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States of Disunion: American Marriage and Divorce, 1867–1906

By

Alexander Fort Roehrkasse

A dissertation submitted in partial satisfaction of the

requirements for the degree of

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in

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of the

University of California, Berkeley

Committee in charge:

Professor Neil Fligstein, Chair
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Abstract

States of Disunion: American Marriage and Divorce, 1867–1906

by

Alexander Fort Roehrkasse

Doctor of Philosophy in Sociology

University of California, Berkeley

Professor Neil Fligstein, Chair

This dissertation comprises three essays on the historical relationship between capitalist development, state formation and marriage and divorce patterns in the United States.

The first examines the effects of liberalizing women's property rights on divorce. In the late nineteenth century, most American states gave married women new rights to own and control assets and earnings. Using administrative data on most U.S. divorces between 1867 and 1906, I show that rights transfers gave women financial independence from husbands that enabled them to exit undesirable unions at greater rates. However, husbands also filed for more divorces following women's economic gains, suggesting that the violation of traditional gender norms of household governance also destabilized unions.

The second essay explores the legal behavior of men and women who faced significant restrictions on divorce. Before the 1970s, U.S. states allowed legal divorces only for specified causes. Examining data on the causes cited by divorce seekers, I document the routinization of divorce procedure over historical time. I also exploit legal changes to demonstrate that individuals adapted to divorce regulations by changing the causes they cited. Strategic legal behavior was widespread but differed by gender, with men being more prone toward routinization and women being more likely to adapt to new rules.

The third essay reflects critically on the quality of available data on nineteenth-century marriages. I compare vital records of marriages to census microdata on marital duration, showing that the latter exhibit significant measurement error. Despite growing interest by elite state actors in measuring marriage and divorce at the population level, vital recording, which had administrative origins in the clarification of individual legal statuses, seems to have elicited more reliable participation in official knowledge projects. I analyze the extent and distribution of mismeasurement, which has consequences for both the study of American political development and the validity of quantitative historical research on the family.

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CONTENTS

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Throughout the life of this dissertation, Sarah Louise Cowan has been my partner. It has benefited in untold ways from her intellectual acuity, her physical and emotional labors, and her brave and patient heart. I dedicate it to her.

1. INTRODUCTION

This dissertation is a sociological study of the relationship between marriage, capitalism, and the state. It is motivated in large part by a critical engagement with two foundational approaches to the historical sociology of marriage. Durkheim's sociology of the family, expressed in *The Division of Labor in Society* ([1893] 2014) and in several lesser-known essays ([1888, 1906, 1921] 1978), and Engels' *The Origin of the Family, Private Property and the State* ([1884] 2010), have proven to be enduring resources for contemporary researchers because they exemplify the analytic power of morphological and materialist approaches to explaining marriage and divorce (Rubin 1975; Lamanna 2002).

Both writing in the late nineteenth century, Durkheim and Engels similarly observed that over many centuries, cooperation, control, and identification that were once diffuse throughout kinship networks increasingly centered on relationships between spouses and their dependent children. The primacy of the conjugal family was a hallmark of modern, capitalist, democratic societies. But the two differed in their explanations of this historical trend, with consequences for their understanding of their own time and for their predictions about the future.

Engels saw changes in family forms as organizational responses to evolving conditions of economic production and accumulation, and saw bourgeois monogamy as a sexual microcosm of broader capitalist exploitation. For this reason, he believed that modern capitalism inaugurated the possibility of "individual sex love" in marriage, but, ironically, only among the propertyless classes who could neither instrumentalize nor coerce marriage. He also argued that communism would break women's and children's economic dependence on husbands and fathers, and that this would bring an end not to marriage itself but rather to its "indissolubility."

By contrast, Durkheim believed that new family forms emerged as changes in the density and structure of social interactions created new kinds and degrees of social solidarity. He argued that intensification of the division of labor led to greater sexual differentiation, increased the importance of emotional rather than material transfers and inheritances, and in making families smaller made them more personal. Affective marriage, therefore, was a function of increased social density, not class conflict. However, Durkheim was deeply concerned about the anomic potential of conjugalism, and held that the modern state justly played a growing role in defining and regulating marriage. In particular, he supported the state's discouragement of divorce, increases in which he believed would be accompanied by other indices of anomy such as suicide.

The historical scope and explanatory ambition of this dissertation are much smaller than both Engels' and Durkheim's. It focuses on marriage and divorce in the United States in the half century following the American Civil War. But the case—contemporaneous with these theorists' own writings—offers a good opportunity to evaluate and extend their arguments because important changes in the character of marriage, the rate and process of divorce, and the role of the state in regulating conjugal relationships were occurring alongside changes in the economic, demographic, and political structure of American society. Ongoing debates about the relationship among these factors continue to draw on

the basic explanatory frameworks provided by Engels and Durkheim, and in doing so inherit some of their strengths and weaknesses while improving upon others.

Arguably the strongest theme in contemporary research on marriage and divorce in the late nineteenth-century United States is that of the changing economic status of women. As women attained more education and entered the labor market in greater numbers, women's economic dependence on marriage decreased (Degler 1980; Kessler-Harris 1982; Solomon 1985; Stanley 1998). This new autonomy was associated with substantial increases in legal divorce and informal separation (Ruggles 1997; Cherlin 2009). Thus, the expansion and intensification of capitalism brought about many of the changes to marriage that Engels associated with its abolition. But research also complicates the argument that women's economic empowerment increased divorce simply by enabling exit from undesirable marriages. Evidence also suggests that women's increased formal employment undermined husbands' normative identities as breadwinners (May 1980; Oppenheimer 1994), perhaps suggesting that Durkheim observed a high-water mark in the sexual division of labor, which, as it lessened, destabilized unions. Bullish economic conditions also increased marriage rates, with partners more prone toward marital disruption selecting into marriage (Cvrcek 2011). Moreover, with existing data it remains difficult to disentangle the effects of women's economic independence from households' financial status (Killewald 2016), and the effects of economic factors in general from those of changing attitudes, contraceptive practices, or household technologies (Preston 1997). Taken together, then, contemporary research drawing inspiration from historical materialism suggests that the influence of capitalist development on marriage and divorce was complex, and that the relative importance of various mechanisms remains uncertain.

Another important theme of contemporary research on marriage and divorce is the role of increased social density brought about by urbanization, transportation, and mass media. The outcomes of these changes support important aspects of Durkheim's morphological theory. Although first characteristic of cosmopolitans, "companionate" marriage became increasingly diffuse and institutionalized among most social and economic groups (Coontz 2005). With the loosening of clerical and kin-based control over marriage and divorce, American legislators assumed ever more aggressive postures in defining and regulating the terms of conjugal relationships (Cott 2000), and sought to enhance their control over marriage by collecting new kinds of information about spouses (Dunn 1954). But the consequences of these changes for the stability of marriages was not as Durkheim expected. The emergent ideal of marriage based on love heightened spouses' expectations, and therefore also their dissatisfaction, leading to more divorce (May 1980). Although American states tightened regulations on divorce, stigma against it lessened and divorce rates accelerated (Cherlin 2009). As a result, tension grew between moralistic laws and pragmatic spouses (Friedman and Percival 1976). In the long run, this tension has largely been resolved in favor of increasing flexibility and self-definition in legal marriage, exemplified by the institutionalization of prenuptial contracts, the diversification of marriage rituals, and the legalization of unilateral divorce. Pace Durkheim, these changes have tended to be associated with fewer suicides, not more (Stevenson and Wolfers 2006). Therefore, while the importance of social structure to marriage and divorce is evident, the

ways in morphological changes open up and resolve gaps between conceptions and practices is still unclear.

This dissertation aims to advance sociological understanding of modern marriage and divorce in two distinct ways. First, it argues that many of the challenges encountered in both classical and contemporary explanations of modern marriage and divorce result from the difficulty of considering simultaneously the role of capitalism *and* the state, and that the *law* of marriage and divorce is a site where the interacting influence of these two factors can be fruitfully explored. The relevance of law to the meanings of marriage and divorce and to demographic behavior during the period of analysis has long been debated. The earliest statistical studies of American divorce argued naively that because tighter divorce laws were followed by divorce rate increases, they had no effects on divorce behavior (Willcox [1891] 1897). Since then, qualitative researchers have consistently pointed to the relevance of marriage and divorce law to shaping conjugal beliefs, identities, and behavior (Grossberg 1985; Basch 1999; Cott 2000). Some demographic studies acknowledge the potential influence of law, but none to my knowledge attempt to measure that influence (Haines 1996; Ruggles 1997; Cvrcek 2011; cf. Ruggles 2012). Recently, however, economic historians have increased their interest in the role of marriage law in men's and women's decisions-making (Khan 1996; Geddes, Lueck and Tennyson 2012; Koudijs and Salisbury 2016; Koudijs and Salisbury 2018; Koudijs, Salisbury and Sran 2018; MacDonald and Dildar 2018; Alshaikhmubarak, Geddes and Grossbard 2019).

As the three essays contained in this dissertation demonstrate, the direct causal effects of the law are often difficult to identify, but the law can nevertheless be a gateway to appreciating the interdependence of capitalist development, state formation, and marriage and divorce patterns. For example, the significance of women's growing labor force participation is difficult to understand unless one accounts for the fact that women's legal rights to own and control their earnings depended on their marital status in ways that were changing significantly over the period of analysis. Therefore, taking Engels' approach seriously requires not only attention to changing productive processes but also to shifting property rights regimes. Because the "weak" American state has long relied on the management of property rights as an instrument of economic governance (Campbell and Lindberg 1900), the redefinition of marital property laws can also be seen as one way that state actors used the economic redefinition of marriage to shape capitalist development. The mutual co-constitution of families, markets, and states becomes more evident through the lens of the law.

Of course, neither economic nor conjugal regulation was unified in the late nineteenth-century United States. Marriage and divorce were overwhelmingly governed by diverse and often conflicting state statutes and state court decisions. Hence the title of this dissertation, "States of Disunion"—just as individuals moved through various states of union and disunion in the life course of their marriages, so too did American states experience varying degrees of unity and discord about the meanings and rules of marriage and divorce. In some places I exploit this variation for analytical leverage. In other places, this variation is itself part of the explanation or a thing to be explained.

The second principle contribution of this dissertation is to historical data and methods. The cornerstone of the study is a large administrative dataset containing

information on most marriages and divorces in the United States between 1867 and 1906 (Wright 1891; U.S. Bureau of the Census 1909b). These data have been used in prior research in various reduced forms (Willcox [1891] 1897; Howard 1904; Lichtenberger 1931; Friedman and Percival 1976; MacDonald and Dildar 2018). This dissertation relies on a much larger and more detailed subset of the data that I have digitized and will make public upon the filing of this dissertation. This will expand resources available to current and future researchers and serve as a compliment to other ongoing efforts to expand public access to historical data on marriage and divorce (Ruggles et al. 2018).

The data I have collected make it possible to apply new methods of computational statistical analysis to long-standing questions about the relationship between marriage, capitalism, and the state. A theme of this dissertation is critical engagement with the unique costs and benefits of different types of historical data and the methods that can be used to analyze them. For example, my new large-scale quantitative data on divorce proceedings allows me to evaluate the generalizability of arguments originating from historians' close reading of small samples of legal texts. In turn, some of my exploratory analyses lead to suggestive findings about mechanisms that can only be confirmed or refuted by other researchers using different data and methods. Rather than engaging in multiple methods in a single study, the dissertation attempts to demonstrate the value and technique of hypothesis generation and testing across different disciplines, data sources, and analytic methods.

This dissertation contains three chapters that are substantive essays. The first, "Marriage, Divorce, and the Gendered Organization of Private Property," highlights the ways in which the role of marriage in capitalism is legally constructed, and shows how this construction shapes demographic behavior. In the late nineteenth century, most American states granted married women new rights to own and control property and wages, and this greatly altered the economic calculus of women's decision-making about marriage. In particular, it ensured that husbands could not entirely appropriate wives assets and earnings, which afforded women more financial security in divorce. The chapter shows that states' intervention into the economics of marriage led to substantial (if unintentional) increases in divorce. But cause and effect are linked not only through the mechanism of women's newfound financial independence from men. Because property rights changes were associated with increases in divorces sought by husbands as well as by wives, I conclude that the laws' disruption of traditional gender norms about household governance also had destabilizing effects on marriage. The essay therefore shows that an explicit focus on property rights can enhance materialist understanding of the relationship between marriage and capitalism, and also that a more nuanced understanding of gender reveals multiple mechanisms linking changes in the economy to changes in conjugal behavior.

The second essay, "Strategizing in the Face of the Law: Gender and the Performance of Marital Breakdown," considers the role of law in the divorce-seeking behavior of men and women in the late nineteenth-century United States. At this time, each state specified by statute its own list of acceptable "grounds" for divorce, such as adultery, cruelty, or neglect. Scholars disagree about whether these menus exerted an effect on the overall rate of divorce, but divorce law necessarily shaped the legal strategies of divorce seekers, which were not always successful. The chapter examines the relationship between

law and legal strategy. I show that over the period of analysis, divorce litigants simplified their claims, suggesting the rationalization of divorce procedure. I also demonstrate significant gender differences in strategic legal behavior, with women being much more likely than men to modify their behavior in response to legal changes. I consider the role of judicial patriarchy and the politics of respectability in shaping this gender difference. The chapter shows why quantitative data can enable inferences about legal behavior that are not possible using textual historical data. The findings also enhance understanding of the lived experience of marriage and divorce in the context of states' growing ambition to regulate conjugal life. Individuals were compelled to respond to new legal rules, but they learned to do so in creative ways that circumvented state actors' intended outcomes.

The third essay, "Don't Count on Love: Discrepant Historical Measures of American Marriages," considers the role of the state in creating the conditions of possibility for systematic knowledge about marriage and divorce. I compare the completeness of counts of marriage events in my newly digitized administrative data to that of decennial census microdata, currently the standard resource for scientific historical analysis of marriage and divorce. I show surprisingly large undercounts of marriages in census data. I consider the reasons why vital records of marriage should outperform census data, exploring in particular the fact that the former arose through citizens' demand for clarified legal statuses—principal among them property rights—whereas the later arose through state actors' demand for population knowledge. The findings offer not only useful methodological caution to historical family demographers, but also insight into the diverse and uneven capacities of the American state.

A conclusion following these three substantive chapters reviews the basic empirical findings and discusses directions for future research. Two appendices discuss, respectively, details of the data collection process and sensitivity analyses for Chapter 2.

2. MARRIAGE, DIVORCE, AND THE GENDERED ORGANIZATION
OF PRIVATE PROPERTY

Introduction

The causal relationship between married women's economic power and divorce is a central interest of research on the family. Sociological theory suggests two primary mechanisms linking the two. On the one hand, wives' independent access to economic resources decreases their financial reliance on husbands, allowing them to exit unsatisfying relationships (Sayer and Bianchi 2000; Sayer et al. 2011). On the other hand, married women's greater economic autonomy contravenes patriarchal norms of marital identity and behavior, generating marital dissatisfaction among men and even women (West and Zimmerman 1987; Cooke 2006). Both mechanisms are cited as drivers of major historical increases in marital dissolution (Ruggles 1997; Killewald 2016).

Existing sociological research on divorce measures married women's economic power almost exclusively in terms of their labor market positions. This gives an incomplete account of economic power relations between spouses. In socially and historically specific ways, marriage and divorce law organize husbands' and wives' legal claims to income and wealth, shaping within-household gender inequalities in access to economic resources (Cherlin 2009). If wives' economic power matters to marital dissolution, accounting for variation and change in married women's property rights is necessary. Does women's independent ownership of wealth also enable marital exit? Does it also subvert husband's patriarchal expectations about household financial governance?

This study investigates one of the most significant historical expansions of women's economic rights in the United States, asking whether it contributed to the simultaneous emergence of mass divorce. Until the mid-nineteenth century, the common-law doctrine of "coverture" gave U.S. husbands near total rights of ownership and control over assets and earnings that wives brought to or acquired in marriage. As such, marriage was an important mechanism for men's primitive accumulation of wealth (Marx [1867] 1992). Because women also faced inferior labor market opportunities and lacked meaningful spousal support or governmental assistance in divorce, ending a marriage posed severe economic risks for women not faced equally by men. Over the course of the late nineteenth century, however, married women's legal rights to own and control economic resources greatly expanded. One such expansion occurred with the adoption of Married Women's Property Acts (MWPAs), which granted married women rights to pre-marital assets and gifts and inheritances received in marriage. The resulting expansion to married women's wealth holding gave them more economic power in marriage and greater economic security in divorce.

Available decennial census data does not allow for rigorous testing of the causal impact of MWPAs on divorce, so I digitized county- and state-level administrative datasets offering detailed information on nearly all legal divorces in the United States between 1867 and 1906. Analysis of this new data shows that although the influence of MWPAs was by definition limited to property-holding women, because legal divorce remained

concentrated among middle- and upper-class couples, reforms led to increases in the divorce rate of approximately 18 to 27%. Less than one fifth of these increases can be attributed to selection into marriage resulting from the laws. The effects of rights expansions were larger where women's employment was less common and where prior marital property regimes were more patriarchal. MWPAs led to proportional increases in divorces sought by men and women, suggesting that wives' expanded property rights enhanced their economic capacity to exit unsatisfying marriages at the same time that they led to increased marital dissatisfaction among husbands. Causal effects were also durable, indicating that the reforms changed the economic calculus of divorce for men and women in lasting ways.

By examining divorce-seeking by men and women separately, the study responds to calls for theorization of divorce that is more attentive to gender asymmetries (Sayer et al. 2011). Findings confirm previous hypotheses about the direct relationships between women's economic independence, the disruption of gender norms, and marital dissolution, but show that these relationships result not only from changing labor market conditions, but also from the legal construction of marriage. This suggests a useful synthesis between family demography on the one hand and economic sociology, political economy, and legal history on the other.

The remainder of this chapter is organized as follows. I develop a theoretical approach to conceptualizing the legal construction of economic power among husbands and wives, before outlining the history of marital property and divorce in the late nineteenth century and advancing hypotheses about the case. I then present two sets of empirical analyses. The first uses county-level data to establish robust causal estimates of the effect of MWPAs on divorce. The second uses more detailed, state-level data to add precision to estimates and test mechanisms accounting for the causal effects. I conclude with a discussion of the significance of marital property regimes to theories of class and gender inequality and to public policy. Appendix B presents a variety of sensitivity analyses.

Married Women's Economic Power and Divorce

I define economic power as the relative capacity to control present or potential economic resources. Sociological theories of the impact of married women's economic power on divorce have been motivated by the analysis of labor markets, particularly modern increases in married women's formal employment. Early theories of the family emphasized the interlocking importance of efficiency and solidarity, viewing sexual specialization as a core function of marriage (Durkheim [1893] 2014; Parsons 1949; cf. Becker 1993). Because married women's employment marked a departure from sexual specialization, it diminished the economic benefits of marriage, in turn eroding commitment between spouses.

Contemporary theories reframe the effects of women's employment on divorce (Amato 2010). Married women's monetary income decreases their economic reliance on husbands, and such independence is thought to increase divorce risk by providing wives the financial means to exit unsatisfying marriages (Sayer and Bianchi 2000; Schoen et al.

2002; Rogers 2004; Teachman 2010; Sayer et al. 2011). But married women's employment may also drive divorce through the violation of traditional norms of gendered economic behavior. Although evidence from the contemporary United States increasingly suggests a correlation between marital stability and gender parity in household and non-household work, in social and historical contexts where patriarchal marriage norms reward female domesticity, women's market labor may violate men's or even women's expectations for a successful or fulfilling marriage (Ross and Sawhill 1975; England and Kilbourne 1990; South 2001; Cooke 2006; Killewald 2016).

Sociological analysis of the sexual division of labor has provided powerful explanations of historic trends in divorce. But by overlooking the diverse and changing ways that legal marriage and divorce organize spouses' ownership and control of economic resources, they give an incomplete account of gender disparities in economic power within households. In some legal contexts, married women have enjoyed considerable rights to maintain separate accounts or share jointly in household income, while in others, women's entrance into marriage has meant the total forfeiture of wealth and earnings to husbands. Economically dependent divorce seekers have sometimes been able to rely on divorce laws that grant them claims to spouses' assets and earnings, but in other times not at all. At the very least, these various legal regimes determine who enjoys the returns to married and divorced men's and women's labor, creating mediating conditions for historical changes in women's employment. More broadly, by shaping gender inequality in formal economic agency, marital property law helps create relationships of cooperation or domination, autonomy or dependency between spouses, in turn shaping the gender-specific calculus of marriage and divorce decisions (Basch 1982; Grossberg 1985; Salmon 1986; Cott 2000). Accounting for husbands' and wives' respective ability to own and control—not simply generate—economic resources is therefore necessary to a complete understanding of the role of economic power in marriage and divorce patterns.

Attending to the legal construction of gendered economic power relations between spouses brings together three disparate trends in research. First, a growing body of inequality research suggests that exclusive attention to employment and income at the expense of wealth and accumulation serves to understate economic inequality along various dimensions (Killewald, Pfeffer and Schachner 2017). Most often, however, wealth is conceptualized and measured as a property of households (Spilerman 2000). But increasing evidence of gender inequality in wealth suggests that the assumption that wealth is an attribute shared equally by spouses should be challenged. In the United States, since first measured in the nineteenth century and continuing through today, gender wealth gaps have been appreciably larger than disparities in income (Shammas 1994; Conley and Ryvicker 2004; Deere and Doss 2007; Joyce 2007; Edlund and Kopczuk 2009; Chang 2010; Ruel and Hauser 2013; McDevitt and Irwin 2017). Although few surveys measure individual wealth holding by couples, limited evidence suggests that gender wealth gaps between husband–wife pairs are in fact wider than those between men and women in general (Siermisnska, Frick and Grabka 2010).

A variety of factors shape gender wealth inequalities, not least gender disparities in employment and income (Chang 2010; Ruel and Hauser 2013). Historically, laws and norms of inheritance have also prioritized sons over daughters in intergenerational wealth

transfers (Goody 1976; Shammas, Salmon and Dahlin 1987). But the gender analysis of marital property rights provides both a theoretical and methodological strategy for addressing gender wealth gaps. On the one hand, marital property laws and systems for the distribution of property in divorce are potential mechanisms through which wealth becomes stratified—not just across households, but also within them. On the other hand, in research settings where direct, individual-level measures of wealth are unavailable, variation and change in gendered property rights provide an indirect measure of gender disparities in the ownership and control of economic resources.

Second, gender analysis of marital property rights also extends and synthesizes insights from political sociology. Social policy scholars have shown how welfare states create rewards and penalties for husbands and wives to invest in specific sexual divisions of labor, and provide financial safety nets that mediate the economic returns to marriage (Orloff 1993; Skocpol 1995; O'Conner, Orloff and Shaver 1999). Social policy therefore creates a system of incentives and capabilities that influence men's and women's willingness and ability to dissolve unions (Bitler et al. 2004; Cooke 2006). Although the American welfare state during the period of analysis was too meager to have meaningfully influenced marriage and divorce decisions, the logic of social policy analysis can be fruitfully applied to the property rights analysis of marriage (Cherlin 2009:34–5). By shaping costs and benefits of household allocations and marriage and divorce decisions, marital property rights in the nineteenth century had many of the same effects that social policy had in the twentieth century.

Third, economic sociologists studying economic governance have long noted that small and subtle changes to property rights can have far-reaching, powerful and lasting effects on economic behavior (North 1981; Campbell and Lindberg 1990; Fligstein 2001; Carruthers and Ariovich 2004). Although developed in the analysis of firms and economies, sociological insights about property rights have clear applications to studying households and populations. Indeed, the gendering of claims to income and wealth in legal marriage has been a mainstay of feminist political economy, which regards property rights as central to the institutions of sex, gender and the family (Engels [1884] 2010; Rubin 1975; Deere and Doss 2007). Combining these two perspectives suggests that significant changes in the respective property rights of husbands and wives should have large-scale impacts on demographic trends in marriage and divorce (e.g. MacDonald and Dildar 2018).

Building on these insights, I ask whether and why a major historical expansion of married women's economic power—the passage of Married Women's Property Acts—played a role in driving the modern emergence of mass divorce in the United States.

Historical Background

American women's economic rights in the nineteenth century were tightly linked to the law of marriage and divorce. The American colonies adopted from English common law the doctrine of coverture, under which wives became *femes covert*, or women legally "covered" by their husbands. As English jurist William Blackstone described it, "[...] the very being and existence of the woman is suspended during coverture, or entirely merged

and incorporated in that of the husband" (1775–1779:433). In the United States, coverture almost entirely divested married women of economic agency, disallowing them from making contracts, suing or being sued, drafting wills, or owning property (Bishop 1875; Wells 1879). This meant that any wealth held by unmarried women became their husbands' property in and after marriage, as did any assets that accumulated during marriage from married women's household work or market labor. Although equity law and prenuptial agreements sometimes mitigated the operation of coverture among elites, married women's near total economic subjugation was the default (Chused 1983; Hartog 2000).

All but eight Western and Southern states adopted the doctrine of coverture. These others inherited a different, "community property" tradition that derived from Visigothic law by way of the Spanish and French civil codes.[1] In community property states, premarital estates were treated as "separate property," and any wealth generated by the couple during marriage—called "community property"—was managed by husbands but was jointly owned. As a result, although they still faced severe constraints on their economic agency, the small minority of women who lived in community property states were much more likely than those in common law states to have legal claims to property if they sought divorce.

Beginning in the 1840s and accelerating greatly after the Civil War, all American states adopted some combination of statutory reforms that expanded married women's economic agency. One of the most important reforms was the widespread adoption of Married Women's Property Acts (MWPAs), which allowed married women to own, manage, profit from, sell, and will away personal and real property that they owned before marriage or received or inherited from a third party during marriage. Although MWPAs did not alter the accrual to husbands of the returns to wives' household and market labor, the laws arrested men's appropriation of women's estates through marriage. Importantly, they gave married women much more secure claims to meaningful amounts of property in divorce (Shammas 1994).

The passage of MWPAs presents a puzzle: why would all-male voters and lawmakers voluntarily relinquish economic power by sharing property rights with married women? Pathways to reform were diverse (Chatfield 2014). States adopted MWPAs at least partly in response to women's increasing social and demographic power. Acquiescing to property reform was in men's perceived interests insofar as they believed it would stave off mounting demands for suffrage (McCammon, Arch and Bergner 2014). Women in Western states—where men sometimes outnumbered women four to one—enjoyed considerable bargaining power in marriage markets, and lawmakers there competed to attract female migrants with favorable reforms (Lemke 2016).

The adoption of MWPAs also reflected the states' transitions from agrarian to industrial societies. This transition strained the economic rationality of feudal legal institutions like coverture. With married women's increasing economic activity outside the home, patriarchal property rights created principal–agent problems between husbands and

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[1] These states were: Arizona, California, Idaho, Louisiana, Nevada, New Mexico, Texas, and Washington. Wisconsin adopted the community property system in 1984 and Alaska allows couples to elect into the community property system.

wives and uncertainty in economic transactions (Shammas 1994; Geddes and Lueck 2002; Dannin 2010). The increasing use of equity courts and prenuptial contracts to circumvent coverture became legally cumbersome (Rabkin 1974; Warbasse 1987). With the intensification of business cycles, allowing married women to own property also provided some insurance against economic downturn by helping to protect household assets from husbands' creditors (Basch 1982; Chused 1983, 1985; Hoff 1991).

Men's support for women's property rights may also have reflected their sometimes-conflicting interests as husbands and fathers (Rabkin 1980). Even if men preferred not to sacrifice any economic rights to their own wives, fertility declines, mortality improvements, and changing labor market conditions increased the value of children, tipping the balance of men's preferences toward supporting property rights for their daughters (Doepke and Tertilt 2009; Fernández 2014; cf. Zelizer 1985). Moreover, increases in the returns to education and mothers' higher investments in children than fathers may have led fathers to support greater economic rights for the mothers of their children's future spouses.

The direct financial impacts of MWPAs were obviously limited to families that owned private property. As such, they had class-specific effects. The availability of historical wealth data problematizes the measurement of nineteenth century wealth holding and MWPAs' effects on it. My own analysis of microdata on wealth available in 1% samples of the Censuses of Population in 1860 and 1870 (Ruggles et al. 2018) shows that across both censuses, 50% of households reported owning real property and 68% of households reported owning personal property. Studies of probate records show that by the late nineteenth century, women represented roughly 40% of testators (Shammas 1994, McDevitt and Irwin 2017). Therefore, while poor families and poor women were largely exempt from MWPAs, the influence of these laws was not confined merely to the economic elite.

Scholars have differed, however, in their evaluation of the magnitude of the effects of MWPAs on married women's property holding. Most studies of probate records suggest that MWPAs in the United States, Canada, and the United Kingdom grew women's wealth and shifted its composition from real to personal property (Shammas 1994; Combs 2004, 2006; Inwood and Sligtenhorst 2004), but McDevitt and Irwin (2017) show that in many cases meaningful gains in women's wealth preceded the laws' passage. My own analysis of census wealth data indicates that MWPAs passed between 1860 and 1870 did not have statistically significant effects on the percentage of married women that held wealth, decreased the real value of married women's personal property holdings by 69%, and increased the real value of real property by 218%. No effects on women's overall wealth holding are evidenced, suggesting that observed changes were indeed caused by changes in marital property laws.

The indirect effects of MWPAs on other social and economic behaviors have also been debated. Early studies of MWPAs argued that their impact was limited (Lebsock 1977; Basch 1982), but more recent research has shown however that the effects of MWPAs were diverse. MWPAs increased women's new patent filings (Khan 1996); intensified human capital investment in daughters (Geddes, Lueck, and Tennyson 2012); spurred the formation of suffrage organizations (Rabkin 1974; Geddes and Tennyson

2013); compounded assortative mating (Koudijs and Salisbury 2016); and altered the economic risk taking of married men (Koudijs and Salisbury 2018; Koudijs, Salisbury and Sran 2018).

Simultaneous to marital property reforms, divorce patterns in the United States were changing in important ways. Figure 2.1 shows that by today's standard, divorce in the nineteenth century was a rare event. But the frequency of divorce increased steadily in the postbellum period, quintupling between 1867 and 1906.[2] The emergence of mass divorce garnered substantial public attention. Divorce novels proliferated (Barnett 1939; Freeman 2003). Leading academics, lawyers, and religious figures founded divorce reform groups that successfully pressured state and federal agencies to begin collecting and compiling divorce statistics (Dike 1893). Early statisticians and sociologists used these figures to study the emerging "divorce problem" (Willcox 1897; Hill 1909; Lichtenberger 1909).

Figure 2.1. Adoption of MWPAs and increase in divorce rate, 1867–1906
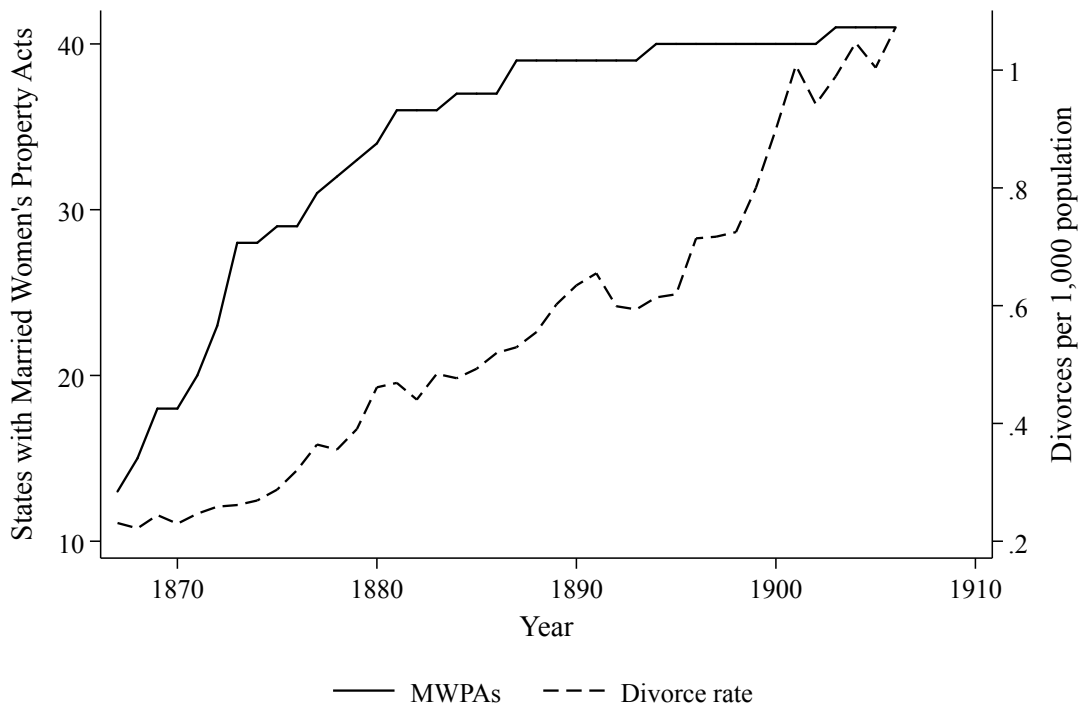
[2] No national data are available before 1867. Regional samples collected by Schultz (1984, 1986, 1990) show that divorce rates in the Northeast, North Central, and Mid-Atlantic regions were increasing over the early nineteenth century at a pace considerably slower than after the Civil War.

Divorce increases were driven by a variety of factors. On the one hand, new motivations for divorce arose. Urbanization, transportation, and mass media fostered expectations of personal fulfillment and intolerance of unsatisfying marriages (May 1980; Riley 1991; Coontz 2006; Cherlin 2009). New reasons to convert informal separations into legal divorces also emerged. Growing rates of property ownership gave the legal dissolution of marriage greater consequences. In a frontier society, widespread internal migration and underdeveloped systems of marital registration had meant that bigamy was fairly common, but with improved communications technology and administrative state-building, divorce became a prerequisite to remarriage (Schwartzberg 2004).

On the other hand, constraints to divorce eroded. Urbanization and migration diminished the social control of religious and kinship networks, and the popularization of marital dissolution in mass media dampened social stigma against divorce. Formal impediments also loosened. In the eighteenth century, divorce was generally obtainable only through a private bill passed by a state legislature. Over the course of the nineteenth century, states abolished legislative divorce and institutionalized judicial divorce (Basch 1999). The rationalization of the divorce process and the professionalization of divorce lawyering greatly reduced financial barriers (Hartog 2000). Divorce remained an adversarial process that required petitioners to prove that their partner had committed a statutorily specified offense while defending against their partner's recrimination. But lawmakers added new statutory grounds for divorce such as cruelty, and judges gave them more expansive interpretations, such as that cruelty included mental harm (Griswold 1986).

One of the most important factors affecting divorce, however, was the changing economic status of women. In the late nineteenth and early twentieth centuries, a growing number of married women engaged in market labor (Kessler-Harris 1982; Stanley 1998). Evidence from divorce dockets suggest that changes to the sexual division of labor strained conjugal relations from both sides (May 1980). Husbands chafed at wives' market work because it subverted Victorian expectations about wifely domesticity. Employed women were usually reluctant about work outside the home, and almost always cited their husbands' failure to provide financially as a source of marital dissatisfaction. Although women faced intense labor market segregation and discrimination, women's increased education and employment decreased their economic dependence on marriage (Degler 1980; Solomon 1985; Cherlin 1992, 2009; Ruggles 1997).

Married Women's Property Acts and Divorce

At the same time that labor market changes were destabilizing marriages, the gendered economic terms of legal marriage were being radically reconstructed. Is there an overlooked, independent causal relationship between historic increases in married women's property rights expansions and corresponding increases in divorce? If so, what are the causal mechanisms? MacDonald and Dildar (2018) show that states' adoption of MWPAs also led to increases in divorce, and explain this relationship in terms of married women's improved bargaining power in marriage (Manser and Brown 1980; McElroy and

Horney 1981; Lundberg and Pollack 1996). Data limitations prevent them from testing alternative mechanisms linking wives' economic empowerment to marital dissolution. Moreover, the empirical implications of the household bargaining framework are unclear: with property rights gains, women's marital exits became more feasible, but because wives' divorce threats became more credible, they were presumably able to leverage more satisfactory arrangements in marriage that enhanced rather than undermined marital stability. Notably, arguments based on women's economic returns to marriage also suffer from ambiguous predictions (Becker 1993): with property rights gains, the marginal benefits to wives of husbands' economic support diminished at the same time marriage became more attractive to women because it no longer divested them of their assets. Therefore, drawing equal inspiration from contemporary sociological theories of married women's employment (Amato 2010; Killewald 2016) and historical evidence about family economics and MWPAs, I generate distinct hypotheses—testable with newly available data—about where, how much, and through what causal pathways women's property rights gains should have increased divorce rates.

Conveniently, because MWPAs were zero-sum transfers between husbands and wives, they should have had no direct impacts on household income or wealth. As a result, the frequently confounding impact of household income effects—in which married women's earnings reduce divorce risk by reducing financial strain on families—can be ruled out (Dechter 1992; Brines and Joyner 1999). This allows me to isolate the impacts of mechanisms relating specifically to gendered economic power.

Like employment, MWPAs increased wives' economic independence vis-à-vis their husbands. Independence effects can be expected to lead to increases in divorces sought by wives. For women, divorce in the late nineteenth century was costly and risked impoverishment. Although expanding, labor market opportunities did not provide financial security comparable to that offered by marriage (Kessler-Harris 1982). Most married women did not work for wages, and therefore lacked valuable employment experience. Experienced and inexperienced women alike faced intense labor market segregation and discrimination. Alimony was rare (Donovan 2017), and government support for low-income women was meager (Dubler 1998). Therefore, by preventing husbands' seizure of wives' pre-marital estates, gifts, and inheritances, MWPAs created a financial buffer that made marital dissolution less costly and precarious for property-holding women. As a result, women previously dissuaded by the economic risks of exiting unsatisfying marriages but possessed of new financial resources should have sought legal divorces in greater numbers. The independence mechanism suggests:

Hypothesis 1: The passage of MWPAs increased rates of divorces sought by wives.

Changes in married women's economic power also cut against traditional gender norms of spousal behavior. In the case of married women's employment, gender-institutional effects worked upon both husbands and wives. As a result, with existing historical data it can be difficult to disentangle husbands' violation of norms of provision from wives' violation of norms of domesticity (Oppenheimer 1994; Killewald 2016). By

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contrast, the case of property rights transfers provides a clearer test. Wives' newfound property rights are not as likely as their employment to have contravened their desired role for themselves or their husbands in marriage. By contrast, men's traditional role in the financial governance of households was suddenly and greatly diminished (Grossberg 1985). Many husbands seem to have felt emasculated by this shift in power. Joel Prentiss Bishop (1875:728), the leading American writer on the law of domestic relations, argued that with the adoption of MWPAs, a wife could

> leave her babes for [her husband] to look after and nurse, and her meals for him to prepare with his own, while she engages in business on her separate account, and accumulates money not a cent of which or its increase is she required to appropriate to the support of her family or even herself,—all must be borne by the husband.

In a personal letter, a Massachusetts probate judge claimed that following the adoption of an MWPA in that state, he

> could name case after case that has proceeded from bickerings and disagreements on the property question to legal proceedings and a divorce. The wife has her own purse, the man his. The children are the children of both. She insists that none of her money shall be used for them, as he is obliged by law to support them, and he insists that she ought to use her own funds for her own children as much as he; and so they fall out. The breach widens and they separate. (Smith 1884:13)

The fact that mothers were in fact more likely than fathers to invest in children illustrates just how irksome married women's independent control of assets was to husbands and to male lawyers, judges, and lawmakers (Doepke and Tertilt 2009). Therefore, insofar as MWPAs, by economically empowering married women, cut against old ways of "doing" gender in marriage, increases in divorce resulting from gender-institutional effects should have been driven by husbands rather than by wives. The gender institution mechanism therefore suggests:

Hypothesis 2: The passage of MWPAs increased rates of divorces sought by husbands.

It is also possible to make predictions about social and geographic variation in the impact on divorce of marital property rights reforms. By definition, MWPAs affected property-holding families. Because formerly enslaved people and their descendants faced severe disadvantages in the accumulation of wealth (Penningroth 2003), impacts of MWPAs can be expected to have been smaller in counties with larger black shares of the population. Therefore:

*Hypothesis 3: The passage of MWPAs increased rates of divorce less
in areas with larger black shares of the population.*

Similarly, because common law and family norms prioritized sons for land inheritance and because real property was less likely to be divided in intergenerational transmission (Goody 1976; Shammas, Salmon and Dahlin 1987), MWPAs should have had larger effects in counties with greater degrees of urbanization. Therefore:

*Hypothesis 4: The passage of MWPAs increased rates of divorce
more in areas with larger urban shares of the
population.*

Finally, the effects of MWPAs on divorce can be expected to have been smaller in counties where women's employment was more common. There are two possible reasons why this should be so. Negative interdependence of the effects women's employment and property rights gains could indicate the substitutive relationship between income and wealth in married women's economic empowerment. This, however, relies on the unlikely assumption that the households in which wives were likely to have worked outside the home were also those in which women were likely to inherit wealth.

The relationship between women's employment, MWPAs, and divorce is more likely to reflect differences in the class composition of county populations, and class-specific pathways to wives' relative economic empowerment. On the one hand, women who worked outside the home did so overwhelmingly to substitute for husband's insufficient earnings. For example, a random sample of divorce suits brought in Los Angeles in the 1880s showed that 73% of employed wives cited husbands' "neglect to provide," 18% claimed they were forced to work against their will, and none indicated a desire for employment (May 1980:170). While such cases sometimes hailed from semiprofessional classes, they most often characterized households headed by semi- and unskilled laborers. On the other hand, the majority of legal divorces during the period of analysis occurred in households headed by farmers, petty proprietors, and skilled laborers (U.S. Bureau of the Census 1909a:43). As contemporaneous observers noted, it was these very "middle classes" who were also most affected by the adoption of MWPAs (Smith 1884:21). Relative to their working-class counterparts, middle-class women were more likely to inherit or to be gifted some wealth. In contrast to upper-class women, whose access to elite lawyering had always afforded them some exceptions from the common law (Chused 1985), middle-class women would have seen their rights to property meaningfully altered by MWPAs. In other words, women's work was likely to have been a driver of divorce among working class couples for whom MWPAs had less import, while property rights reforms mostly affected middle class households in which wives were less likely to seek employment. Class-specific pathways to economic empowerment suggest that the effects on divorce of women's employment and property rights gains should be negatively interdependent. Therefore:

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Hypothesis 5: The passage of MWPAs increased rates of divorce less in areas with larger employed shares of the adult female population.

Finally, heterogeneity in preexisting marital property regimes suggests where MWPA adoption should have had larger effects on divorce. Before the passage of MWPAs, married women in community property states already enjoyed the right to own separate property, although statutory law often granted husbands rights to manage that property (McDevitt and Irwin 2017). As a result, the effect of MWPAs in common law states was to grant women ownership and control of new property, whereas in community property states the change applied only to control. As a result, the effects on post-divorce property claims were likely negligible. Moreover, because women in community property states possessed claims of ownership to substantial parts of community property in divorce, the marginal effects of any property rights gains were smaller than in common law states, where married women were broadly prevented from property ownership before reforms. Therefore:

Hypothesis 6: The passage of MWPAs increased rates of divorces less in areas with community property regimes.

The Causal Relationship Between Property Rights and Divorce

This section describes analyses using county-level data to establish the basic causal effect of gendered property rights changes on marital stability, and to show that this impact depended on the economic and legal context.

Data and Measurement

Decennial census data for the period of analysis lacks information on the timing of marriage and divorce events, problematizing estimation of the causal effects of property rights changes. I therefore digitized administrative divorce data published by Wright (1891) and the U.S. Bureau of the Census (1909b). In 1887–1888 and again in 1907, the Census Bureau surveyed local administrative records covering the previous twenty years, collecting divorce data from clerks of courts with divorce jurisdiction. In most cases the Census Bureau dispatched agents to collect data directly from localities, but where state-level vital statistics registration systems were already in place, these records were used. In both surveys the Census Bureau collected records by mail from local authorities in approximately 765 "sparsely settled or distant counties," and in the second survey it employed agricultural surveyors in 206 Southern counties. The two surveys covered all counties in all continental U.S. states and the District of Columbia between 1867 and 1906. However, I exclude three states: South Carolina prohibited divorce for all but a few years during the period of analysis, and county border changes within and across North and South Dakota make county-level analysis impractical.

Several aspects of the divorce data warrant careful use and interpretation. First, a moderate amount of data is missing. Some 3.0% of divorce records were destroyed locally, overwhelmingly in the South and by fire. A further 0.5% of divorce records were coded by Census officials as lost, incomplete, or defective, and divorces in 4.9% of county–years were never recorded by local authorities or never collected by or reported to the Census Bureau. The resulting unbalanced panel comprises 90,268 county–years. Second, the data count only successful divorce suits. Only very limited information about divorce applications is available for the period 1887–1906 (U.S. Bureau of the Census 1909b): application outcomes were fairly stable across time and similar across geographic region, with an average of 74% of cases approved, 21% denied, and 5% remaining unresolved after five years. The earliest data on sex differences in success rates comes from May (1980:181), who shows that in Los Angeles in 1920, divorces were granted to men and women at nearly equal rates. Finally, the divorce data combine counts of "absolute" or conventional divorces and "limited" divorces, the latter being a rare form of legal separation. Limited divorce occurred so infrequently that its inclusion in the data is unlikely to bias substantially any estimates of the impact of women's property rights on conventional divorce. But because limited divorces were more likely to include a property settlement for the wife's maintenance, any such bias would be toward a null effect.

The newly digitized administrative divorce data are annual county flows. Constructing the outcome of interest—rates of divorce—requires a denominator. Ideally this denominator would represent the population "at risk" for the event in question—married persons (or married men or women). However, marital status was first observed by the Census Bureau only in 1880, and many individual-level records for 1890 were destroyed (Ruggles et al. 2018). Micro-level data can be used more reliably to infer stocks of cohabiting married couples (e.g. Ruggles 1997; Bloome and Muller 2015; Bloome, Muller and Feigenbaum 2017). While suitable for estimating the risk set for marital dissolution, because this measure excludes large numbers of separated individuals it underestimates the population at risk of legal divorce. The paucity of data on stocks of married persons in the United States leads even scholars of late twentieth-century American divorce patterns to adopt total population as a denominator (Friedberg 1998; Wolfers 2006). I adopt this approach, constructing an outcome variable *divorces per 1,000 persons* by linearly interpolating annual county populations from decennial census data (Haines and Inter-university Consortium for Political and Social Research [ICPSR] 2010). I address possible shifts in sex, age and marriage structures through modelling strategies discussed in the following section and through alternative constructions of the divorce rate presented in Appendix B.

The basic unit of analysis is the county–year, but frequent changes in county borders during the period of analysis make nominal counties incomparable across time. GIS-based methods for standardizing borders are appropriate for analyzing decennial census data (Hornbeck 2010). But with annualized data, thousands of unique border changes must be accounted for rather than the net change between censal observations. In this case, the key assumption of the approach—that attributes of counties are evenly distributed across geographic space—risks severe measurement error. More realistic assumptions—such as that counties' attributes are distributed evenly across their

populations—become unfeasible in complex systems of border changes. I therefore adopt a simpler and more conservative strategy, which is to form groups of counties whose shared borders changed at any point over the period of analysis, and to aggregate the attributes of all counties in each group for the entire period of analysis.[3] I use the same strategy for a small number of counties, mostly in the South, that exhibited "judicial attachment," in which divorce jurisdiction was shared among multiple counties. Group–years containing county–years with missing or defective data are dropped. As a result, 918 counties are joined into 199 groups, with group–years representing 6.5% of all observations. Hereafter, for simplicity I refer to both counties and groups as counties.

The explanatory variable of interest is the adoption of a Married Women's Property Act. Researchers have disagreed about the appropriate dating of MWPAs (Hoff 1991; Khan 1996; Geddes and Lueck 2002; Geddes and Tennyson 2013; MacDonald 2015). I use dates proposed in Geddes and Tennyson (2013), but in Appendix B I report estimates using alternatives. The variable *MWPA* is therefore a dummy indicating whether or not the property rights regime of a given county in a given year granted married women rights of ownership and control to pre-marital property and marital gifts and inheritances. During the period of analysis (1867–1906), thirty states passed MWPAs; eleven states passed MWPAs before 1867, five passed them after 1906, and Alabama never passed one.

Some models also make use of county-level covariates as explanatory variables, motivated by prior research and theory. Because currently available microdata samples from the decennial Censuses of the Population are not large enough to construct valid county-level measures for the entire period of analysis, I use aggregate data from Haines and ICPSR (2010), linearly interpolating for intercensal years. Sex ratios in some Western counties were highly male-skewed, but equalized with migration and development over the period of analysis. Such ratios may have shaped marriage markets and therefore divorce decisions (South and Lloyd 1995). I therefore include a measure of the *percentage population male*. While recent scholarship shows that divorce among Southern blacks was not as infrequent as previously believed (Penningroth 2008; Bloome and Muller 2015; Bloome, Feigenbaum, and Muller 2017), the legacy of slavery shaped marriage and divorce among formerly enslaved people in important ways (Hunter 2017). For this reason, I include a measure of the *percentage population black*. By consensus, urbanization is recognized as a strong correlate of divorce during the period of analysis (Schultz 1984, 1986, 1990; Ruggles 1997; Cherlin 2009; Cvrcek 2011). I therefore include a measure of *percent population urban*, with urban places defined as having at least 2,500 residents. Finally, women's market labor was an important historical driver of divorce (May 1980; Ruggles 1997). I proxy for this construct using a measure of the *percentage of adult women working in manufacturing*, where adult women are defined as aged 16 and older, and

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[3] Data on county border changes come from Wright (1891), U.S. Bureau of the Census (1909b), Long (2012), and Aiken and Kane (2013). An extreme degree of county border volatility both within and across North and South Dakota makes the linking of divorce and census data unfeasible, and I drop these two states.

manufacturing is defined according to contemporaneous Census Bureau industrial classifications.[4]

Table 2.1 summarizes the dependent variable and covariates. Figure 2.1 shows national changes over the period of analysis in the outcome of interest—the divorce rate— and the key explanatory variable—the adoption of Married Women's Property Acts. The central question for research design is how to identify the causal relationship between the latter and the former.

Table 2.1. Summary statistics, dependent variable and covariates

	N	Mean	Std. dev.	Min.	Max.
Divorces per 1,000 population	68,522	.524	.703	0	113.5
% population male	68,522	52.0	3.44	45.5	100
% population black	68,520	12.4	20.1	0	93.4
% population urban	68,516	13.4	20.8	0	100
% adult women in manuf. labor	68,257	1.05	2.78	0	56.5

Research Design and Methods

I use ordinary least squares to estimate fixed-effects models of divorce that generalize the quasi-experimental logic of the difference-in-differences design.[5] The design exploits states' staggered adoption of MWPAs to simulate "treatment" and "control" groups over time, where a treated county–year is one that is exposed to an MWPA. The fixed-effects framework allows me to control for a large set of possibly endogenous factors in the face of significant historical data constraints. I estimate four different models with successively more stringent methods for identifying the causal effect of MWPA adoption on divorce rates. In all models I use Huber-White heteroskedasticity-robust standard errors. Because my data are serially autocorrelated, in this and all models I cluster standard errors at the county level, which corrects for arbitrary forms of error correlation within counties across years (Bertrand, Duflo, and Mullainathan 2004;

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[4] Ideally, my measure of women's market labor would include non-manufacturing labor. More detailed labor market classifications are available in the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. 2018). However, currently available IPUMS samples are too small to generate county–level estimates of women's market labor (cf. Ruggles 1997). I therefore derive the manufacturing proxy from aggregate full-count census data (Haines and ICPSR 2010). This is the only reliable county-level measure of women's labor over the period of analysis. As a robustness check, however, I use IPUMS data to construct state-level percentages of adult women employed in all occupations and in non-farm occupations. Results from all models using this measure are comparable to those presented.
[5] In the appendix I report and discuss population-weighted least squares estimates. Coefficients for preferred specifications are smaller but statistically significant.

Cameron and Miller 2015). Appendix B presents and discusses standard error corrections for spatial autocorrelation, which for preferred models are comparable to the main results.

Model 1, the baseline fixed-effects model, takes the form:

$$Y_{ct} = \tau MWPA_{ct} + \gamma_c + \delta_t + \varepsilon_{ct} \tag{1}$$

where the outcome variable Y_{ct} represents divorces per 1,000 persons in county c in year t; $MWPA_{ct}$ is a dummy variable indicating treatment exposure, with τ being the coefficient of interest; γ_c is a vector of county fixed effects; and δ_t is a vector of year fixed effects. Model 1 is flexible specification of the difference-in-differences design that controls for all unobserved effects that are stable within counties over time or change universally among them over time. Estimated coefficients for the effect of MWPAs will be biased, however, by region-specific shocks that influenced both the adoption of MWPAs and the divorce rate.[6] To control for such shocks, Model 2 modifies Model 1 by making year fixed effects regionally specific, replacing δ_t with θ_{rt}, where subscript r indexes the U.S. Census Bureau's nine geographic divisions:

$$Y_{ct} = \tau MWPA_{ct} + \gamma_c + \theta_{rt} + \varepsilon_{ct}. \tag{2}$$

This approach effectively compares counties only to other counties in the same region (cf. Dube, Lester and Reich 2010).

Estimates of coefficients for MWPAs in Model 2 will continue to be biased if states adopted MWPAs in response to local demographic, economic, and social changes that also affected the divorce rate over the long term. I address this problem through two separate strategies. Model 3 incorporates county-level covariates that measure expected correlates of marital dissolution, yielding:

$$Y_{ct} = \tau MWPA_{ct} + \beta X_{ct} + \gamma_c + \theta_{rt} + \varepsilon_{ct} \tag{3}$$

where X_{ct} is a matrix of covariates including *percent population male*, *percent population black*, *percent population urban*, and *percent adult women working in manufacturing*, and β is a vector of corresponding coefficients. A covariates-based strategy for identifying the causal impact of MWPAs is constrained, however, by data limitations: coefficient estimates for MWPAs will remain biased if states adopted reforms in response to time-varying endogenous factors other than those observable in historical census data. Such factors could include changes in women's non-manufacturing labor, attitudes about marriage and divorce, or power relations between men and women. Coefficient estimates will also be biased if MWPA adoption responded to changes in the divorce rate itself. In order to account for these potentially confounding factors, Model 4 modifies Model 2 by

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[6] For example, financial panics in the nineteenth century affected agricultural, industrial, and commercial sectors differently (Temin 1969). These panics directly prompted some states to adopt MWPAs (Chused 1983), and also had independent effects on rates of marital dissolution (Cvercek 2011).

adding parameters for county–specific quadratic time trends, yielding the so-called random trend model (Wooldridge 2010:374–377):

$$Y_{ct} = \tau MWPA_{ct} + \gamma_c + \theta_{rt} + \lambda_c t + \pi_c t^2 + \varepsilon_{ct}. \tag{4}$$

Whereas identification of the causal effect of MWPAs on divorce in Model 3 requires that we observe all sub-regional heterogeneity in time-varying endogenous factors, Model 4 allows any such factors to remain unobserved and requires only that they change in basic linear or curvilinear patterns (which may be idiosyncratic). For example, if county-specific changes in women's educational attainment increase divorce risk and also increase the likelihood of MWPA adoption, such changes will be controlled for by the random trend parameter $\gamma_c \lambda_t$ insofar as they are approximated by a quadratic equation. This specification is particularly well suited to the case because the treatment is discrete, and so Model 4 is preferred to the other models because of its robustness to bias from reverse causality and omitted variables (cf. Friedberg 1998; Wolfers 2006).

Results

Table 2.2 reports ordinary least squares estimates of models predicting counties' annual rates of divorce over the period 1867–1906. In all models, two singleton observations are dropped; in Model 3 and other models with covariates (5–8), 289 observations with missing covariates are listwise deleted.

Across all models, estimated effects of married women's property rights expansions on divorce are positive and statistically significant. Model 1, the baseline difference-in-differences model with county and year fixed effects, suggests that MWPA adoption raised the divorce rate by an average of .050 divorces per 1,000, a 13.1% increase at means values of divorce in reform state–years. Estimates are smaller with the addition of region–year fixed effects in Model 2, suggesting that the baseline model does not account for some endogenous regional shocks. In Models 3 and 4, the addition of county-level controls in the form of covariates and county trends, respectively, raises estimates of the impact of MWPAs on divorce. The covariates model also shows that although counties' marriage markets and racial compositions did not impact divorce rates, urbanization and women's employment exerted strong positive influence as expected. Under the random trend model, the preferred specification, MWPA adoption raised the divorce rate by an average of .071 divorces per 1,000 population, an 18.7% average increase. Results presented in Table 2.2 provide consistent evidence that expansions in married women's property rights increased divorce. They also allow for a comparison of the magnitude of the effects of women's gains in property rights and labor markets: in terms of its impact on divorce, MWPA adoption was equivalent to a 1.4 standard deviation increase in adult women's manufacturing employment.

Table 2.2. Divorces per 1,000 population, county-level estimates

	Model 1	Model 2	Model 3	Model 4
MWPA	.050	.041	.050	.071
	(.016)**	(.016)**	(.017)**	(.035)*
% male			-.003	
			(.009)	
% black			.002	
			(.002)	
% urban			.006	
			(.001)***	
% adult women			.013	
manuf. labor			(.003)***	
Adjusted R2	.277	.297	.299	.365
N	68,520	68,520	68,231	68,520
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No
Region–year FE	No	Yes	Yes	Yes
County trends	No	No	No	Yes

Note: Standard errors, clustered at the county level, are in parentheses.
* p<.05 ** p<.01 *** p<.001

That estimates are larger when accounting for random trends is a notable finding. It suggests that insofar as MWPA adoption was associated with pre-treatment trends in divorce, such trends were in fact negative. Such a pattern would be perplexing if MWPAs were intended to ease divorce, as with twentieth century liberalizations of divorce law (cf. Wolfers 2006:1805). In such a case, reform could be expected to follow from pressure created by pre-reform divorce increases. But in the nineteenth-century setting, the pattern revealed by the trend model points instead to the unintended consequences of MWPA adoption. Women's organizations exerted positive influence on the adoption of MWPAs (McCammon, Arch and Bergner 2014). But with the exception of Elisabeth Cady Stanton (1884), leading feminists opposed easing divorce, at least publicly. Rather, women's groups supported equalizing marriage in order to strengthen it, not subvert it (Stanley 1998:178–185). Male legislators and judges who implemented and interpreted MWPAs supported women's divorce access even less (Lebsock 1977; Chatfield 2014), and many of the same male reformers favoring MWPAs also campaigned for stricter divorce legislation (e.g. Dike 1906). The complex historical context of "progressive" family reform during this period makes the observed patterns of divorce and property rights changes less surprising.

Table 2.3. Divorces per 1,000 population, county-level estimates

	Model 3	Model 5	Model 6	Model 7
MWPA	.050	.056	.058	.064
	(.017)**	(.022)*	(.018)**	(.016)***
MWPA x % black		-.0004		
		(.0005)		
MWPA x % urban			-.001	
			(.139)	
MWPA x % adult women manuf. lab.				-.021
				(.006)***
% male	-.003	-.003	-.003	-.003
	(.009)	(.009)	(.009)	(.009)
% black	.002	.003	.002	.002
	(.002)	(.002)	(.009)	(.009)
% urban	.006	.006	.007	.006
	(.001)***	(.001)***	(.001)***	(.001)***
% adult women manuf. lab.	.013	.013	.014	.031
	(.003)***	(.003)***	(.003)***	(.006)***

Note: All models include county and region–year fixed effects. Standard errors, clustered at the county level, are in parentheses. For all models, adjusted $R^2 = .299$ and N = 68,231.
* p<.05 ** p<.01 *** p<.001

   Although the random trend model is preferred for causal identification, the covariates-based strategy remains useful for evaluating treatment effect heterogeneity. Did the effects of MWPAs on divorce depend on local demographic, economic, or social conditions? In Table 2.3, Model 3 is restated for comparison and Models 5–8 add to Model 3 interaction terms between the treatment variable and specific covariates contained in X_{ct}. Results for Models 5 and 6 do not support the expectations that the effects of MWPAs on divorce depended on the racial or urban composition of counties (Hypotheses 3 and 4). But results of Model 7 strongly support the hypothesis that married women's employment and property holding represented distinct, class-specific pathways to divorce (Hypothesis 5). In counties with higher incidences of women's market labor, the positive impact of MWPAs on divorce was smaller—at mean values, a 1% increase in the former reduced the latter by nearly one half. Conversely, in states with MWPAs, increases in women's market labor had positive effects on divorce that were two-thirds smaller than those in states without MWPAs. This relationship is corroborated by evidence presented in Appendix B

that MWPAs and laws granting married women greater rights to their market earnings had positive but negatively interdependent effects on divorce.[7]

Changes to marital property rights regimes resulting from MWPAs depended on prior legal context, with large gains in common law states but ambiguous ones in community property states. Table 2.4 gives results for the covariates and random trend models, estimated on separate samples of common-law and community property states. As expected, estimates of MWPAs' impact on divorce in common law states are slightly larger than for the full sample. By contrast, estimates for all models on the sample of community property states fail to reject the null hypothesis of no effect. Negative point estimates for community property states are a curious result, and suggest the need for more detailed study of the Western context. But discrepant results across prior property regimes support the Hypothesis 6, which predicted that divorce patterns responded proportionally to the magnitude of property rights changes.

Table 2.4. Divorces per 1,000 population, county-level estimates, by prior property rights regime

	Model 3		Model 4	
	Common law states	Community property states	Common law states	Community property states
MWPA	.052	-.032	.077	-.049
	(.017)**	(.116)	(.039)*	(.121)
% pop. male	-.012	.025		
	(.007)	(.016)		
% pop. black	.002	.004		
	(.002)	(.005)		
% pop. urban	.006	.009		
	(.001)***	(.002)***		
% adult women manuf. lab.	.011	.032		
	(.003)***	(.009)***		
Adjusted R^2	.317	.383	.400	.569
N	60,110	8,118	60,334	8,183
County FE	Yes		Yes	
Year FE	No		No	
Region–year FE	Yes		Yes	
County trends	No		Yes	

Note: Standard errors, clustered at the county level, are in parentheses.
* p<.05 ** p<.01 *** p<.001

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[7] Models using IPUMS data to construct state-level percentages of adult women employed in all occupations and in non-farm occupations also show that the effects of MWPAs depend negatively on women's employment.

Manipulating the timing of treatment can be an important strategy in difference-in-differences studies for evaluating the robustness of causal inferences and the durability of causal effects. Figure 2.2 reports estimates of the random trend model using the sample of common law states, augmented with treatment leads and lags. Dummy variables mark the five years before adoption, the year of adoption, the nine years after adoption, and ten years forward.[8] The figure graphs coefficients and confidence intervals for these treatment lead and lag dummies. Recall that the random trend model attempts to control for pre-treatment trends. For causal estimates to be valid, pre-treatment coefficients should not be significantly non-zero. Although Figure 2.2 does show small increases in point estimates in the year before treatment, treatment leads are each statistically insignificant and an F-test fails to reject the null hypothesis that pre-treatment coefficients are jointly equal to zero (p = .409).

Figure 2.2. Time-varying effects of MWPAs, common-law states

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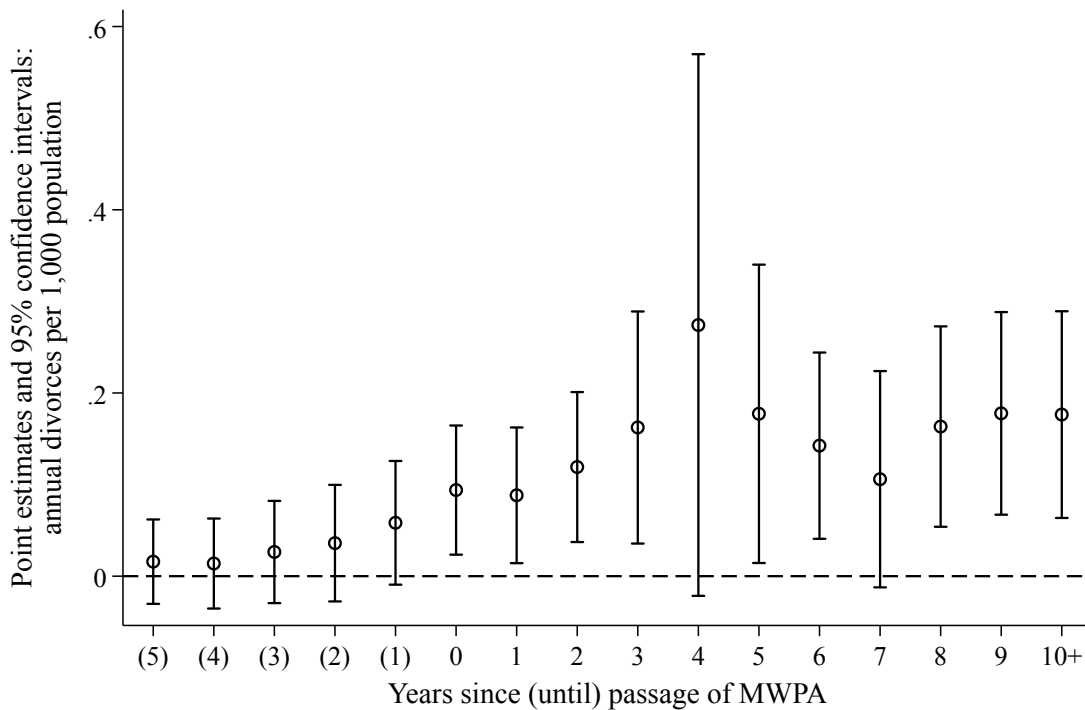
[8] Note that all but the last variable equal one only in the relevant year, while the final variable equals one in each year following the tenth year of adoption (cf. Autor 2003). The inclusion of all counties in the lead–lag model yields very similar point estimates and slightly larger confidence intervals. As expected from comparisons of coefficients for MWPAs in Models 3 and 4 in Table 2.2, graphical analysis of lead-lag models without random trends shows modest, statistically insignificant negative pre-treatment trends.

On the other hand, examination of treatment lags can help give a sense of the impact of MWPAs on divorce over time—whether it grows, stabilizes, or mean reverts. Figure 2.2 illustrates that the effects of MWPA on divorce were at first somewhat modest but then fluctuated with time and settled at approximately .176 (p = .002) after 10 years. Fluctuation may result from variation in the dissemination of knowledge about MWPAs or in process through which judges interpreted and gave effect to marital property statutes (Chused 1985; MacDonald 2015). Regardless, the results in Figure 2.2 add further evidence in support of the claim that expanded women's property rights had lasting causal effects on divorce.

The construction of the outcome variable—divorces per 1,000 population—does not directly rule out the possibility that apparent increases in rates of divorce are in fact the product of stable divorce rates and increasing rates of marriage. Moreover, there are good theoretical reasons to believe that MWPAs increased marriage rates because they made marriage more attractive to propertied women. But two pieces of evidence suggest that spurious inference is unlikely. First, prior research using national time-series data shows that while marriage rates strongly predicted rates of separation and abandonment during the period of analysis, no such impact on rates of legal divorce are evident (Cvrcek 2009). Second, results from models using county-level marriage data (Wright 1891; U.S. Bureau of the Census 1909b), presented in Appendix B, are inconclusive but indicate that MWPAs are not likely to have increased marriage rates by more than 3.4%. Increased selection into marriage can therefore explain no more than 18.2% of the increase in divorce resulting from married women's property rights expansions, and may explain none of it.

Bounding Estimates, Testing Mechanisms

County-level analysis allows me to correct for data quality issues, to generate robust causal estimates, to evaluate heterogeneity in treatment effects, and to measure the impact of marriage selection on divorce. But because county-level divorce data are reported only as annual counts, analysis based on it has two important limitations. First, it risks underestimating treatment effects due to unobserved heterogeneity in the treatment construct. An implicit assumption of the fixed effects models presented above is that the treatment—the adoption of a married women's property act—applied retroactively to all couples who were already married. In fact, some MWPAs were retroactive, others applied only to couples married after adoption, and still others were limited to proactivity by judges at some point after adoption (Wells 1879; Chused 1985; Shammas 1994; MacDonald 2015). To the extent that laws were proactive rather than retroactive, models using county-level data overestimate the treated population by including couples married before MWPA adoption, and therefore underestimate the average treatment effect. In the analyses that follow, I use state-level data for which divorces are cross-tabulated by duration of marriage. This allows me to date marriages to before and after MWPA adoption, and to identify the treated population under the alternative assumption that MWPAs were exclusively proactive. Therefore, the county- and state-level analyses can be thought of as generating lower- and upper-bounded estimates, respectively.

A second limitation of county-level analysis is that annual counts do not allow me to distinguish between distinct mechanisms linking property rights and marital dissolution through women's greater ability to exit marriage and the subversion of men's gendered expectations of marriage (Hypotheses 1 and 2). In order to solve this problem, I exploit state-level counts of divorce suits brought by husbands and wives, which allow me to examine how property rights propelled divorce seeking in gender specific ways (cf. Sayer et al. 2011).

Data and Measurement

I digitized additional state-level data reported in Wright (1891) and the U.S. Bureau of the Census (1909b), who cross-tabulate state-level annual divorce counts by duration of marriage in one-year increments up to twenty (1–2 years, 2–3 years…, 20–21 years) and by sex of divorce petitioner. Durations greater than 21 years are aggregated, and so I censor them because treatment assignment cannot be determined. Divorces occurring after less than one year of marriage are not reported. (Note that because the state-level data aggregate county-level data, data quality issues that were accounted for in the county-level analysis appear as measurement error in the state-level analysis.) The unit of analysis is therefore the state–marriage-cohort–divorce-year, or state–cohort–year for short. Given the results of the county-level analysis, I restrict the sample to common-law states. (State-level analyses of community property states indicate that MWPAs had no statistically significant effects on total divorces or divorces sought by husbands or by wives.) I construct the outcome variable by dividing the state–cohort–year count by the total state population in the cohort's year of marriage, yielding observations for 28,301 state–cohort–years having a mean value of .035 divorces per 1,000 population.

One measurement issue is that the divorce petitioner may not correspond precisely to the initiator of marital dissolution. Among husbands and wives who mutually agreed to separate, decisions about who would file for divorce may have been based on social or legal expedience rather than real grievance. That said, laws in every state required petitioners to demonstrate the commission of some specified fault, prohibited divorce by mutual consent, and penalized collusion between spouses. A more common case was that in which a petitioner filed for divorce following abandonment by their spouse. But evidence presented in Appendix B shows that an overwhelming majority of increases in legal divorce resulted from new marital dissolutions rather than from petitioners seeking to legalize already existing states of separation.

Research Design and Methods

I use ordinary least squares to estimate linear regression models taking the following form:

$$Y_{smgt} = \tau(MWPA_{st}) + \lambda_{st} + \varepsilon_{smgt}. \tag{5}$$

28

where Y_{smgt} represents divorces per 1,000 population sought in state s by spouse of marriage cohort m and gender g in year t. There are two important differences between this model and those from the county-level analysis. The fixed effects design is not numerically equivalent to a first-differences estimator because treatment does not vary across marriage cohorts. However, because we observe treated and untreated cohorts, it is possible to add state–year dummies λ_{st} that difference out all variation in divorce rates resulting from variation across state–years in all possible explanatory factors. As a result, treatment effects are identified solely off of differences between cohorts within state–years. Marriage models presented in Appendix B indicate that MWPA adoption did not lead to significant increases in marriage cohort size. Causal inference in state-level analysis therefore depends only on the assumption that there is no unobserved qualitative heterogeneity in marriage cohorts that is correlated with MWPA adoption, such as the increased selection into marriage by couples with higher propensities for divorce.

Results

Table 2.5 presents estimates of the model described in Equation 5. The first three columns contain estimates based on the full sample of divorces occurring between 1867 and 1906. Cohort-specific divorces per 1,000 population were .013 higher among couples married after MWPAs than before, a difference of 37.1%. Across treatment groups, absolute differences in divorces sought by wives were more than twice as large as men's differences. However, because women successfully petitioned for divorce at much higher rates than men (Friedman and Percival 1976), proportional increases in men's and women's divorce were very similar—36.4% and 39.1% at mean values of divorce, respectively.

Table 2.5. Divorces per cohort per 1,000 population, state-level estimates, common-law states

| | Model 8 | | | | | |
| | Divorce year-balanced sample | | | Marriage cohort-balanced sample | | |
	Total	Men	Women	Total	Men	Women
MWPA	.013	.004	.009	.010	.003	.006
	(.003)***	(.001)***	(.002)***	(.003)**	(.001)**	(.002)*
Adj. R^2	.714	.661	.652	.764	.678	.699
N	28,301	28,301	28,301	15,226	15,226	15,226

Note: All models include state–year fixed effects. Standard errors, clustered at the county level, are in parentheses.
* p<.05 ** p<.01 *** p<.001

State-level estimates are expected to be higher than county-level estimates because they represent upper bounds for the true effect of MWPAs. But they may also differ from county-level results for two unintended reasons. First, the censoring of the data and the positively skewed distribution of marital duration among divorcers is likely to generate upward bias because the sample includes an artificially high (low) proportion of treated (untreated) cohorts that are newly married, and which therefore have a higher marginal risk of divorce. To illustrate this problem, consider the adoption of an MWPA in Illinois in 1861: divorces by couples married in 1861 (1862, 1863, etc.) are only observed if those couples survive at least 6 (5, 4, etc.) years, and because divorce hazard peaks at 4 years of marriage and declines steadily thereafter, cohorts go unobserved in disproportionately risky years. Second, using marital duration data also incorporates into the analysis an additional 13 treatment events—those MWPAs adopted between 1847 and 1866. A straightforward way to address both problems is to restrict the sample to those married between 1866 and 1886, which balances the panel across marriage cohorts rather than across divorce-years. This strategy limits estimation to the same sample of 27 treatment events captured in the county-level analysis, with the exclusion of two MWPAs passed in Kentucky in 1887 and Montana in 1894. The last three columns of Table 2.5 contain estimates for the sample balanced on marriage cohorts. As expected, point estimates are lower. Divorces sought by husbands increased by 27.3% at mean values of the outcome variable, and divorces by wives increased by 26.1%. The results therefore give equal support to the gender-institutional and independence mechanisms (Hypotheses 1 and 2): married women's property rights gains drove marital dissolution by contravening husbands' expectations about traditional spousal behavior, as well as by financially enabling married women to dissolve undesired unions.

Conclusion

Women's expanded ownership and control of private property played an important role in the modern emergence of mass divorce in the United States. Between 1867 and 1906, Married Women's Property Acts increased divorce rates by between 18 and 27%. No more than one fifth of additional divorces are attributable to additional marriages. Divorce increases were durable and were more pronounced where rights increases were more significant and where married women's employment was less likely to provide an alternative path to economic autonomy. MWPAs drove equal increases in divorces sought by husbands and by wives, suggesting that expansions of married women's economic power both undermined men's expectations of patriarchal household governance and financially empowered women to exist unsatisfying marriages.

The study demonstrated the empirical importance of attending to variation and change in the legal environments that economically empower men and women to make family decisions, and which give those decisions disparate economic consequences. The use of large-scale, previously unexamined administrative data highlights the ways that overlooked changes in legal marriage and divorce can have far-reaching historical and demographic consequences. At the same time, limitations to the study—such as a lack of

thorough data measuring wealth directly and an inability to measure the relationship between formal rights changes and practical changes in gendered household governance (Braukman and Ross 2000)—mean that quantitative research should proceed carefully alongside more detailed archival, survey, and ethnographic research on the family.

Although power relations among spouses are complex and irreducible to economics (Rollins and Bahr 1976; Komter 1989), the study also advanced a theoretical understanding of gendered economic power in marriage that centers the ownership and control of economic resources. This conception promises to enrich research on both class and gender. On the one hand, although property rights feature centrally in theories of stratification (Marx [1867] 1992; Weber [1922] 1978; Sorensen 2000; Wright 2002), a strong tendency to treat households as integrated financial units having a singular class position can obscure important formal inequalities between husbands and wives. Opening the black box of families shows how property rights, as organized in marriage and divorce, contribute to economic gender inequality. On the other hand, gender inequality research on the labor process—whether formal or informal—takes on new significance when it is understood that by marrying and divorcing, men and women greatly alter their legal rights to the returns to one another's labor (England and Kilbourne 1990; Combs 2006). It should come as no surprise that it was Marianne Weber ([1907] 2003)—not Max—who identified in the modern expansion of married women's property rights a fundamental precondition for translating women's historical labor market gains into meaningful self-determination.

Despite significant change in the United States over the last 150 years that have made legal marriage and divorce less explicitly patriarchal, these formal institutions continue to be mechanisms for the gendered cultivation and exercise of economic power. On the one hand, marriage and divorce law continues to shape gendered wealth stratification. Laws governing the valuation of care work, the distribution of property in divorce, and spousal and child support continue to shape how legal marriage and divorce enable men's and women's appropriation and accumulation of one another's labor and capital (Doepke, Tertilt and Voena 2012; Voena 2015). On the other hand, resulting wealth inequalities have important implications for divorce decisions. Evidence that divorce continues to economically benefit men but harm women suggests that current laws and policies create economic costs to divorce that likely prevent some women from exiting undesired marriages (Holden and Smock 1991; Peterson 1996; Smock, Manning and Gupta 1999; Poortman and Selzer 2007). At the same time impediments to divorce are associated with greater rates of domestic violence, female suicide, and wife murder (Stevenson and Wolfers 2006). Insofar as economic risks continue to create effective barriers to marital exit, measures that make divorce less costly for women are therefore likely to have important impacts for their well-being. Public welfare expansions and stronger provisions against labor market discrimination would likely ease divorce and improve divorce outcomes, but so would further equalitarian reforms to the gendered distribution of property rights in marriage and divorce. These could include, for example, the adoption of the Uniform Marital Property Act, the standardization and strengthened enforcement of spousal and child support, or wages for housework.

3. STRATEGIZING IN THE FACE OF THE LAW: GENDER AND PERFORMANCE OF MARITAL BREAKDOWN

Introduction

An apparent paradox characterizes perspectives on family law and gender inequality in the late nineteenth- and early twentieth-century United States. On the one hand, feminist historians and activists, both then and now, saw family law as explicitly and effectively patriarchal. Legal patriarchy shaped married and divorced women's rights to property, reproductive autonomy, and custody (Grossberg 1984; Hoff 1991). It also constrained their freedom to enter and exit marriages. In calling for more liberal divorce laws, Elizabeth Cady Stanton (1884) argued that "no words [could] describe the infinite outrages to which women [were] subject in compulsory relations for which the law gives no redress." Contemporary critics have seen in the fault-based divorce system a formal structure that reinforced wifely norms of dependence and domesticity (May 1980; Fineman 1994).

On the other hand, socio-legal scholars and participants in the divorce process have seen in this period an abundance of fraud and legal fiction, in which the law in action was grossly decoupled from the law on the books. State divorce statutes may have been severely moralistic, but in the practice of divorce, husbands and wives, both as individuals and as couples, increasingly bent and broke the law in order to dissolve their marriages, benefitting from an emergent profession of divorce lawyers and a population of judges and clerks who were increasingly willing to accommodate divorce seekers (Friedman and Percival 1974; Basch 1999; Hartog 2000; Friedman 2005). As early as the 1860s, newspaper editors, legislators, and social reformers decried widespread perjury and malfeasance in divorce litigation (Jacob 1988:33–4). By 1932, an influential legal treatise observed that it was "well known that the parties often seek to evade the statutory limitations and that there is great danger of perjury, collusion, and fraud" (Vernier 1932:93). Moreover, if legal patriarchy constrained women's access to divorce, why were more than two thirds of successful divorce petitioners women, and more than three quarters of all divorce suits successful?

Was divorce patriarchal? Did divorce law matter to divorce outcomes? Can these two perspectives—one seeing divorce law as consequential and discriminatory, the other seeing divorce process as flexible and performative—be reconciled? This study proposes a double intervention. The first is to combine feminist scholars' attention to gender inequality with socio-legal scholars' emphasis on legal process rather than case or statutory law. Rather than seeing law as straightforwardly defining unequal terms, I treat the restrictive, adversarial system of fault-based divorce as a legal framework in which men and women had to strategize in distinctive ways to achieve their respective goals. Rather than examining the role of gender in legislative or jurisprudential discourse, I focus on the operation of gender in lawyers' offices and county courtrooms. The central explicandum of this chapter is therefore *gendered legal strategy*—how to conceptualize and measure it, and what it can tell us about the legal production of gender inequality.

The second intervention this study makes relates to the sources and methods used to examine the relationship between the history of divorce and gender inequality. Prior historical evidence of strategic divorce behavior—especially during the emergence of "modern" judicial divorce in the postbellum period—has been fairly unsystematic, based either on cursory examination of aggregate statistics (Friedman and Percival 1974) or on careful consideration of a small number of cases in non-representative jurisdictions (May 1980; Basch 1999; Buckley 2002). Neither approach has taken full account of the role of diverse and changing divorce laws, nor clarified the ways in which divorce strategies varied by region or gender. I therefore introduce a new administrative dataset offering detailed legal information on nearly all American divorces between 1867 and 1906, and combine it with a sample of state divorce statutes to evaluate the relationship between divorce law and legal strategy. I show that men routinized their divorce petitioning more than women, but that women adapted their petitioning more readily to restrictive legal regimes.

The rest of the chapter is organized as follows: after briefly describing the fault-based divorce system, I conceptualize strategic legal behavior in this context; I then describe the data and methods used, before presenting results and discussing implications for research on law and gender inequality and for cross-disciplinary historical methodology.

Late Victorian divorce on the books and in action

Following the American Civil War divorce rates accelerated, growing fivefold between 1867 and 1906. Figure 3.1 shows the gender-specific national divorce rate for the period of analysis, plotted as a dotted black line and scaled on the right-hand y-axis. The causes of this acceleration were many. States replaced cumbersome systems of legislative divorce with streamlined judicial divorce procedures (Chused 1994; Basch 1999). Improved labor market conditions for women decreased wives' economic dependence on husbands, and more precarious economic conditions for many men created financial strains on couples (Oppenheimer 1994; Ruggles 1997; Cvrcek 2011; Bloome and Muller 2015). Urbanization, transportation, and mass media intensified expectations of personal fulfillment in marriage and increased marital dissatisfaction, but also eroded the social control of religious and kin networks and chipped away at the stigmatization of divorce (May 1980; Riley 1991; Coontz 2005; Cherlin 2009).

Divorce increases occurred in spite of a legal system of fault-based divorce that remained restrictive. Spouses' mutual consent was not sufficient for divorce, so a "private ordering" of marital dissolution was not formally possible (Mnookin and Kornhauser 1979). Each state specified by statute its own menu of legally actionable "causes" or "grounds" of divorce. Figure 3.1 also plots the percentage of all divorces citing the five "principal," or most common, causes of divorce—adultery, cruelty, desertion, habitual intoxication, and neglect to provide. Individuals could successfully petition for divorces only if they could prove that their spouse had committed one of these offenses and could defend themselves against recrimination. Because divorce was considered a public malady, not only spouses but also state prosecutors could contest the claims of a divorce petitioner (Connolly 1954).

Figure 3.1. Divorces and cited causes, United States, 1867–1906
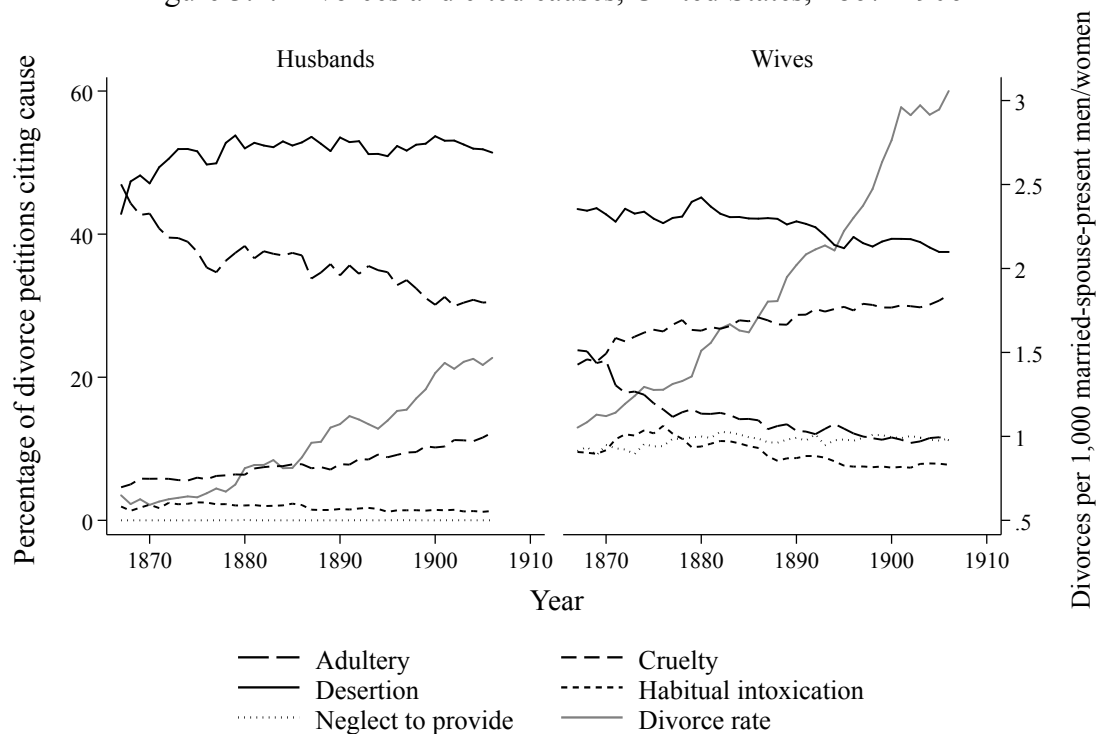
The antebellum period experienced a general liberalization of divorce statutes. Many states introduced new divorce causes and shortened residency requirements for migratory divorce-seekers (Phillips 1988). But as the postbellum divorce rate accelerated rapidly, state legislatures reacted with new restrictions. Between 1889 and 1906, five states newly provided for the defense of an absent party in a divorce suit, fifteen established waiting periods for remarriage, eighteen lengthened residence requirements for divorce, and six eliminated some grounds for divorce (May 1980:4).

At the time, the unfettered acceleration of divorce was widely understood as evidence that legal restrictions were inconsequential (Willcox 1897; Cahen 1932). But of course, the one does not follow from the other: divorce rates might have risen even faster in the absence of legal backlash. Nevertheless, contemporary observers of divorce during this period point to an increasing divergence between divorce law on the books and divorce law in action (Basch 1999; Friedman 2005). Confronting formal impediments to divorce, individuals and couples developed new methods for exaggerating, concealing, and colluding to achieve their desired legal ends. Whether or not divorce law curbed divorce rates, it necessarily shaped divorce behavior, refashioning divorce seeking into a strategic performance. Bracketed by a colonial era of true "fault"-based divorce and a contemporary era of true divorce "freedom," late Victorian divorce was a transitional period marked by strategic legal "fiction" (Friedman and Percival 1974).

Strategy and performance in divorce seeking

Strategic behavior in a nominally fault-based divorce system is most systematically conceptualized in terms of departures from a non-strategic ideal type: the *genuine petitioner*. The genuine petitioner is the legal subject that the law on the books presupposes. This ideal type has two characteristics: genuine petitioners do not cite specific causes of divorce that have not in fact occurred; and they cite all causes of divorce that have in fact occurred. In other words, these actors cite causes of divorce *if and only if* they correspond to their actual lived experience. Importantly, this conception does not imply that genuine petitioners will sue for divorce if an actionable cause occurs—only that any suit they bring will be faithful to such occurrences.

In contrast to the ideal type of the genuine petitioner, one can imagine two contrasting ideal types of strategic legal action (LoPucki and Weyrauch 2000). One is marked by routinization. Routinization is common in the consolidation of legal fields (Kagan 1984), and involves the standardization and increased taken-for-grantedness of concrete practices and procedures and the meanings thereof. In turn, it may also involve the decoupling of formal representations of processes (e.g. the law on the books) from the processes themselves (e.g. the law in action) (Meyer and Rowan 1977).

Studies of late Victorian divorce in the United States argue that routinization occurred as litigants, lawyers, and judges developed and diffused simple, standardized scripts for divorce-seeking. With significant increases in divorce, the administration and adjudication of divorce became much more standardized. Divorce paperwork increasingly took the form of pre-printed forms (Basch 1999). The publication and circulation of legal treatises established a self-identified field of domestic relations law, brought greater uniformity to judges' understanding of divorce, and helped professionalize divorce lawyers who consolidated modular strategies for divorce-seeking (Marshall and May 1933; Hartog 2000). These strategies obscured the messier realities of marital dissolution, reducing the complexity of legal representations of emotions and behaviors to a tested, efficacious narrative (cf. Sarat and Felstiner 1986, 1997). The *routinized petitioner* adopted these scripts to expedite the pursuit of divorce and increase its likelihood success. The routinized ideal type therefore relaxes the sufficiency ("if") condition of the genuine petitioner, declining to cite all the available legal causes for which they are eligible.

The second ideal type that diverges from the genuine petitioner is characterized by strategic adaptation to limited menus of divorce. If routinization characterizes management of abundant legal options, adaptation characterizes legal strategy in the face of constrained options. The ideal type of the *adaptive petitioner* diverges from the genuine petitioner in terms of the necessity ("only if") condition: when faced with a limited menu of grounds upon which to file for divorce, adaptive petitioners pursue divorce on grounds other than those that they would choose if they were legally available. If the performance in routinization is reductive, adaptive performances are substitutive. Rather than simplifying one's experience, one outright redefines it.

In the case of late Victorian divorce, adaptation is most easily observed in states with extreme controls on divorce. New York, for example, allowed divorce only for

adultery. Lawrence Friedman (2000:1512) documents the resulting strategy, which continued well into the mid-twentieth century:

> "In light of this, there developed a most interesting practice, which we might call soft-core adultery. This involved a little drama performed in a hotel. The cast of characters included the husband, a woman (generally a blonde who was hired for the occasion), and a photographer, of course. An article in the New York Sunday Mirror magazine section, published in 1934, had the intriguing title: 'I was the Unknown Blonde in 100 New York Divorces.' The 'unknown blonde' usually charged $50 for her work."

If extreme, the practice of soft-core adultery illustrates the ways in which men and women, sometimes individually and sometimes collusively, would embellish or outright fabricate evidence in order to be eligible for divorce.

Can we measure routinization and adaptation in divorce seeking using large-scale administrative data? Can we observe systematically its distribution across time and place? Are gender differences in strategic divorce-seeking observable, and if so, do they offer a different perspective on historical gender inequality before the law?

Data

Quantitative historical research on divorce in the United States relies on two kinds of sources. Census data allow researchers to observe the marital statuses of the entire population, but give no insight into the divorce process (e.g. Ruggles 1997; Bloome and Muller 2015). Samples of divorce dockets allow for more detailed analysis, but are limited in their scope and generalizability (e.g. May 1980; Schultz 1984, 1986, 1990). To navigate this challenge, I digitized administrative records published by Wright (1891) and the U.S. Bureau of the Census (1909b) that cover nearly all recorded divorces in the continental states and the District of Columbia between 1867 and 1906.[9]

Individual-level divorce data can be recovered from published cross-tabulations (N = 1,274,341). The data include information on the gender of the divorce petitioner; the specific, legally defined "causes" of divorce cited by the petitioner; and the state and year

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[9] The data were collected in two retrospective surveys each covering the previous two decades. The Census Bureau dispatched agents to collect data directly from clerks of courts with divorce jurisdiction, but where state-level vital statistics registration systems were already in place, these records were used. Records were collected by mail from local authorities in approximately 765 "sparsely settled or distant counties," and by agricultural surveyors in 206 Southern counties in the second survey. I exclude South Carolina, which prohibited divorce for all but a few years during the period of analysis. The surveys include only successful divorce suits, a problem common also to archival docket studies (Basch 1999). The Census Bureau (1909b) reported that nationally, 74% of petitions were approved, 21% denied, and 5% remaining unresolved after five years, with little variation across geographic region or historical time. Docket studies suggest that the overwhelming majority of "denied" cases were in fact withdrawn (Marshall and May 1933; Penningroth 2008). The divorce data combine counts of "absolute" divorce with counts of "limited" divorce, the latter being a form of legal separation that prevented remarriage and was very rare compared to absolute divorce.

in which the divorce was granted. After recategorizing nominally distinct but substantively similar causes,[10] the data include 49 unique causes of divorce and 528 unique combinations of causes. For some analyses I calculate divorce rates by dividing the annual number of divorces occurring in a given state by the number of married-spouse-present men/women in the state, calculated from decennial census data and linearly interpolated for intercensal years (Ruggles et al. 2018).[11]

I link a subsample of the administrative divorce data to data on state divorce laws compiled by the U.S. Bureau of the Census covering the period 1886–1906 (1909a). Menus of causes legally available causes varied widely by state, but I focus on the five "principal" causes. Over the period of analysis, 91.1 percent of all divorces cited at least one of these causes, and 89.3 percent cited only these causes.[12] Because some states allowed only wives to petition for cruelty and no states allowed husbands to petition for neglect to provide, I generate sex-specific binary variables indicating whether husbands or wives were able to petition for divorce for a particular principal cause in a given state–year, and aggregate these binary variables into count variables that index gender-specific legal access to divorce.

Methods

I use several descriptive and analytic approaches to measure gendered legal strategies of divorce in the late nineteenth century. I measure routinization in two distinct ways, in the aggregate and at the individual level. In the aggregate, routinization implies that, given some menu of legally available causes, divorce petitioners and divorce lawyers experimented with methods of divorce-seeking and developed scripts for legal action consolidated around a set of causes smaller than the full menu. To measure this emergent property of routinization, for each state–year I calculate the diversity of causes cited by male and female divorce petitioners respectively. I use the Herfindahl–Hirschman index, a standard tool for measuring the concentration of firms in industries, defining:

$$H_{stg} \equiv \sum_{c=1}^{C} p_{stgc}^2 \qquad (1)$$

where p_{stgc} is the proportion of divorces in state s in year t by petitioner of gender g for specific cause or combination of causes c. The index, H_{stg}, has a range of $(0, 1]$ with higher values indicating greater degrees of concentration (i.e. lower levels of diversity).

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[10] Nominal distinctions without substantive differences appear to result from idiosyncratic language in state statutes. For example, divorces for "illicit carnal intercourse before marriage" are recategorized under the more commonly occurring description "illicit premarital intercourse." Twenty-three causes are reclassified.

[11] Because marital status is not measured in census data before 1880, I follow Bloome and Muller (2015) in using married-spouse-present persons as a denominator for calculating divorce rates. Robustness checks show no meaningful differences in results when using marital status to calculate divorce rates. In analyses of divorce rates I exclude North and South Dakota, Oklahoma, and Indian Territory because border changes prevent the valid measurement of denominators.

[12] I limit my analysis to laws permitting "absolute" divorce and not laws allowing for "limited" divorces. Such divorces represented a small minority of all legal divorces. See note 1.

A second measure of routinization relies instead on the degree to which causes were combined in individual divorce petitions. Obtaining a divorce formally required that the petitioner prove just one "cause" of divorce. Because the evidentiary burdens and financial costs of divorce seeking increased with additional causes (Marshall and May 1933), routinization implies that petitioners combined causes less frequently. I therefore construct a simple individual-level measure of the number of causes cited in each divorce.

The aggregate concentration and individual combination of causes both depend on the legal availability of causes, which often differed by gender. Measuring gender differences in routinization therefore requires controlling for legal diversity and change. For concentration, I estimate the state-level linear model:

$$H_{stg} = \text{female}_g \beta_1 + \text{causes}_{stg} \beta_2 + \delta_{st} + \varepsilon_{stg} \tag{2}$$

where H_{stg} is the Herfindahl–Hirschman index; causes_{stg} is the number of "principal" causes of divorce legally available to each gender in each 2; δ_{st} is a state–year fixed effect; and β_1 is the coefficient of interest. In this and all subsequent models I use Huber-White heteroskedasticity-robust standard errors, clustered at the state level. For combination, I estimate the individual-level linear model:

$$\text{num\_causes}_{istg} = \text{female}_g \beta_1 + \text{causes}_{stg} \beta_2 + \delta_{st} + \varepsilon_{istg} \tag{3}$$

where num_causes_{istg} is the number of causes cited by individual i and β_1 is the coefficient of interest. Both models control for all legal and unobserved state-level heterogeneity and identify gender differences in routinization on the assumption that actual aggregate diversity or individual co-occurence of spousal misbehavior does not vary by gender within state–years.

Similarly to routinization, I measure adaptation at both the macro and micro levels. In both cases I estimate counterfactuals of legal strategy by exploiting changes in state statutes. Figure 3.2 illustrates the logic of the approach. Until 1895, divorces in North Carolina were generally available only for adultery. In that year, the state legislature added desertion a cause of divorce. In 1903, however, legislators again limited access to divorce for desertion. The resulting pattern highlights two processes. On the one hand, unsurprisingly, divorces for desertion clearly increased in response to increased legal availability of divorce for that cause; when desertion was again restricted, divorces for desertion decreased. This suggests the existence of "genuine" petitioners who followed the law on the books, seeking divorces for desertion *if and only if* they had actually been deserted. On the other hand, however, is evidence of strategic behavior: divorces for adultery decreased as divorces for desertion increased, and vice versa. This apparent substitution between causes of divorce suggests that some men and women seeking divorce before 1893 cited adultery because it was the only available cause, but would have cited desertion if that option had been available to them; moreover, after the 1903 reform, at least some spouses who would have cited desertion if they could instead cited adultery. In other words, petitioners strategically adapted their suits to the legal context.

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Figure 3.2. Divorces by cause, North Carolina, 1867–1906
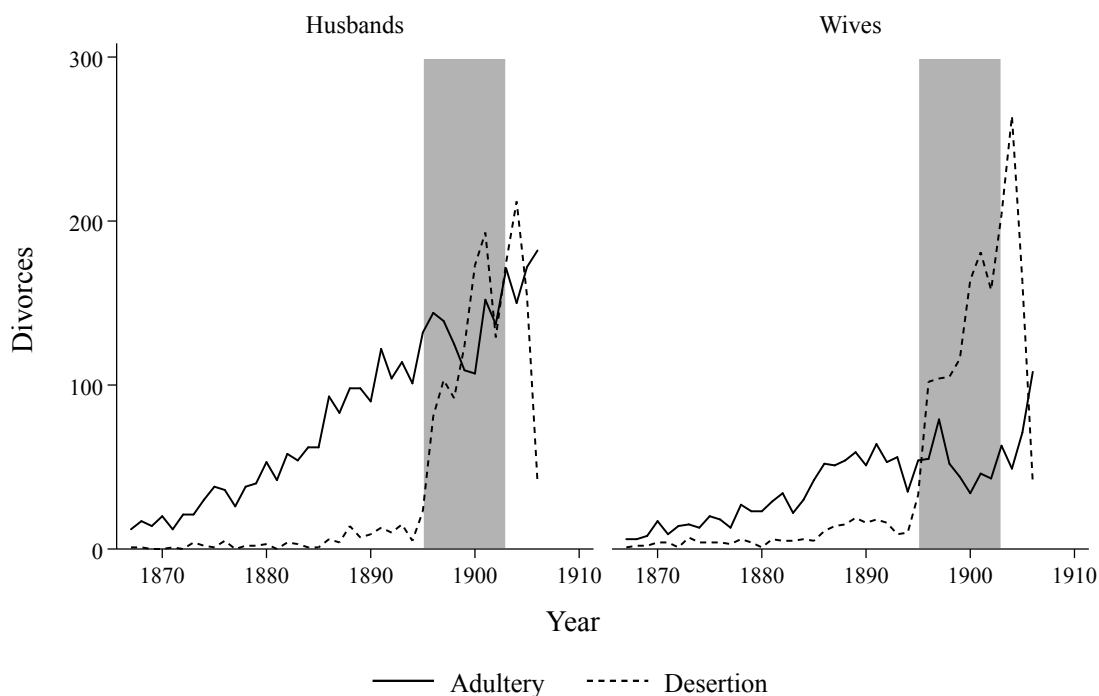
I generalize the logic of this example to a regression framework. At the macro level, I estimate a linear model:

$$d_{st} = \text{other\_causes}_{st}\beta + \delta_s + \gamma_t + \varepsilon_{st} \tag{4}$$

where the model is estimated separately for each gender g and cause c; d_{st} is the divorce rate in state s and year t; other_causes$_{st}$ is a variable counting the number of principal causes of divorce other than cause c that were legally available; δ_s and γ_t are vectors of state and year fixed effects, respectively; and β is the parameter of interest.[13] At the micro level I estimate a logit model:

$$\frac{\mathrm{p}_{ist}}{1-\mathrm{p}_{ist}} = \exp(\text{other\_causes}_{st}\beta + \delta_s + \gamma_t + \varepsilon_{ist}) \tag{5}$$

where the model is estimated separately for each gender g and cause c; p_{ist} is the probability of petitioner i citing specific cause c; and β is the parameter of interest. Models

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[13] This model is numerically equivalent to a first-differences estimator, and controls for all unobserved time-invariant heterogeneity across state–gender–cause clusters and all universal heterogeneity across years.

4 and 5 are identified on the assumption that legal changes are uncorrelated with other unobserved state shocks.

Routinization in divorce, 1867–1906

The ideal type of routinized divorce seeking implies that petitioners and lawyers increasingly converged on a set of standardized scripts for achieving desired legal outcomes more efficiently and reliably. Do statistics on divorce provide meaningful evidence of routinization? If so, are there identifiable patterns in the process across time, place, and gender?

Figure 3.3 shows the concentration of divorce causes, calculated by sex at the state level and presented as three-year weighted regional averages over the period of analysis. The graph demonstrates that concentration was increasing overall with historical time, but that levels and trends diverged by region and gender. In general, concentration decreased as one moved westward across the country, correlating broadly with legal and institutional development. (The exception is western husbands, who showed the most marked trend toward concentration.)

Figure 3.3. Concentration of divorce causes, by region, 1867–1906
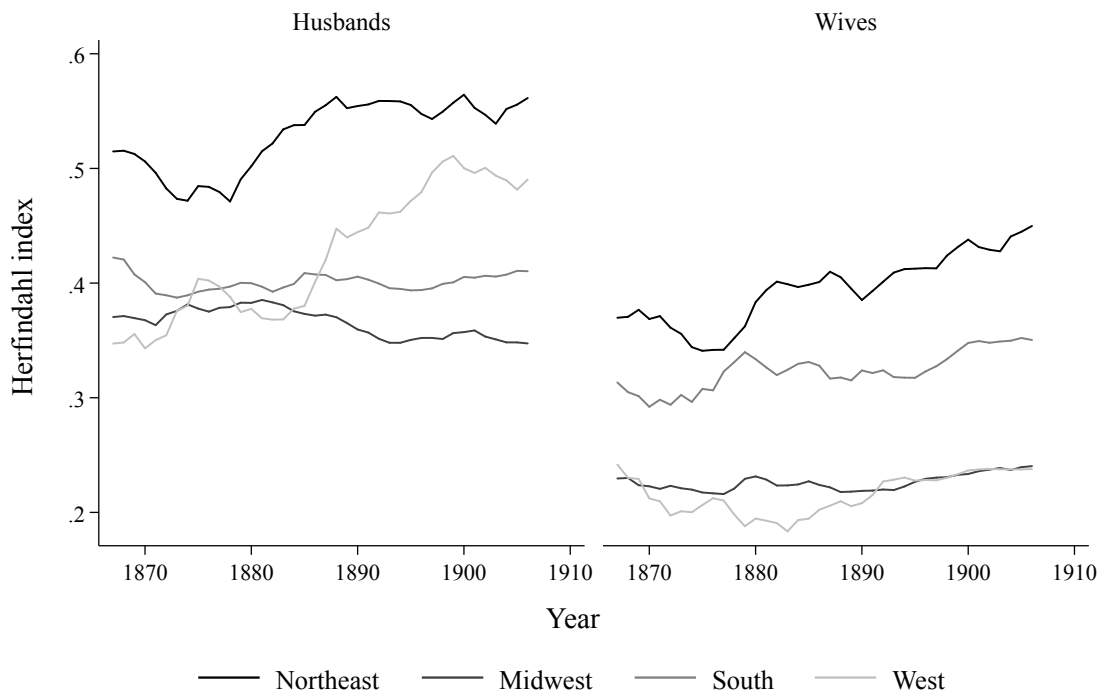
The most striking variation, however, is across gender, with wives citing a much greater diversity of causes than men. Because many states offered more legal causes to women than to men, descriptive evidence of gender differences in concentration may overstate differences in legal strategy by obscuring differences in legal opportunity. In order to correct for this problem, I estimate Model 2, a state-level gender-specific fixed-effects model that controls for legal variation and change (N=1,890). Figure 3.4 shows point estimates and 95 percent confidence intervals for several alternative specifications. Although a portion of the observed gender difference in concentration is attributable to legal differences, even after accounting for this, women petitioners exhibited substantially lower levels of concentration, with mean differences in Herfindahl–Hirschman indexes of -.105 (p < .001).

Figure 3.4. Effect of legal availability and gender on concentration, 1886–1906
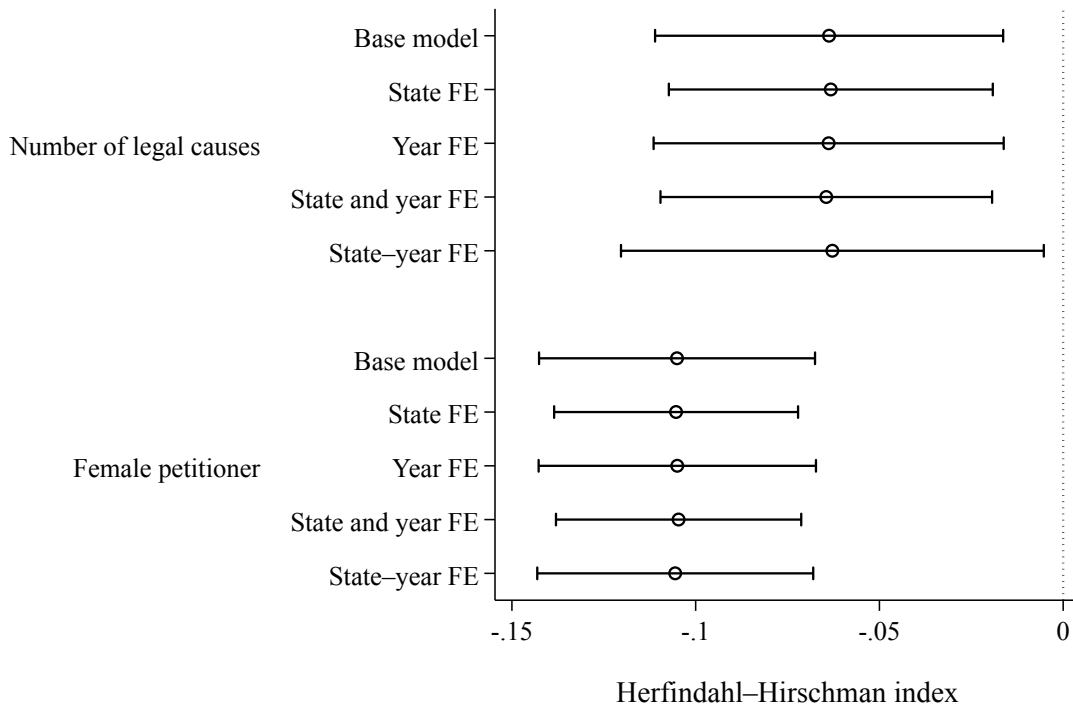
A second empirical implication of routinization is that individual divorce petitioners and divorce lawyers combined causes less frequently. Figure 3.5 shows three-year weighted regional averages of the mean number of causes cited by divorce petitioners, decomposed by sex of petitioner. Overall differences and trends in combination mirror

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those in concentration, offering robustness to the validity of each as a measure of routinization. Decreases in combination were most pronounced in the northeast, especially among women, and in every region throughout the period of analysis women were more likely to combine causes than men. Again, however, descriptive evidence obscures the role of legal variation and change, and so I estimate Model 3, an individual-level model of combination with legal controls (N=964,230). Figure 3.6 shows point estimates and 95 percent confidence intervals for several alternative specifications. Heterogeneity in legal environments did not meaningfully influence petitioners' combination of causes. Women displayed only slightly but statistically significantly higher levels of combination than men (p < .05).

Figure 3.5. Number of causes cited in divorces, by region, 1867–1906
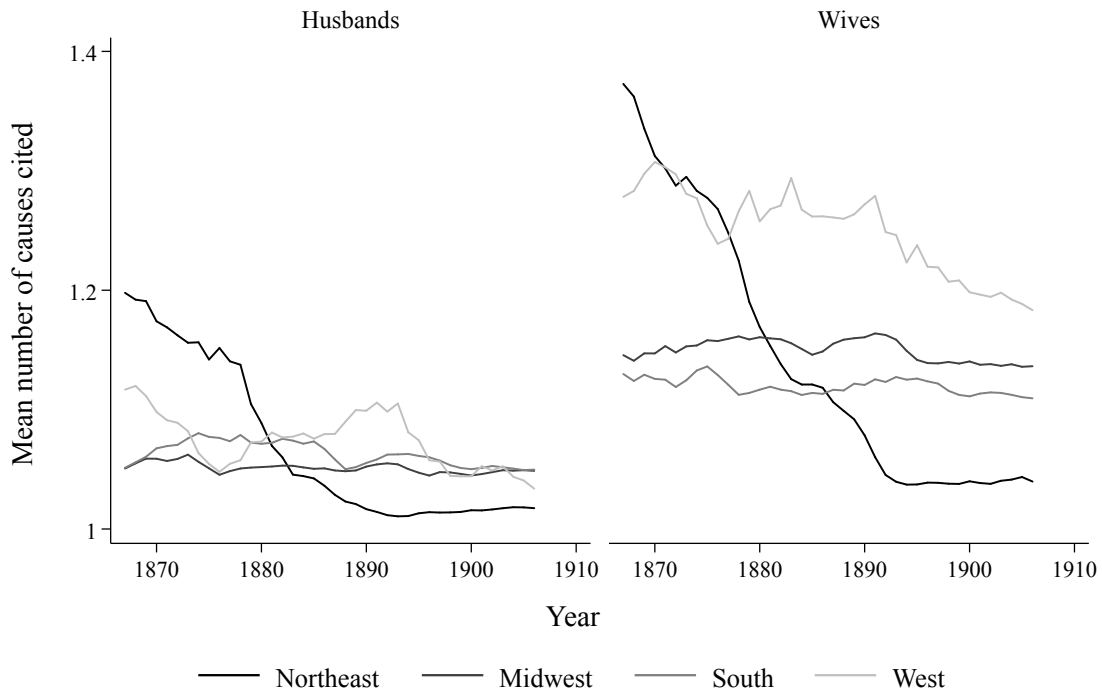
What conclusions can be drawn from gender differences in routinization? For one, women's relative lack of routinization is at odds with Friedman and Percival's (1974) argument that the preponderance of female over male petitioners reflected expedience among couples colluding to achieve a divorce. If this were so, we should expect to see women and not men citing fewer causes, as both a group and as individuals. Because wives filed for almost twice as many divorces as husbands, women's lower levels of routinization

are not likely to have reflected legal illiteracy or a lack of access to routinized scripts of divorce seeking. Indeed, many routinized processes centered on women petitioners. For example, as divorce proceedings in New York City relied increasingly on printed forms, such forms identified the wife as the plaintiff by default, requiring attorneys to cross out the *s* in "she" for male plaintiffs (Basch 1999:102).

Figure 3.6. Effect of legal availability and gender on combination, 1886–1906

Number of causes cited
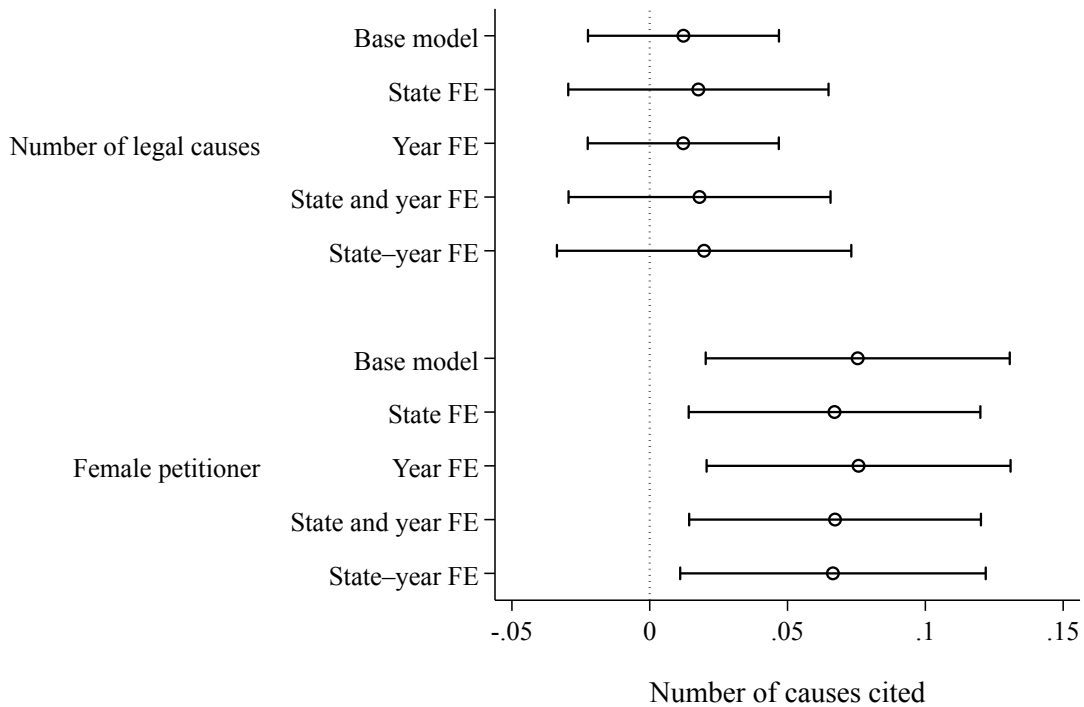
Women's low concentration and high combination of causes may best be seen as an alternative legal strategy rather than as a lack of strategy. What factors might have motivated such an alternative? For one, gender differences in routinization may have resulted from differences in the credibility afforded to the claims of men and women in courtrooms and judges' chambers. Higher evidentiary standards for women may have propelled them to be more genuine, sharing the full extent of their husbands' offenses, even at the expense of more elaborate and expensive suits. Conversely, a gender credibility gap may have led women to adopt an elaborative strategy, maximizing the number of causes cited to increase the odds that at least one cause would suffice. Perhaps most plausible, the citation of multiple causes was perhaps intended to establish female petitioners as

honorable women, "long-suffering and dutiful" wives who had made a sustained and good faith effort to maintain the union (Dayton 1995).

For another, women's more expansive divorce claims may reflect gender differences in the stakes of divorce. In a socioeconomic context in which most women's earning potential and wealth holding were severely limited, divorce had grossly unequal economic consequences for husbands and wives. (Indeed, contemporary divorce still poses modest economic benefits to men and significant costs to women [Peterson 1996]). Women's citation of multiple causes may have operated at a different margin than whether or not they acquired a divorce, instead bearing on whether their settlement would include financial support from their ex-husbands or a distribution of personal or shared property. Because women's access to household resources after marriage often depended on the faultiness of their husbands, women may have eschewed routinization in favor of maximizing an expected settlement.

Adaptation in divorce, 1886–1906

The ideal type of the adaptive divorce seeking implies that petitioners were able and willing to switch entirely the causes of divorce they cited in order to achieve their desired ends. Do the proposed methods of measuring adaptive behavior offer meaningful evidence of this phenomenon? Does such evidence correlate with routinized behavior, or does it exhibit some other pattern? Figure 3.7 presents results from Model 4 (range of N = [452, 945]). Point estimates represent the effect of introducing a new legal cause other than cause c on rates of divorce for cause c. In other words, negative coefficients indicate adaptation. Results are standardized for comparability: a coefficient of -1 marks a decrease in the divorce rate of one standard deviation. Results indicate that women were highly creative in navigating the strictures of divorce law, substituting claims of adultery, cruelty, and habitual intoxication for other causes of divorce when the latter were not legally available. Men appear to have engaged in similar strategies with regards to cruelty only, and the point estimate is smaller although not statistically differentiable from that for women.

Differences in effects across cause support the validity of the measure of adaptation. Divorce seekers were most likely to substitute causes with subjective or more easily falsifiable evidentiary bases (cruelty, habitual intoxication), and were least likely to substitute causes that were more difficult to conjure (desertion, adultery). Results for Model 5, in Figure 3.8, lend robustness to the finding that women relied on cruelty in particular as a substitute for other causes they would have cited were they legally available (cf. Griswold 1986).

Figure 3.7. Effect of other legal causes on divorce rate, by cause and sex, 1886–1906

Adultery
Husbands
Wives
Cruelty
Husbands
Wives
Desertion
Husbands
Wives
Habitual intoxication
Husbands
Wives
Neglect to provide
Wives

-1.5   -1   -.5   0   .5

Divorces per 1,000 married-spouse-present men/women
(standardized coefficients)
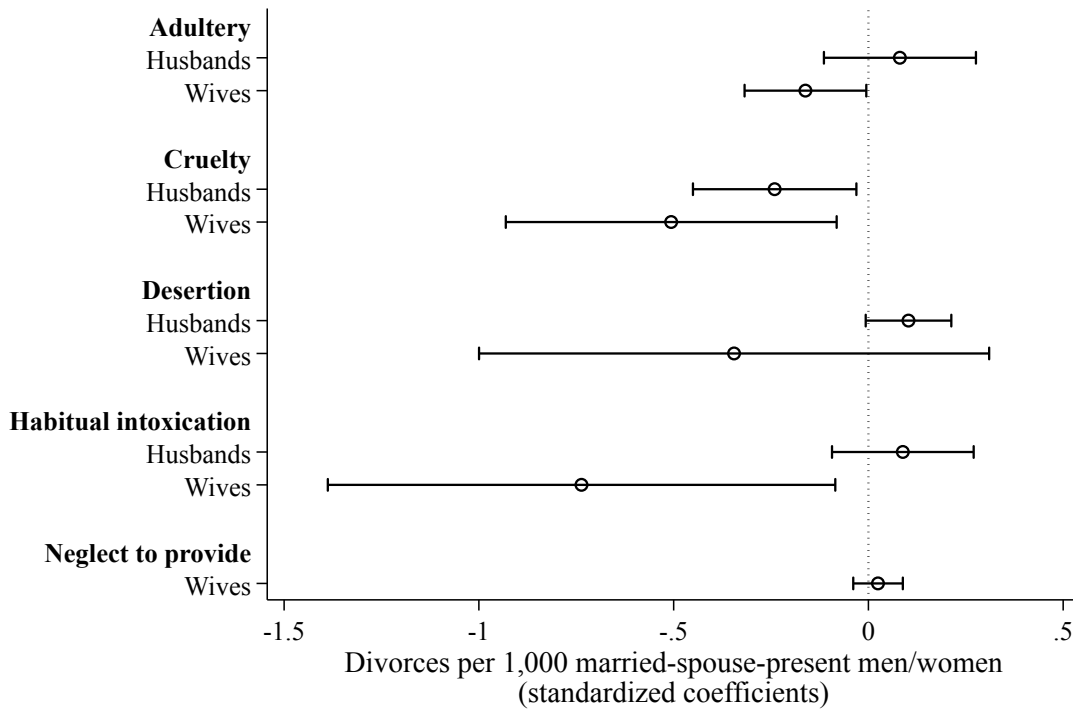
Why did women take up a strategy of adaptation more often than men? The pattern may reflect women's greater demand for divorce. For men, who relied on marriage less for economic subsistence, and for whom greater geographic mobility made remarriage without divorce possible, formalizing union dissolution in the form of a legal divorce was less important than for women. Women whose husbands neglected or deserted them faced significant economic costs if they allowed restrictive laws to deter divorce and prevent a financial settlement or remarriage.

Adaptation may also have been a strategy for managing the noneconomic costs of divorce. Several scholars have observed that under judicial patriarchy, men's claims about women's sexuality were particularly influential to jurists and consequential for women's post-marital life (May 1980; Basch 1999; Penningroth 2008). Women, especially women of color, navigated a complex "politics of respectability" in seeking divorce or contesting their husbands' divorce suits. Adaptation may have been a legal strategy for managing reputational costs by minimizing the risk of exposure to dishonorable discussions about intimate details, particularly about female sexuality.

Figure 3.8. Effect of other legal causes on cause choice, by cause and sex, 1886–1906
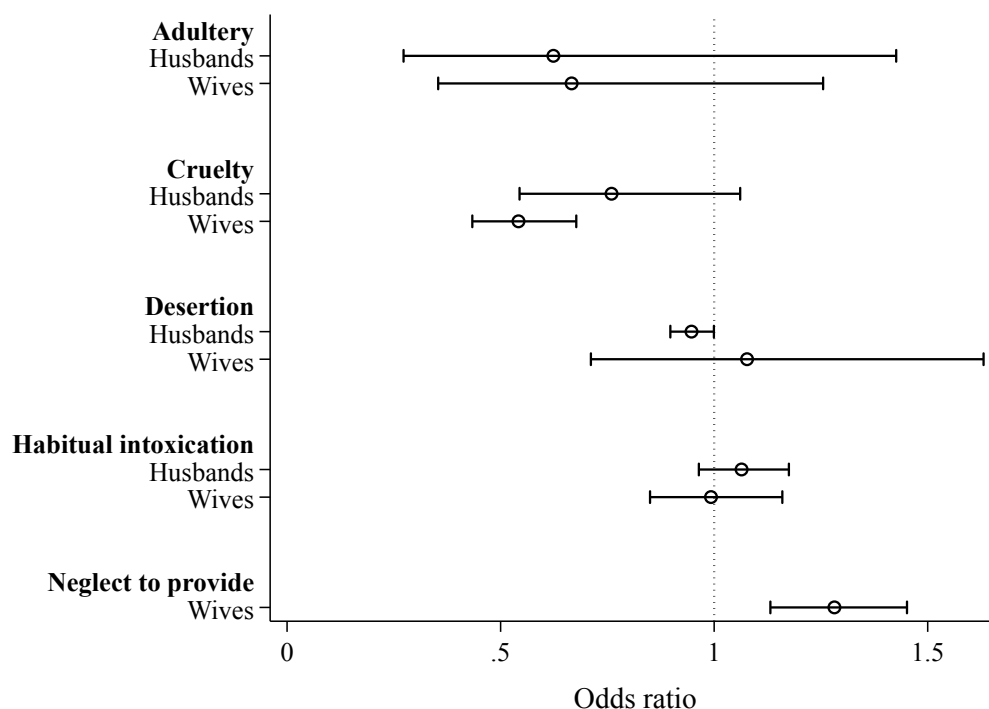
Conclusion

In the late nineteenth century, family law remained deeply patriarchal at the same time the law in action diverged greatly from the law on the books. In a period of historic acceleration in American divorce and rapid institutionalization of judicial divorce procedure, legal decoupling had complex implications for husbands and wives. Men and women alike exhibited strategic behavior in the face of restrictive divorce statutes, bending and breaking the law to achieve their desired ends. But men's and women's strategies were different. Husbands were much more likely to routinize their legal strategies, converging on reliable and efficient scripts for achieving divorce by concentrating divorce seeking around smaller sets of causes and combining multiple causes less frequently. By contrast, wives' divorce strategies were more adaptive than men's. In the absence of a legally available cause corresponding to genuinely occurring malfeasance by their spouse, women substituted other causes, tailoring their petitions to circumvent the constraints of their legal environment.

While my findings confirm some hypotheses generated from prior historical work, the widespread extent of strategic divorce behavior evidenced in this study is an inconvenient fact for studies that use divorce petitions and testimony to make direct

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inferences about men's and women's lived experiences and motivations for legal action (Basch 1999; Buckley 2002). Reflecting on divorce petitions from the late nineteenth and early twentieth centuries, social historian Elaine Tyler May (1980:10–11) argues that the "obligation to persuade" that was built into the fault-based divorce process provides the researcher an advantage because it "compelled the spouses to express their grievances against each other in court." Although May acknowledges that this obligation sometimes led to "distorted" accusations, in general, she believes, spouses "were not forced to lie or mold their difficulties into a particular formula in order to fit the laws." My findings show that spouses, especially wives, were in fact so compelled, and that husbands systematically excluded meaningful information about wives' conduct from their divorce petitions.

My approach therefore highlights the ability of quantitative social history to uncover patterns of behavior that are not visible through close examination of historical texts. Late nineteenth-century divorce-seekers' true reasons are not reliably indexed by textual traces of their performances in court. This is especially true for women, who faced a legal apparatus of evaluation and adjudication designed and enforced entirely by men. Through the exploration of aggregate trends, emergent properties, and counterfactuals, my approach offers a complementary method for making inferences about the reasoning and motivation behind historical social action.

If my statistical analysis gives newly clear and systematic evidence that men and women engaged in distinctive legal strategies when seeking divorce, it does not provide conclusive answers about why they engaged in the particular strategies that they did. Wives' citation of comparatively diverse and multiple divorce causes provides strongly suggestive evidence that women faced credibility gaps in nineteenth century divorce courts. Such gender gaps are increasingly well documented among witnesses and litigators in historical and contemporary legal settings (Dayton 1995; Basch 1999; Gilmore 2017). To reduce this gap, female divorce petitioners may have committed to genuine legal strategies or even embellished petitions to increase the chances of substantiating at least one cause or to perform the sympathetic character of the faithful but aggrieved wife. Wives' relatively greater adaptation to constrictive legal regimes also indicates that discrepancies between women's formal legal access to and effective demand for divorce were more acute than for men. Women's adaptive strategies may also be evidence of their management of reputational costs to the divorce process.

But such conclusions are speculative, and a return to archival sources is necessary to explain newly discovered patterns. Hopefully, therefore, the study illustrates the importance of researchers of different methods proceeding in close dialog, iteratively generating and testing hypotheses about complex legal processes that have left rich but fraught historical evidence.

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4. DON'T COUNT ON LOVE: DISCREPANT HISTORICAL MEASURES OF AMERICAN MARRIAGES

Introduction

The United States has been a historic laggard in the collection of vital statistics (Shryock, Siegal and Larmon 1980). Most advanced countries established national compulsory civil registration systems for marriages and divorces in the early twentieth century, whereas the United States has none. A national system for compiling detailed statistics was first implemented only in the 1950s, but never achieved complete participation by states and was discontinued in 1996 due to limited reporting and reduced funding. The poor quality and paucity of U.S. marriage and divorce data has led, even in very recent years, to basic misunderstandings about trends in contemporary family life (Kennedy and Ruggles 2012).

In contrast, the United States has been a historic leader in the development of official censuses (Anderson 1990; Emigh, Riley, Ahmed 2016a, 2016b). Therefore, the recent integration and publication of large-scale historical census microdata is quickly increasing researchers' ability to leverage "big data" to investigate the history of nuptial life in the United States. For example, the bibliography of the Integrated Public Use Microdata Series (IPUMS) project (Ruggles et al. 2018) lists more than 1,200 studies of marriage and the family relying on such data.

The ease with which such data can quickly be put to use, however, has a downside: it risks allowing researchers to overlook or ignore important data quality issues. A growing but widely overlooked literature explores the validity and reliability of historical census microdata, examining the undercounting of individuals (Ruggles 1991; King and Magnussen 1995; Hacker 2013), the misreporting of individual attributes (Conk 1981; Preston, Lim, and Morgan 1992; A'Hearn, Baten and Crayen 2009; Crayen and Baten 2010), and the consequences of measurement error for analytic results (Clogg, Massagli, and Eliason 1989; Raley 2002).

This study examines the accuracy of historical census measures of marriage events by comparing them to a new national dataset of vital records of marriage. Specifically, I examine discrepancies between marriage events enumerated in the 1900 U.S. decennial census of the population and those recorded in county-level vital records. Both datasets are assumed to be error-prone, but simple assumptions about overcounting allow me to examine the relative performance of each type of data (Kennedy and Ruggles 2012).

I find significant and pervasive discrepancies between counts of marriages from alternative sources. At most 64% of marriages recorded in vital records during census year 1900 appear in census data. Marriage rates per 1,000 unmarried men (women) 15 years and older are 38.2 (42.4) when calculated with Census data compared to 57.0 (63.4) using

vitals data.[14] I demonstrate that the misreporting of marital duration—used in this analysis to date marriage events—was nearly three times more common than misreporting of age, but that such misreporting does not explain persistent count discrepancies in excess of a known 5.2% overall census undercount (Hacker 2013). Remaining discrepancies must result either from the misreporting of marital status or the disproportionate underenumeration of married people. I argue that this evidence provides support for a model of successful official knowledge production that places greater emphasis on individual incentives to interpolate themselves into state legers than on states' abilities to coerce compliance by political subjects.

In what follows, I discuss prior research on historical census and vital records quality and generate predictions about the case. I then outline the data and methods used to compare historical U.S. marriage data quality. After describing record discrepancies and their administrative and population correlates, I conclude by discussing methodological and theoretical implications.

Official knowledge problems

What Scott (1998) calls states' "projects of legibility" fall into three broad categories. Censuses are universal and compulsory cross-sections of populations undertaken directly by centralized governments. Civil registration systems organize local bureaucrats' regular recording of vital events and systematize their reporting to centralized authorities. With contemporary advances in statistical methodology, sample-based surveys perform an increasingly large proportion of the work once done by both censuses and civil registration systems, but because the focus of this chapter is on historical data, I focus only on censuses and vital records.

Censuses

Census taking is a central activity of modern statecraft. It is, fundamentally, an effort by state actors to develop knowledge of populations as such to facilitate their governance (Foucualt 2003). Although the enumeration of persons for purposes of political apportionment was originally foremost among the mandates of American census takers, over the nineteenth century state actors' biopolitical projects expanded to include the measurement of personal attributes like race, nativity, and marital status, which they saw as central to the management of social order.

Census takers' ability to measure populations and their characteristics is not a foregone conclusion, and numerous political sociological studies examine the political and administrative struggles within and between state bureaucracies and civil society that characterize efforts to mount censuses (Loveman 2014; Emigh, Riley, Ahmed 2016a,

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[14] Vital records-based estimates of the 1900 marriage rate published by the Centers for Disease Control are slightly higher than my estimates: 61.3 for men and 68.2 for women (https://www.cdc.gov/nchs/data/series/sr_21/sr21_024.pdf).

2016b). A separate, methodologically focused literature examines measurement error in censuses, generally categorizing it into two broad classes: failures to capture individuals in censuses, and failures to record accurately the attributes of observed individuals.

Undercounting in censuses is a near-universal phenomenon, and results from a combination of enumerator error, foreign migration, language problems, and complex living arrangements. Although some people are often double-counted in censuses, it is usually a much larger number that are never counted. U.S. decennial censuses are thought to have undercounted the population by 6.0% in 1850, 5.2% in 1900, 5.4% in 1940, 1.2% in 1980, and 0.1% in 2000 (Robinson 2001:23; Hacker 2013:88).

When individuals are observed, a variety of factors affect the likelihood that information about them is accurately recorded. Many of these factors mirror the measurement challenges characteristic of survey research. On the one hand, census enumerators may make errors. In the United States, before a permanent census bureau was established in 1902, each decennial census was conducted ad hoc, and census administrators had very little control over enumerators, whose recruitment by federal marshals was characterized by political patronage more than professionalism (Anderson 1990).

On the other hand, census respondents may report inaccurate information about themselves. Misreporting may result from respondents' lack of self-knowledge or from weak incentives to provide accurate information (Tollnek and Baten 2016). Respondents may also misreport information when census questions and categories are unintelligible (Emigh, Riley, and Ahmed 2016a, 2016b). And of course, respondents may purposely misreport information, a tendency that is particularly relevant for the case of marriage. For example, black women in the 1910 census overreported their incidence of widowhood and the length of their marital unions, in part to conceal separations and children born out of wedlock (Preston, Lim, and Morgan 1992). Such misreporting may result from social desirability bias, in which respondents seek to manage their social interactions with enumerators, or from the perceived downstream consequences of honest responses. When the public salience of a census is high, the latter set of motivations can also affect undercounting (Kaneshiru 2013; American Sociological Association 2018).

Civil registration systems

Like censuses, civil registration systems seek to develop measures of the population. Differently than censuses, though, civil registration systems do so by locally recording vital events (i.e. births, deaths, marriages, divorces) as they occur, and aggregating local records. The two steps in this process—local recording and centralized reporting—highlight the two-fold origin and function of civil registration systems. On the one hand, vital recording itself has its origin not in the measurement of populations but rather in the official establishment of legal statuses for the purpose of clarifying rights and obligations (Dunn 1954). States may therefore promote vital recording not for biopolitical reasons, but rather in pursuit of the rule of law and the stabilization of property rights. On the other hand, the standardization of vital recording and the centralized aggregation of

vital records have a later, nineteenth century provenance, one explicitly linked to the calculation of population parameters.

Political sociological research also treats struggles to erect civil registration systems as a case of historical state building, with national state actors vying to mobilize local ones, and in turn, local bureaucrats vying to register political subjects (Emery 1993; Loveman 2005, 2007). Whereas measurement error in census data consists of both misrecording and underenumeration, problems in civil registration systems are almost exclusively one of missing records. The causes of underrecording in vital records depend on what is being measured. Most literature focuses on the undercounting of births and infant deaths, particularly among marginalized populations (Patterson 1980; Setel et al. 2007). With respect to marriage, several factors are particularly relevant to historical record quality. On the one hand are factors relating to marrying persons and their agents. Marrying people may have insufficient incentives to formalize their unions, particularly where the rule of law is weak or when personal wealth is minimal (Dubler 1998). This is especially true when populations are dispersed and transportation costs to official centers are nontrivial. Marrying people may also and actively resist registering unions if such unions are socially unsanctioned or if individuals are suspicious of incursions by modern officials into traditional institutions like marriage (Outhwaite 1995; Diamont 2001). On the other hand are factors relating to local officials. Because civil registration systems are often built upon preexisting local vital records systems, the political histories of localities can exert path-dependent influence on the administration and recording of marriages, with consequences for their completeness (Deporte 1926; Richmond and Hall 1929).

Counting marriages in the late nineteenth-century United States

What predictions do theories of official knowledge production and prior research on measurement error in censuses and civil registration systems offer about the accuracy of U.S. census and vital statistics of marriage at the turn of the twentieth century?

I contrast two major strains of theorizing which correspond loosely with Michael Mann's (1984) distinction between the despotic and infrastructural powers of states. By "despotic" power Mann means a coercive force "over" society possessed by the state elite and independent of regular negotiation with civil society groups. By "infrastructural" power Mann refers to the power of the state to work "through" civil society, penetrating and centrally coordinating its activities using its own methods and resources.

Many canonical theories of the state emphasize the importance of despotic power to the construction of official knowledge (Foucault 2009; Bourdieu 2014). Here, elite state actors and rational bureaucratic structures are central to the success of knowledge projects imposed from above. Although the 1900 U.S. census antedated the formation of a permanent Census Bureau in 1903, the project was administratively centralized and participation by political subjects was compulsory. By contrast, federal officials looked upon local marriage recording practices with frustration, noting that "many states lacked compulsory requirements for the proper return and record of marriages, while some of the states which had such a requirement lost the value of it because they imposed no penalty for its non-observance" (Wright 1891:18).

A second strain of theorizing focuses more closely on state's infrastructural power. Here, political subjects offer consent and cooperation with official knowledge projects because they help organize the business of civil society. In the case of marriage, individuals have incentives for their unions to be present in vital records because they increase certainty about one's ability to establish important legal statuses relating to legitimacy, support, and inheritance (Weber 1978; Swedberg 2003). By contrast, individuals lack "skin in the game" in responding to the inquiries of census enumerators, and may treat such interactions as cheap talk games or avoid them altogether.

These two theoretical perspectives on despotic and infrastructural state power each have implications for the counting of marriages in censuses and vital records. Despotic theories emphasizing state's coercive capacity suggest that:

> *Hypothesis 1: Census counts of marriages should be absolutely higher than vital records counts.*

Meanwhile, theories of infrastructural power emphasizing everyday incentives for cooperation suggest that:

> *Hypothesis 2: Census counts of marriages should be absolutely lower than vital records counts.*

However, as I discuss below, because my measures of record quality in census and vital records are relative, these hypotheses are poorly identified. A failure to confirm Hypothesis 1 does not imply a refutation of its corresponding theory, but rather a preponderance of support for its alternative Hypothesis 2, with the converse being equally true. Tests of Hypotheses 1 and 2 do not clarify which is absolutely true, but rather only which is truer than the other.

Clearer links can be made between theories of official knowledge and the correlates of differentials between census and vital records. Several states had compulsory systems for centralizing county-level vital records. Despotic theories imply that these sub-national but nevertheless top-down, elite projects of generating population-level knowledge should be associated with relative improvements to vital records data relative to any given benchmark. Because such civil registration systems cannot be expected to have affected census quality, despotic theories suggest:

> *Hypothesis 3: Census counts of marriages should be lower relative to vital records of marriage in counties in states with compulsory civil registration systems.*

Moreover, if censuses represent cheap talk games, as suggested by theories emphasizing infrastructural power, census performance relative to vital records should be negatively correlated with other known measures of census misreporting, such as the duration of one's marital status. Thus, infrastructural theories suggest:

Hypothesis 4: Census counts of marriages should be lower relative to vital records of marriage in counties where misreporting of marital duration is more severe.

Finally, each perspective offers predictions about the way that local bureaucratic professionalization should affect discrepancies between census and vital records of marriage. Some states established laws specifying penalties for local bureaucrats who failed to perform their marriage recording duties. Attempts to bolster record production by expressly coercing bureaucrats arguably represents the despotic corollary of coercion exacted upon subjects of measurement. Therefore:

Hypothesis 5: Census counts of marriages should be higher relative to vital records of marriage in counties where local bureaucrats were penalized for the non-performance of duties.

Finally, some states assigned duties to record marriages to dedicated, non-judicial county-level personnel, suggesting that they allocated administrative resources to the recording of marriages rather than relying on existing officers such as judges and court clerks to perform the task. Such local administrative behavior corresponds to the type of institutionalized penetration of civil society described by infrastructural theories. Therefore:

Hypothesis 6: Census counts of marriages should be lower relative to vital records of marriage in counties where local bureaucrats were dedicated to marriage recording.

To aid interpretation, note that odd- and even-numbered hypotheses correspond respectively to despotic and infrastructural theories of official knowledge production.

Data and methods

Measuring official knowledge problems

How can the accuracy of official statistics be evaluated? Information about populations collected by contemporary states, especially advanced industrial ones, is usually collected using methods designed to allow for tests of validity, reliability, and missingness. Less so for historical censuses. In the United States, the 1870 census was the first to stimulate widespread concern about record quality, and the 1900 U.S. census was the first to employ modern strategies for evaluating data quality, such as by asking respondents to report their age in years and their month and year of birth. Where such precautions were not taken at the time of enumeration, historical researchers must rely on more creative techniques.

With respect to the misrecording of individuals' attributes, record quality can be evaluated by comparing observed distributions to distributions strongly expected for theoretical or empirical reasons. One common example is the analysis of "age heaping." The true distribution of age in populations is relatively continuous, and the distribution of terminal digits in age-in-years is roughly uniform. However, observed distributions of age-in-years in many censuses exhibit sharp discontinuities at cognitively "preferred" terminal digits such as 0 and 5 (Zelnik 1961). Such discontinuities can be analyzed to quantify the misreporting of age.

Evaluating the undercounting of individuals is more difficult. Historical data quality can be assessed through contemporary accounts. Although such accounts can offer helpful clues and guiding details, they are usually anecdotal and often reflect motivated reasoning (King and Magnuson 1995). Demographic methods use alternative sources of data, such as records of births, deaths, and migration to generate independent estimates of populations which can then be compared to census counts. These methods provide the most convincing estimates of overall census undercounts, but they also have drawbacks. First, they rely on accurate mortality and age data, when both are known to be subject to nontrivial measurement error (Preston, Lim and Morgan 1992; Hacker 2013). Second, they are often only feasible for subpopulations, and rarely provide geographic or demographic detail about measurement error.

A third approach attempts to compare the same measure across multiple sources. Most common is to estimate undercounting by seeking to locate specific individuals from other records in census data. This approach has been particularly useful for identifying which types of people have been most likely to be excluded from historical censuses—the foreign-born; residents of large cities or frontier areas; infants and young children; the indigent; and itinerate groups such as borders, lodgers, and servants—groups whose undercounting is largely corroborated by contemporaneous accounts (Steckel 1991). But record linking studies have also tended to generate inflated and unreliable estimates of overall undercounts.

This study blends the logic of the demographic and record linking approaches, but also simplifies them. Rather than seeking to identify individual records in alternative sources, I compare aggregate counts, but over thousands of geographic subunits, namely, U.S. counties. And rather than using life tables to project population estimates onto census years, I derive flow data from the census which can then be directly compared to vital records. This approach has the merit of relying on many fewer assumptions than alternative methods, but is also subject to unique forms of bias, discussed below. It bears strong similarity to a recent effort by Kennedy and Ruggles (2012) to evaluate the quality of contemporary divorce data by comparing alternative sources under simple assumptions.

Discrepancies in marriage counts

The primary quantity of interest is the discrepancy between census and vital records counts of marriages. Census data are full-count microdata from the 1900 decennial census, published by the Integrated Public Use Microdata Series, or IPUMS (Ruggles et al. 2018).

The 1900 census asked married respondents about the duration of their current marriage.[15] Marriages occurring within the census year (June 1, 1899, to May 31, 1900) were coded "0." Of 27,812,405 married people, 34 men and 118 women have missing data on marital duration. Counts of marriages with durations less than one year therefore offer a measure of the number of legal marriages contracted in the census year. Domestic migration will cause measurement error at the state and county level but not at the national level. Net foreign immigration and mortality will cause, respectively, upward and downward bias in census measures of marriage events at all geographic levels. Notably, however, these sources of bias are confined to individuals whose legal marriage occurred during the census year *and* who migrated or died after their legal marriage and before census enumeration. Rates of legal divorce within the first year of marriage were trivial (Unites States Bureau of the Census 1909b). I calculate census counts of marriages separately using male and female responses, and compare them as a robustness check.

I digitized vital records of marriage published by the United States Bureau of the Census (1909b). In 1887 and again in 1907, the Census Bureau dispatched agents to most counties to compile the previous twenty years of local marriage and divorce records. Some states with civil registration systems reported county-level statistics directly, and records were collected by mail from local authorities in approximately 765 "sparsely settled or distant counties," and by agricultural surveyors in 206 Southern counties in the second survey. Data were patchy in the first survey, with 59% of counties missing marriage data. By 1900, though, outside of South Carolina, which did issue marriage licenses or record returned licenses, marriage counts were reported for 2,704 of 2,766 counties, or 97.8%. The data are annual flows of marriage "returns" at the county level. Marriage returns are certificates of marriage returned by officiants of celebrated marriages to local record keepers.[16] Vital records tabulate marriages by calendar year, so for comparison with census data I assign vital records counts of marriages to census year 1900 using vital records data from 1899 and 1900 and the monthly distributions of marriages in Massachusetts and Michigan in those years.[17] All subsequent references to years refer to census years—that is, to the twelve-month period preceding June 1 of the referenced year.

At national, state, and county levels I calculate absolute differences in counts of marriage d_a by subtracting vital records counts c_v from census counts c_c. Variation in true population of married people across geographies complicates comparison of discrepancies. I therefore construct the normalized difference:

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[15] Instructions to census enumerators were: "Number of years married. — Enter in this column for all persons reported as married (column 9) the number of years married (to present husband or wife), as 5, 9, 29, etc.; for persons married during the census year, that is, from June 1, 1899, to May 31, 1900, write '0;' for all other persons leave the column blank. Notice that this question can not be answered for single persons and need not be for widowed or divorced persons." (Barrows 1976)

[16] The Census Bureau also collected information on marriage licenses, but many states issued licenses irregularly or not at all, and so these figures were never reported.

[17] Results are robust to alternative methods of assigning marriages to census years, including using a larger sample of states' monthly marriage distributions in alternative years.

$$d_n = \begin{cases} \dfrac{d_a}{c_v} * 100, & c_c < c_v\,; \\[2mm] 0, & c_c = c_v\,; \\[2mm] \dfrac{d_a}{c_v} * 100, & c_c > c_v\,. \end{cases}$$

Using a piecewise denominator to calculate d_n avoids creating outlying values in sparsely populated geographies and yields a continuous measure with range [-100, 100] that can be interpreted as the *percentage difference in marriage counts*, with negative values indicating census undercounting relative to vital records data, and positive values indicating vital records undercounting relative to census data.

Because vital records are annual and the census measures marital duration in integer values, it is possible to compare census and vital records counts in prior census years. Importantly, however, older estimates will experience increasing measurement error resulting from migration and mortality, as well as from legal divorce and spouses' misreporting of separations as divorces, deaths, or non-existent unions (Preston, Lim and Morgan 1992). Counties with missing vital records of marriage are listwise deleted.

Marriage data quality

Discrepancies between census and vital records counts of marriage are a joint function of the quality of both data sources. Interpreting discrepancies is not, therefore, straightforward. For example, a moderate discrepancy could indicate that one source is valid and the other is moderately biased, or that one source is moderately biased and the other severely so. To help address this problem I develop several measures of data quality that are independent of count discrepancies, and compare them to discrepancies. This helps to better understand which source of data is to blame.

To measure the quality of census data, I examine the misreporting of marital duration. I use a strategy identical to that used to analyze age misreporting. Absent the ability or incentive to provide accurate information about one's age, respondents tend to supply cognitively convenient numbers, usually ones ending with digits 0 or 5 (Zelnik 1961).[18] The aggregate result of misreporting due to digit preference is "heaping," in which response frequencies cluster around predictable responses. Because marital duration, like age, is measured in years, it can be expected to exhibit a similarly discontinuous distribution.

To measure heaping in marital duration I use the Whipple Index, a widely used and validated measure of heaping (A'Hearn, Baten and Crayen 2006). The index takes the form:

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[18] It is noteworthy that U.S. Census officials appear by 1900 to have been aware of this behavior, alerting enumerators that "[a]n answer given in round numbers, such as 'about 30,' 'about 45,' is likely to be wrong" and encouraging them in such cases to "endeavor to get the exact" value (Barrows 1976:206). The 1900 census was the first to ask respondents about their month and year of birth as well as their age in years, to evaluate the accuracy of the latter.

$$W = \frac{\sum(n_5 + n_{10} \ldots + n_{55} + n_{50})}{\frac{1}{5}\sum_{i=2}^{51} n_i} \times 100$$

where n_i represents the frequency of respondents reporting marriages of length i. I restrict the range of values over which I evaluate heaping to 2 to 51 years. This balances the frequency of terminal digits, minimizes the number of counties with married populations too small to evaluate heaping (56 counties, for which $\sum_{i=2}^{51} n_i < 250$), and avoids misreporting issues specific to marital durations of less than two years, discussed below. In reporting results, I use a simple transformation of the Whipple index:

$$\widetilde{W} = \begin{cases} \dfrac{W - 100}{4}, & W > 100; \\ 0, & W \leq 100; \end{cases}$$

that can be interpreted as the *percentage of respondents heaping marital duration* (cf. Crayen and Baten 2010).

To measure vital records quality, I examine legal and administrative characteristics of counties relevant to marriage recordkeeping, as tabulated in United States Bureau of the Census (1909a). I create three indicator variables. The first measures whether a county belonged to a state that required the *centralized reporting* of marriage records to the state. By 1900, ten states comprising 448 counties had such requirements. The second measure indicates whether a county belonged to a state requiring that marriage licenses be returned to and recorded by a *non-judicial officer*. By 1900, twenty-nine states comprising 1,726 counties had such provisions. The third measure indicates whether a county belonged to a state specifying *penalties*—financial, criminal, or otherwise—for marriage recorders' non-performance of duties, including specifying that parties whose marriages were not recorded had a right to bring personal legal suit against the negligent official. In 1900, twenty-five states comprising 1,405 counties allowed for such penalties. It should be noted that because these measures are state-level policy indicators, they fail to capture variation across states in policy details, or variation across counties within states in the quality and intensity of their implementation.

Other explanatory variables

I also use IPUMS microdata to generate several other measures of counties' demographic characteristics. Because literacy can be expected to bey positively correlated with accurate census reports, I measure the percentage of the population that is *literate*, defined as being able both to read and to write. Because historical censuses are known to have undercounted individuals who were *foreign born*, *urban* residents, or *black* (Hacker 2013), I include measures of the percentages of the population at least 16 years old comprising each of these groups, where urban places are defined as having 2,500 or more people. I also include a measure of the percentage of the population *born in another state*

to control for bias resulting from internal migration. Because census quality is also known to have varied by *region*, I also include dummies for the Census Bureau's four geographic regions.

Missing marriages?

How exactly to census- and vital records-based counts of marriages compare? Figure 4.1 shows the distribution of county-level discrepancies in counts of marriages occurring in census year 1900, with negative values indicating census undercounts as a percentage of vital records and positive values indicating the opposite. Discrepancies calculated using male and female census responses are displayed separately. For both sexes, 90% of counties had a census undercount, with the median (mean) county having a 31% (29%) census undercount.

Figure 4.1. Percent difference, census and vital records counts of marriages, U.S. counties, 1900.
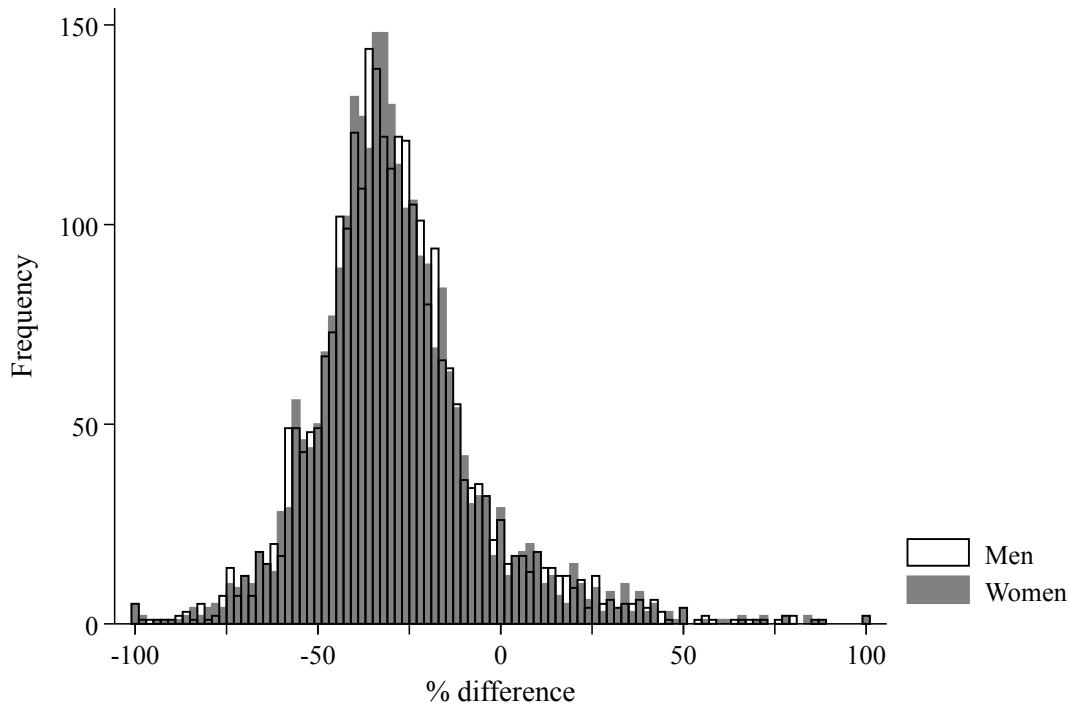
An examination of the distribution of marital duration in the census, displayed in Figure 4.2, indicates a possible explanation for such severe discrepancies. Considerably more people reported being married 1–2 years (1,217,733) than being married less than one year (887,233). Explaining such a discrepancy is difficult except in terms of measurement error: no historical evidence indicates a negative shock to marriage rates in 1900, and neither immigration, mortality, nor divorce patterns offer a plausible explanation for the gap. A much more likely scenario is that individuals married less than 1 year were recorded as having been married 1–2 years. This could result from rounding—it would be unsurprising if nontrivial numbers of newlyweds identified as having been married "one year." It is even more likely that such misrecording resulted from respondents' misunderstanding of the marital duration question. Census enumeration began on June 1, 1900, and the reference date for marital duration was that day. On the one hand, it is possible that responses given at later enumeration dates were systematically upwardly biased—for example, an individual married June 15, 1899 and enumerated July 1, 1900 should have been recorded as having been married less than one year (as of *census* day), but might instead have been misrecorded as having been married for more than one year (which, as of the date of *enumeration*, they in fact had been). On the other hand, respondents may simply have interpreted enumerators' questions to refer to calendar years, upwardly biasing measurement of marital duration for all marriages contracted between June 1, 1899 and December 31, 1899.

Figure 4.2. Distribution of marital duration in the 1900 U.S. census.

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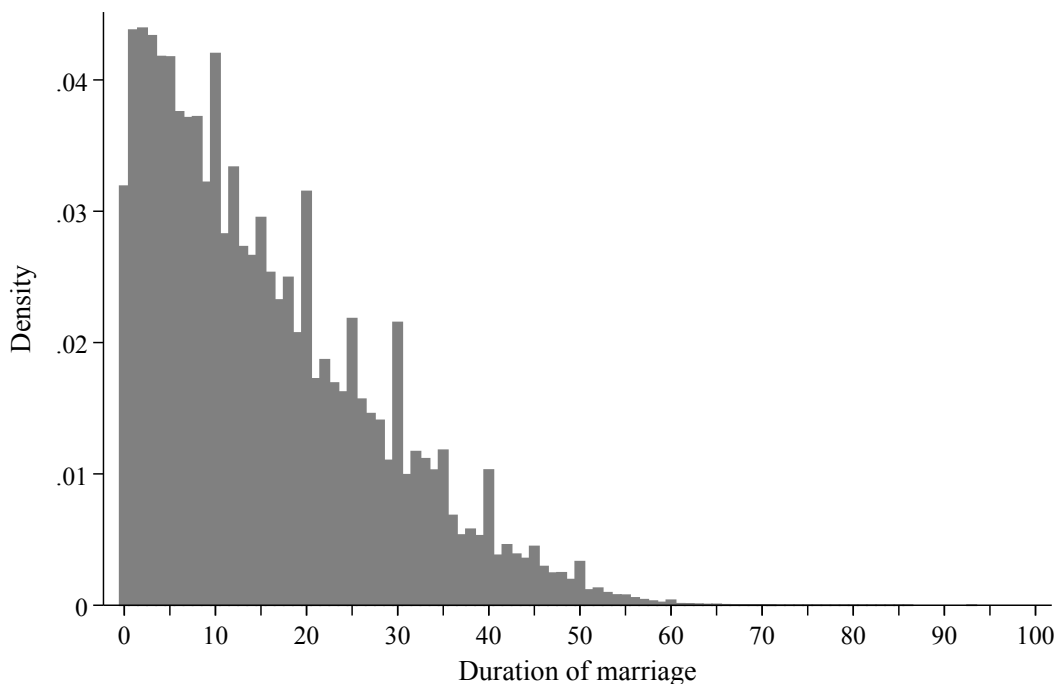
Other evidence suggests that marital duration was widely misrecorded in the census. Figure 4.2 also clearly indicates heaping at terminal digits 0 and 5. Transformed Whipple indices indicate that 4.7% of married individuals expressed digit preference in reporting marital duration in the 1900 U.S. census, compared to 1.8% who heaped their self-reported age.[19] But equally compelling evidence indicates that misrecording of marital duration cannot explain away count discrepancies. If apparent undercounting of marriages contracted in 1900 was in fact an artifact of misrecorded marital duration, this would result in excessively high counts of marriages lasting 1–2 years. In fact, Figure 4.2 shows that slightly *fewer* such marriages were recorded than those lasting 2–3 years. It is considerably less plausible to suppose that newlyweds would report having been married two or more years.

Figure 4.3. Annual discrepancies, census and vital records counts of marriages, U.S., 1889–1900.

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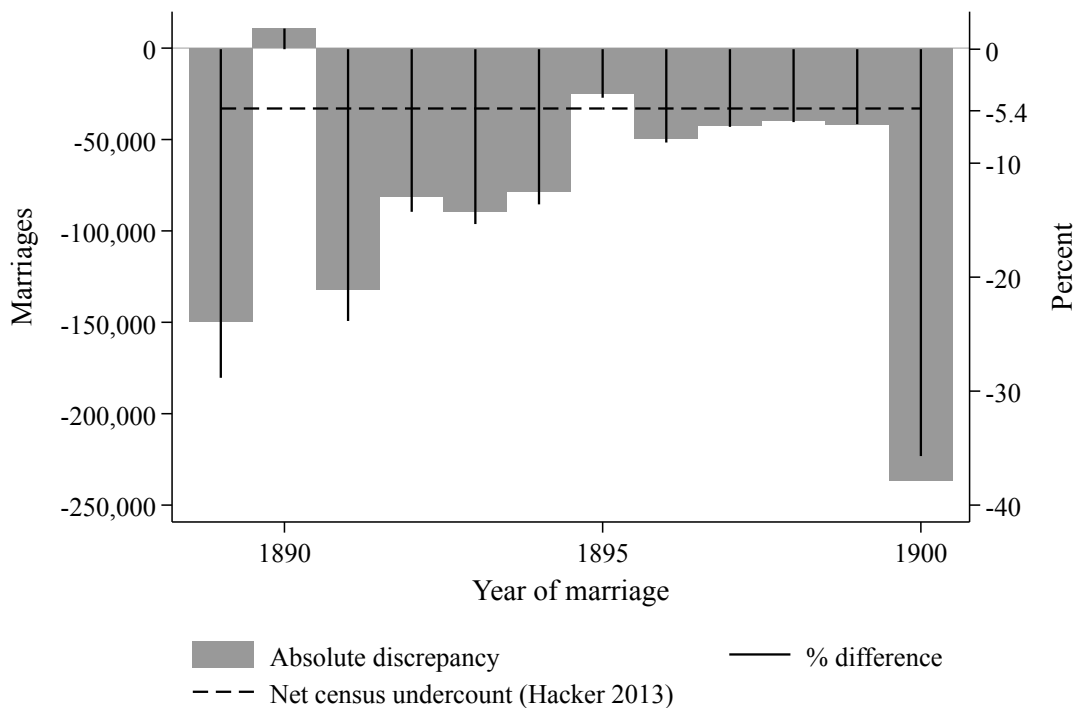
[19] The unweighted average of percentage of county residence heaping marital duration was 4.2%, compared to 1.5% for age. County-level heaping in marital duration and age were strongly correlated: a bivariate OLS regression model of marital duration heaping on age heaping yields a correlation coefficient of 1.025 (p < 0.0000, state-clustered standard errors). Demographic correlates of heaping in both marital duration and age corroborate prior research on patterns of census misreporting.

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Figure 4.3 gives additional evidence of missing marriages, displaying the total discrepancies in counted marriages by year of occurrence. Discrepancies are calculated by totaling only counties with non-missing marriage data from both sources, and should be interpreted with increasing caution in earlier years because potential bias from foreign migration, mortality, and divorce increases. Known overall census undercounts of the population were approximately 5.2% in 1900 (Hacker 2013), plotted as a dashed line. If apparent count discrepancies were simply the result of wrongly recorded marital duration, the census should have overcounted marriages in years prior to 1900 (that is, relative to the -5.2% baseline), and approximated a net count discrepancy of -5.2% over multiple years. To the contrary, census undercounts of marriages exceeded the baseline in all prior years excepting 1890 and 1895, outliers clearly attributable to heaping (cf. Figure 4.2). Comparing counties with non-missing census and vital records data, marriages contracted in 1900 were undercounted in the census by 35.7%; 21.4% contracted in the period 1899–1900 were undercounted; 12.0% contracted 1894–1900 were undercounted; and 13.7% in the period 1889–1900.

Therefore, although misreporting of marital duration was widespread, accounting for this fact appears to explain at most about one third of census net undercounts of marriages occurring in 1900. On the assumption that vital records do not overcount marriages, one is therefore left to conclude that census–vital records discrepancies in counts of marriages result primarily from the erroneous recording of unmarried status for married individuals who were enumerated, and/or the disproportionate underenumeration of married individuals. Taken together, evidence of a general undercount of marriages in the census relative to vital records suggests support for Hypothesis 1 and a lack of support for Hypothesis 2—in other words, the influence of despotic power on the success of marriage measurement appears to have been smaller than the influence of infrastructural power.

Data quality and bounded estimates

National net census undercounts relative to vital records indicate that the latter measure of marriage has higher overall validity. But Figure 4.1 evidences wide variation across counties in the severity of count discrepancies. Variation in discrepancies could be attributable to the unreliability of census data, vital records, or both. What do exogenous indicators of data quality suggest about the sources of count discrepancies?

Table 4.1 gives summary statistics for attributes of U.S. counties in 1900. Table 4.2 reports estimated coefficients and standard errors for models of county-level count discrepancies measured over three different intervals—census years 1900, 1899–1900, and 1894–1900. Models are estimated separately by whether men or women were used to generate census counts of marriages, with sex differences reported. If variation in count discrepancies was driven by unreliable census data, discrepancies should be correlated with exogenous measures of census data quality, such as the degree of heaping in continuous self-reported variables. The first row of Table 4.2 shows that heaping in marital duration is strongly correlated with census undercounts of marriage in 1900: for male-based

estimates, a one standard deviation increase in marital heaping led to a .15 standard deviation widening of the gap between census and vital records counts of marriages. The strength of this effect decays considerably, however, when discrepancies are measured over multiple years. In other words, counties whose residents more frequently misreported marital durations between years 2 and 51 were also more likely to undercount newlyweds, but not necessarily marriages overall. This pattern offers additional evidence in support of the conclusion that discrepancies in 1900 resulted in part from misreports of marital duration, but that such misreports cannot explain overall net undercounts. This offers modest support for Hypothesis 4, but cannot rule out the possibility that misunderstanding rather than cheap talk explains the decay in effects moving upward in the distribution of marital duration.

Table 4.1. Summary statistics, U.S. counties, 1900.

	N	Mean	S.d.	Min.	Max.
% difference in counts					
1900, men	2,698	-28.6	22.8	-100.0	100.0
1900, women	2,698	-28.8	22.6	-100.0	100.0
1900, sex difference	2,698	0.2	4.0	-50.0	38.5
1899–1900, men	2,691	-16.5	20.8	-90.3	89.1
1899–1900, women	2,691	-17.9	19.8	-90.3	88.2
1899–1900, sex difference	2,691	1.4	5.4	-24.0	71.4
1894–1900, men	2,647	-9.7	20.7	-89.1	89.8
1894–1900, women	2,647	-10.3	19.5	-89.3	89.2
1894–1900, sex difference	2,647	0.6	3.6	-15.2	32.4
% mar. dur. heaping	2,783	4.2	2.8	0.0	19.0
% age heaping	2,816	1.5	1.9	0.0	16.5
% literate	2,838	84.8	14.0	26.2	100.0
% urban	2,838	14.0	22.2	0.0	100.0
% black	2,838	12.9	20.6	0.0	93.3
% foreign born	2,838	13.7	16.2	0.0	89.0
% born other state	2,839	29.6	22.4	0.0	97.9
Population in 1,000s	2,838	26.8	73.6	0.004	2,067.9

Two pieces of evidence, however indicate that vital records do not themselves provide reliable counts of marriages, and contributed as well to inter-county variation in count discrepancies. First, Table 4.2 shows that census undercounts relative to vital records were larger in counties that had dedicated personnel for the recording of marriages. This is consistent with Hypothesis 6: counties' devotion of more administrative resources to marital recordkeeping led to more accurate—in this case, higher—counts of marriages, and therefore larger census undercounts relative to vital records. Estimated coefficients for

centralized reporting (Hypothesis 3) and personal penalties for recorders (Hypothesis 5) have the expected signs, but are not statistically significant.

Second, net census *overcounts* of marriage in some 236 counties, visualized in Figure 4.1, suggest that in some places, census data outperformed vital records outright. Because these counties had disproportionately small populations and were overrepresented in Western states, it is difficult to draw general implications from cases of vital records undercounts. But they may indicate that vital records overall contained nontrivial degrees of marital undercounting, and that observed net census undercounts of vital records understate the true undercount of U.S. marriages.

Population correlates of measurement error

Table 4.2 further reports estimates of the association between county-level marital count discrepancies and the characteristics of county populations. Less literate counties might be expected to have higher rates of census misreporting, but any such effect appears to be captured by the parameter for marital duration heaping.

By contrast, counties with greater proportions of urban residents exhibit substantially larger census undercounts of marriages. For male-based estimates, a one standard deviation increase in urban residents leads to a .15 standard deviation increase in the census undercount of marriages. The strength of this effect diminishes by about a third when considering multi-year discrepancies, but remains statistically significant. Counties with larger proportions of black residents also had larger census undercounts of marriage in 1900: a one standard deviation increase in the former is associated with a .09 standard deviation increase in the latter. No such association is evidenced in multi-year counts.

As discussed above, national census estimates of marital events occurring in the United States will be biased upward by foreign migration, and local estimates will be similarly biased by domestic migration. Such bias should increase with multi-year estimates. As expected, larger foreign-born populations are associated with larger census counts of marriage relative to vital records, but only in multi-year estimates and increasingly so with larger periods—almost certainly indicating the inclusion of foreign contracted marriages in census data. Domestic migration, however, seems to contribute no meaningful bias to estimates of count discrepancies. The most striking correlates of disparities between census and vital records counts of marriage are geographic. Figure 4.4 plots normalized discrepancies at the state level. Discrepancies appear to be most concentrated in the South, corroborating prior historical research documenting problems of census enumeration in that region. However, estimated models described in Table 4.2 include region dummies (the reference category is the Northeast), alongside administrative and population covariates. This affords a better understanding of the marginal effects of place itself. The results complicate the conventional understanding of Southern underdevelopment. The influence of geography on count discrepancies in 1900 alone is ambiguous, but considering multi-year discrepancies shows clear patterns. Census undercounts were indeed severe in the South, but even more so in the Midwest, where, net of other factors, some 13 to 19% of men's marriages went uncounted in the census.

Table 4.2. Percent difference, census and vital records counts of marriage, U.S. counties, 1900.

Basis of census count	1900			1899–1900			1894–1900		
	Men	Women	Dif.	Men	Women	Dif.	Men	Women	Dif.
% mar. dur. heaping	-1.576***	-1.480***	-0.096*	-1.007**	-0.865**	-0.142*	-0.494	-0.350	-0.144**
	(0.289)	(0.289)	(0.042)	(0.311)	(0.301)	(0.065)	(0.262)	(0.253)	(0.053)
Centralized reporting	-2.204	-1.947	-0.257	-1.366	-1.242	-0.124	-3.624	-3.266	-0.359
	(2.525)	(2.515)	(0.152)	(2.043)	(1.883)	(0.456)	(2.597)	(2.244)	(0.458)
Non-judicial recording	-3.361**	-3.248**	-0.114	-4.502**	-4.404**	-0.098	-5.158*	-5.096*	-0.062
	(1.172)	(1.187)	(0.192)	(1.543)	(1.440)	(0.349)	(2.034)	(1.907)	(0.318)
Non-recording pen.	0.582	0.493	0.088	1.334	1.209	0.126	1.441	1.236	0.204
	(1.253)	(1.232)	(0.177)	(1.637)	(1.509)	(0.286)	(1.854)	(1.775)	(0.239)
% literate	-0.192	-0.199	0.006	-0.239	-0.231	-0.007	-0.196	-0.189	-0.007
	(0.138)	(0.137)	(0.013)	(0.147)	(0.141)	(0.019)	(0.161)	(0.152)	(0.018)
% urban	-0.158***	-0.159***	0.001	-0.110**	-0.128***	0.019*	-0.083**	-0.092**	0.009
	(0.032)	(0.031)	(0.004)	(0.033)	(0.034)	(0.007)	(0.030)	(0.028)	(0.005)
% black	-0.126*	-0.135**	0.009	-0.048	-0.060	0.012	-0.073	-0.076	0.003
	(0.048)	(0.049)	(0.005)	(0.059)	(0.057)	(0.011)	(0.054)	(0.052)	(0.010)
% foreign born	0.058	0.031	0.027***	0.250**	0.181*	0.069***	0.416***	0.352***	0.065***
	(0.070)	(0.068)	(0.006)	(0.075)	(0.068)	(0.015)	(0.072)	(0.065)	(0.016)
% born other state	0.061	0.046	0.014**	0.067	0.052	0.016	0.110	0.095	0.015
	(0.040)	(0.038)	(0.004)	(0.049)	(0.044)	(0.010)	(0.059)	(0.055)	(0.008)

Table 4.2. *(continued)*

Basis of census count	1900			1899–1900			1894–1900		
	Men	Women	Dif.	Men	Women	Dif.	Men	Women	Dif.
Region									
Midwest	-6.893*	-5.754	-1.139***	-13.344***	-10.985***	-2.359***	-19.080***	-17.007***	-2.073**
	(2.933)	(2.884)	(0.266)	(3.175)	(2.803)	(0.620)	(4.379)	(3.869)	(0.626)
South	-6.507	-5.935	-0.572*	-11.674**	-10.284**	-1.390*	-10.893*	-9.772*	-1.121
	(3.466)	(3.407)	(0.272)	(3.867)	(3.515)	(0.630)	(4.455)	(3.997)	(0.622)
West	5.373	6.002	-0.630	2.031	1.403	0.628	-0.386	-2.070	1.684
	(3.825)	(3.656)	(0.502)	(4.261)	(3.588)	(1.028)	(5.253)	(4.626)	(0.881)
Observations	2,671	2,671	2,671	2,664	2,664	2,664	2,624	2,624	2,624
Adjusted R^2	0.130	0.122	0.019	0.144	0.116	0.091	0.215	0.170	0.221

Note: Standard errors, clustered at the state level, in parentheses
* p < 0.05, ** p < 0.01, *** p < 0.001

Figure 4.4. Percent difference, census and vital records counts of marriages, U.S. states.

1900

% difference
☐ -25 – -12
▨ -32 – -25
▨ -35 – -32
▨ -39 – -35
■ -64 – -39
☐ No data

1899-1900

% difference
☐ 0 – 6
▨ -16 – 0
▨ -22 – -16
▨ -26 – -22
■ -46 – -26
☐ No data

1894-1900

% difference
☐ 0 – 25
▨ -9 – 0
▨ -12 – -9
▨ -18 – -12
■ -29 – -18
☐ No data
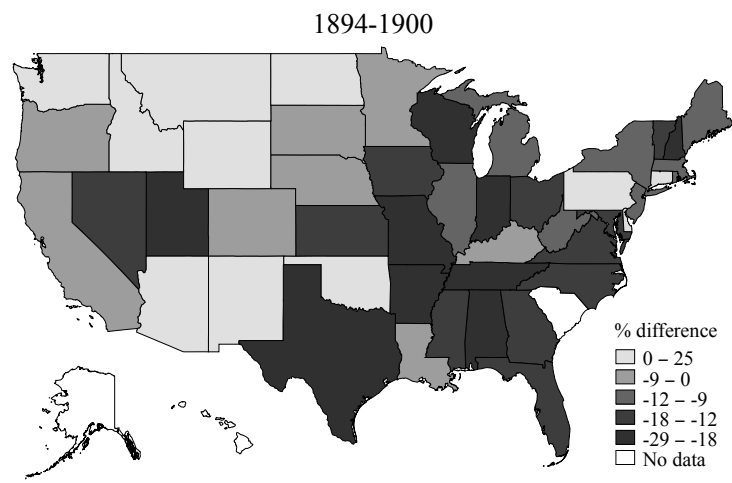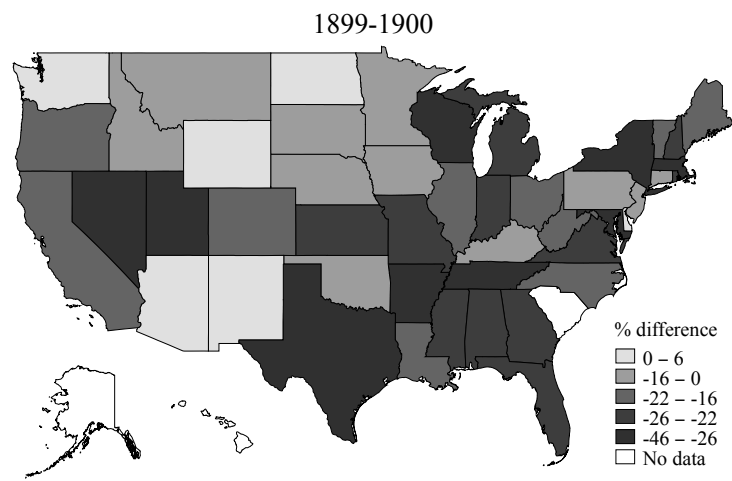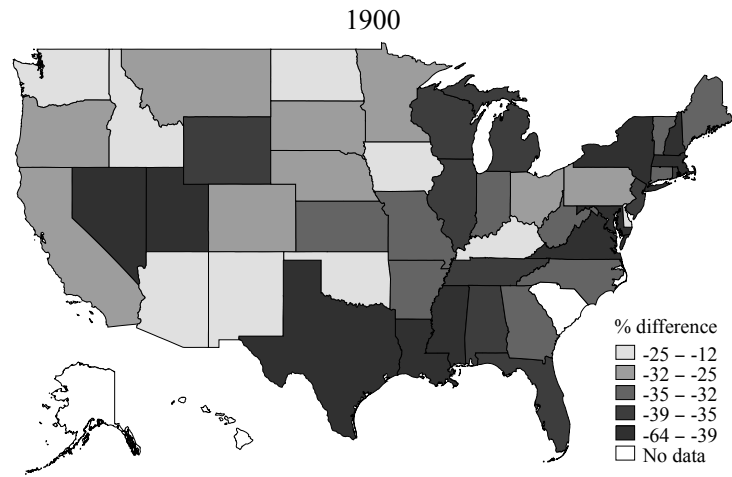
Conclusion

Comparison of U.S. census and vital records data for marriages occurring in 1900 indicates serious flaws in census-based measures of marriage. Analysis of the distribution of marital duration in the census gives evidence of enumeration error consistent with respondents' expression of digit preference and possible misunderstanding of census reference periods. But marital duration misreporting cannot account for multi-year census shortfalls of recorded marriages relative to vital records. These shortfalls indicate either the inaccurate enumeration of large numbers of married individuals as unmarried, or the disproportionate underenumeration of married people, particularly newlyweds. The large number of marriages missing from the census—and the unevenness of their missingness across groups and geographic areas—has significant consequences for scientific analysis of historical family patterns in the United States. This study compared measures of marital *events* in census data and vital records, but the findings can only be explained if the marital *status* of large numbers of individuals is misrecorded, or individuals are missing not at random from the census, specifically on the basis of their marital status. Marital status measures in historical census microdata have been used to calculate national historical trends in nuptiality (Haines 1996) and marital dissolution (Ruggles 1997; Cvrcek 2009), and are even more commonly relied upon in the analysis of the causes and consequences of marriage and divorce (Bloome and Muller 2015; Bloome, Feigenbaum and Muller 2017). The results of this study recommend caution when using census-based measures of marital status before the institutionalization of the Census Bureau in 1902.

This study extended the exercise carried out by Kennedy and Ruggles (2012), who compared alternative sources of contemporary marriage and divorce data and, using a simple set of assumptions, were able to show that conventional sources of data on family patterns were plagued by gross undercounts. Fortunately, their critique pointed to a preferred alternative in new census data. The historical situation is less auspicious because vital records, which provided a heuristic for evaluating census data, seem to provide a substitute in only a very limited sense. Vital records are in the first place limited in the information they contain: they measure marital flows but not stocks, and for early periods do not decompose marital events by the characteristics of marrying persons. Nevertheless, such exercises continue to provide helpful background knowledge to those using newly available marriage and divorce data—contemporary or historical—in new ways.

The study also has implications for the sociology of state formation and the sociology of official statistics. Comparing strains of theory that emphasize, respectively, the despotic and infrastructural powers of states, this study showed that the latter appeared to be more important to generating accurate counts of marriages in the United States at the turn of the twentieth century. Contemporary political sociology is taking greater interest in how states "see" (Scott 1998) at the same time it increasingly understands states as "many-handed" (Orloff and Morgan 2017). This is to say, states construct political subjects and governed populations through their "principles of vision and division" (Bourdieu 1985), but because they are complex, heterogenous, and semi-autonomous organizations, they do so in variegated and sometimes internally inconsistent ways. Synthesizing these two physical metaphors, we might say that the findings of this study provide evidence that the state requires different prescriptions for each lens. Future research should exploit comparisons in the ways that different parts of states see the same objects differently. Such comparisons offer special opportunities to identify the diverse ways that complex states construct and control political subjects and populations.

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5. CONCLUSION

This dissertation examined the relationship between the modernization of marriage, capitalist development, and state formation. Using a variety of methods to analyze a new, detailed data on most marriages and divorces occurring between 1867 and 1906 in the United States, the study showed that these three historical factors are highly interdependent. Despite the complex relationship among them and the challenges to inference presented by data limitations, each chapter of this study provided some novel empirical findings that also advanced sociological theory. Each also pointed toward further, necessary research.

Chapter 2 showed that expansions of women's property rights led to substantial increases in divorce. This finding partially corroborates recent research on property rights and divorce in the late nineteenth-century United States that concludes that divorce increases were driven by women's increased financial independence from men (MacDonald and Dildar 2018). My research shows that this was not the only mechanism at play—by destabilizing traditional gender norms, new property rights for women also led husbands to seek more divorces. A quickly growing body of research considers the far-reaching effects of married women's property rights (Lueck and Tennyson 2012; Koudijs and Salisbury 2016; Koudijs and Salisbury 2018; Koudijs, Salisbury and Sran 2018; MacDonald and Dildar 2018; Alshaikhmubarak, Geddes and Grossbard 2019). But disentangling the causal mechanisms that bear on decision-making about marriage and divorce is difficult (Killewald 2016). The chapter demonstrates the usefulness of considering simultaneously the relative explanatory power of economic bargaining models (Becker 1993) and institutional theories of gender (West and Zimmerman 1987), and further research on the gendered economics of marriage and divorce should extend efforts to test alternative mechanisms generated across different disciplines and scholarly traditions. The findings also suggest caution in considering the ability of specific data sources and specific longitudinal methods to identify specific mechanisms. My own future research on this topic will apply regression discontinuity methods to cohort data to better distinguish between treatment effects that were specific to rights-holders and effects that impacted entire populations as shocks to normative definitions of marriage. Finally, because this study showed that wealth matters greatly for divorce, and because contemporary research documents gender wealth gaps that are greatly in excess of gender gaps in income, future divorce research should expand beyond the traditional focus on labor markets, pursuing a more thoroughgoing examination of the role of family finance in husbands' and wives' decision-making.

Chapter 3 combined legal and administrative data to demonstrate that individuals frequently engaged in strategic performances of marital breakdown in order to acquire legal divorces. Over time, divorce litigants cited fewer causes of divorce in each case, and in the aggregate converged on a set of preferred scripts for convincing judges to dissolve their marriages. Moreover, litigants adapted to new legal constraints on divorce by altering their claims about conjugal disfunction. Importantly, these two types of strategic behavior were taken up by men and women with different frequencies, suggesting gender disparities in

the politics of respectability. The findings help reconcile conflicting scholarly arguments about the strength of the American state in enforcing marriage and divorce regulations in the late nineteenth century (Friedman and Percival 1974; Grossberg 1984; Hoff 1991; Basch 1999; Hartog 2000; Friedman 2005). Patriarchal divorce laws did indeed restrict divorce seeking, but more so by dictating the gendered self-presentations of divorce seekers than by forestalling the dissolution of marriages. The chapter also demonstrates a little explored resonance between the promises of feminist historiography and cliometrics. Men and women were differentially compelled to specific legal speech acts in order to achieve their aims in divorce court, making their testimonies rich but unreliable records of actual conjugal experiences. By exploiting change in divorce law to generate exogeneous variation in legal behavior, statistical methods for analyzing large-scale patterns in legal behavior can offer new interpretations of the meaning and motivation of legal testimony that cannot reliably be inferred from textual evidence because of historical gender inequalities. Textual evidence from contemporary divorce proceedings are almost completely foreclosed to researchers. But the chapter offers a more general suggestion to researchers using data generated through legal administrative processes to consider carefully the reasons actors have for presenting themselves and offering testimony in the manner that they do, and to weigh according the relative merits of close reading, counting, and other methods of measurement. Future research should further consider new ways in which large-scale data—including textual data—can be used to make inferences about legal motivation and legal reasoning that might be belied by straightforward observation.

Chapter 4 compared vital records and census measures of marriage events, finding that the latter include substantial misrecording of marital duration and underenumeration of marital events. This discovery has two principal implications. Substantively, it suggests that citizens' demand for clarity in legal statuses—especially those such as marriage that dictated claims to property—may have meaningfully contributed to the success of official knowledge production. By comparison, the systematization of such records into population statistics and the enumeration of population censuses, because they were fundamentally top-down biopolitical projects by elite state actors, generally failed to collect equally accurate measures of marriages. This suggests an important rethinking of research on official knowledge production. Unsurprisingly, a consistent finding in such research is that in order to mitigate mistrust of centralized state actors and to overcome weak individual incentives to aid efforts to estimate population parameters, the consent and cooperation of civil society groups is necessary to motivate political subjects' thorough and forthcoming participation. The case of vital records of marriage—which both antedate and undergird modern population-oriented knowledge projects—appears, however, to illustrate an alternative mechanism of official knowledge creation in which individuals have personal incentives to interpolate themselves into legers of the state. Future research should inquire further into political subjects' self-interpolation into administrative records, and the relationship of this process to the creation and distribution of various rights and statuses. The history of cultivating one's own legibility is sure to be of growing interest at a time when social and political concerns over privacy and surveillance are greatly increasing.

Methodologically, because measurement errors observed in Chapter 4 were substantial and not uniform, they have troubling consequences for both the descriptive and

analytic use of census microdata to explore marriage and divorce patterns in the late nineteenth and early twentieth centuries. The study extends recent efforts to compare the validity of alternative measures of marriage and divorce, which have varied in quality as federal funding for data collection has ebbed and flowed (Kennedy and Ruggles 2014). Standard demographic methods for evaluating errors in census data are powerful but usually rely on some strong or untestable assumptions (Hacker 2013). Further research should continue to find opportunities to compare alternative measures or alternative data sources to generate clear and transparent measures of misrecording and underenumeration (Steckel 1991).

Questions about the relationship between marriage, capitalism, and the state that captured the interest of Durkheim and Engels continue to present difficult but exciting puzzles to contemporary researchers. Many of their insights into the role of the organization of economic production and social interaction continue to inspire more subtle contemporary explanations, including those in this study. We now possess considerably more powerful data and methods to help solve these puzzles, but definitive answers remain elusive. This study made a modest contribution to a long-standing and ongoing effort by social researchers to understand the ways that marriage and divorce are sites of economic stratification, gender domination, and social control. By continuing this effort, we may better come to know how to continue reshaping marriage and divorce so that they might be the basis of deep personal connection, social inclusion, economic security, and even freedom.

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APPENDIX A: DETAILS OF DATA COLLECTION

This appendix details the collection of marriage and divorce data for this dissertation. The first section discusses data on divorce and marriage events. The second section addresses data on the law of marriage and divorce. The third discusses plans to publish the data.

Vital records of marriage and divorce

The principal data contribution of this dissertation is the rendering of a large volume of historical vital records of marriage and divorce in computer-analyzable form. This section reviews the political and cultural origin of efforts compile vital records data, their method of compilation by the U.S. Census Bureau, the methods used to key them into computer-analyzable format, and the measures taken to code them to aid interpretation of their accuracy and meaning.

Origins of compilation

Concern among religious and political leaders about rising divorce rates led to the formation of the New England Divorce Reform League in Boston in 1881, which was reconstituted as the National League for the Protection of the Family in 1885. In 1884 this group coordinated petitioning and testimony by politicians, jurists, educators, and clergy to the U.S. Congress, unsuccessfully calling for federal funding for research on divorce. In 1887, the general convention of the Protestant Episcopal Church issued a memorandum to the U.S. Congress, reiterating the general case for a national survey of divorce: that state laws governing divorce were lax and various, that legal stringency and uniformity were needed, and that appropriate legislative reform depended on extensive and reliable information about both divorce laws and divorce patterns (Wright 1891:13). Within two months a national survey was funded to compile the previous twenty years with of vital records of marriage and divorce.

The first report, covering year 1867–1906, underwent several printings and was widely read both domestically and abroad (1909a:3). Between 1902 and 1905, Congress again began to receive petitions urging the federal government to bring divorce statistics up to date, but again failed to appropriate funds. However, immediately following a meeting with delegates from an ecumenical conference in 1905, President Theodore Roosevelt issued a message to Congress noting "a widespread conviction that the divorce laws are dangerously lax and indifferently administered in some of the States, resulting in a diminishing regard for the sanctity of the marriage relation" (U.S. Bureau of the Census 1909:4). Roosevelt hoped for the institutionalization of a uniform marriage and divorce law, and recommended another survey of marriage and divorce records to inform legislation to that end. A second twenty-year retrospective study spanning 1887–1906 was promptly funded. In 1916 funds for a ten-year retrospective study were allocated, but on account of the war effort only data for 1916 were collected (U.S. Bureau of the Census 1918:6-7). The national compilation of vital records was eventually annualized beginning

with data for 1922, but was discontinued after 1932. Due to resource constraints and the relative value of having continuous panel data, this study focuses on the first two surveys, representing the years 1867–1906.

Methods of compilation

The first retrospective compilation of vital records of divorce was conducted by the newly created U.S. Department of Labor. Its first commissioner, Carroll D. Wright, had previously served as the chief of the Massachusetts Bureau of Statistics of Labor, in which capacity he had organized one of the first state-wide compilations of divorce statistics from local vital records (Bureau of Statistics of Labor 1880). At the national level, Wright employed a twofold method of data collection. He attempted to collect divorce records in sparsely populated areas by mail. But because some court clerks charged "exorbitant fees" or "declined to furnish the facts called for by mail at any price," only 766 counties supplied records this way and the method was deemed "not effectual" (Wright 1891:15–17). The remainder of divorces were compiled by "special agents and experts" dispatched by the U.S. Department of Labor to tabulate directly the records of local court clerks (U.S. Bureau of the Census 1907a, 1907b). Roughly 92% of divorces were compiled through this "personal canvass." Combining the two methods, "each libel [i.e. divorce petition], for the twenty years named [1867 to 1886, inclusive], in every court in the whole country having divorce jurisdiction, was examined, as well as the dockets of the courts, and, oftentimes, such evidence as might be on file." Discrepancies in figures generated by the national survey and state vital statistics systems were found to be "insignificant" and were attributed to unsystematic administrative error. The survey of divorces was deemed a success, with Wright concluding that it was "[p]ractically […] complete."

Wright developed the survey instrument in close consultation with Rev. Samuel W. Dike, head of the National League for the Protection of the Family. The instrument focused on collecting information what was universally included in divorce "libels," or petitions for divorce. These documents were greatly limited in detail compared to other documents available in court dockets, but they had the merit of being largely standardized across states in their basic features. This allowed Wright to compile complete information not only on the number of divorces occurring in each county in each year, but also on the party that filed for divorce, the cited legal cause of the divorce, the duration of the marriage, and the parental status of the parties. Other available information, such as the testimony, age, nationality, race, religion, occupation, and number of prior marriages of parties to divorces were not universally available, and so they were not tabulated.

The second survey, comprising divorces from 1887 to 1906, was conducted by the newly created Bureau of the Census under William C. Hunt, chief statistician for population, with advice from Wright (1909:xiii, 3–6). Again, statistics were supplied by mail from temporarily deputized court clerks in 765 smaller and more remote counties. In addition, Census agents appointed to collect cotton statistics in 206 Southern counties were used to canvass court records of divorce. In remaining counties, "regular special agents and detailed clerks" of the Bureau visited county seats and obtained statistics directly from public court records. The sole exception was Escambia County, FL, "where the court clerk refused either to permit the examination of the court records by an agent of the Census or to furnish the data himself at a reasonable compensation." Data were collected, at least in

part, from all but 6 of 2,803 counties or county equivalents, and were deemed "approximately complete" (1909:6, 11).

In both surveys federal statisticians also sought to collect information about marriages from local vital records in the same manner as divorce (U.S. Bureau of the Census 1907a, 1907b). In contrast to the divorce survey, however, the early marriage survey covering 1867–1886 was deemed "thoroughly incomplete and unsatisfactory" (Wright 1891:18). Only 1,728 of 2,627 counties supplied any marriage data (U.S. Bureau of the Census 1909:4–6). Because a number of states did not require the issuance of marriage licenses, counts were made of marriage returns, or records generated when the officiants of marriage celebrations recorded such events with local authorities. The incompleteness of marriage returns was attributed to "the fact that many states lacked compulsory requirements for the proper return and record of marriages, while some of the states which had such a requirement lost the value of it because they imposed no penalty for its non-observance," and Census Bureau statisticians argued that "more severe penalties, should compel the officer officiating to make a return at once." States that implemented their own marriage registration systems were thought to have reported statistics that were "fairly complete," but federal agents noted that "in some the work of compilation at the central office [was] so carelessly and inaccurately done as to detract greatly from their value" (Wright 1891:18).

The later marriage survey, comprising 1887–1906, was considerably more successful. Of 2,844 counties existing in 1906, those with "ostensibly complete returns for all years" numbered 2,598, and some 179 counties supplied returns that were known to be lacking or incomplete in certain years. Sixty-seven counties provided no records, but 41 of these were in South Carolina, which had no legal provisions for the issuance of marriage licenses or the recording of marriages (U.S. Bureau of the Census 1909:7). The diffusion of marriage license laws made possible the comparison of counts of licenses and returns, and Hunt indicated on this basis that counts of returns appeared "fairly accurate," but that the "laws and practices in respect to the return and record of marriages" still prevented the collection of "thoroughly satisfactory statistics" (1909:5).

Keying

The data were accessed in their published form. Based on my own extensive inquiries with historical scholars and librarians, no data from the original surveys are known to exist in any other form. A hard copy of Wright (1891) was maintained and digitized by the University of Michigan, and the hard copy of U.S. Bureau of the Census (1909b) was maintained by The Pennsylvania State University and digitized by Google. Both were accessed via HeinOnline, and are part of the public domain.

Figure A.1 gives a representative example of the vital records data in published form. The complex and irregular formatting of data tables prevented the use of automated optical character recognition software. Therefore, data were manually keyed into electronic spreadsheets by three data entry professionals in India and the Philippines between October and November 2016. Data entry personnel were hired directly via the online labor platform Upwork and paid the U.S. federal minimum hourly wage. Fortunately, each data table

included summative information, such as totals across counties, states, or years. This way, errors in keying could be easily identified using simple algorithms, preventing the need for double keying and reducing cost. I manually fixed errors identified by algorithm and revalidated them.

Coding of record quality and missingness

Three sets of difficult choices were necessary to make the published data machine readable and to give accurate interpretation to them. The first is specific to county–level data. Two counties in Massachusetts jointly recorded marriages, and 27 counties in several states adjudicated divorces jointly with at least one other county. Clusters of such counties were given unique identifiers and the county in which clustered vital records were recorded were given an identifier for inclusion in analysis. More complicated was the issue of county border changes. Although adjustment for border changes is feasible when longitudinal analysis is confined to censal years, the annualized structure of analyses in this study made it impractical to apportion county attributes in intercensal years. Therefore, the problem required a simpler approach—combining counties that shared a border that changed over the period of analysis. Again, clusters of counties—comprising 811 counties for marriages and 820 counties for divorces—were given unique cluster identifiers that enable the researcher to combine vital records across clusters of counties to make longitudinal analysis valid. Analysis of robustness of results to county clustering is highly recommended in future research.

The second set of choices relates to a nontrivial degree of record destruction and recording error over the period of analysis. Fortunately, the Department of Labor and the Census Bureau offered thorough annotations of record destruction and known administrative error, as illustrated in Figure A.1. Footnotes were keyed by the aforementioned data entry professionals, and I read and coded footnotes into 28 record quality categories, given in Table A.1. Divorce records in 3.0% of county–years were destroyed locally, overwhelmingly in the South and by fire. Records in a further 0.5% of county–years were coded by Census officials as lost, incomplete, or defective, and in 0.3% of county–years were not recorded, registered, or reported. For marriages, records in 1.7% of county–years were destroyed locally, in 0.4% of county–years were coded by Census officials as having been lost, incomplete, or defective, and in 2.5% of county–years were not recorded, registered, or reported. "Attached" counties are those in which marriage or divorce records are pooled across counties, with counties of record being those counties in which the data tables record pooled data. In this study, any county–years with record codes (except those identifying counties of record in clusters of attached counties) are listwise dropped from the analysis. But because the data tables often contain non-missing values for years with record quality issues, researchers can decide for themselves about the appropriate course of action depending on their analytic goals.

Figure A.1. County-level marriage and divorce data from Wright (1891).

TABLE I.—MARRIAGES AND DIVORCES, 1867 TO 1886, BY COUNTIES.

ALABAMA.

	Counties.	Population 1870.	Population 1880.	Marriages and divorces.	1867.	1868.	1869.	1870.	1871.	1872.
1	Autauga	11,623	13,108	Mar						
				Div	1	1	1		2	3
2	Baldwin (a)			Mar						
				Div						
3	Barbour	29,309	33,979	Mar	2	2		9		6
				Div						
4	Bibb	7,469	9,487	Mar						
				Div					1	1
5	Blount (b)	9,945	15,369	Mar	5	1		1		
				Div						
6	Bullock	24,474	29,066	Mar	1	5	7	2	3	2
				Div	1					
7	Butler (c)	14,981	19,649	Mar	193	172	244	218	200	210
				Div	1	1	4	3	4	2
8	Calhoun	13,980	19,591	Mar						
				Div	3		3	1	1	
9	Chambers	17,562	23,440	Mar	1	1			1	1
				Div						
10	Cherokee	11,132	19,108	Mar	3	4				2
				Div						
11	Chilton (d)	6,194	10,793	Mar						
				Div						
12	Choctaw (e)	12,676	15,731	Mar					70	170
				Div	1			3	4	3
13	Clarke	14,663	17,806	Mar						
				Div						
14	Clay	9,560	12,938	Mar						
				Div						
15	Cleburne	8,017	10,976	Mar						
				Div						
16	Coffee	6,171	8,119	Mar						
				Div	9	13	10	16	14	4
17	Colbert (f)	12,537	16,153	Mar						
				Div					2	2
18	Conecuh (c, g)	9,574	12,605	Mar						
				Div						
19	Coosa	11,945	15,113	Mar						
				Div						
20	Covington (h)			Mar						
				Div						
21	Crenshaw (h)	16,024	17,365	Mar	1			1		1
				Div						
22	Cullman (i)		6,355	Mar						
				Div						

a The counties of Baldwin, Mobile, and Washington form one judicial district and the divorces are not separable; hence all, as well as population, are credited to Mobile county. Divorces reported prior to 1876 are incomplete, as the courthouse was destroyed by fire January 20, 1888, before the investigation was completed.
b In 1877 a part set off to Cullman county.
c From 1867 to 1872 divorces for Conecuh county are included with those for Butler county.
d For several years prior to 1880 the records of divorce are defective. Organized as Baker in 1868; name changed since 1870.

1873.	1874.	1875.	1876.	1877.	1878.	1879.	1880.	1881.	1882.	1883.	1884.	1885.	1886.	Total.	
3		1			2	8	1	5	10	5		11	5	59	} 1
															} 2
3	7	1	9	4	2	2		1	7	2	17	14	18	106	} 3
	3	2	4	2	5	12	2	2	5	8	5	4	8	64	} 4
2		2	2	5	5		3	1	3	5	7	1	2	45	} 5
6	3	4	2	2	3			12	16	14	14	17		113	} 6
215	223	220	227	197	256	195	256	207	212	249	195	369	261	4,519	} 7
3	3	3	2	2	10	14	10	13	21	8	19	17	15	155	
2		1		2	5	1	3	3	5	4		5	7	47	} 8
9	6	3	8	3	5	5	8	2	4	4	2	2	16	81	} 9
3	3		2	2		5	1		3	5	2	1		41	} 10
2		1	3	5	2	3	6	2	3	4	5	3	3	42	} 11
132	151	131	142	133	152	162	165	155	136	127	102	152	172	69	} 12
1	4	6	5	1	3		4	4	7	5	7	10			
	1	1	1	3		8	2	1	5	10	13	5	10	60	} 13
															} 14
	1		1		3		3	1	1					10	} 15
18	13	11	8	7	5	12	8	15	11	12	12	13	40	251	} 16
	2	7	5		3			1	1			2		25	} 17
	3		1	3	5	12	16	8	22	12	7	11	10	110	} 18
															} 19
															} 20
	4	7	2	5	13	13	11	12	17	11	16	17	19	150	} 21
			48	56	58	64	60	52	89	75	102	96		700	} 22

e Marriage records destroyed by fire April 11, 1871.
f First court with divorce jurisdiction established in 1871.
g Divorces for Escambia county prior to February, 1885, are included with those for Conecuh county.
h The counties of Covington and Crenshaw form one judicial district and the divorces are not separable; hence they, as well as population, are credited to Crenshaw county.
i Organized in 1877 from parts of Blount, Marshall, Morgan, and Winston counties. No divorces granted.

Table A.1. Codes for record issues, vital records of marriage and divorce, 1867–1906

Code	Label	Code	Label
1	Destroyed, fire (see 26)	15	Defective
2	Partially destroyed, fire (see 27)	16	"No systematic registration"
3	Destroyed, other/unnamed cause	17	Partially reported
4	Partially destroyed, other/unnamed cause	18	Not dated to year
5	Lost/missing or destroyed	19	Not yet reported when data were gathered
6	Partially lost/missing or destroyed	20	No divorces granted
7	Lost/missing	21	No divorce jurisdiction.
8	Partially lost/missing	22	No marriages solemnized.
9	Incomplete	23	Not disaggregated by county
10	Not reported/no report	24	Attached; not county of record
11	Licenses; returns not fully reported	25	Attached; county of record
12	Row/column does not sum	26	Attached; county of record; destroyed by fire
13	"Colored only"	27	Attached; county of record; partially destroyed by fire
14	No record/no record kept	28	All zeros but no record note; inferred not reported

The third set of choices relates to ambiguities in the presentation of the data by the Department of Labor and the Census Bureau. Unfortunately, as illustrated in Figure A.1, both agencies represented both missing data and "0" values using two ellipses ("……"), creating confusion about the interpretation data in cells with this marker.[20] I addressed this issue in the following way. First, to deal with counties that come into existence over the period of analysis, I used data on county formation from Aiken and Kane (2013) to create two variables indicating the years in which each county was for the final time administratively organized. This gives clear interpretation to the missingness of marriage and divorce data before the onset of county organization. Second, in counties for which only ellipses are given for the entirety of a survey (1867–1886 and/or 1887–

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[20] Wright (1891:17) explained this practice as follows: "In those counties where figures are not given in the tables there have been either no divorces during the twenty years covered by the investigation, or so few that had the number been given it would not have appreciably affected the results. As a matter of fact, negative information has been given that there were no divorces, for in all cases, with perhaps two or three exceptions, the clerks of courts have either assured the office positively that no divorces were granted during the twenty years named, or failed to reply to the statement of the office that if a return was not made by a given date it would be assumed that no divorces had been granted. The latter conditions, however, apply to only a very few, very remote, and very sparsely settled counties—counties where it is perfectly safe to assume that there had not been half a dozen divorce cases in a score of years."

1906) but for which no footnote is given indicating the quality of records, I treat the cell as missing and assign record code 28, "All zeros but no record note; inferred not reported," signifying that it is my interpretation that no data were recorded by local authorities and/or collected by or reported to the Department of Labor or the Census Bureau. This code is applied to divorces in 4.6% of county–years and to marriages in 14.2% of county–years, the latter coming overwhelmingly in the first survey. For divorce, remaining ellipses, which occur in organized counties with non-missing data, are coded as "0." Figure A.1 illustrate that these overwhelming occur in county–year cells adjacent to other cells with small counts of divorce, supporting the assumption that these represent true zero values. For marriages, ellipses are treated as missing, reasoning that in all but a few small and remote counties, populations were large enough and marriage rates high enough that the assumption of at least one marriage per county–year is a reasonable default.

The quality of county marriage statistics was so poor that Wright and Hunt declined to publish state-level figures. Divorce data were, however, were tabulated at the state level, and important information about the gender of divorce petitioner, marital duration, and cause of divorce are available only in state-level cross tabulations. It is essential, however, to note that these data collapse county level data without regard to problems of record quality or data missingness. Therefore, any analyses using state-level data should be treated with special caution.

Coding causes of divorce

Chapter 3 of this dissertation uses state-level data on the causes of divorce cited by divorce litigants. The number and kind of causes legally available to litigants varied by state and by year. In both surveys of divorce, published data included cross-tabulations of divorces by state, year, gender of petitioner, and "specific" cause of divorce. While neither the Department of Labor (Wright 1891) nor the Census Bureau (1909b) offered information on how they categorized individual divorces into specific causes, these causes appear to correspond closely to the language in corresponding state statutes. As a result, it is assumed that no meaningful loss of information occurred in the organization and presentation of data in this way.

The resulting data comprise 63 specific causes of divorce. However, because states employed different language in statutes defining substantively similar causes of divorce, I collapse 14 of these to create a set of 49 substantively distinct causes of divorce. Causes of divorce are merged only when their meanings are substantively similar *and* no state recognizes both as distinct causes at the same time. So, for example, "habitual drunkenness" is recoded as "habitual intoxication," because the two were synonyms employed by different states to describe similar circumstances. By contrast, "gross misconduct" and "lewd conduct" were treated as distinct causes because Indiana allowed divorce petitioners to cite them separately in divorce suits.

Legal data

A major goal of the early federal surveys of marriage and divorce was to understand the diverse state statutes regulating marriage and divorce. Citing "conflicting statements in different works relating to marriage and divorce legislation," Wright (1891:20) employed two lawyers, "expert in the matter of compilation," to collect and compare the status of marriage and divorce laws in 1867. The new digest relied entirely on published revised statutes rather than on commentaries or pre-existing digests, and Wright insisted it was "thoroughly verified." This first effort, however, provided only a cross section of marriage and divorce laws.

The second survey undertook a more ambitious compilation of laws. Beginning with a survey of state marriage and divorce laws as of 1887, they then traced changes in such laws through the end of 1906, noting both the nature and date of such changes. Although the Census Bureau did not detail its methods of compilation, it offered detailed information on the source materials used in developing its longitudinal digest (U.S. Bureau of the Census 1909a:182). I used this longitudinal digest to develop a machine-readable dataset comprising annualized observations of state marriage and divorce laws.

The hard copy of longitudinal digest (U.S. Bureau of the Census 1909a) was maintained by the University of Michigan, digitized by Google, and accessed via HeinOnline. Marriage and divorce data were single coded by two undergraduate students at the University of California, Berkeley between September 2016 and May 2017. The coders were given detailed decision rules about categorizing laws into quantitative variables, and received course credit for their work.

Publication

There is a growing consensus that a variety of practices and conventions in the social science research community—such as model selection and sampling to achieve statistically significant results (e.g. "p-hacking") and suppression of null and replicative findings (e.g. the "file drawer effect")—fail to uphold basic tenets of the scientific method but nevertheless remain quite common (Miguel et al. 2014). One such practice that touches on many others is data hoardings. Researchers face incentives against sharing data with other researchers because such sharing decreases one's comparative advantage in the market for innovative research and because it increases the risk of one's errors becoming exposed. When such a risk is especially low, not only mistakes but fraudulent practices can go unchecked. As a result, a growing number of social science journals encourage or require authors of published studies to publicize their data. But this norm has yet to diffuse widely among sociologists. Indeed, in a recent armchair study by Cristobal Young and Aaron Horvath (2015), only 15 of 53 (28%) sociologists contacted with requests for replication packages provided them.

Therefore, I intend to publish upon the completion of this dissertation the entirety of the data I have rendered in machine readable format, along with documentation that will make it widely interpretable and analyzable. A common practice is to release such

data through one's on Web site, but this limits access. I will instead publish the data on Open ICSPR, a platform that maximizes the flexibility, stability, and accessibility of the data to future researchers. I will separately provide replication packages for specific research output that include modified data and computer code.

APPENDIX B: SENSITIVITY ANALYSES FOR CHAPTER 2

The main analyses rely on several data management and modeling choices to which there are reasonable alternatives. Following Young's (2009) call for greater transparency and more thoroughgoing analysis of the robustness of results to modeling and other research design decisions, this appendix reports results under several other defensible specifications.

Selection into Marriage

Using total population to construct divorce rates will generate upward bias in estimates of MWPAs' impact on divorce if the reforms also led to increases in marriage. Positive shocks to the denominator will go unmeasured, leading to systematic overestimates of the rate following treatment. Moreover, there is reason to suspect that MWPAs would indeed impact marriage rates. Although MWPAs decreased the attractiveness of marriage to bachelors seeking propertied wives, they increased the attractiveness of marriage to propertied bachelorettes. While the absolute changes in property rights resulting from MWPA were exactly inverse, the marginal gains for women—who on average possessed less wealth—were larger than the marginal losses for men. For this reason, we might expect MWPAs to have had a net positive effect on marriage rates.

To evaluate this possibility, I digitized data comprising annual county-level counts of marriage, also reported in Wright (1891) and the U.S. Bureau of the Census (1909b). The marriage data suffer from several unique issues. Like the divorce data, some 1.7% of marriage records were destroyed locally, and a further 0.4% of marriage records were coded by Census officials as having been lost, incomplete, or defective. But marriages in 30.4% of county–years were never recorded by local authorities and/or collected by or reported to the Census Bureau. Marriage records were maintained by county recorders who often had vague directives and weak professional incentives (U.S. Bureau of the Census 1909a). As expected, missingness is highly correlated with smaller county populations and greater rurality. In addition to bias from missing data, administrative failure may also generate downward bias in those marriage counts that were reported. Because only some states required licenses for marriage, statistics were gathered from records of marriage "returns," documents submitted by the person solemnizing the marriage to the local recording official. State laws varied in directing solemnizers to make returns, requiring clerks to record them, and applying fines or criminal punishments for the nonperformance of these duties. Therefore, marriages may be undercounted in more laxly regulated states and in larger and more rural counties where transportation costs were higher. Given substantial nonrandom missingness and measurement error, extreme caution is warranted in interpreting results based off of these data. Nevertheless, they provide the only localized, annualized, large-scale resource for evaluating the possibility that divorce results are biased by selection into marriage.

Table B.1. Marriages per 1,000 population, county-level estimates

	Model 1	Model 2	Model 3	Model 4
MWPA	.109	-.343	-.297	.309
	(.126)	(.153)*	(.152)^	(.133)*
% pop. male			-.062	
			(.052)	
% pop. black			.010	
			(.027)	
% pop. urban			.019	
			(.006)**	
% adult women manuf. lab.			.021	
			(.019)	
Adjusted R^2	.555	.569	.574	.749
Observations	56,719	56,700	56,357	56,700
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No
Region–year FE	No	Yes	Yes	Yes
County trends	No	No	No	Yes

Note: Standard errors, clustered at the county level, are in parentheses.
^ p<.10 * p<.05 ** p<.01 *** p<.001

After grouping 909 counties with border changes into 198 groups, marriage data represent an unbalanced panel of 56,719 county–years. Table B.1 reports estimates of models identical to those presented in Table 2.2 and described by Equations 1–4. It is readily apparent from comparing coefficients across columns that the results are highly sensitive to model specification. The reasons for this sensitivity are not clear, and experimentation with other specifications and samples of common law and community property states does not yield more stable results. One possible explanation is that models without county trends (1–3) are negatively biased, as would be the case if the adoption of MWPAs was systematically associated with decreasing trends in marriage rates. This scenario is not implausible: mean age at first marriage was increasing over the period of analysis (Haines 1996), as was the incidence of spinsterhood among affluent females (Shammas 1994). Male lawmakers may have been more likely to adopt MWPAs, which made marriage more attractive to propertied women, specifically in those places where such women were increasingly forgoing marriage. Regardless, results for the preferred random trends model indicate that the adoption of MWPAs had a statistically significant positive impact on marriage rates, as expected. But point estimates suggest that the marriage rate increased by an average of only .309 per 1,000 population, a 3.4% increase at mean values for the outcome variable. Based on county- and state-level divorce analyses, this suggests that selection into marriage could not have accounted for more than 18.2% of the additional divorces resulting from married women's property rights expansions.

Standard Error Corrections

The standard errors presented in the main analysis are robust to serial autocorrelation and heteroskedasticity. However, the estimation strategy assumes the spatial independence of counties—in other words, that residuals are contemporaneously uncorrelated across panels. If this assumption is violated, estimates of standard errors—and therefore statistical inference—will be biased. (Estimates of coefficients will remain unbiased, but will be inefficient). Modeling spatial dependence directly is hindered by the frequency of county border changes, the length of panels, and the grouping strategy employed to make units comparable across time. I therefore resort to reporting estimates of standard errors under various assumptions about the spatial dependence of observational units.

One simple approach is to increase the level at which standard errors are clustered. Generally speaking, necessary and sufficient conditions for clustering are the correlation of both regressors and residuals along a particular unit of space or time (Cameron and Miller 2015). Because treatment assignment—in this case, the adoption of MWPAs—varies at the state level, the presence of any intrastate correlation of residuals justifies clustering at the state level (Barrios et al. 2012).

Clustering at the state level continues to rely on the assumption that residuals or regressors are independent across states. Like other state policies, MWPAs adoption followed patterns of mimicry and diffusion (Chatfield 2014). Therefore, treatment assignment is non-independent, and in the presence of any cross-state correlation in residuals, estimates of standard errors will be biased. A simple way but limited to address this problem is to employ a variance estimator that allows cluster-robust inference along multiple dimensions (Cameron, Gelbach, and Miller 2011). Clustering standard errors by year controls for within-year policy diffusion and other year-specific shocks. For key models presented in the main analysis, Table B.2 reports estimates of standard errors clustered on different combinations of geographical and temporal units. Model 4 is the preferred specification for estimating the main effect of MWPAs. Surprisingly, the variance of the estimator is actually slightly reduced by clustering at the state rather than county level. This suggests that after controlling for county and region–year fixed effects and county trends, residuals were negatively correlated across counties within states. Because most states required divorce seekers to be residents of the state but not the county in which they brought suit, the negative correlation of residuals is consistent with a model of strategic action in which divorce seekers may have shopped for sympathetic judges or successful lawyers. Increases in estimated standard errors when accounting for year clusters are small, but neglect dependency in states' MWPA adoption with lags greater than one year.

Table B.2. Divorces per 1,000 population, alternative standard error corrections

	Unit of clustering in error covariance matrix	Model 4	Model 7	Model 4	Model 4
				Common	Community
		All	All	law	property
MWPA		.071	.064	.077	-.049
	County	(.035)*	(.015)***	(.039)*	(.121)
	State	(.032)*	(.031)*	(.034)*	(.087)
	County, year	(.038)^	(.026)*	(.044)^	(.130)
	State, year	(.034)*	(.036)^	(.041)^	(.106)
% adult women manuf. lab.			-.021		
	County		(.006)***		
	State		(.009)*		
	County, year		(.007)**		
	State, year		(.009)*		
Adjusted R^2		.365	.299	.343	.517
Observations		68,520	68,231	60,334	8,183
County FE		Yes	Yes	Yes	Yes
Year FE		No	No	No	No
Region–year FE		Yes	Yes	Yes	Yes
County trends		Yes	No	Yes	Yes

Note: For brevity, coefficients and standard errors for some covariates for Model 7 are not displayed, but are consistent with results presented in Table 2.3. Standard errors are in parentheses.
^ $p<.10$ * $p<.05$ ** $p<.01$ *** $p<.001$

Model 7 establishes the substitutive effects of women's economic empowerment via capital and labor markets. Differently than the random trend model, the covariates-based model sees substantial increases in standard error estimates resulting from both state- and year-level clustering. Nevertheless, estimated coefficients for both the main effect of MWPAs on divorce and the dependence of that effect on women's labor market conditions remain statistically significant. Standard error corrections for other models excluding random trends (Equations 1 and 2), not shown here, exhibit more sizable increases in standard errors that render estimates statistically insignificant. This recommends the preferred random trend model for its relative robustness to spatial autocorrelation. Negligible differences—in fact, small decreases—in standard errors when clustering at higher geographic levels suggest that this model adequately controls for spatial autocorrelation (Cameron and Miller 2015).

Alternative Constructions of Divorce Rates

Marriage models account for shocks to marriage stocks resulting directly from MWPAs. Another concern, however, is that total population-based divorce rates will obscure longer-term trends in the age and marriage structure of counties that may be correlated with MWPA adoption. For example, if MWPAs were part of larger progressive reform agendas that included public health efforts, adult mortality improvements may have increased the proportion of total populations at risk for divorce (Cutler and Miller 2005). To a certain extent, these changes will be captured by random trends parameters. But an alternative strategy is to construct divorce rates that are sensitive to changes in age and marriage structures.

The first alternative divorce rate construction is *divorces per 1,000 divorce-aged persons*, with divorce age defined as 16–60. Aggregate census data pool age-groups irregularly across censuses (Haines and ICPSR 2010). I therefore use IPUMS samples to estimate county age structures (Ruggles et al. 2018). Because these samples are too small to generate reliable county-level estimates directly, I use them to generate state–level proportions of men and women who are divorce–aged, and then assign divorce-aged people to counties based on the sex composition of counties reported from complete, aggregate data.

The second construction is *divorces per 1,000 married, spouse-present persons*. Married spouse-present (MSP) persons are identified using IPUMS data (Ruggles et al. 2018). After 1880 MSP persons are identified by their reported marital status; before, MSP status is imputed by IPUMS using information on census record ordering, household age structures, and other individual and household attributes (cf. Bloome and Muller 2015). I construct county MSP figures using the same method used to calculate divorce-aged populations. (Models tested on IPUMS data indicate that sex outperforms the alternative, urban residence, as the basis for assignment of age structures to counties. However, constructions of the divorce rate using the latter method, as well as those using divorce-aged women and married spouse-present women as denominators, yield near-identical results.)

Table B.3 presents results using these alternative constructions of the dependent variable. Note that coefficients differ from the main results in part because mean values of the rates also differ. In proportional terms, however, estimates using alternative rates are similar to those in the main analysis. In the preferred specification (Model 4), MWPAs increased divorces per 1,000 divorce-aged persons by .151, or 16.7% at mean value of the outcome variable, and increased divorces per 1,000 married, spouse-present persons by .252, or 17.1%. In addition to supporting the general conclusions of the main results, a comparison of results based on divorces per MSP population suggests that the overwhelming majority of additional divorces resulting from MWPAs were driven by increases in married men and women dissolving their marriages rather than increases in already separated spouses filing for divorce (cf. Cvercek 2009). This pattern is consistent with the specific causal mechanisms tested in the main analysis.

Table B.3. Divorces, alternative rate specifications

	Model 1	Model 2	Model 3	Model 4
	Per 1,000 divorce-aged persons (16–60)			
MWPA	0.084	0.061	0.079	0.151
	(0.031)**	(0.029)*	(0.033)*	(0.071)*
% pop. male			-0.009	
			(0.016)	
% pop. black			0.003	
			(0.004)	
% pop. urban			0.013	
			(0.002)***	
% adult women			0.019	
manuf. lab.			(0.005)***	
	Per 1,000 married, spouse-present persons			
MWPA	0.134	0.093	0.116	0.252
	(0.049)**	(0.047)*	(0.052)*	(0.109)*
% pop. male			0.002	
			(0.027)	
% pop. black			0.006	
			(0.006)	
% pop. urban			0.019	
			(0.003)***	
% adult women			0.029	
manuf. lab.			(0.009)***	
Observations	68,334	68,334	68,118	68,334
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No
Region–year FE	No	Yes	Yes	Yes
County trends	No	No	No	Yes

Note: Standard errors, clustered at the county level, are in parentheses.
^ p<.10 * p<.05 ** p<.01 *** p<.001

Border Changes and Grouping

The method of grouping counties is the most parsimonious and conservative method for dealing with county border changes using a large panel of annualized data. Table B.4 presents results that test the consequences of this method for model estimates. The first column reiterates results presented for the random trends model in Table 2.2. In this case, group–years were dropped in any year in which any county belonging to the group had missing or defective data. The second column reports estimates when such group–years are not dropped. The third column is identical to the main sample except that

counties in Texas and Washington are removed. Several states experienced "judicial attachment," in which the divorces of one county's population were administered and therefore recorded in a neighboring county. Outside of Texas and Washington, all such cases are treated in the same manner as county border changes. But in these two states, judicial attachment was so frequent and volatile that U.S. Census Bureau officials were not able to identify attachments reliably (Wright 1891:398, 426). Dropping these states does not reduce standard errors, and but leads to slightly higher point estimates.

Table B.4. Divorces per 1,000 population, grouped and ungrouped county–years

	Main sample	Group–years with missing county–years included	TX and WA excluded	Ungrouped county–years only	Group–years only
	Model 4	Model 4	Model 4	Model 4	Model 4
MWPA	.071	.075	.078	.069	.081
	(.035)*	(.035)*	(.036)*	(.035)*	(.035)*
Adjusted R^2	.421	.422	.401	.414	.807
Observations	68,520	71,430	63,216	64,062	4,454

Note: Standard errors, clustered at the county level, are in parentheses.
* p<.05 ** p<.01 *** p<.001

Finally, the last two columns report estimates for the non-overlapping samples of ungrouped and grouped county–years. Point estimates are higher in group–years than ungrouped county–years. This may reveal a modest degree of aggregation bias, or it may represent the disproportionate effects of MWPAs in areas where border changes were common. Regardless, the general consistency of results across samples and aggregation strategies suggests that the chosen grouping method for addressing county border changes and judicial attachment does not meaningfully bias estimates in the main analysis.

Weighting

Some difference-in-differences studies using aggregate divorce data estimate models weighted on total population at the level of aggregation (Friedberg 1998; Wolfers 2006). Although perhaps intuitive—Los Angeles County, CA should "count" more than Weston County, WY—weighted least squares estimates do not necessarily yield more consistent or more efficient estimates, nor do they necessarily approximate the population average partial effect better than ordinary least squares (Winship and Radbill 1994; Solon, Haider, Wooldridge 2015). Nevertheless, comparison of weighted and unweighted least

squares estimates is a helpful check on model misspecification. Therefore, Table B.5 reports least squares estimates weighted on interpolated annual county populations. Because divorce was more common in population-dense places, weighted mean values of the outcome variable are higher—.642 divorces per 1,000 persons compared to an unweighted mean of .524. Compared to unweighted estimates, standard errors are somewhat larger for Models 1–3, but much smaller for Model 4. Point estimates are larger in the baseline specification, but are considerably lowered by the addition of region–year fixed effects in Models 2 and 3. The preferred random trends model, which also includes region–year fixed effects, suggests that divorces increased by .05 per 1,000 population, a 7.8% increase at population-weighted mean values of the dependent variable. Taken together, the divergence in estimates between OLS and WLS recommends concern about functional form specification, most especially for Models 1–3. But efforts to model heterogeneity in treatment effects with total population produce inconsistent results. WLS results offer some indication that the magnitude of the effects of MWPAs was inversely related to total county population, but more research is needed to establish this relationship clearly and give it meaningful interpretation.

Table B.5. Divorces per 1,000 population, weighted-least squares estimates

	Model 1	Model 2	Model 3	Model 4
MWPA	0.085	0.006	0.013	0.050
	(0.022)***	(0.021)	(0.021)	(0.015)**
% pop. male			-0.021	
			(0.008)*	
% pop. black			0.005	
			(0.003)^	
% pop. urban			0.006	
			(0.001)***	
% adult women manuf. lab.			0.005 (0.003)	
Adjusted R^2	.654	.683	.687	.765
Observations	68,520	68,520	68,231	68,520
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No
Region–year FE	No	Yes	Yes	Yes
County trends	No	No	No	Yes

Note: Standard errors, clustered at the county level, are in parentheses.
^ p<.10 * p<.05 ** p<.01 *** p<.001

Measurement of Legal Change

Some disagreement persists about how properly to date the treatment in question (Hoff 1991; Khan 1996; Geddes and Lueck 2002; Geddes and Tennyson 2013; MacDonald 2015). Most recently, MacDonald and Dildar (2018) argue for a modification of Geddes and Tennyson's (2013) dating scheme that accounts for some heterogeneous lags in MWPA effectiveness resulting from diverse judicial interpretation of the legislation.

Table B.6 reports estimates of models identical to those in Table 2.2, except for the modification of treatment timing to reflect MacDonald and Dildar's measurements of legal effectiveness rather than legal adoption. Estimates fall within the range of those using the original dating scheme. Of course, which dating scheme is preferable is a substantive question rather than a modeling one, and this chapter does not present new evidence or argumentation on these questions (see Chused 1985; MacDonald 2015). Rather, a comparison of the results in Tables 2 and B6 simply shows that the influence of increased women's property rights on divorce is not particularly sensitive to choices between alternative defensible constructs of the treatment.

Table B.6. Divorces per 1,000 population, alternative MWPA dates

	Model 1	Model 2	Model 3	Model 4
MWPA (alt. date)	0.049 (0.017)**	0.055 (0.017)***	0.059 (0.018)**	0.061 (0.037)^
% pop. male			-0.003 (0.009)	
% pop. black			0.003 (0.002)	
% pop. urban			0.006 (0.001)***	
% adult women manuf. lab.			0.013 (0.003)***	
Adjusted R^2	.277	.297	.299	.365
Observations	68,520	68,520	68,231	68,520
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No
Region–year FE	No	Yes	Yes	Yes
County trends	No	No	No	Yes

Note: Standard errors, clustered at the county level, are in parentheses.
^ p<.10 * p<.05 ** p<.01 *** p<.001

Legal reforms other than the adoption of MWPAs also affected the divorce rate. These included changes to states' lists of legal "grounds" for divorce, to residency

requirements for divorce, and to other marital property laws. Such changes may bias estimates of MWPAs' impact on marriage and divorce if their timing is not independent of MWPA adoption. Over the period of analysis, many states adopted so-called Earnings Acts (hereafter "EAs"), which allowed married women to own and control wages from market labor. During the period of analysis, twenty-nine passed EAs; eight states passed EAs before 1867, seven passed them after 1906 and four states never passed them. But because the theoretical arguments regarding married women's property are also applicable to married women's earnings, and because 14 states passed EAs in the same year as MWPAs, it is important to account for their possible contribution to marital stability.

Table B.7. Divorces per 1,000 population, inclusion of earnings acts

	Model 1	Model 2	Model 3	Model 4
MWPA	0.055	0.028	0.037	0.085
	(0.024)*	(0.026)	(0.027)	(0.041)*
EA	-0.012	0.023	0.024	-0.029
	(0.023)	(0.027)	(0.025)	(0.015)^
% pop. male			-0.003	
			(0.008)	
% pop. black			0.002	
			(0.002)	
% pop. urban			0.006	
			(0.001)***	
% adult women			0.013	
manuf. lab.			(0.003)***	
Adjusted R^2	.277	.297	.299	.365
Observations	68,520	68,520	68,231	68,520
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No
Region–year FE	No	Yes	Yes	Yes
County trends	No	No	No	Yes

Note: Standard errors, clustered at the county level, are in parentheses.
^ $p<.10$ * $p<.05$ ** $p<.01$ *** $p<.001$

Table B.7 reports estimates of models identical to those in Table 2.2, except for the inclusion of dummies indicating the adoption of an EA. EA adoption dates come from Geddes and Tennyson (2013). Results are comparable for the baseline model, but point estimates for MWPAs are somewhat lower in Models 2 and 3 and higher in Model 4. Consistent with previous research, the exact influence of EAs appears unclear (Cott 1987; MacDonald and Dildar 2018). Though statistically significant only at the 10% level, explaining the negative coefficient for EAs in the preferred random trend model is difficult.

Of five states that passed EAs before MWPA during the period of analysis, four were in the deep South. Alternative parameterizations of legal change (e.g. dummies for MWPA only, EA only, both laws) suggest that in these states, the sole adoption of EAs had positive but statistically insignificant effects on divorce, while the adoption of EAs alongside or after MWPAs generated smaller positive impacts on divorce than the sole adoption of MWPAs. States' likelihood of adopting EAs alongside or after MWPAs did not depend clearly on geography or historical time. While explaining the influence of EAs therefore requires additional research, the results presented in Table B.7 give some evidence that the main results were not upwardly biased by omitted, coinciding expansions of women's economic rights.