(0.0001)*
Black	**0.1872**	**0.208**
	(0.0190)	*(0.0129)*
Other race*	0.0615	0.0314
	(0.0373)	*(0.0280)*
Children	**-0.3697**	**-0.3732**
	(0.0102)	*(0.0106)*
Children under 17	**0.0959**	**0.0974**
	(0.0102)	*(0.0099)*
Sex ratio	0.2603	**-0.3073**
	(0.1317)	*(0.0990)*
Own wages	**-0.0365**	**0.1031**
	(0.0011)	*(0.0025)*
Partner's wages	**0.1011**	**-0.0374**
	(0.0026)	*(0.0011)*
State fixed effects	✓	✓
State time trends	✓	✓

1. Table reports coefficients from linear regression of estimated penalty on simulated instrument and other controls. Standard errors clustered by state in parentheses
2. Other controls as in table 2
3. Other race: not white or black
4. Bold indicates significance at 5% level
5. 570751 observations

instrument for the actual penalty faced.[18]

The joint estimation of the two equations also allows for a test of the exogeneity of the actual marriage penalty faced. The correlation between the error terms (ρ) in the two equations is estimated to be significantly positive (around 0.22), supporting the hypothesis that there are unobserved characteristics which increase the likelihood of marriage and the size of the penalty faced. This may, for example, be related to higher socioeconomic status providing access to high status jobs for both men and women, increasing their marriage penalty, but at the same time providing social pressure for marriage.

Throughout this estimation I control for demographic characteristics including education, age, race, children and the sex ratio faced. Controlling for income is important since the actual marriage penalty faced is a function of income, albeit highly nonlinear. Table 2 reflects previous literature in finding that being older, white, having more dependents and having a higher income is associated with a higher probability of marriage. Marriage rates have also been falling over time. Women are more likely to be married given a favourable marriage market, whilst men do not appear to exploit a strong marriage market to choose a particular relationship status (given that they are in a coresidential relationship).

4.1 Sensitivity checks

The instrumented results presented above are strikingly different from the simple probit results. Here I show that these instrumented results suggesting a larger impact of financial incentives on the marriage decision than previously thought are robust to three important variations. First, I show that the results are robust to using different calculation methods for the marriage penalty, and second I show that changing the base year for the calculation of the simulated instrument does not change the conclusions drawn. Finally, I show that responses to marriage penalties

[18]Table 9 in the appendix provides further confidence that my instrument is appropriate. This shows results from a probit estimation of marital status as a function of the simulated instrument (directly) and other covariates. The coefficient on the simulated instrument is negative, as would be expected if living in a high penalty state and year is associated with a lower probability of marriage.

Table 4: Sensitivity check: the effect of different measures of the marriage penalty on the probability of marriage

	Men			Women		
	Raw	Probit	Probit (IV)	Raw	Probit	Probit (IV)
Measure	(1)	(2)	(3)	(4)	(5)	(6)
Children to female	**-0.0058** *(0.0003)*	**-0.0023** *(0.0003)*	**-0.0167** *(0.0013)*	**-0.0058** *(0.0003)*	**-0.0023** *(0.0003)*	**-0.0170** *(0.0014)*
Children to male	**0.0026** *(0.0004)*	**-0.0010** *(0.0004)*	**-0.0187** *(0.0013)*	**0.0026** *(0.0004)*	**-0.0011** *(0.0004)*	**-0.0190** *(0.0014)*
Children to higher earner	**0.0025** *(0.0004)*	**-0.0013** *(0.0006)*	**-0.0188** *(0.0013)*	**0.0025** *(0.0004)*	**-0.0012** *(0.0006)*	**-0.0190** *(0.0014)*
Children to minimise tax	**0.0030** *(0.0004)*	-0.0004 *(0.0005)*	**-0.0180** *(0.0012)*	**0.0030** *(0.0004)*	-0.0003 *(0.0005)*	**-0.0182** *(0.0012)*

1. Table reports marginal effects (at average characteristics) from probit estimation. Standard errors clustered by state in parentheses
2. Other controls as in table 2
3. Bold indicates significance at 5% level
4. 570751 observations
5. Instrument base year 1998 in all cases

are of a similar magnitude as those to marriage subsidies.

Table 4 outlines results equivalent to those shown in table 2 for four different penalty calculation methods. The first line is the same as in table 2, with children assigned to the female partner. Line two shows the same results but where the marriage penalty faced and simulated instruments are calculated assuming the male partner is assigned any children. Line three assigns any children to the higher earner, and line four assigns children to minimise the overall tax burden. As discussed in section 3.1 above, these different assumptions generate similar patterns of changes in the marriage penalty, but at different levels.[19]

From table 4 it is clear that the penalty calculation method used has a large impact on the standard cross sectional estimates. Assigning the children to the female as shown above suggests a modest negative relationship between the penalty faced and probability of marriage having controlled for observable characteristics. However, this is not the case for other measures of the marriage penalty. Raw

[19]Figures 5 and 6 in the appendix graph the average penalties and simulated instruments for each calculation method.

Table 5: Sensitivity check: the effect of the marriage penalty on the probability of marriage using different base years for instrument

	Men	Women
Base year	(1)	(2)
1998	**-0.0167**	**-0.0170**
	(0.0013)	*(0.0014)*
1988	**-0.0162**	**-0.0165**
	(0.0013)	*(0.0014)*
2008	**-0.0167**	**-0.0169**
	(0.0013)	*(0.0013)*
Combined	**-0.0165**	**-0.0168**
	(0.0013)	*(0.0014)*

1. Table reports marginal effects (at average characteristics) from probit estimation. Standard errors clustered by state in parentheses
2. Other controls as in table 2
3. Bold indicates significance at 5% level
4. 570751 observations
5. Marriage penalties calculated assuming woman assigned children in all cases

relationships suggest there may be a positive relationship between the penalty faced and probability of marriage, whilst any relationship becomes negative or disappears when observable characteristics are controlled for.

Once the estimated penalty is instrumented with the simulated instrument (simulated using the penalty calculation method in question), the results are broadly consistent for all marriage penalty calculations, ranging from a $1000 increase in the marriage penalty resulting in a 1.7 to 1.9 percentage point reduction in the probability of marriage. The consistency of these results suggests that attenuation bias from measurement error is a problem when estimating this relationship using a standard probit model.

Table 5 presents estimates equivalent to columns 3 and 6 of table 2 for simulated instruments with different base years. Row one replicates the 1998 base year results shown above. Rows 2 and 3 show results for base years of 1988 and 2008 respectively, and row 4 shows results for a combined simulated instrument. Again, despite these instruments having different levels (as shown in figure 7 in the appendix), the point

Table 6: Effect of the marriage penalty or subsidy on the probability of marriage for subgroups of those experiencing penalties and those experiencing subsidies.

	Raw (1)	Probit (2)	Probit (IV) (3)	Inst. (4)	ρ (5)
Men					
Penalties	**0.0271**	**0.0023**	**-0.0280**	**925**	**0.2185**
	(0.0013)	*(0.0007)*	*(0.0022)*		
Subsidies/Neutral	**0.0230**	**0.0044**	**0.0298**	**363**	**0.3130**
	(0.0008)	*(0.0004)*	*(0.0043)*		
Women					
Penalties	**0.0271**	**0.0027**	**-0.0292**	**904**	**0.2277**
	(0.0013)	*(0.0007)*	*(0.0022)*		
Subsidies/Neutral	**0.0230**	**0.0044**	**0.0290**	**362**	**0.3067**
	(0.0008)	*(0.0004)*	*(0.0042)*		

1. For the penalties group, the estimates show the effect of a $1000 increase in the marriage penalty on the probability of marriage. For the subsidies group, estimates show the effect of a $1000 increase in the marriage subsidy on the probability of marriage
2. Table reports marginal effects (at mean characteristics) from probit and instrumental variable probit estimation of marital status, in subsamples of couples facing marriage penalties and those facing subsidies. Standard errors clustered by state in parentheses
3. Bold indicates significance at 5% level
4. Subsidies includes 15,233 observations with neither a penalty nor subsidy (taxes neutral to marriage)
5. Column 4 gives F statistic from test that instrument's coefficient is equal to zero in the marriage penalty equation. Column 5 gives estimated ρ from that estimation

estimates are remarkably consistent, ranging from 1.6 to 1.7 percentage points.

When considering the policy implications of these results, it is worth noting that the UK proposal to privilege marriage in the tax system aims to provide a subsidy to marriage. It is possible that the effects of a subsidies and penalties for marriage may not be symmetric. The analysis presented above is based on the full range of subsidies and penalties, so some understanding of whether my results are driven by the encouragement of subsidies for marriage or the discouraging effect of penalties is important in understanding the policy implications.

Table 6 presents results splitting the sample into those that experience a penalty and those that experience a subsidy (or where income tax is marriage neutral). Amongst the group experiencing penalties, the raw correlations and standard probit estimation suggest a positive relationship: the higher the penalty experienced,

the higher the probability of being married. This is in contrast to the expected relationship, perhaps reflecting that amongst those facing a marriage penalty, the penalty is highest in absolute terms amongst higher income couples with similar incomes. These couples may also be more likely to marry for social reasons. In contrast, once the endogeneity of marriage and the estimated penalty is controlled for, the coefficient on the marriage penalty becomes strongly significantly negative, with a $1000 increase in penalty suggesting a 2.8 to 2.9 percentage point reduction in the marriage probability.

Amongst the subsample facing subsidies, estimates from the instrumental variables probit estimation are of a similar magnitude to those in the subsample experiencing penalties: a $1000 subsidy is associated with a 2.9 to 3.0 percentage point increase in the probability of being married. This suggests symmetric responses to marriage subsidies and penalties, and reinforces the conclusion that a marriage tax subsidy could do more to increase marriage rates than previously thought. Since there is likely to be selection into the subgroups of penalties and subsidies, my preferred results remains those from the full sample.

4.2 Results by educational group

Table 7 presents results from separate regressions within three educational groups: those with no college education (low), those with some college education (medium) and those with 4 years or more college education (high). This allows us to consider the differential effects at different socioeconomic levels, proxying for different levels of permanent income. This table reinforces the findings from the full sample, but highlights heterogeneity. Columns 1 and 2 show that the marriage penalty is more strongly correlated with marital status for the low and medium education groups than for the highest education group. The differences between groups are even more pronounced in the causal effects shown in column 3: low education individuals are 2.7 (men) or 2.2 (women) percentage points less likely to marry if their marriage penalty increases by $1000 (around 2.5% of their household earnings), whilst the

Table 7: Effect of the marriage penalty on the probability of marriage: by education

Education group	N	Raw (1)	Probit (2)	Probit (IV) (3)	Inst. (4)	ρ (5)	Mean (6)
Men							
Low	268021	**-0.0082**	**-0.0027**	**-0.0265**	**1733**	**0.2460**	0.881
		(0.0006)	*(0.0005)*	*(0.0027)*			
Medium	142081	**-0.0069**	**-0.0021**	**-0.0122**	**1374**	**0.1515**	0.903
		(0.0005)	*(0.0005)*	*(0.0028)*			
High	160649	**-0.0038**	**-0.0017**	**-0.0065**	**2749**	**0.1911**	0.944
		(0.0003)	*(0.0002)*	*(0.0015)*			
Women							
Low	265410	**-0.0097**	**-0.0023**	**-0.0220**	**1746**	**0.2219**	0.892
		(0.0005)	*(0.0005)*	*(0.0022)*			
Medium	155509	**-0.0090**	**-0.0034**	**-0.0164**	**2061**	**0.1874**	0.900
		(0.0004)	*(0.0004)*	*(0.0028)*			
High	149832	**-0.0025**	**-0.0011**	**-0.0048**	**2001**	**0.1368**	0.935
		(0.0002)	*(0.0002)*	*(0.0013)*			

1. Table reports marginal effects (at average characteristics) of marriage penalty ($1000) on marriage probability from probit estimation. Standard errors clustered by state in parentheses
2. Other controls as in table 2
3. Bold indicates significance at 5% level
4. Column 4 gives F statistic from test of coefficient on the instrument being equal to zero in marriage penalty equation
5. Low education: no college education; medium education: some college education; high education: 4 years of college education or more
6. Column 6 shows mean of married dummy for each group

response for the highest educational group is 0.7 (men) or 0.5 (women) percentage points (where $1000 is around 1.2% of their household earnings). The larger response of the lower education groups is seen in the associated elasticities: a 1% increase in household income for low education men reduces their probability of being married by around 1.3%; for the highly educated men the response is lower at 0.6%. In addition, column 4 shows that my instrument remains strong in all subsamples.[20]

4.3 Results by presence of children and age

Since many arguments promoting marriage over cohabitation are centred on the benefits for any children the couple has, it is instructive to consider the differential responses to marriage penalties for couples who have younger and older children, and those who do not have dependent children. Table 8 sets out results for these groups. The general pattern seen in the aggregate data exists for all groups: whilst there is a moderate negative association between the marriage penalty faced and the probability of being married, this becomes much larger once the endogeneity of the marriage penalty is accounted for in column 3. Parents with young children (aged less than five years) have a similar response to an $1000 increase in the marriage penalty as for the aggregate population at around 1.7 percentage points. Parents of older dependent children respond somewhat less, but still far in excess of previous estimates at 1.1 percentage points.[21] In all cases, column 4 shows that the instrument remains strong in each subgroup.

The group most responsive to the marriage penalty regime they face is couples with no children. A lower proportion of these couples are married (81% versus 94% of those with children), and a $1000 increase in the marriage penalty faced results

[20]The results in table 7 use the simulated instrument calculated from the aggregate random sample. Table 10 in the appendix shows results when the simulated instrument is calculated only from couples with the relevant education level. These results are close to using the aggregate instrument, giving the same qualitative conclusions.

[21]Table 8 presents results using the aggregate simulated instrument used for the full sample above. Table 11 in the appendix shows results when the simulated instrument is calculated from a random sample with the relevant characteristic. These results are very similar to using the aggregate instrument.

Table 8: Effect of the marriage penalty on the probability of marriage: by presence of children

Demographic group	N	Raw (1)	Probit (2)	Probit (IV) (3)	Inst. (4)	ρ (5)	Mean (6)
Men							
Some children	407437	**-0.0032**	**-0.0033**	**-0.0140**	**2064**	**0.2498**	0.940
		(0.0002)	*(0.0002)*	*(0.0012)*			
Young children	187617	**-0.0033**	**-0.0038**	**-0.0173**	**1197**	**0.2931**	0.932
		(0.0003)	*(0.0003)*	*(0.0023)*			
Older children	219820	**-0.0037**	**-0.0027**	**-0.0110**	**2730**	**0.2115**	0.947
		(0.0002)	*(0.0002)*	*(0.0012)*			
No children	163314	**-0.0040**	**-0.0108**	**-0.0303**	**2343**	**0.1024**	0.815
		(0.0010)	*(0.0008)*	*(0.0061)*			
No children (under 35)	75575	0.0021	**-0.0174**	**-0.0368**	**1548**	0.0544	0.721
		(0.0029)	*(0.0017)*	*(0.0147)*			
No children (over 35)	87739	**-0.0037**	**-0.0063**	**-0.0213**	**1766**	**0.1492**	0.895
		(0.0006)	*(0.0007)*	*(0.0045)*			
Women							
Some children	407437	**-0.0032**	**-0.0032**	**-0.0139**	**2052**	**0.2504**	0.940
		(0.0002)	*(0.0002)*	*(0.0012)*			
Young children	187617	**-0.0033**	**-0.0037**	**-0.0172**	**1135**	**0.2941**	0.932
		(0.0003)	*(0.0003)*	*(0.0023)*			
Older children	219820	**-0.0037**	**-0.0027**	**-0.0109**	**2755**	**0.2108**	0.947
		(0.0002)	*(0.0003)*	*(0.0012)*			
No children	163314	**-0.0040**	**-0.0112**	**-0.0324**	**2395**	**0.1105**	0.815
		(0.0010)	*(0.0009)*	*(0.0060)*			
No children (under 35)	75575	0.0021	**-0.0168**	**-0.0399**	**1465**	0.0651	0.721
		(0.0029)	*(0.0018)*	*(0.0149)*			
No children (over 35)	87739	**-0.0037**	**-0.0071**	**-0.0211**	**1756**	**0.1326**	0.895
		(0.0006)	*(0.0008)*	*(0.0046)*			

1. Table reports marginal effects (at average characteristics) of marriage penalty ($1000) on marriage probability from probit estimation. Standard errors clustered by state in parentheses
2. Other controls as in table 2
3. Bold indicates significance at 5% level
4. Column 4 gives F statistic from test of coefficient on the instrument being equal to zero in marriage penalty equation
5. Some children: any dependent children; young children: child under five; older children: children but not under five; No children: no dependent children
6. No children (under 35) comprises couples in which the woman is aged 35 or under; no children (over 35) comprises couples in which the woman is aged 36 or more
7. Column 6 shows mean of married dummy for each group

in a 3 percentage point reduction in the probability of marriage. This may reflect a lower social value of marriage amongst couples who do not have children. The population of couples with no dependent children is bimodal in age. If this group is split into couples where the female is aged 35 or under, and those where she is over 35, we can see that the effect exists in both age groups.

In contrast to the other sub-groups, there is no raw association between marital status and the experienced marriage penalty for younger couples without children. In addition, the estimates in columns 2 and 3 are not significantly different from each other (and ρ estimated in the instrumental variable probit is insignificantly different from zero),[22] suggesting that these couples select themselves into marriage mainly based on observable characteristics. Therefore the estimated effect from the standard probit model of 1.7 percentage points is the preferred estimate of the effect of the marriage penalty on marital status, still placing this group as one of the more responsive subgroups. If these younger couples are yet to have children but are responsive to the financial incentives for marriage, then increasing such incentives could increase the proportion of children born to married rather than cohabiting couples.

Together these results suggest that the potential for financial incentives to induce more marriages is larger than previously thought. Whilst the subset of the population who respond most are childless couples, this is also the group who are most likely to subsequently have children, so such a policy might reduce extramarital childbearing.

5 Conclusion

This paper estimates the extent to which marriage penalties discourage marriage in favour of unmarried cohabitation, and conversely to what extent marriage subsidies encourage marriage. It explicitly deals with the endogeneity between marital status and the marriage penalty experienced by using a simulated instrument. I find that

[22] In all other subgroups, the instrumental variable probit result is significantly different from the standard probit result and so the preferred estimate is that from the instrumental variable probit estimation.

a $1,000 increase in the marriage penalty reduces the probability of marriage by around 1.7 percentage points or 1.9%, an income elasticity of -1.1. This estimate is more than four times greater than that found in the existing literature, though remains small as a policy lever. Providing financial incentives can affect marital status decisions and so increase the marriage rate.

This result has strong implications for policy in the context of concern about declining marriage rates and subsequent adverse effects. Although there are many more personal and likely more important reasons for getting married, there is real potential for increasing the marriage rate through the provision of financial incentives, going beyond any symbolic effect. Whether this effect is economically, rather than statistically, significant depends upon the magnitude of any tax benefit. The proposal that has received the most attention in the UK suggests that up to £750 of a married individual's unused personal allowance could be transferred to their spouse, giving a maximum benefit of £150 per annum for a married couple (Adam, Brewer, Browne and Phillips 2010). This would be withdrawn in households with a high rate taxpayer, and so concentrated amongst lower income tax paying households. Since the prevalence of cohabitation amongst couples in coresidential relationships is higher in the UK at just over 20% of all partnerships,[23] it is possible that the UK has more marginal couples whose behaviour may be changed. However, given the elasticities calculated in this paper, the modest value of the transferable tax allowance could be expected to increase the proportion of cohabiting couples who are married by at most 0.25%.[24]

However, this paper does not provide evidence to suggest whether the additional marriages caused by a financial incentive are desirable. Indeed, the evidence suggests that the part of the population of couples who is most responsive to such a financial incentive is couples without children. This casts doubt on the ability of

[23]UK General Household Survey data, 2004-2007. Calculated from data in table 6, p.16 of Beaujouan and Bhrolchain (2011).

[24]The IFS estimated a maximum average household income increase of 0.18% from the policy (Institute for Fiscal Studies 9 April 2010). Given the marriage penalty elasticity of -1.1 and a marriage rate of just under 80% of cohabiting couples, this implies a maximum 0.25% increase in the proportion of married couples.

this policy to target child outcomes. Whilst there is a wealth of evidence suggesting strong correlations between marital status and various beneficial outcomes (Waite and Gallagher 2000), the causality is often not clear (see Ribar (2004) for a survey of some evidence). There should be particular concern about the marriages induced by financial incentives since they will be marginal marriages: a couple motivated by the financial incentive may not change any behaviour beyond gaining the marriage certificate and the tax benefit. On the other hand, if the process of getting married is of itself transformative, either to the individuals themselves or to the attitudes they encounter from others, then a direct financial subsidy to marriage may have desirable outcomes.

A further issue raised by the analysis in this paper is that couples are able to, and choosing to, alter their legal marital status to affect their tax liabilities. It is widely agreed that the tax code should treat families with equal income equally to achieve horizontal equity (Eissa and Hoynes 2000a) and this is reflected in the married schedule in the US income tax system. With the increasing prevalence of cohabitation, using legal marriage as the marker by which families are defined may no longer achieve this goal.

References

Adam, Stuart, Mike Brewer, James Browne, and David Phillips, "Taxes and Benefits: The Parties' Plans," 2010 Election Briefing Note 13, Institute for Fiscal Studies 2010.

Alm, James and Leslie A. Whittington, "Income taxes and the marriage decision," *Applied Economics*, 1995, *27* (1), 25–31.

____ **and** ____ , "Income taxes and the timing of marital decisions," *Journal of Public Economics*, 1996, *64* (2), 219–240.

____ **and** ____ , "The rise and fall and rise... of the marriage tax," *National Tax Journal*, 1996, *49* (4), 571–589.

____ **and** ____ , "For Love or Money? The Impact of Income Taxes on Marriage," *Economica*, August 1999, *66* (263), 297–316.

____ **and** ____ , "Shacking up or shelling out: Income taxes, marriage and cohabitation," *Review of Economics of the Household*, 2003, *1* (1), 169–186.

____ **, Stacy Dickert-Conlin, and Leslie A. Whittington**, "Policy Watch: The Marriage Penalty," *Journal of Economic Perspectives*, Summer 1999, *13* (3), 193–204.

Angrist, Josh, "How Do Sex Ratios Affect Marriage and Labor Markets? Evidence from America's Second Generation," *The Quarterly Journal of Economics*, 2002, *117* (3), 997–1038.

Beaujouan, Eva and Maire Ni Bhrolchain, "Cohabitation and marriage in Britain since the 1970s," *Population Trends*, 2011, *145.*

Burman, Leonard E., Elaine Maag, and Jeff Rohaly, "The Effect of the 2001 Tax Cut on Low- and Middle-Income Families and Children," *Tax Notes*, 2002, *96* (2).

Carter, J. Scott and Casey A. Borch, "Assessing the Effects of Urbanism and Regionalism on Gender-Role Attitudes, 1974–1998," *Sociological Inquiry*, 2005, *75* (4), 548–563.

Congressional Budget Office, "For better or for worse: marriage and the federal income tax," CBO Study, CBO June 1997.

Conservative Party, "Invitation to Join the Government of Britain: The Conservative Manifesto 2010," General Election Manifesto 2010.

Crawford, Claire, Alissa Goodman, Ellen Greaves, and Robert Joyce, "Cohabitation, marriage and child outcomes: an empirical analysis of the relationship between marital status and child outcomes in the UK using the millennium cohort study," *Child and Family Law Quarterly*, Jul 2012, *2.*

Crossley, Thomas F. and Sung-Hee Jeon, "Joint taxation and the labour supply of married women: evidence from the Canadian tax reform of 1988," *Fiscal Studies*, 2007, *28* (3), 343–365.

Currie, J. and J. Gruber, "Health insurance eligibility, utilization of medical care, and child health," *The Quarterly Journal of Economics*, 1996, *111* (2), 431–466.

Daily Telegraph, "Tory tax break for marriage 'more about message than money'," 18 January 2010.

Department for Work and Pensions, "Social Justice: transforming lives," DWP Strategy Document March 2012.

Dickert-Conlin, Stacy, "Taxes and transfers: their effects on the decision to end a marriage," *Journal of Public Economics*, 1999, *73* (2), 217 – 240.

___ **and Amitabh Chandra**, "Taxes and the timing of births," *Journal of Political Economy*, February 1999, *107* (1), 161–177.

Eissa, Nada and Hilary Hoynes, "Explaining the fall and rise in the tax cost of marriage: the effect of tax laws and demographic trends, 1984-1997," *National Tax Journal*, September 2000, *53* (2), 683–712.

___ **and** ___ , "Tax and transfer policy and family formation: marriage and cohabitation," 2000.

Ellwood, David T., "The impact of the Earned Income Tax Credit and social policy reforms on work, marriage and living arrangements," *National Tax Journal*, December 2000, *54* (4), 1063–1106.

Feenberg, Daniel R. and Harvey S. Rosen, "Recent developments in the marriage tax," *National Tax Journal*, March 1995, *48* (1), 91–101.

Feenberg, Daniel Richard and Elizabeth Coutts, "An introduction to the TAXSIM model," *Journal of Policy Analysis and Management*, Winter 1993, *12* (1), 189–194.

Furstenberg, Frank F., "Should government promote marriage?," *Journal of Policy Analysis and Management*, 2007, *26* (4), 951–964.

Holtzblatt, Janet and Robert Rebelein, "Measuring the effect of the EITC on marriage penalties and bonuses," *National Tax Journal*, 2000, *53* (4), 1107–1134.

Institute for Fiscal Studies, "Conservatives to recognise one third of marriages in the tax system," Press Release, Institute for Fiscal Studies 9 April 2010.

Kennedy, Sheela and Larry Bumpass, "Cohabitation and children's living arrangements: new estimates from the United States," *Demographic Research*, 2008, *19* (47), 1663–1692.

LaLumia, Sara, "The effects of joint taxation of married couples on labor supply and non-wage income," *Journal of Public Economics*, 2008, *92*, 1698–1719.

Leigh, Andrew, "Who Benefits from the Earned Income Tax Credit? Incidence among Recipients, Coworkers and Firms," *The B.E. Journal of Economic Analysis & Policy*, 2010, *10* (1 (Advances)), article 45.

Moffitt, Robert A., *Welfare, the Family, and Reproductive Behavior:Research Perspectives*, The National Academies Press, 1998.

Popenoe, David, "Cohabitation, Marriage, and Child Wellbeing: A Cross-National Perspective," *Society*, 2009, *46*, 429–436.

Ribar, David C., "What do social scientists know about the benefits of marriage? A review of quantitative methodologies," Discussion Paper 998, IZA January 2004.

Sjoquist, David L. and Mary Beth Walker, "The marriage tax and the rate and timing of marriage," *National Tax Journal*, 1995, *48* (4), 550–564.

Stephens, Melvin and Jennifer Ward-Batts, "The impact of separate taxation on the intrahousehold allocation of assets: evidence from the UK," *Journal of Public Economics*, 2004, *88*, 1989–2007.

Stock, James H., Jonathan Wright, and Motohiro Yogo, "A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments," *Journal of Business & Economic Statistics*, 2002, *20* (4), 518–29.

The Centre for Social Justice, "It is time to back marriage," CSJ Report February 2012.

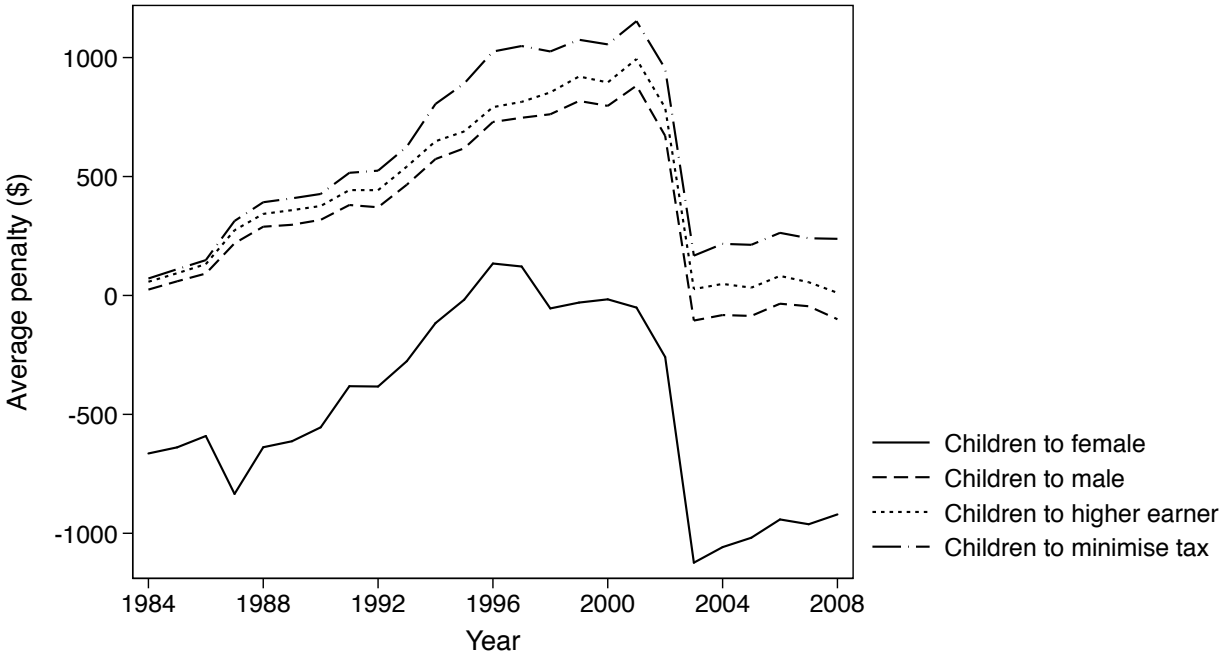
Waite, Linda J. and Maggie Gallagher, *The Case for Marriage*, Broadway Books, 2000.

Whittington, Leslie A. and James Alm, "'Til death or taxes do us part: the effect of income taxation on divorce," *Journal of Human Resources*, 1997, *32* (2), 388–412.

Whittington, Leslie A, James Alm, and H Elizabeth Peters, "Fertility and the Personal Exemption: Implicit Pronatalist Policy in the United States," *American Economic Review*, June 1990, *80* (3), 545–56.

Appendix

Figure 5: Average marriage penalties over time: different calculation methods



Average marriage penalties by year for the CPS sample, calculated assuming different allocations of children if cohabitation occurred.

Table 9: Effect of simulated instrument on marital status

Marital status	Men	Women
Simulated instrument ($000)	**-0.0167**	**-0.0169**
	(0.0013)	*(0.0014)*
Education	**-0.0161**	**-0.0176**
	(0.0009)	*(0.0014)*
Education^2	**0.0009**	**0.0010**
	(0.0001)	*(0.0001)*
Age	**0.0096**	**0.0114**
	(0.0007)	*(0.0005)*
Age^2	**-0.0001**	**-0.0001**
	(0.0000)	*(0.0000)*
Black	**-0.0525**	**-0.0386**
	(0.0039)	*(0.0040)*
Other race*	-0.0102	-0.0065
	(0.0091)	*(0.0073)*
Children	-0.0016	-0.0000
	(0.0013)	*(0.0012)*
Children under 17	**0.0348**	**0.0329**
	(0.0013)	*(0.0013)*
Sex ratio	-0.0190+	**0.0363**
	(0.0115)	*(0.0097)*
Own wages	**0.0012**	**-0.0006**
	(0.0000)	*(0.0000)*
Partner's wages	**-0.0003**	**0.0014**
	(0.0001)	*(0.0000)*
State fixed effects	✓	✓
State time trends	✓	✓

1. Table reports marginal effects (at mean characteristics) from probit estimation of marital status on full set of controls and simulated instrument (in place of estimated penalty). Standard errors clustered by state in parentheses
2. Other race: not white or black
3. Bold indicates significance at 5% level

Table 10: Robustness check: effect of the marriage penalty on the probability of marriage by educational group. Comparing aggregate instrument with instrument simulated using observations from that subgroup.

Education	Aggregate instrument			Instrument from subgroup		
Group	Probit (IV)	Inst.	ρ	Probit (IV)	Inst.	ρ
Men						
Low	**-0.0265**	**1733**	**0.2460**	**-0.0269**	**1467**	**0.2497**
	(-0.0027)			*(-0.0027)*		
Medium	**-0.0122**	**1374**	**0.1515**	**-0.0117**	**1464**	**0.1460**
	(-0.0028)			*(-0.0027)*		
High	**-0.0065**	**2749**	**0.1911**	**-0.0059**	**2696**	**0.1709**
	(-0.0015)			*(-0.0015)*		
Women						
Low	**-0.0220**	**1746**	**0.2219**	**-0.0207**	**1522**	**0.2083**
	(-0.0022)			*(-0.0024)*		
Medium	**-0.0164**	**2061**	**0.1874**	**-0.0150**	**2107**	**0.1676**
	(-0.0028)			*(-0.0027)*		
High	**-0.0048**	**2001**	**0.1368**	**-0.0051**	**1834**	**0.1456**
	(-0.0013)			*(-0.0014)*		

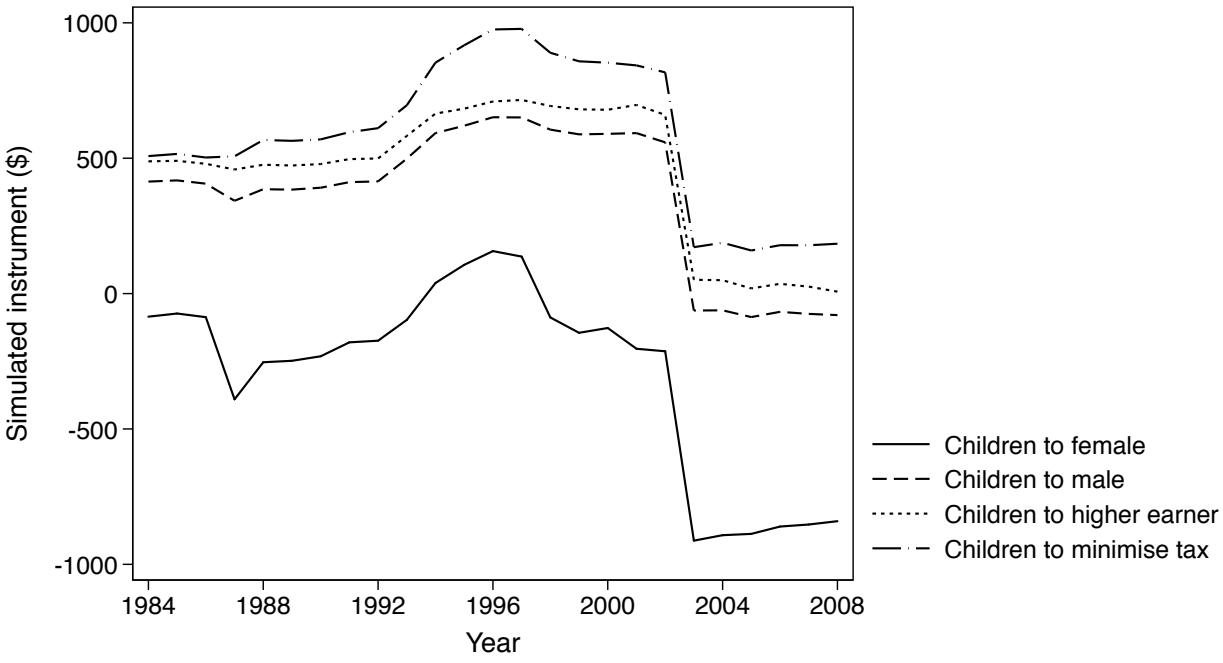
1. Table reports marginal effects (at average characteristics) of marriage penalty ($1000) on marriage probability from IV probit estimation using aggregate instrument (as in table 7) and with instrument simulated only with observations from relevant subgroup. Standard errors clustered by state in parentheses
2. Controls as in table 7
3. Bold indicates significance at 5% level
4. Low education: no college education; medium education: some college education; high education: 4 years of college education or more

Table 11: Robustness check: effect of the marriage penalty on the probability of marriage by presence of children. Comparing aggregate instrument with instrument simulated using observations from that subgroup.

	Aggregate instrument			Instrument from subgroup		
	Probit (IV)	Inst.	ρ	Probit (IV)	Inst.	ρ
Men						
Some children	**-0.0140**	**2064**	**0.2498**	**-0.0131**	**2091**	**0.2337**
	(0.0012)			*(0.0012)*		
Young children	**-0.0173**	**1197**	**0.2931**	**-0.0169**	**1256**	**0.2854**
	(0.0023)			*(0.0024)*		
Older children	**-0.0110**	**2730**	**0.2115**	**-0.0102**	**2596**	**0.1946**
	(0.0012)			*(0.0012)*		
No children	**-0.0303**	**2343**	**0.1024**	**-0.0271**	**1976**	**0.0857**
	(0.0061)			*(0.0043)*		
No children (under 35)	**-0.0368**	**1548**	0.0544	**-0.0227**	**1069**	0.0150
	(0.0147)			*(0.0106)*		
No children (over 35)	**-0.0213**	**1766**	**0.1492**	**-0.0201**	**1091**	**0.1385**
	(0.0045)			*(0.0041)*		
Women						
Some children	**-0.0139**	**2052**	**0.2504**	**-0.0131**	**2091**	**0.2360**
	(0.0012)			*(0.0012)*		
Young children	**-0.0172**	**1135**	**0.2941**	**-0.0171**	**1194**	**0.2930**
	(0.0023)			*(0.0024)*		
Older children	**-0.0109**	**2755**	**0.2108**	**-0.0102**	**2627**	**0.1935**
	(0.0012)			*(0.0012)*		
No children	**-0.0324**	**2395**	**0.1105**	**-0.0279**	**1913**	**0.0879**
	(0.0060)			*(0.0044)*		
No children (under 35)	**-0.0399**	**1465**	0.0651	**-0.0222**	**1069**	0.0152
	(0.0149)			*(0.0110)*		
No children (over 35)	**-0.0211**	**1756**	**0.1326**	**-0.0196**	**1080**	**0.1192**
	(0.0046)			*(0.0042)*		

1. Table reports marginal effects (at average characteristics) of marriage penalty ($1000) on marriage probability from IV probit estimation using aggregate instrument (as in table 7) and with instrument simulated only with observations from relevant subgroup. Standard errors clustered by state in parentheses
2. Controls as in table 8
3. Bold indicates significance at 5% level
4. Some children: any dependent children; young children: child under five; older children: children but not under five; No children: no dependent children
5. No children (under 35) comprises couples in which the woman is aged 35 or under; no children (over 35) comprises couples in which the woman is aged 36 or more

Figure 6: Simulated instrument: different calculation methods



Simulated instrument calculated using different allocations of children if cohabitation occurred.

Figure 7: Simulated instrument: different base years

Simulated instrument using different base years for simulation sample.