Credit and the Family: The Economic Consequences of Closing the Credit Gap of U.S. Couples

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Abstract

Closing disparities in credit access between spouses can help reduce consumption inequality in the household. The 2013 reversal of the Truth-in-Lending Act increased the borrowing capacity of secondary earners in equitable-distribution states but not in community-property states, where division-of-property laws superseded the policy change. Using a matched difference-in-differences design and administrative financial-transaction records measuring the credit and consumption of each spouse, I show that this reversal increased secondary earners' credit card limits by $1,506. In turn, spouses shared consumption more equally, closing their pre-reversal consumption gap by half. Household spending shifted toward goods that benefit both spouses. Delinquency rates were not measurably impacted, suggesting that household financial standing did not worsen. These results are consistent with a model of joint decision-making under limited commitment, in which credit causes a shift in marital bargaining power.

JEL: D13, D14, D18, G28, J12, J16

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1 Introduction

Promoting fair and equal access to consumer credit has long been a policy goal in the United States.[1] But is credit shared equally in the household? And do disparities in access to credit between spouses lead to disparities in consumption? There are reasons to believe that disparities in banking services and credit persist within the household. Survey evidence shows that perceived financial inequity in the household is among the top predictors of divorce, and roughly half of marriages in the U.S. actually end in divorce.[2] Moreover, for married couples with a single household income–roughly half of U.S. couples (U.S. Bureau of Labor Statistics, 2020*a*)–breadwinners likely have higher borrowing capacity than their spouses, because individual rather than household income determines at least part of one's ability to borrow. Even for dual-income households, gender norms make Americans see men as the financial providers (Pew Research Center, 2017). However, we know little about the extent and implications of credit disparities in the household. And while consumption inequality at least in part reflects differences in access to credit markets (Krueger and Perri, 2006; Blundell, Pistaferri and Preston, 2008), whether this association is causal remains an open question.

In this paper, I examine how policies that aim at reducing credit disparities affect the consumption gap of U.S. couples. Specifically, I analyze the 2013 reversal of the Truth-in-Lending-Act (TILA) as a source of exogenous variation in the amount of credit card limits extended to secondary earners. Before 2013, TILA required card issuers to evaluate card applicants' *independent* income in their lending decisions. The statute was reversed in 2013 to allow card issuers to consider *household* income, facilitating access to credit for secondary earners and stay-at-home spouses. Using detailed data on spouse-level financial accounts and a matched difference-in-differences design, I show that the reversal had the intended effect of increasing secondary earners' borrowing capacity. My central finding is that spouses shared consumption more equally, narrowing their pre-reversal consumption gap by half after two years. Specifically, secondary earners' spending on "private" goods (for example, clothing) increased while that of primary earners' decreased. Household spending on "public" goods (for example, home improvement) increased as well, thus leading to a moderate overall increase in household

[1] Examples of financial policies aiming at equalizing access to credit include the Fair Housing Act of 1968, Equal Credit Opportunity Act of 1974, and Community Reinvestment Act of 1977.

[2] See Dew, Britt and Huston (2012) for survey evidence. The divorce to marriage rate was 44 percent in 2019 (Centers for Disease Control and Prevention, 2021).

spending. Despite higher spending, the reversal did not worsen the financial spending of the household, such as delinquency rates or overdraft probabilities.

After establishing the causal link between credit and consumption disparities in the household, I use cross-sectional analysis and a calibrated model to clarify the economic mechanism. The limited-commitment (LC) channel posits that factors that improve the outside option of secondary earners (that is, the value of being divorced) should shift the consumption allocation in their favor because a better outside option strengthens their bargaining power in the marriage (Chiappori and Mazzocco, 2017; Kocherlakota, 1996). Several institutional features make credit a plausible factor that can increase outside options.[3] Alternatively, liquidity constraints, imperfect information (Wang, 1995; Ashraf, 2009; Dubois and Ligon, 2011), or self-control stemming from differences in spouses' time preferences (Ashraf, Karlan and Yin, 2006; Bertaut, Haliassos and Reiter, 2009) can also lead to higher consumption shares for secondary earners. Heterogeneity analysis reveals patterns consistent with the predictions of the LC channel and at odds with other plausible channels. Motivated by this empirical result, I calibrate a dynamic model of household decision-making and show that the LC channel is quantitatively important, as it explains roughly one-third of the observed increase in secondary earners' consumption share.

My empirical strategy is a difference-in-differences design that compares secondary earners in equitable-distribution (ED) U.S. states, the treatment group, with those in community-property (CP) states, the control group. Secondary earners in CP states are a valid control group because card issuers were allowed to consider household income even before the reversal under marital division-of-property laws, which recognize household income as joint property regardless of who earns it. The identifying assumption is that, in the absence of the reversal, secondary-earner and household outcomes for the two groups would have evolved in parallel. To strengthen this parallel-trends assumption, I conduct nearest-neighbor propensity score matching to ensure that the treated and control groups have similar pretreatment characteristics that are thought to be associated with the dynamics of the outcome variables (Abadie, 2005).

[3]First, credit limits are portable in the sense that secondary earners or stay-at-home divorcées can keep the high credit limits that they obtained using household income while they were married because credit card issuers are prohibited from making lending decisions based on one's marital status. Second, since having a sole credit card account helps to build one's own credit history, secondary earners' access to credit can improve after divorce. Finally, while debt obligations are divided between spouses upon divorce according to marital division-of-property laws, credit *limits* are not considered marital property and do not get contested in divorce proceedings.

Because there is a never-treated group and a simultaneously absorbing treatment, my estimation does not suffer from the negative-weighting or underidentification problems that can arise in difference-in-differences setups with variation in treatment timing (Sun and Abraham, 2020). See Figure A.1a for where CP and ED states are located in my sample.

I use a panel data set of monthly spending, income, and credit card borrowing covering roughly 66,200 opposite-sex couples, constructed using de-identified financial-account records from the JPMorgan Chase Institute (JPMCI). This data set has the unique advantage of tracking the spending and credit use of *individual* spouses, which allows me to overcome the key measurement hurdle in the intrahousehold literature that spending is only observed at the household level. I proxy for each spouse's consumption by summing spending on their debit card, credit card, and checking-account transactions, such as cash withdrawals or electronic transfers;[4] I proxy for their independent credit by summing the credit limits on their sole credit card accounts; and I proxy their total credit by summing the limits on all the credit card accounts the spouse has access to, either as a primary account holder or as an authorized user. I define spouse-specific consumption shares as each spouse's spending relative to total household spending, and the household's consumption gap as the difference in the two spouses' spending shares. Credit shares and gaps are constructed in the same manner.

An important concern is that the spending-based measure I describe above may be a poor proxy for individual consumption. For instance, if spouses spend individually but consume purchased goods together, such as when there is a designated shopper, individual spending will not accurately reflect individual consumption. I address this concern by constructing an alternative consumption measure based on the "gender-intensity" in spending shares. Specifically, I calculate the share of spending by gender for 100 spending categories using 2.4 million individuals who are active users and sole account holders of Chase checking and credit card accounts. I use this out-of-sample spending shares and assume that a given spending category was consumed by the spouse with greater than 55 percent of gender-intensity in spending shares.[5] For exam-

[4]For joint checking accounts that are shared between spouses, I measure who spent what on these accounts by identifying which debit card is assigned to whom. For any transactions for which the identity of the spender cannot be clearly assigned, I assume that the spending was split equally by the spouses.

[5]My results are not sensitive to the choice of spending-share threshold. I choose 55 percent as my baseline threshold because it captures a broad set of categories without reducing the accuracy of *gendered* consumption. Thus, this measure is akin to the "gender-assignable" measure used in prior research, which assumes that gender-specific clothing can only be consumed by one member of the household regardless of who purchased them, but significantly improves external validity by capturing a broader set of categories. The gender-intensity measure

ple, I assume that wives consume total household spending on "cosmetics," since 82 percent of total spending on cosmetics is incurred by women. This measure provides an alternative proxy for consumption, under the assumption that gender-intensive goods are more likely to be consumed by only one member of the household regardless of who purchased them.

I find four main results. First, the TILA reversal had the intended effect of increasing access to credit for secondary earners. The estimated increase in secondary earners' credit card limits two-years after the reversal is 59 percent of their average pre-reversal monthly consumption, or roughly $1,506. Put in context, the effect corresponds to 16 percent of the typical credit limit for secondary earners in my sample. This can be considered a first-stage effect since the reversal would not trigger a change in the consumption allocation between spouses without also affecting borrowing capacity. The treatment effect is driven by changes in secondary earners' income-reporting behavior rather than differential effects on other credit card terms. Specifically, the reversal did not differentially affect secondary earners' propensity to open credit card accounts in the treated group relative to the control group; rather, conditional on opening an account, it differentially increased the credit limit for secondary earners in the treated group relative to the control group. My estimates are not confounded by selection bias, as borrowing costs (the annual percentage rate) and the propensity to open or close credit card accounts were invariant to the reversal.

Second, the central finding of the paper is that the TILA reversal reduced the consumption gap between spouses by shifting consumption toward secondary earners. I find that the reversal increased secondary earners' consumption by 66 percent relative to their pre-reversal monthly mean over the two-year-period after the reversal, or $1,685.[6] The reversal not only increased the secondary earners' *level* of consumption, but also increased their *share* of consumption in the household by 11 percent by year two. The cumulative increase in household consumption is smaller ($953) than the increase observed for secondary earners ($1,685), suggesting that the consumption reallocation between spouses operated through primary earners cutting back their consumption. As a result, the consumption gap in the household narrowed by half

captures roughly 54 percent of spending-based measure in dollar terms (i.e., $1,374/$2,534).

[6]Secondary earners' consumption increased by $1,685 based on the spending-based measure and by $820 based on the gender-intensity measure, although both measures indicate an increase in spending by more than 60 percent. The discrepancy in dollar terms reflects the fact that the gender-intensity measure only considers a subset of spending categories with more than 55 percent in gender-intensity in spending. The implied marginal propensity to consume (MPC$\approx$0.55) is comparable to that of other papers in the literature that proxies for consumption not just with interest-accruing credit card debt but with total purchase on credit cards (e.g., Agarwal et al. (2018)).

relative to the pre-reversal mean. These results are robust to using a battery of alternative specifications, samples, and measures, such as gender-intensity measure that considers a smaller subset of spending categories with less or more conservative spending-share threshold (50 and 60 percent), or an alternative measure that takes non-Chase credit card spending into account. Placebo tests involving (1) households that see no change in credit limit; and (2) those that only see a change in the joint credit limit without a change in secondary earners' independent credit access show no effects on secondary earners' consumption share, corroborating the interpretation that secondary earners' *independent* borrowing capacity shapes how spouses distribute consumption.

Third, while there is no clear cut evidence of a commensurate rise in income to finance higher consumption, the reversal did not worsen the financial standing of the household over the two-year period I analyze. Specifically, a variety of financial-solvency outcomes were not materially impacted, including delinquency rates, overdraft probabilities, and the propensity to take out high-interest loans, such as payday or subprime loans. In addition, households became more likely to settle existing debt and to pay down more expensive debt first while borrowing more on cards that carry lower interests, improving the overall family financial health. Moreover, the increase in household borrowing is not statistically significant and substantially smaller than the increase in secondary earners' consumption. That, combined with the fact that the indicators of financial solvency did not deteriorate, is consistent with spouses coordinating their consumption decisions to satisfy the family budget constraint. Accordingly, the results indicate that the narrowing of a consumption gap in the household did not come at the cost of worsened financial standing.

Finally, I find support for the LC channel as a primary explanation for these findings. Under the canonical collective-household model with LC, since spouses cannot precommit to future allocations of resources, credit can empower secondary earners to act in their best interest and voice their opinions in the marriage, to the extent that higher borrowing capacity improves their outside options. This channel is relevant even for couples who are not on the verge of divorce insofar as there is some risk of divorce. That said, in the cross-section, the channel predicts that the effect of the reversal on secondary earners' consumption share should be larger (smaller) for couples with weaker (stronger) marital commitment because they will be more (less) sensitive to changes in the outside option. Consistent with this prediction, I find that the

estimated effect is 86 percent larger for couples that are most likely to divorce and 50 percent smaller for couples that are least likely to divorce.[7] Placebo analysis confirms that marital commitment does not influence consumption reallocation for households in which secondary earners' borrowing capacity does not change. I find no clear-cut support for other channels, such as imperfect information, financial constraint, self-control, or limited attention.

Motivated by the reduced-form estimates showing support for the LC channel, I analyze the TILA reversal through the lens of household-decision-making model under limited commitment and borrowing constraints and show that the LC channel is quantitatively important. In this model, primary and secondary earners jointly decide how much to save and consume, whether to work, and whether to divorce by maximizing the weighted sum of their utilities, where the weights are their bargaining powers. The key feature of the model is that a spouse's bargaining power can change over time: whenever a spouse's outside option increases to the point at which the value of being divorced exceeds the value of staying married, the bargaining power adjusts just enough to make the spouse who prefers to divorce indifferent between divorcing and staying married. Higher bargaining power, in turn, leads to a higher consumption share in the household. I use this standard setup (Mazzocco, Ruiz and Yamaguchi, 2014; Voena, 2015) to test the quantitative importance of the LC channel by incorporating a key feature of the reversal–namely, the expansion of secondary earners' borrowing limits that they can keep even after divorce–and track how secondary earners' share of consumption evolves in equilibrium. For realistic parameter values, I show that the model-generated consumption path can account for up to 37 percent of the observed increase in secondary earners' consumption share.

The contribution of this paper is threefold. First, this paper speaks directly to the policy debate on the credit access and inequality by providing evidence that there are credit disparities in the U.S. even within the household and that these credit disparities exacerbate consumption disparities. The traditional case for regulation of consumer financial products is based on consumer protection from opportunistic or predatory practices (Mullainathan, Barr and Shafir, 2009; Campbell et al., 2011; Stango and Zinman, 2011; Agarwal et al., 2015, 2018; Cambell, 2016; Alan et al., 2017). The more recent policy debate highlights that another important rationale for regulation is to reduce disparities in credit access (Federal Reserve Board, 2023).

[7] I proxy for weaker marital commitment using spending on dating/escort services or counseling services, such as couple counseling. Conversely, I proxy for stronger marital commitment using baseline spending on mortgage, based on the idea that homeownership increases marital commitment (Lafortune and Low, 2017), and on children.

A small but growing literature examines the distributional consequences of credit card policies (Cuesta and Sepúlveda, 2021; Nelson, 2023; Keys, Mahoney and Yang, 2023). But to the best of my knowledge, there has been to date no evidence on the extent of credit disparities within U.S. households and their consequences for consumption disparities.[8] The TILA reversal provides an ideal setting to fill this gap because the financial status of only one of the two spouses in the household is affected, allowing me to document the substantive effect of reducing within-household credit disparities on consumption reallocation between spouses. My finding that credit market policies have an uneven impact on individual family members also contributes to the related literature on marginal propensity to consume (MPC) after credit expansions (Gross and Souleles, 2002; Aydin, 2022; Gross, Notowidigdo and Wang, 2020) by showing that the household-level consumption response may mask sizable inequality and reallocation between spouses. A related contribution is that this paper is the first to evaluate the impact of the 2013 TILA reversal and to apply the family-economics perspective to consumer financial policies.

Second, by leveraging administrative financial-accounts records and comprehensive measures of intra-household spending allocations, this paper provides the first direct evidence on how spouses allocate consumption within U.S. households. Prior studies in the intra-household literature emphasize the importance of measuring within-household economic outcomes for understanding inequality (Chiappori and Meghir, 2015; Blundell, Pistaferri and Saporta-Eksten, 2016; Lise and Yamada, 2019) and effects of policies designed to improve child's development (Lundberg, Pollak and Wales, 1997; Attanasio and Lechene, 2014; Schönberg and Ludsteck, 2014) and family's well-being (Voena, 2015), but the paucity of data on spouse-specific consumption has hindered progress in the literature.[9] Relative to prior studies (see Appendix A for a comprehensive overview), I provide direct evidence of how spouses allocate consumption in the U.S that does not require additional model-based assumptions.[10] The JPMCI dataset allows

[8] A related set of studies evaluate how financial regulations or practices in the financial sector affect inequality. Recent studies document the role of bias in underwriting (Dobbie et al., 2020), information disparity (Blattner and Nelson, 2021), mortgage market policies (Kermani and Wong, 2021), or technological innovation in consumer lending markets (Bartlett et al., 2021; Fuster et al., 2021) in shaping disparities in consumer credit outcomes.

[9] Three studies to date have attempted to directly measure spouse-level consumption using Danish, Dutch, and Japanese survey datasets (Browning and Gørtz, 2012; Cherchye, De Rock and Vermeulen, 2012; Lise and Yamada, 2019). While these studies make an important progress, they either use (i) one-time, ad-hoc survey modules that makes it difficult to track consumption allocation within the same household over time or (ii) rotating panel surveys that collect coarse measures of recall-based expenditures only from female respondents for one specific month in a year using a small sample of couples (200 $\sim$ 1,150).

[10] Several studies made progress toward quantifying within-household disparities using a structural model of intra-household bargaining (Browning et al., 1994; Lise and Seitz, 2011; Knowles, 2013), but model-based esti-

me to track *who spent what* precisely in monthly-frequency for the same couples over time and provides information on the breakdown of each expenditure and spending method (e.g., checking, debit card, credit card) by individual household members. In addition, I develop a new approach to measuring spouse-specific consumption based on a broad set of gender-specific consumption categories and to measuring marital commitment based on spending on dating services, couple counseling, joint assets (i.e., mortgages) and children, both of which can be applied more broadly in future work on family economics and gender.

Finally, at a substantive level, the main novelty of the paper is to establish that credit cards have bargaining effects. This result contributes to the literature on the interaction between marital property laws and bargaining. It is widely recognized that marital property laws have shaped many aspects of women's rights and economic outcomes (Khan, 1996; Gray, 1998; Doepke and Tertilt, 2009; Goldin and Shim, 2004; Fernández, 2014; Anderson and Bidner, 2015) and improved their bargaining positions in marriage (Chiappori, Fortin and Lacroix, 2002; Bayot and Voena, 2014; Voena, 2015; Lafortune and Low, 2017). I show a new credit-based mechanism through which marital property laws shapes spouses' decision power. Because marital property laws govern the underwriting decisions of credit card issuers, and these decisions, in turn, influence spouse's ability to borrow both within *and* outside of marriage, these laws can have material impact on one's marital bargaining position. A related literature in family economics highlights that marital commitment is less than full (Mazzocco, 2004, 2007) and that exogenous changes in income or control over earnings have substantive effects on household expenditure decisions by altering spouses' relative bargaining position (Lundberg, Pollak and Wales, 1997; Duflo, 2003; Duflo and Udry, 2004; Bobonis, 2009; Lise and Yamada, 2019). The contribution to this literature is to propose a complementary but distinct factor that affects spouses' bargaining position – i.e., credit cards. An important policy implication is that allowing both spouses to take advantage of joint marital assets not only expands credit access but also helps to strengthen secondary earners' marital bargaining power.[11]

mates may underestimate the true extent of disparities if other mechanisms beyond bargaining are at play, such as noncooperation (Lundberg and Pollak, 1993; Basu, 2006), information frictions (Ashraf, 2009), or strategic motives (Hertzberg, 2016; Schaner, 2015; Choukhmane, Goodman and O'Dea, 2021).

[11] A related literature in development evaluates the empowerment effects of microcredit programs or savings products targeting women's financial independence (Ashraf, Karlan and Yin, 2010; Banerjee, Karlan and Zinman, 2015; Field et al., 2021). This study differs from the development literature in terms of institutional setting and the type of financing considered. While microcredit is a targeted policy tool in countries where cultural and gender norms make it difficult for women to obtain credit (Fletschner, 2009), the U.S. credit card market is highly so-

2 Institutional Background and Research Design

The 2013 reversal of the Truth-in-Lending Act Section 150, or the ability-to-pay provision, exogenously increased secondary earners' access to credit in the credit card market. Section 2.1 discusses the institutional background and Section 2.2 describes my empirical design.

2.1 The Truth-in-Lending Act

The 1968 Truth-in-Lending Act (TILA) is a federal statute that requires lenders to disclose terms and cost – such as the annual percentage rate (APR) – to consumers and bans lenders from using deceptive advertising practices (CFPB, 2021).[12] The TILA governs a wide range of consumer credit products including credit cards, mortgages, auto, and installment loans.

This study examines the reversal of an amendment to TILA Section 150, which applies to the credit card market. In October 2011, roughly two years before the reversal, the Federal Reserve Board (the Board) introduced an amendment to Section 150, mandating credit card issuers to specifically consider the consumer's "independent" ability to pay when they issue credit. Prior to the amendment, Section 150 did not offer any specific guidance and stated that:

> a card issuer may not open any credit card account for any consumer under an open end consumer credit plan, or increase any credit limit applicable to such account, unless the card issuer considers the ability of the consumer to make the required payments under the terms of such account (12 CFR §1026, 2012).

After the amendment, card issuers were required to either (i) consider the consumer's independent means of repaying through information collected on a credit card application; or (ii) obligate the consumer to have a cosigner who has such means and can assume joint liability for the account. The original intent of this amendment was to restrict card issuers from extending credit to consumers under the age of 21 to address a growing concern at the time that young adults were being offered credit cards on the basis that their parents had enough income, without the parents' consent. However, the amendment raised an unexpected concern that it may

phisticated and affects more than 80 percent of all American adults (CFPB, 2019). Since cultural and institutional differences play a central role in how family members interact (Bau and Fernández, 2021), whether independent ability to access credit markets strengthens women's bargaining position in the U.S. is an open question. I show that there is a meaningful scope for improving women's financial independence through credit cards even in a setting where cultural norms do not dictate women's ability to borrow and regulations prevent discrimination on the basis of gender.

[12] Stango and Zinman (2011) evaluates the 1981 reform of the Truth-in-Lending Act that required disclosure of APR to consumers.

restrict secondary earners and stay-at-home spouses who have limited income of their own but access to their spouse's income from establishing access to credit.

Growing concerns about the 2011 amendment having discriminatory effects on secondary earners and stay-at-home spouses prompted a Congressional hearing to consider reversing the amendment. The nature of these concerns are reflected in the opening statement of the June 2012 Congressional hearing by Senator Shelley Capito (R-WV):

> This rule could be especially punitive for women who are in a failing marriage or an abusive relationship. As I think about what some of the fundamental steps somebody who is maybe in an unhappy marriage or an abusive relationship would take, one of the fundamental, I am sure, pieces of advice is to try to establish credit, try to establish a financial footprint. Similarly, stay-at-home spouses whose husband or wife dies unexpectedly or divorces them could face similar challenges if they have not maintained a credit history.... The ability to pay rule threatens to further complicate the situation by potentially limiting their access to credit. (House Hearing: 112th Congress, 2012)

The 2011 amendment was reversed in 2013, allowing card issuers to "consider income and assets to which consumers have a reasonable expectation of access" for consumers over the age of 21. The Consumer Financial Protection Bureau (CFPB) announced this change in May, 2013, and compliance with this rule was required by November, 2013.

2.2 Research design

I examines the effect of the 2013 reversal by exploiting the fact that the TILA reversal was superseded by state marital property laws in some states but not in others. Specifically, for couples living in community property (CP) states, since any income earned during marriage is considered to be jointly owned regardless of who earned it, card issuers were allowed to consider secondary earners' "household income" when issuing credit. In equitable distribution (ED) states, however, card issuers were required to consider secondary earners' "independent income" prior to the reversal because income earned during marriage is considered to be separately owned. I use this feature and consider households living in CP states as the "control" group and those living in ED states as my "treated" group. Figure A.1a shows the map of where CP and ED states are located in my sample.

I use the following difference-in-differences (DiD) regression specification:

$$Y_{h,t}^{i} = \alpha_h + \gamma_t + \beta \mathbf{1}[Treat \times Post]_{h,t} + \epsilon_{h,t} \tag{1}$$

where $Y_{h,t}^{i}$ is an outcome for secondary earner i in household h at month t. α_h are household fixed-effects, γ_t are time (month-year) fixed-effects, and $\mathbf{1}[Treat \times Post]_{h,t}$ is an interaction term between treatment and post ($t \geq$ November 2013) indicators. The coefficient of interest, β, captures the differential change in the outcome for the treated group relative to the control group following the reversal. I refer to β as "the monthly effect" of the TILA reversal. In addition to monthly effects, I document cumulative effects, $\Phi_\tau = \sum_{j=0}^{\tau} \phi_j$, over 6-, 12-, 18-, and 24-month after the reversal, from running the following dynamic DiD specification:

$$Y_{h,t}^{i} = \alpha_h + \gamma_t + \sum_{j \neq -1} \phi_j (Treat_h \times 1_{j=t}) + \epsilon_{h,t} \tag{2}$$

I omit the month prior to the reversal, $j \neq -1$, so ϕ_j can be interpreted as a change relative to this pre-reversal period. To facilitate a comparison between stock and flow variables at different points in time, I report ϕ_j for stock (e.g., credit limit) and Φ for flow (e.g., consumption) variables throughout. For all regressions, I cluster standard errors at the state level.

The identifying assumption is parallel trends: the average outcomes for treated and controls would have followed parallel paths over time in the absence of treatment. While the parallel trends assumption does not require outcomes to look similar in levels across treated and control units, this assumption may be violated if pre-treatment characteristics that are thought to be associated with the *dynamics* of the outcome variable are unbalanced between the treated and the control group (Abadie, 2005). To strengthen the "parallel trends" assumption, I apply the nearest neighbor propensity score matching method by matching households based on their conditional probability of being treated given the covariates. I choose pre-treatment covariates (X) based on factors that may influence the card issuer's underwriting criteria and use a logit regression to estimate the propensity score $p(X) = P(Treat = 1|X)$. Because propensity score has a balancing property, the matched sample has the same distribution of covariates, conditional on the propensity score (Rosenbaum and Rubin, 1983).[13] Appendix B discusses

[13] After propensity score matching, my sample remains representative and captures over 80 percent of the pre-matched sample size (see Table 1). Specifically, sample size is 81,134 pre-matching and 66,000 post-matching.

how the reversal provides a credible identification setting in practice.

3 Data and Descriptive Evidence

I use a panel dataset of monthly spending, income, and credit card borrowing of 66,200 opposite-sex couples from October 2012 to December 2015, covering a year before and two years after the TILA reversal. I do not analyze the 2011 amendment because the data starts from 2012.

3.1 Analysis Sample

I construct my sample in three steps– (i) identify couples in the dataset; (ii) obtain information on each spouse's checking; and (iii) credit card accounts.

I identify couples using a record of account linkages that links family members to a unique household identifier. Individuals must share personally identifiable information, such as address and last name, to be linked to the same household unit. Since I do not directly observe individuals' marital status, I apply several data filters to focus on individuals that are likely to represent married couples. Specifically, I restrict the sample to opposite-sex, two adult-member households in which members have the age gap of less than 16 years.[14] The age gap restriction is applied to filter out siblings or parent-child pairs residing in the same address. Given that more than 92 percent of individuals in my sample have joint checking accounts shared with the other member in the same household unit (see Figure A.4) – an alternative proxy for identifying couples (Ganong and Noel, 2019) – my sample is likely to capture married couples. I further restrict the sample to spouses in their prime working age (25 to 65 years old) at the timing of the reversal to mitigate confounding effects from retirement.

Next, I obtain each spouse's checking account information to ensure that individual member's spending can be tracked both before and after the reversal. I require *both* spouses to have at least one active checking account at JPMC *either* as a primary or secondary account holder, where active means having at least 5 transactions every month. I do not require individual spouses to have separate financial accounts, but only that they are account holders of at least one (sole or joint) checking account. This allows me to capture couples with a diverse set of

[14]This sample can include two adult-member households with children. Since I do not directly observe if a couple has children, I proxy for this by household spending on child care and children's clothing.

financial account structure, including those that only have joint accounts as well as those with a mix of joint and separate accounts. For couples with joint checking accounts, I require spouses to have their own debit cards associated with these shared accounts to be able to track each spouse's spending on these joint accounts. I further restrict the sample to couples that make above-poverty annual labor income of at least $17,000 in 2013,[15] to focus on couples that generate sufficient income and primarily use JPMC checking accounts to manage their finances.

In the final step, I obtain each spouse's credit card account information to focus on the sample relevant for studying the impact of the credit card policy change. I require (i) at least one spouse in the household to be a credit card holder at some point during my sample period; and (ii) secondary earners to not have a *sole* credit card account at the beginning of my sample period (October 2012). The first restriction allows me to focus on couples that rely on the credit card market. The second restriction allows me to focus on couples where secondary earners have the highest propensity to open credit cards. The reversal primarily applies to new card openers rather than existing card holders because new card openers must report income on their credit card applications whereas existing card holders rarely update their income. Thus, focusing on this sample helps me to isolate the primary margin through which the reversal operated, which is the change in income consideration standards.

At the end of the data construction steps, I conduct propensity score matching and obtain 66,200 couples. I refer to this sample as the *All Sample* and use this sample to document descriptive patterns. For my regression analysis, I further restrict All Sample to 11,682 households where secondary earners open a sole credit card at some point during my sample period. I refer to this sample as the *Regression Sample*. Since card issuers use income primarily for deciding how much credit limit to extend to new card openers, focusing on this sample allows me to test whether secondary earners that happened to be living in equitable distribution states get a bigger lift in credit limits among secondary earners who are equally likely to open a new credit card around the same time. I confirm that treated and control secondary earners in the Regression Sample have similar pre-reversal characteristics and card opening rates.

The sample restriction I apply focuses on marginal households that are most likely to be affected by the policy. While focusing on the sample is sensible from a policy evaluation perspective, one potential concern is that my findings cannot be generalized to a broader population

[15] $17,000 is the U.S. Department of Health and Services' 2013 poverty threshold for two-member household.

due to the restrictive sample selection criteria. However, the sample of households I study represents a non-trivial fraction of the overall couples, with the *All Sample* representing more than 50 percent of all couples I identify with active checking accounts during my sample period (this share is 82 percent before propensity score matching) and the *Regression Sample* representing roughly 20 percent of all couples with active checking and credit card accounts. And while the minimum income criteria eliminates low-income couples from my sample, given that making sufficient income is a key requirement for credit card underwriting, excluding poorer, underbanked couples likely makes my sample more representative of households that participate in the credit card market. I further address the sample selection concern by (1) benchmarking my sample to a representative sample of U.S. couples using the CEX and PSID; and (2) showing that my results are robust to using a broader sample of 137,904 households with no restrictions on secondary earners' credit card ownership status in Sections D and 3.4.

3.2 Variable Construction

Consumption I proxy for spouse-specific consumption, c^i, by summing spending on each spouse's sole and joint credit card, debit card, and checking accounts, including cash withdrawals and electronic transfers:

$$\begin{aligned} c^i = &\text{Dept store} + \text{Discount Store} + \text{Clothing} + \text{Entertainment} + \text{Flights} + \text{Hotels/Rental} + \\ &\text{Medical} + \text{Transport} + \text{Food Away} + \text{Dur Retail} + \text{Nondur Retail} + \text{Checks} + \text{Cash} + \\ &\text{Prof. Svcs} + \text{Personal Svcs} + \text{Auto Repair/Parts} + \text{Fuel} + \text{Utilities} + \text{Grocery} + \\ &\text{Home Improvement} + \text{Home Cleaning/Repairs} + \text{Child} + \text{Insurance} + \text{Tax} \end{aligned} \tag{3}$$

I aggregate account-level transaction records into detailed spending categories using the Merchant Category Code, transaction counter party, and JPMCI's internal categorization variables. I track who spent what on the couples' joint checking account by (i) identifying which debit card is linked to whom and (ii) attributing spending to the respective debit card holder on the shared accounts. For any joint account transactions for which the spender cannot be identified, I assume that they are shared expenses– e.g., $100 electronic bill payment is shared $50-$50. This is a conservative assumption that pushes consumption shares to be more even.

Once I have spouse-specific consumption measure, c^i, I further break this measure into pri-

vate and public components and construct within-household consumption measures.[16] Private consumption refers to spending on exclusive goods that are consumed privately and only benefits the spouse who spends the money. Public consumption refers to spending on goods that are consumed jointly by the household. I build on existing studies to determine whether spending is private or public (Chiappori, Fortin and Lacroix, 2002; Mazzocco, 2007). See Table A.1 for detailed categorization. The within-household consumption measures include:

$$c^{hh} = c^P + c^S; \qquad c^i_{sh} = \frac{c^i}{c^{hh}}; \qquad c_{gap} = c^P_{sh} - c^S_{sh}$$

Household consumption, c^{hh}, is proxied by summing primary and secondary earners' consumption; spouse-specific consumption share, c^i_{sh}, is proxied by dividing i's consumption by total household consumption; and the consumption gap in the household, c_{gap}, is measured as the difference between primary and secondary earners' consumption shares.

Spending may be a poor proxy for consumption if spouses spend individually but consume the purchased goods together. To address this concern, prior research has used *gender-assignable* consumption measure that infers c^i using household expenditures on clothing, since gender-specific clothing can only be consumed by one member of the household regardless of who purchased it. I build on this idea and construct a *gender-intensity* measure of consumption that uses a more data-driven approach to capture a broader range of spending categories. Specifically, I take 2.4 million individuals who are active users and sole account holders of Chase checking and credit card accounts during my sample period. I then compute the share of spending done by men and women for 100 spending categories, and use this statistic to broaden the category of goods that could be considered *gendered* beyond just spending. Thus, i's gender-intensity consumption measure sums spending categories for which i's gender has more than 55% in spending share. This approach excludes spending categories with gender-intensity between 50 to 55 (i.e., categories that are less clearly gendered), and captures roughly half of spending-based consumption in dollar terms. I conduct robustness with less and more conservative gender-intensity thresholds (50% and 60%). See Section D for detailed discussion on how spending shares are calculated. Since clothing represents a small fraction of total

[16] I include public consumption in spouse-specific consumption share calculation to capture each spouse's broader spending preferences. I provide a decomposition analysis in Section 4.4 to ensure that the change in consumption is not simply capturing the nominal change in who shops for public goods.

household spending (1%), gender-intensity measure helps to improve the external validity of gender-assignable measure by capturing more than 54% of spending-based consumption.

Credit I construct two credit measures – independent credit and total credit. Spouse i's *independent* credit access is proxied by the sum of credit limits on i's sole credit card account; and *total* credit access is the sum of credit limits on any credit card account he or she has access to either as a primary account holder or as an authorized user. Household credit access is measured as the sum of total credit limit extended to spouses, where limits on joint accounts are only counted once in the household-level aggregation. Credit shares and gaps are constructed in the same manner as consumption shares and gaps.

Income Monthly spouse-specific income is measured as the sum of labor income (payroll direct deposits), government transfers, and other income deposited to spouses' sole and joint checking accounts *for which they are the primary account holder*.[17] One potential concern with this assumption is mis-classifying which spouse is the primary earner. Given that husbands are primary earners for the majority (84%) of opposite-sex married couples in the U.S. (Current Population Survey, 2020), mis-classification is likely to arise for couples that only have a shared checking account where the wife is the primary account holder (16.1% in my sample). While over-classifying wives as primary earners can bias consumption gaps downward because wife's consumption share tends to be smaller than that of the husband (Lise and Seitz, 2011), mis-classification is unlikely to be a concern for causal inference because treated and control households have a similar distribution of account structure types (see Figure A.5) – i.e., mis-classification is uncorrelated with the treatment assignment. Household income is measured as the sum of each spouse's income. A spouse is primary earner if he or she earned higher average monthly labor income relative to the other spouse in the pre-reversal period.[18]

[17] Government transfers include unemployment insurance, veteran's benefits, and tax refunds; and other income includes business or gig income.

[18] I classify households as double-income if (i) it receives more than 4 payroll direct deposits in a month; or (ii) receives more than 2 payroll deposits in a month and the difference in the amount deposited in each paycheck is larger than one standard deviation of monthly labor income that households receive on average. This is based on the fact that workers typically receive income on a bi-weekly basis (U.S. Bureau of Labor Statistics, 2020*b*).

3.3 Pre-Treatment Characteristics and Sample Representativeness

Treated and control households have similar pre-treatment characteristics. Before propensity-score matching, Table 1 show that the treated group has higher baseline average income and liquidity, and is more likely to have credit cards. Columns 4 through 6 show that the matching procedure yields 66,200 households with similar pre-treatment characteristics. Table A.5 compares secondary earner characteristics for the Regression Sample of 11,682 households and shows that treated and control secondary earners have similar pre-treatment characteristics.

My sample of households look similar to a representative sample of U.S. households. Table A.3 compares average characteristics of my sample to a representative sample of two-member households using the Consumer Expenditure Survey (CEX) and the Bureau of Labor Statistics (BLS). Compared to the benchmark mean, individuals in my sample tends to be younger and consume and earn more. The discrepancy can be driven by differences in sample and measurement: the CEX includes retirees, while I focus on couples in their prime working age that presumably have higher consumption and income; and it also has a well-known underreporting concern (Mian and Sufi, 2016). Despite the differences in levels, the ratio of consumption to income or the ratio of public (or private) consumption to household consumption match the CEX closely. The share of double-income households also match the BLS share.[19]

There is substantial heterogeneity in credit access and consumption both within and between couples. Panel A of Table 2 shows monthly pre-reversal household characteristics. Couples on average consume and earn total income of $6,005 and $9,017, respectively, while the median household consumes and earns roughly 23 percent less than the average household. Couples on average have access to credit 74 percent of the time, while the median couple always has access to credit before the reversal. Panel B illustrates heterogeneity within the household. On average, primary earners earn 8 times more and consume 30 percent more than secondary earners. Secondary earners are substantially less likely to be able to borrow independently before the reversal relative to primary earners. As discussed in Section 3.2, the income gap is likely to be overstated because I attribute all income streams to the primary account holder when both spouses deposit income into their joint checking accounts.

[19] See Table A.4 for statistics of account ownership structure for married couples in the U.S.

3.4 Descriptive Evidence

I document three novel facts that motivate understanding the link between disparities in credit access and consumption in the household. The descriptive analysis uses all sample period.

First, there are large gaps in credit access within the household. Figure 1 plots the average share of accessible credit by earner type. Primary earners have access to 92% and secondary earners 35% of total credit limits available at the household-level, indicating a within-household credit gap of 57%. The independent credit gap is even larger ($0.61\% = 0.80 - 0.19$), suggesting that secondary earners are much less likely than primary earners to be able to borrow independently from credit markets. The large within-household credit gap is driven in part by the fact that All Sample is limited to households where secondary earners did not have a sole credit card account at the beginning of my sample period. However, the within-household credit gap is large even in a broader sample of households without this sample restriction. Figure A.6 shows total and independent credit gaps of 11% to 12% among households where secondary earners had credit card accounts at the beginning of the sample period.

Second, there are large gaps in consumption within the household. Secondary earners on average consume 44% and primary earners 56% of total household consumption, indicating a consumption gap of 12%. In other words, secondary earners consume 78 cents for every dollar consumed by primary earners. The consumption gap in the household cannot be fully explained by differences in spouses' income. If income determines consumption shares of each spouse, individuals that make similar levels of income should consume similar shares of consumption in their respective household. However, Figure 2 shows that relative – rather than nominal– financial power in the household explains how much consumption is allocated to each member within the household. Specifically, this figure shows the average consumption share of individuals in the same income bin by earner status in their respective household. With the exception of the highest income bin, individuals in every income bin has higher consumption share relative to their spouse if they are primary earners, but not if they are secondary earners.

Finally, secondary earners' *independent* credit access is positively correlated with their share of consumption in the household. Figure 3a plots secondary earners' average consumption share against their share of accessible household credit. Secondary earners' consumption share increases monotonically with their share of total accessible household credit, suggest-

ing that having a higher relative borrowing capacity is associated with higher consumption allocation in the household. Interestingly, Figure 3b shows that the positive correlation between credit and consumption shares disappears when consumption share is plotted against the amount of credit access secondary earners have as an authorized user. This suggests that simply having higher borrowing capacity does not explain whether a spouse gets higher consumption allocation in the household. Rather, it illustrates that ability to access credit independently is associated with how consumption is shared between spouses.

4 Effect of the Reversal on Inequality in the Household

The descriptive evidence shows a clear link between credit and consumption gaps. Motivated by this, I examine the causal effect of the reversal on credit and consumption in the household. For causal analysis, I use the Regression Sample of 11,682 households.

4.1 Effect on Secondary Earner Credit

Table 3 presents DiD estimates on secondary earners' independent (sole) and total credit limit. The outcomes are scaled by secondary earners' monthly pre-reversal consumption mean, so the estimated coefficient can be interpreted as a percent change in spending power relative to their typical monthly spending. Column 1 reports the average monthly effect of the reversal, β, obtained from Equation 1. Columns 2 through 5 report cumulative estimates obtained from Equation 2. Column 6 reports implied effects of the reversal by converting 24-month effect into dollars. Pre-reversal monthly average of the outcome variables are reported in brackets.

The TILA reversal expanded credit access for secondary earners. Column 1 shows that secondary earners' monthly sole credit limit increased by 38 percent relative to their pre-reversal consumption mean. Total credit limit also increased by 40 percent, or \$1,009 ($0.398 \times \$2{,}534$), suggesting that nearly the entire increase in total credit access for secondary earners is driven by changes in independent credit. This increase was large and persistent. Columns 2 through 5 show that secondary earners' total credit limit continued to expand, increasing by 31 percent after one- and 59 percent after two-years, or \$1,506, as shown in Column 6. The monthly average (conditional) credit limit extended to secondary earners before the reversal was \$1,595

($9,115), implying that the estimated effect is as large as (17 percent of) the pre-reversal mean. Section C shows that the reversal did not differentially affect other credit terms, such as APR, card opening or closing rates, and non-Chase credit card usage, suggesting that treated and control secondary earners have a similar credit quality.

Figure 4a provides visual evidence that shows how secondary earners' credit limit evolved over time.[20] The figure plots dynamic estimates obtained from Equation 2 and shows that treated secondary earners' credit limit trended in parallel with respect to that of the control group before the reversal; increased differentially following the reversal; and leveled off one year after the reversal. The gradual increase in credit limit reflects more credit card opening over time for both the treated and the control group. Section C shows that the event-study results are robust to parametrically controlling for the linear pre-trend.

4.2 Effect on Secondary Earners' Consumption

The TILA reversal had a large and persistent effect on secondary earners' consumption. Panel A of Table 4 reports monthly and cumulative effects on secondary earners' consumption using the spending-based measure. Secondary earners' monthly consumption increased by 3 percent relative to their pre-reversal mean, or $73. The cumulative estimates show that consumption increased by 15 percent after 12-months and 66 percent after 24-months, or $1,685.

The central result of the paper is that the TILA reversal reduced the consumption gap in the household by increasing secondary earners' share of consumption. The second row of Panels A shows that the share of consumption allocated to secondary earners increased by 0.5 percent on a monthly basis and by 11 percent after 24 months relative to their pre-reversal mean. Put another way, the reversal differentially increased secondary earners' consumption share by 0.05 percentage points (0.46×1.11) more for the treated group relative to the control group. This is an economically meaningful increase, as typical monthly fluctuation in consumption shares in the pre-reversal period is 0.19 percent. The shift in consumption toward secondary earners reduced the consumption gap between spouses. The third row shows that the consumption gap in the household closed by 2.3 percent on a monthly basis and was halved after two-years.[21]

[20] See Figure A.8 for secondary earners' credit limit and consumption share in levels. This figure illustrate that the estimated effects are driven by changes in the treated group.

[21] The average difference in consumption shares between primary and secondary earners implied by the consumption share estimate Φ_τ need not equal the estimate for the consumption gap because the estimates are obtained

Using the *gender-intensity* consumption measure delivers similar estimates. As discussed in Section 3.2, an important concern for my spending-based consumption measure is that individual spending may not be an accurate proxy for consumption because it hinges on the assumption that spender is the consumer. I mitigate this concern by using an alternative measure that proxies for consumption by summing spending categories that are more likely to be consumed by wife vs. husband based on whether the category has higher than 55 percent of gender-intensity in spending shares.[22] For example, 82 percent of all cosmetics purchases incurred on women's financial accounts, so I treat household spending on cosmetics as the wife's consumption, regardless of who made the purchase. Panel B of table 4 shows secondary earners' consumption and consumption shares increased by 60 percent ($821) and 12 percent, respectively, and the consumption gap in the household declined by 40 percent after two years. The implied dollar effect on consumption using the GI measure is smaller than spending-based measure because GI measure constructs consumption by restricting spending categories to those that are clearly gendered (i.e., more than 55% GI threshold). However, the magnitude of estimated effects in percent terms (i.e., percent change relative to the pre-reversal mean of clearly gendered subset of consumption categories) are similar for both consumption measures.

My implied marginal propensity to consume (MPC) out of credit limits is comparable to that of other papers in the literature that proxies for consumption not just with interest-accruing credit card debt (e.g., Gross and Souleles (2002); Aydin (2022)) but with more comprehensive measure that include total purchases on credit cards (Agarwal et al. (2018)). Consumption estimates in Panels A and B suggest nearly 0.55 cents to a dollar increase in consumption per dollar of credit limit expanded after two years, depending on the consumption measure. Figures 4b and 4c show these results visually by estimating Equation 2. There is no detectable trend in secondary earners' consumption share before the reversal, and the share increases after the TILA reversal. Figure A.9 reports visual evidence using the gender-intensity measure. Taken together, these results suggest that evening out credit disparities between spouses led spouses to share consumption more equally.

from using scaled outcomes with household-specific scaling factors.

[22] Note that my results are not sensitive to the choice of spending-share threshold because using less ($\geq$50%) or more ($\geq$60%) conservative thresholds leads to the same qualitative conclusions. I chose 55% as the threshold by trading-off the accuracy versus the generalizability of this measure, as having less (more) conservative threshold improves (weakens) generalizability by capturing a broader set of categories but reduces (improves) the accuracy of capturing gendered purchases. Relative to the spending-based measure, the gender-intensity measure captures roughly 54 percent of all spending in dollar terms (i.e., 1,374/2,534).

I present further evidence that my findings are robust to using alternative consumption measures, specification, and other samples in Section D. To highlight a few examples, my results are robust to using (1) a consumption measure that includes card payments to other financial institutions to take non-JPMC credit card spending into account; (2) less ($\geq$50%) or more conservative spending-share thresholds (i.e., $\geq$60% in spending shares) for constructing gender-intensity measures; (3) excluding spending categories that may reflect public consumption (i.e., food away, travel, cash, checks, etc); (4) using gender-assignable measure; (5) a specification including state-specific trends; (6) a broader sample of households in which secondary earners had credit card accounts at the beginning of my sample period. Placebo tests confirms that secondary earners' consumption shares did not change for couples that did not see a change in credit allocation, and permutation test addresses a potential concern that estimates may be influenced by unequal treated and control U.S. state sizes.

4.3 Household Credit, Consumption, and Other Financial Outcomes

The reversal expanded credit access at the household-level. The first row of Table 5 shows that the reversal increased credit limit available at the household-level by 28 percent relative to pre-reversal consumption mean after 24-months of the reversal, or $1,523. The increase in household credit is economically meaningful, representing about a third of pre-reversal household credit. The magnitude of the increase is similar to the credit limit increase observed for secondary earners, suggesting that the reversal did not crowd out primary earner's access to credit. The effect on household consumption is positive but substantially smaller than the effect on secondary earner consumption. Specifically, household consumption increased by 17 percent relative to the pre-reversal mean, or $953, after 24-months, which represents 57 percent of the effect on secondary earners' consumption ($1,685). Thus, consumption allocation in the household operated through primary earners cutting back consumption.

I document suggestive evidence that households increased labor supply to finance higher consumption. Panel B of Table 5 reports estimates on household income, cash on hand, and interest-accruing credit card debt. The reversal had a large, positive impact on household income ($946), a small increase in credit card debt ($14), and a decline in liquid cash balance ($75), although these effects not statistically significant over the two-year horizon I analyze

(e.g., t-stat 1.24 for income). Thus, households may be able to sustain higher consumption in the long-run if income continues to rise. Interpreted through the lens of the canonical collective household model, these results suggest that primary earners increased labor supply as a result of the TILA reversal. Under the collective household model, standard income effects should, all else equal, lead to a reduction in secondary earners' labor supply and an increase in primary earners' labor supply to the extent that higher credit leads to an increase in secondary earners' bargaining power and thereby their demand for goods and leisure (Chiappori, Fortin and Lacroix, 2002). While my data does not allow me to test whose labor supply increased because I cannot track spouse-specific labor supply decisions, the increase in household income is consistent with the theoretical prediction of credit altering couples' labor supply decisions.

The TILA reversal did not worsen the financial standing of the household. Higher consumption with no clear cut commensurate increase in income raises a concern that the financial standing of the household may be worsened. However, Table 6 shows that a variety of financial solvency outcomes were not materially impacted, including overdraft probabilities, delinquency rates, or the likelihood of borrowing high-interest loans, such as payday loans. I find a small improvement in debt settlement rates and "optimal" debt repayment behavior for households with multiple credit cards, in which households pay down expensive debt first while borrowing more using cards that carry lower interest (Ponce, Seira and Zamarripa, 2017; Gathergood et al., 2019).

Overall, these results indicate that narrowing of a consumption gap in the household did not come at the cost of worsened financial standing. The divergence of consumption effects estimated at the household- and the secondary earner-level, combined with the indicators of financial solvency not deteriorating, highlight the importance of analyzing household behavior through the lens of individual family members. Specifically, household consumption effect being more muted than that of secondary earners indicate that household averages can mask the heterogeneity in consumption response of individual family members.

4.4 Private and Public Consumption

Decomposing total consumption effect into detailed spending categories reveal that consumption reallocation mainly operated through secondary (primary) earners increasing (reducing)

their private consumption. Table 7 shows that secondary earners' private consumption increased by 32 percent relative to their monthly pre-reversal consumption mean after 24-months. On the other hand, private consumption at the household-level was unaffected, showing a statistically insignificant effect of 5 percent increase relative to the average monthly pre-reversal household consumption. Interpreted in dollars, the increase in secondary earner private consumption is substantially larger ($821) than the increase in household private consumption ($245), suggesting that primary earners reduced private consumption by $576.

While consumption reallocation operated primarily through changes in spouses' demand for private goods, part of the reallocation effect reflects who shops for public goods. Panel A shows that secondary earners' public consumption increased by 34 percent relative to their monthly pre-reversal consumption mean, or $863, after 24-months. Panel B shows that households' demand for public consumption also increased by 13 percent relative to household's pre-reversal monthly consumption mean, or $707. Given that secondary earners' demand for public goods increased by more than that of the household, this suggests that primary earners reduced their spending on public goods by $156 ($863-$707). Overall, these results hold up to using the gender-intensity measure. Figure 5 provides visual evidence of the reallocation effect in the household. While both secondary earner- and household-level demand for private and public goods increased, primary earners cut back both types of consumption. In particular, primary earners cut their private consumption more drastically than they do public consumption. Assuming that primary earner's reduction in public consumption reflects a nominal change in who shops for public goods, the estimates imply that 91% ($\frac{\$1,528}{\$1,684} = \frac{\$1684-\$156}{\$1,684}$) of the total increase in secondary earners' consumption captures reallocation effect. Table A.11 reports results using the gender-intensity measure and similarly shows that reallocation operated mainly through private consumption.

The detailed breakdown of consumption patterns shows that the TILA reversal led to a substantial change in the types of goods demanded by secondary earners and households. Panel A of Figure 6 shows the percent change in secondary earners' spending on specific categories of goods relative to their pre-reversal average monthly consumption, along with the magnitudes interpreted in dollars and in terms of pre-reversal mean of each category. Among public goods, secondary earners increased spending on groceries, home improvement (e.g., home or garden supply stores, florists, etc), and fuel (e.g., gas station) the most. Among private goods,

secondary earners increased spending on paper checks, food away from home, clothing, and nondurable retail the most.[23] Panel B shows that household consumption patterns largely mirror that of secondary earners. The substantial reduction in cash spending suggests that secondary earners not only changed the type of goods purchased but also their spending method, as credit cards allowed secondary earners to substitute cash with card spending. Figure A.12 shows similar results using the gender-intensity measure. Note that the gender-intensity measure excludes categories with less than 55 percent in spending shares, such as paper checks.

5 Mechanism for TILA Consumption Effects

This section explores mechanisms that could explain this paper's main findings.

5.1 The Limited-Commitment Channel

The limited-commitment (LC) channel has the potential to explain my main findings. Under the canonical collective-household model with LC, since spouses cannot precommit to future consumption sharing rule, factors that improve the outside option (that is, the value of being divorced) of spouses with lower initial bargaining power should shift consumption allocation in their favor to satisfy their participation constraints in marriage (Chiappori and Mazzocco, 2017). In practice, credit limits can increase secondary earners' outside options because divorcees can keep high credit limits that they obtained during marriage even after divorce because card issuers are prohibited from adjusting account holders' credit limits based on their marital status.[24] Therefore, divorcees' credit limits are "portable" and will stay intact even after divorce as long as they are able to make a minimum monthly payment.[25] Thus, in the cross-section, the LC channel predicts that the consumption reallocation effect should be larger (smaller) for couples with weaker (stronger) marital commitment because they will be more (less) sensitive to changes in the outside option.

[23] The increase in paper checks represents less than 2 percent of average baseline spending on checks. Thus, the large dollar effects capture the fact that typical check payments tend to be large in dollar terms.

[24] Equal Credit Opportunity Act of 1974 prohibits credit card issuers from making lending decisions based on one's marital status. Exceptions apply when one applies for a joint credit card shared with the other spouse.

[25] Figure A.13 shows changes in financial situation after divorce for divorced men and women. Women's (men's) financial situation tends to improve (deteriorate) after divorce relative to when they were married. This suggests that secondary earner divorcees (typically women) are unlikely to become financially delinquent after divorce.

Overall, my results are consistent with credit shaping power dynamics between spouses. Table 8 reports differential effect of marital commitment on secondary earners' credit limit and consumption share using a triple interaction specification that interacts $\mathbf{1}[Treat \times Post]$ with proxies of marital commitment. Panel A shows that secondary earners' credit limits increased after the TILA reversal, but there is no differential increase for couples with weaker or stronger marital commitment. However, Panel B shows that the estimated effect on secondary earners' consumption share is larger (smaller) for couples with weaker (stronger) marital commitment prior to the reversal. Column 1 shows that a standard deviation increase in couples' spending on counseling services or dating services – a proxy for weak commitment – leads to 86 percent increase in secondary earners' consumption share. On the other hand, a standard deviation increase in spending on mortgage payments and children's clothing or child care – proxies for stronger commitment (Lafortune and Low, 2017)– reduces reallocation by 40∼50 percent. Columns 4-6 show that these results are robust to including a battery of baseline covariates that accounts for differences in baseline income, liquid assets, couples' age gap, and secondary earners' consumption share across households with varying marital commitment. Table A.12 shows that limited marital commitment does not have any effect on secondary earners' consumption share for households that do not experience a change in credit limit.

In practice, credit has direct, immediate implications for secondary earners' outside options because credit provides financial buffer to spouses with limited resources to undergo divorce proceedings. However, the LC channel is relevant even for couples that are not on the verge of divorce under a broader interpretation of marital bargaining than credit cards triggering an explicit threat of divorce. Credit can discipline harmonious relationships by giving each spouse agency to act in his or her best interest, for example, by giving secondary earners more latitude to buy goods that better reflect their preferences and primary earners less control over household spending. Moreover, since independent credit access allows secondary earners to establish their own financial footprint by building their own credit history, credit access has a potential to influence couples' bargaining dynamics in the long-run by improving financial risk-sharing in which secondary earners provide insurance against primary earners' downside income risk.

5.2 Alternative Channels

While I cannot fully rule out the importance of alternative economic channels, I do not find clear cut support that they are primary drivers that explain my main findings.

Financial Constraint. Credit can induce secondary earners to spend more by relaxing financial constraint. Thus, the financial constraint channel predicts that secondary earners' consumption share should be larger for couples that were financially constrained prior to the reversal. However, I find no clear cut evidence of consumption reallocation effect being larger for financially constrained couples relative to unconstrained couples. Table A.14 reports monthly effects on secondary earners' credit limit and consumption share for subsample split analysis based on proxies of financial constraint, such as baseline liquid cash balances (i.e., liquidity), utilization rate (i.e., debt capacity slack), and debt-to-income ratio (i.e., disposable income). The estimated effect on secondary earners' consumption share is similar irrespective of whether households have high or low cash liquidity prior to the reversal. Subsample splits by credit card utilization rates or debt-to-income ratio shows that consumption reallocation effect is if anything greater for *less* financially constrained couples. Finally, Table A.10 shows that relaxing households' total credit constraints (i.e., increase in credit limit on joint account) without changing secondary earners' independent credit access does not lead to any change in consumption reallocation. Overall, these results suggest a limited role of the financial constraints channel.

Limited Information. Under imperfect information (Wang, 1995; Ashraf, 2009), primary earners' consumption should not change because spouses cannot observe the realization of each other's outcomes. For example, primary earners would not be aware of secondary earners' spending patterns as secondary earners would try to hide their borrowing ability. However, as discussed in Section 4.4, consumption reallocation operated mainly through primary earners cutting back their consumption. This suggests that spouses coordinate their consumption decisions to satisfy the family budget constraint. Moreover, 92 percent of couples in my sample have a joint checking account. To the extent that couples pay their credit card bills using their joint checking accounts, there's limited scope for hiding one's credit card spending.

Shopping Convenience and Self-Control. Under shopping convenience or self-control, secondary earners' consumption shares should increase either because credit cards make it is easier to spend money or because they have little self-control in overcoming the impulse to indulge (Banerjee and Mullainathan, 2010). Thus, these channels predict that the increase in secondary earners' consumption should (i) be financed with credit cards (due to convenience) and (ii) lead to deterioration in households' financial well-being. Table A.13 decomposes secondary earners' monthly consumption effect by payment medium. While credit card spending constituted 70 percent of the increase in secondary earners' consumption, roughly a quarter of the increase was financed with debit cards for which there was no change in convenience premium since secondary earners always had debit cards even prior to the reversal. Moreover, the fact that the reversal did not materially impact households' financial standing (see Section 4.3) suggests a limited role of self-control.

6 Quantitative Analysis

Given the relevance of the LC channel in reduced-form, I assess the quantitative importance of this channel by calibrating a model of household decision-making under limited commitment. I closely follow and build on Voena (2015) by incorporating key aspects of the TILA reversal. This section provides a high-level summary of the model and its key prediction. See Section E for detailed model set-up and solution method.

Model Overview The household consists of two spouses, primary and secondary earners (P and S). In each month t, spouses jointly decide how much to save, consume, whether to work and to divorce, by maximizing the weighted sum of their individual utilities, where the weights are their bargaining powers, θ_t^i. Thus, household decisions reflect the preferences of spouses according to their bargaining powers.

The key feature of this model is that a spouse's bargaining power can change over time, and this bargaining process depends on the spouses' outside options, $V_t^{i,D}$ – i.e., the value of being divorced. Specifically, in each period, the household decision-making problem consists of two stages. In the first stage, each spouse takes their existing bargaining power as given and computes their values of being divorced, $V_t^{i,D}$, and staying married, $V_t^{i,M}$. In the second stage,

spouses compare these values, and this can lead to three cases:

1. $V_t^{i,M} > V_t^{i,D} \quad \forall i$, both spouses prefer to stay married;
2. $V_t^{i,D} > V_t^{i,M} \quad \forall i$, both spouses prefer to divorce;
3. $V_t^{i,M} > V_t^{i,D}$ and $V_t^{i',M} \leq V_t^{i',D}$, $i \neq i'$, only one spouse prefers to stay married

In the first case, spouses stay married and solve the household value function, V_t^M, by computing the weighted sum of $V_t^{i,M}$. In the second case, spouses divorce and stick to their outside option, $V_t^{i,D}$. The third case triggers marital bargaining, and $\theta_t^{i'}$ adjusts just enough to make the spouse who prefers to divorce indifferent between divorcing and staying married. Specifically, spouses solve for V_t^M subject to i''s participation constraint in marriage, $V_t^{i',M} = V_t^{i',D}$, and the bargaining process, $\theta_t^{i'} = \theta_{t-1}^{i'} + \lambda_t^{i'}$, where $\lambda_t^{i'} > 0$ represents the shadow price of the participation constraint. In summary, when a spouse's outside option increases to the point of negotiation, bargaining power shifts towards that spouse to keep him/her in marriage.

The TILA Reversal I use the standard budget constraints, where spouses pool their financial resources if they're married, and they live off their own resource if divorced. To illustrate the quantitative importance of credit as a factor that increases secondary earners' outside options, I introduce spouse-specific borrowing limits, L_t^i, to the model. Specifically, spouses can borrow up to their combined credit limits if they're married, $L_t = L_t^P + L_t^S$, and they can each *keep their own limits in case of divorce*, just like in real life.

I then mimic the policy experiment by increasing secondary earners' borrowing limit from $\underline{L^S}$ to $\overline{L^S}$, while keeping the primary earners' limit constant $L_t^P = L^P$:

$$L_t = \begin{cases} L^P + \underline{L}^S, & \text{if } t < \text{TILA reversal} \\ L^P + \overline{L}^S, & \text{otherwise} \end{cases}$$

and track how the consumption allocation of the two spouses evolves in equilibrium. Therefore, whether the TILA reversal is quantitatively important for intra-household bargaining will depend on how much secondary earners' higher borrowing limits improve their outside options, $V_t^{S,D}$. Thus, the "portability" feature of credit limit is the key aspect of the reversal that makes individual borrowing capacity relevant for shaping marital bargaining power.

Key Model Prediction The key prediction of this model is that increasing a spouse's outside option leads to a shift in consumption allocation in his or her favor because higher outside option increases marital bargaining power to satisfy participation constraints. This prediction can be shown using the first-order-condition with respect to c_t^i:

$$\frac{u'\left(c_t^{P*}\right)}{u'\left(c_t^{S*}\right)} = \frac{\theta_t^S + \lambda_t^S}{\theta_t^P + \lambda_t^P} = \gamma_t \tag{4}$$

See Appendix Section F for derivation of this prediction. This condition shows that the ratio of marginal utilities of consumption has a one-to-one relationship to the relative bargaining power of the spouses, or the slope of the Pareto frontier (Kocherlakota, 1996). Thus, whenever secondary earner's Lagrangian multiplier associated with her participation constraint is positive, $\lambda_t^S > 0$, primary earner's marginal utility will be relatively higher than that of secondary earner. This implies an increase in secondary earners' consumption share.

6.1 Quantitative Results

Using the model presented above, I examine two questions – (1) by how much did secondary earners' relative bargaining power increase in the household? and (2) how much of the observed increase in secondary earners' consumption share can be accounted for by the LC channel?

Changes in the Bargaining Power I use a sufficient statistics approach to document the size of the change in secondary earners' relative bargaining power in the household as a result of the 2013 reversal. While the spouses' relative bargaining power is not observed in the data, Equation 4 shows that – under certain assumptions about the spouses' preferences – the relative bargaining power (the right-hand-side) can be characterized by observable elements of household behavior: spouse-specific consumption. I obtain average monthly consumption of primary and secondary earners in the treated group before and after the reversal to quantify the size of the change in secondary earners' relative bargaining power.

Figure 7 illustrates that the reversal led to an economically meaningful increase in secondary earners' relative bargaining power in the household. Assuming that both spouses have the CRRA utility with a relative risk aversion of $\gamma = 1.5$, Figure 7a shows that the slope of the Pareto frontier before the reversal was -0.78. After the reversal, Figure 7b shows that this slope

became steeper – i.e., the relative bargaining power tilted toward the secondary earner. The change in the relative bargaining power is 23 percentage points (ppt): 1.01 - 0.78. This change in bargaining power after the reversal is 5 times as large as the typical change in the bargaining power among card openers before the reversal. In addition, the average monthly change in the slope is close to 0 among a broader sample of households that includes secondary earners that did not open a credit card account. This illustrates that the relative bargaining power changes little over-time, but an increase in secondary earners' borrowing capacity generates an economically meaningful shift in their marital bargaining power.[26]

The Limited Commitment channel I use a calibration approach to quantify the extent to which the LC channel can explain the observed increase in secondary earners' consumption share in the data. I obtain parameters from existing literature, and where needed, directly from my dataset.[27] Table 9 reports the parameters used in this exercise. I set each period to be one month and track household consumption behavior for 36 months – 12 months before and 24 months after the reversal – to match the data. Table A.15 compares the outcomes generated by the model and observed in the data and shows that the model generates reasonable estimates of consumption and borrowing behavior.

Figure 8 shows that the LC channel can explain up to 37 percent of the observed increase in secondary earners' consumption share. The figure compares the path of secondary earners' consumption share observed in the data (blue) to that generated in the model (red and green). The model generated paths in Figures 8a and 8b use the first and third terciles of primary earners' credit limit, respectively. Based on the model, secondary earners' consumption share increased by 1.46 to 1.64 percent relative to the pre-reversal period, whereas the change corresponds to 4.38 percent in the data. This implies that the LC channel is quantitatively important, accounting for roughly 33-37% of the observed increase in the data. Comparing Figures 8a and 8b shows that the LC channel matters more for households with larger credit gaps and for those in which the reversal led secondary earners to have higher borrowing capacity than primary earners, consistent with spouses' relative financial power shaping how they allocate resources.

[26] To be clear, this does not imply that the spouses' *level of consumption* stays constant if secondary earners do not open a credit card account. It implies that the share of consumption does not change prior to the reversal.

[27] This exercise is intended to answer *how good of a job does a model used in prior literature explain my empirical results* rather than targeting empirical moments to replicate my reduced form estimates.

Overall, this exercise illustrates that the LC channel plays an important role, but other channels may also play a role since the LC channel only partially explains the total increase.

7 Conclusion

The provision of the Truth-in-Lending Act (TILA) concerning independent ability to pay was reversed in 2013 to facilitate access to credit for secondary earners and stay-at-home spouses who have limited income of their own but have access to household income. I exploit the fact that the 2013 reversal was superseded by state-level marital division-of-property laws in some states but not others to gain identification and leverage administrative financial-transaction data that measure credit and consumption of each spouse. This allows me to examine whether reducing disparities in credit between spouses reduces consumption disparities in the household.

My central finding is that the reversal – which increased secondary earners' credit limits by $1,506 after 24 months – reduced the pre-reversal consumption gap in the household by half. Consumption shifted toward secondary earners, whose private consumption crowded out primary earners' private consumption. Secondary earners increased demand for "public" consumption following the reversal, suggesting that primary earners indirectly benefited from the TILA reversal. A variety of household financial-solvency outcomes were not materially impacted. The limited-commitment channel, which posits that higher borrowing capacity strengthens marital bargaining power, appears to best explain the empirical patterns documented in this paper. Because financial policies can have an uneven impact on individual family members in the household, a key implication of this paper is that policies aimed at reducing financial disparities between spouses can reduce consumption inequality.

I highlight three caveats and corresponding directions in which my work can be extended. First, this paper examined relatively short-run effects of the TILA reversal. Therefore, whether consumption-reallocation and financial-solvency patterns persist in the long run is an open question. Second, this paper took a step toward constructing consumption measures of individual family members, but clearly more can be done to improve the measures' accuracy, as measurement of within-household economic outcomes is crucial for policy-making designed to alleviate poverty (Chiappori and Meghir, 2015). Finally, this paper analyzed the behavior of couples that represent the traditional family structure of a married man and woman. How-

ever, American family structures have changed dramatically over the last few decades, with the rise of same-sex marriage and co-habitation. Analysis of how trends in family structure are associated with within-household inequality would be a fruitful direction of research.

Appendix For Online Publication

A Related Literature

Existing studies in the intra-household literature examining consumption show that giving spouses with low initial bargaining power (typically women) more control over income (Schultz, 1990; Thomas, 1990; Browning et al., 1994; Duflo and Udry, 2004; Blundell, Chiappori and Meghir, 2007; Bobonis, 2009), cash transfers (Lundberg, Pollak and Wales, 1997; Attanasio and Lechene, 2014), savings accounts (Ashraf, Karlan and Yin, 2010), or better outside options in marriage markets (Angrist, 2002; Chiappori, Fortin and Lacroix, 2002) reduces their labor force participation and changes household consumption patterns in a way that better reflect preferences of the wives, with greater spending on education, housing, and nutrition for children. A related of set of studies that evaluate the effect of microcredit programs targeting women's financial independence find that improving women's financial control reduces household consumption of temptation goods and increases female labor force participation (Field et al., 2021), but has no discernible effect on women's empowerment (Banerjee et al., 2015). Other intra-household decisions beyond consumption have been examined, including effects on fertility (Doepke and Kindermann, 2019); charitable giving (Ahn and Ren, 2022), debt repayment (Bertaut, Haliassos and Reiter, 2009; Vihriälä, 2023), financial portfolio choice (Addoum, Kung and Morales, 2016; Ke, 2021; Gu, Peng and Zhang, 2022); learning (Ashraf, 2009; Conlon et al., 2021), and productive efficiency (Udry, 1996). See Lundberg and Pollak (2007) and Chiappori and Meghir (2015) for a comprehensive overview.

B The Reversal of the Truth-in-Lending Act in Practice

Two conditions must hold for the TILA reversal to provide a credible identification setting. First, the card issuer's treatment of treated and control states must be different prior to the reversal for using CP states as the control group to be valid. Second, the card issuer's compliance with the reversal must trigger a change in the income reporting behavior of treated secondary earners. Since card issuers use reported income to determine the amount of credit limit to extend to a card applicant, treated secondary earners should report higher income after the reversal to see a larger increase in credit limit relative to the control group.

I confirmed with JPMorgan Chase & Co. (JPMC) that the first condition holds. JPMC applied the independent ability-to-pay criteria to ED states only, thus validating the first condition of my identification strategy. Despite the fact that card issuers applied different income standards to treated and control states, card issuers did not change their marketing or solicitation strategy across the two types of states. Figure A.2a plots the year-over-year change in credit card solicitations in CP and ED states from all credit card issuers in the U.S. The figure shows that card issuers did not advertise or solicit cards in ED states more than they did in CP states. This result is consistent with the credit card industry's practice of using income to determine credit limits rather than to decide whether to issue or solicit credit.

Secondary earners' reported income on their credit card applications validates the second condition. Figure A.2b shows the average difference in reported monthly income between treated and control secondary earners before and after the reversal. Before the reversal (left bars), secondary earners in treated states reported $380 lower income on average relative to

those in control states. This difference entirely disappears with the TILA reversal (right bars), suggesting full compliance with the policy change. The difference in the reported income is even larger for single income households, corresponding to roughly $500, or 14 percent of median monthly household income. Overall, these two conditions suggest that the TILA reversal is likely to generate a differential increase in treated secondary earners' access to credit. Note that primary earners' income reporting behavior did not change (see Figure A.2c).

C Secondary Earners' Credit Card Robustness Analysis

The reversal did not differentially affect credit card opening or closing rates. Table A.6 shows that treated secondary earners were equally likely to open or close sole credit card accounts as the control group (Panel A), and similar results hold for joint credit card accounts, regardless of whether the account was held by primary or secondary earners (Panel B). In principle, the reversal could have generated differential card opening rates if treated secondary earners increased demand or banks offered more credit cards in treated states in anticipation of the reversal. Yet, I do not find any evidence of differential demand for credit or bank targeting (see Figure A.2a). This is consistent with (i) secondary earners not being aware of the reversal; and (ii) card issuers using income for deciding how much credit limit to extend to card applicants rather than whether to extend credit.[28]

Treated secondary earners received similar APR and were equally likely to use non-Chase credit cards as the control group. If the reversal differentially induced riskier individuals to open credit cards or changed non-Chase credit card use behavior such that they increased spending on cards I observe while reducing spending on cards I do not observe, the consumption effects can reflect sample selection rather than credit-induced consumption reallocation. However, Panel A of Table A.7 shows that the reversal did not lead to differential APR or credit card use behavior at other banks between the treated and the control group. However, treated secondary earners had lower utilization rates, consistent with receiving higher credit limits.

The event-study results are robust to parametrically controlling for the linear pre-trend. The blue shaded area in Figure 4a denotes the "phase-in" period in which the CFPB announced the policy change and allowed card issuers early compliance with the reversal before the law went into effect. Consistent with card issuers phasing into the new income standard regime, the figure shows a differential upward trend in credit limit for the treated group a few months before the reversal. I parametrically control for the linear pretend in event time to ensure that my results are not driven by the pre-existing trend during this phase-in period (Roth, 2020).[29] Figure A.11a illustrates that a linear pre-trend is a reasonable functional form assumption, and Figure A.11b shows that my results are robust to accounting for this differential pre-trend.

D Alternative Measurement, Sample, and Specification

Gender-Intensity (GI) Measure Table A.2 reports the breakdown of category-level spending shares by gender. These statistics are obtained out-of-sample using 2.4 million consumers who

[28] I confirmed with credit card industry practitioners that the reversal did not result in more credit card opening in treated states because income is not the primary metric for underwriting, but is used for determining credit limit.

[29] This approach has been used widely in event study settings where pre-existing trend may confound the treatment effect. See, for example, Wolfers (2006); Dobkin et al. (2018); Gross, Notowidigdo and Wang (2020).

are active users of both Chase checking and credit card accounts during my sample period. The share of spending is re-weighted to take the gender distribution into account, such that the spending share statistic is not driven by over-sampling of men vs. women in the data. Thus, more than 50% spending share refers to spending intensity, taking sampling distribution into account.

I use these out-of-sample statistics to construct the GI consumption measure that attributes total household spending to each spouse for which his/her gender-intensity spending share is greater than 55 percent. I choose 55 percent as the spending share threshold to improve the accuracy of categories that can be considered *gendered*. For example, both men (50.93%) and women (49.07%) have similar spending shares on public transportation, so treating total household spending on public transit as husband's consumption can be misleading. Thus, this approach assumes that one cannot infer consumption allocation from the excluded (i.e., not clearly gendered) categories. The GI measure improves the accuracy of spouse-specific consumption by only considering gendered goods, but only captures 54% (i.e., $1,374 vs. 2,534) of consumption in dollar terms relative to the spending-based measure. The next section discusses that my results are robust to using less (50%) and more (60%) conservative GI threshold.

I assess whether the spending-based consumption measure is a reasonable proxy for consumption by examining the relationship between the spending-based and gender-intensity measure. If "spender as the consumer" is a poor assumption, the two measures should not be positively correlated. Figure A.7 plots each spouse's average monthly consumption share using the spending-based measure against the gender-intensity version. The two measures are positively correlated for both spouses.

Alternative Consumption Measures I consider a battery of alternative consumption measures to test the sensitivity of my results. Panel A of Table A.8 reports monthly effects using alternative spending-based consumption measures and Panel B reports estimates using alternative GI measures. Column 1 of Panel A shows that my results hold up to using a consumption measure that includes credit card payments to other financial institutions. This mitigates the potential concern that secondary earners may have increased spending on financial accounts I observe while reducing spending on accounts I do not observe. Column 2 confirms that my results are robust to excluding potentially work-related expenses. This addresses the concern that consumption effects may be driven by the earner status in the household if, for example, primary earners are systematically more likely to consume than secondary earners. Columns 3 through 6 exclude private consumption categories that may be considered public consumption and show that my results are robust to excluding these categories.

Panel B of Table A.8 consider progressively more conservative definitions of the gender-intensity measure. Column 1 reconstructs consumption using a 50% GI threshold and shows that the estimated effects are similar to using the 55% threshold. Columns 2-4 exclude private consumption categories that may actually capture couples' joint consumption. Column 5 uses a more conservative 60% GI threshold. While this measure can precisely measure one's consumption, it reduces the representativeness, as this measure captures less than 25 percent of consumption in dollar terms relative to the spending-based measure. Finally, Column 6 only consider a narrow subset of gender-specific spending categories considered in prior studies, such as women's clothing or men's footwear, that provides a more precise proxy for consumption. While using more conservative measures reduce statistical power, these measures provide qualitatively similar estimates.

Specification robustness Table A.9 shows that my results are robust to using alternative specifications. Compared to Column 1, which reports my main monthly effects, Columns 2 through 4 show that my estimates are not sensitive to the choice of fixed effects. This illustrates that my empirical strategy is not subject to the "negative weighting" problem that can arise in staggered DiD settings.[30] Column 5 shows that my results are also robust to controlling state-specific time trends that take local economic trends into account. One might be concerned that consumption effects reflect households having a mismatch in the timing of income and expenditure commitments, since households may be more sensitive to credit access when they experience cash shortfalls. However, Column 6 shows that aggregating data into quarterly yields similar estimates, suggesting that the consumption effect is not driven by monthly variation in the timing of income.

Other Samples and Placebo Tests. The first three columns of Table A.10 show that my results robust to using alternative samples. Column 1 shows that my results are robust to restricting the sample to couples with joint accounts, which addresses the potential concern that my Regression Sample may include non-couple family units, such as siblings. Column 2 shows that my results also hold up to restricting the sample to couples with separate checking accounts, for which the measurement of earner status is more precise because income streams can be traced to each spouse cleanly. Column 3 shows that my results are generalizable to using a broader sample of households where secondary earners *had* credit card accounts at the beginning of my sample period. This addresses the concern that my results may not be generalizable because of sample selection. However, the estimated effects are smaller relative to using the Regression Sample, confirming that existing card holders were not affected by the reversal as much as new card openers because they rarely update income.

Placebo tests confirm that secondary earners' consumption shares did not change for couples that did not experience a reduction in the within-household credit gap. Column 4 shows that secondary earners' consumption share did not change for couples that received higher total household credit limit but whose within-household credit gap did not change (i.e., joint account limits increased but secondary earners' independent credit access did not change). This strengthens the interpretation that the consumption reallocation effect is driven by the reduction of the credit gap between spouses rather than capturing a general impact of households having more access to credit. Column 5 shows that secondary earners' consumption share did not change for households where secondary earners' credit limit did not change. Note that the placebo samples are distinct from the regression sample, as they are obtained from a broader sample of credit-card holding households where secondary earners do not necessarily open a credit card during my sample period. Column 6 similarly shows that there's no detectible DiD effect in the pre-period. Finally, Column 7 confirms that the TILA reversal did not affect primary earners' credit access on average. Table A.12 reports that marital commitment does not affect secondary earners' consumption share for households that do not experience any change in credit limit .

[30]Recent advances in econometric theory point to potential pitfalls associated with estimates from two-way fixed effects specifications in a staggered adoption DiD design (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; Athey and Imbens, 2021; Borusyak, Jaravel and Spiess, 2021; Goodman-Bacon, 2021) Since the empirical setting considered in this paper has simultaneous absorbing treatment in which treatment happens in a single date and the never-treated group, OLS estimation does not suffer from negative weights or under-identification problem (Borusyak, Jaravel and Spiess, 2021).

DiD Permutation Tests. My estimates may be influenced by the fact that ED and CP state sizes are highly unequal. Figure A.10 plots the distribution of placebo estimates for secondary earners' credit limits and consumption share where I randomly assign treatment status across CP and ED states 1,000 times. The placebo distribution is centered around 0 and are substantially smaller than the observed treatment effect I find.

E Household Decision-Making under Limited Commitment

E.1 Set-up

The household consists of two spouses, primary and secondary earners, indexed by $i \in (P, S)$, who live until T. In each month t, the spouses decide jointly how much to save, consume, and whether to work and divorce. The spouses have complete knowledge of all variables and preferences dated t and earlier and of probability distributions over all variables in $t' > t$.

Preferences Each spouse has preferences that are separable over time and across states, with diminishing marginal utility over consumption $u(c_t)$ and disutility, ψ, from labor market participation, P_t^i. Each spouse's period utilities take the form $u_t^{i,M} = \frac{c^{i\,1-\gamma}}{1-\gamma} - \psi P_t^i + \xi_t$ in marriage and $u_t^{i,D} = \frac{c^{i\,1-\gamma}}{1-\gamma} - \psi P_t^i$ in divorce, where ξ_t is a taste shock for marriage that follows a random walk, capturing the persistence in the taste for marriage such as the spouses' affection for one another: $\xi_t = \xi_{t-1} + \epsilon_t$ and $\epsilon_t \sim_{iid} N(0, \sigma_\xi^2)$. Primary earner always works ($P_t^P = 1$) and incurs disutility ψ, while secondary earner can choose to work. The spouses have identical discount factor, β, and beliefs.

In marriage, spouses benefit from economies of scale in consumption. Specifically, total household expenditure is given by a constant elasticity of substitution aggregator of primary and secondary earners' consumption: $x = [(c^P)^\rho + (c^S)^\rho]^{\frac{1}{\rho}} e(k)$. For $\rho \geq 1$, the couple gets more utility jointly from the same level of spending because there are gains from marriage.[31] The couple devotes a fraction, $e(k)$ denoting an equivalence scale, of total household expenditures on children. The economies of scale and the cost multiplier take into consideration the existence of goods that are public within the household.[32]

Income The income process, y_t^i, has two components – an endogenous component (h_t^i), and an exogenous component (z_t^i) that is correlated between spouses:

$$ln(y_t^i) = ln(h_t^i) + z_t^i \tag{5}$$

The income shock follows a random walk: $z_t^i = z_{t-1}^i + \zeta_t^i$, with $\zeta_t^i \sim_{iid} N(0, \sigma_{\zeta^i}^2)$. The law of motion for each spouse's human capital takes the functional form, $ln(h_t^i) = ln(h_{t-1}^i) - \delta \cdot (1 - P_{t-1}^i) + (\lambda_0^i + \lambda_1^i \cdot t) \cdot P_{t-1}^i$, such that human capital depreciates at a rate δ if a spouse does not work in the previous period or appreciates with tenure at a rate $\lambda_0^i + \lambda_1^i \cdot t$.

[31] The CES consumption aggregator is a standard assumption. See, for example, Boerma and Karabarbounis (2021) for home production model and Knowles (2013) for intra-household model.

[32] This is a short-hand way to allow for public consumption for married couples. I adopt this approach for tractability because determining the relative shares of public vs. private consumption is not the primary focus of the quantitative exercise. An alternative way to take public consumption into account is by allowing individual spouses to have different preferences over public and private consumption Mazzocco (2007).

Budget Constraints Saving (borrowing), a_t^i, earns (pays) the market rate, $\tilde{r} > 0$. The budget constraints in marriage and divorce are:

$$\begin{aligned} A_{t+1} - (1+\tilde{r})A_t &= Y_t - x_t && \text{if married} \\ a_{t+1}^i - (1+\tilde{r})a_t^i &= y_t^i \cdot P_t^i - c_t^i \cdot e(k) && \text{if divorced} \end{aligned}$$

where $A_t = \sum_{i=P}^{S} a_t^i$, $Y_t = \sum_{i=P}^{S} y_t^i \cdot P_t^i$, and x_t denote total household savings, income, and expenditure. While married, the couple allocates A_t between one another according to their respective bargaining power (θ_t^i) in each period because divorce is possible. Therefore, in the first period after divorce, each spouse enters t with $a_t^i = \theta_{t-1}^i A_{t-1}$. After divorce, spouses live off their individual financial resources and contribute to the consumption of their children as a fraction of their own consumption, according to $e(k)$. Spouses pay higher interest rate when they borrow, but earn lower rate when they save:

$$\tilde{r} = \begin{cases} \overline{r}, & \text{if } a_t^i < 0 \\ \underline{r}, & \text{otherwise} \end{cases}$$

Borrowing Limits The key feature of this model is that spouses have individual borrowing limits, L_t^i, that are determined exogenously and depend on the TILA regime. Specifically, the TILA regime is modeled to capture the stylized feature that secondary earners' borrowing limit is higher after the reversal.

$$A_{t+1} \geq -L_t \qquad \text{if married} \tag{6}$$

$$L_t = \begin{cases} L^P + \underline{L}^S, & \text{if } t < \text{TILA reversal} \\ L^P + \overline{L}^S, & \text{otherwise} \end{cases} \tag{7}$$

The sum of the two spouses credit limits represent the couples' total borrowing capacity, L_t. The borrowing constraint imposes limits on the couples' "net worth" (i.e., assets minus liabilities) and can be interpreted as maximum credit card debt that the couple can cumulate. In case of divorce, each spouse keeps individual borrowing limit, L_t^i:

$$a_{t+1}^i \geq -L_t^i \qquad \text{if divorced} \tag{8}$$

This "portability" feature of borrowing limit is what makes individual borrowing capacity relevant for shaping marital bargaining power.[33] In practice, because borrowing limit is an uncontested marital resource (unlike income, assets, or debt) that belongs to the spouse that holds a credit card account (i.e., primary account holder), borrowing capacity translates into higher outside options by relaxing budget constraint when divorced.

E.2 Decisions and Model Predictions

In each period, the household decision-making problem consists of two stages. In the first stage, each spouse computes the value of being divorced and the value of staying married based on

[33] In practice, divorcees can keep credit limits that they obtained during marriage as long as they are able to make minimum monthly payment. This is because card issuers do not treat divorce any differently than other life events that could trigger financial distress, such as job loss or illness.

the existing bargaining power without taking their participation constraints into account. In the second stage, each spouse compares the value of being divorced to that of staying married and decides whether to stay married, divorce, or negotiate. If couples negotiate, they compute the value of staying married conditional on the adjusted bargaining power. Thus, the optimal value function for each spouse is determined by comparing the value functions of being divorced and staying married, $V_t^i(\omega) = \max\Big\{V_t^{i,D}(\omega), V_t^{i,M}(\omega)\Big\}$.

Stage 1.a: The Value of Being Divorced To compute the value of being divorced, the problem is solved by backward induction using the terminal condition that each spouse consumes all of his/her assets ($a_{t+1}^i = 0$) given the set of state variables, $\omega_{\mathbf{T}}{}^D = \Big\{a_T^i, h_T^i, z_T^i, \Omega_T\Big\}$:

$$\begin{aligned} V_T^{i,D}(\omega_{\mathbf{T}}^{\mathbf{D}}) &= \max_{c_T^i, P_T^i} u(c_T^i) \\ &\text{s.t.} \\ (1+r)a_T^i &= y_T^i \cdot P_T^i - c_T^i \cdot e(k) \end{aligned}$$

In the remaining periods $t = 1, ..., T-1$,

$$\begin{aligned} V_t^{i,D}(\omega_{\mathbf{t}}^{\mathbf{D}}) = \max_{c_t^i, a_{t+1}^i, P_t^i} &\Big\{u(c_t^i) + \beta E\Big[V_{t+1}^{i,D}(\omega_{\mathbf{t+1}}^{\mathbf{D}} | \omega_{\mathbf{t}}^{\mathbf{D}})\Big]\Big\} \\ &\text{s.t.} \\ a_{t+1}^i - (1+r)a_t^i &= y_t^i \cdot P_t^i - c_t^i \cdot e(k) \\ a_{t+1}^i &\geq -L_t^i \end{aligned} \tag{9}$$

given state variables $\omega_{\mathbf{t}}{}^D = \Big\{a_t^i, h_t^i, z_t^i, \Omega_t\Big\}$. I assume that spouses do not remarry after divorce. Ω_t represents the vector of the TILA regime at time t.

Stage 1.b: The Value of Staying Married To compute the value of staying married, the couple first solves the household value function, V_t^M. Then the spouses compute their individual value of staying married, $V_t^{i,M}$ using the optimal choice of consumption, labor supply, and savings decisions from this household problem.

To compute the household value function, the couple chooses the control vector in the terminal period that maximizes the weighted sum of their individual utilities, where the weights are given by the Pareto weights θ_T^i (i.e., bargaining power):

$$\begin{aligned} V_T^M(\omega_{\mathbf{t}}^{\mathbf{M}}) = \max_{c_T^P, c_T^S, P_T^S} &\Big\{\theta_T^P u(c_T^P) + \theta_T^S u(c_T^S)\Big\} \\ &\text{s.t.} \\ (1+r)A_T &= Y_T - x_T \end{aligned}$$

where $\omega_{\mathbf{T}}^{\mathbf{M}} = \Big\{A_T, h_T^S, z_T^P, z_T^S, \theta_T^P, \theta_T^S, \xi_T, \Omega_T\Big\}$ and requiring $A_{T+1} = 0$. The state variables capture current assets, secondary earner's human capital, income shocks, bargaining power, taste for marriage shock, and the TILA regime.

In the remaining periods $t = 1, ..., T-1$, the couple solves:

$$V_t^M(\omega_{\mathbf{t}}^{\mathbf{M}}) = \max_{c_t^P, c_t^S, P_t^S, a_{t+1}^P, a_{t+1}^S} \left\{ \theta_t^P u(c_t^P) + \theta_t^S u(c_t^S) + \beta E[V_{t+1}^M(\omega_{\mathbf{t+1}}^{\mathbf{M}} | \omega_{\mathbf{t}}^{\mathbf{M}})] \right\}$$

$$\text{s.t.}$$

$$A_{t+1} - (1+r)A_t = Y_t - x_t$$

$$A_{t+1} \geq -L_t$$

given state variables $\omega_{\mathbf{t}}^{\mathbf{M}} = \left\{ A_t, h_t^S, z_t^P, z_t^S, \theta_t^P, \theta_t^S, \xi_t, \Omega_t \right\}$. The initial bargaining power of each spouse, θ_0^i is determined exogenously and can be considered a bargaining structure that spouses agreed on (but did not commit to) at the time of household formation. The values of the Pareto weights may reflect factors that influence the decision process–such as relative financial resource–that are known and predicted at $t = 0$ (Chiappori and Meghir, 2015); capture values that clear the marriage market (Choo, Seitz and Siow, 2008); or can result from noncooperative threat points (Lundberg and Pollak, 1993).

Spouses consume and save jointly when computing the household value function V_t^M, but they allocate consumption and savings between one another according to θ_t^i because divorce is possible. They use this individual consumption and saving, $c_t^{i,*}$ and $a_t^{i,*}$, to compute $V_t^{i,M}$. Then given a sequence of optimal solutions $\forall\ \omega^M$, $\{c_t^{i,*}(\omega^{\mathbf{M}}), P_t^{i,*}(\omega^{\mathbf{M}}), a_{t+1}^{i,*}(\omega^{\mathbf{M}})\}_{t=1}^T$, the value of staying married for each spouse:

$$V_t^{i,M}(\omega_{\mathbf{t}}) = u(c_t^{i,*}(\omega_{\mathbf{t}}^{\mathbf{M}}), P_t^{i,*}(\omega_{\mathbf{t}}^{\mathbf{M}}); \xi_t^i) + \beta E[V_{t+1}^{i,M}(\omega_{\mathbf{t+1}}^{\mathbf{M}})] \tag{10}$$

The married couple's optimal value function is the weighted sum of each spouses' value functions, where the weights are the bargaining power from $t-1$:

$$V_{t+1}^{M,*}(\omega_{\mathbf{t+1}}^{\mathbf{M}}) = \theta_t^P V_{t+1}^{P,*}(\omega_{\mathbf{t+1}}^{\mathbf{M}}) + \theta_t^S V_{t+1}^{S,*}(\omega_{\mathbf{t+1}}^{\mathbf{M}}) \tag{11}$$

Stage 2: The Divorce Choice Problem In the second stage, each spouse compares the value of being divorced ($V_t^{i,D}$) to the value of staying married ($V_t^{i,M}$). Three possible cases may arise:

1. The participation constraints are satisfied for both spouses, so it is optimal to stay married:

$$V_t^{i,M} > V_t^{i,D} \qquad \forall\ i \tag{12}$$

   In this case, spouse i's value function is $V_t^{i,M}$ is from the stage 1.b problem.

2. The participation constraints are binding for both spouses, so it is optimal to divorce:

$$V_t^{i,D} > V_t^{i,M} \qquad \forall\ i \tag{13}$$

   In this case, spouse i's value function is $V_t^{i,D}$ from the stage 1.a problem.

3. One spouse prefers to stay married but the other spouses' participation constraint binds. Suppose that only secondary earner's participation constraint binds so that it is optimal for primary earner to stay married but secondary earner prefers to divorce:

$$V_t^{P,M} > V_t^{P,D}$$
$$V_t^{S,M} \leq V_t^{S,D} \tag{14}$$

In this last case, the couple solves the stage 1.b. problem again *under the constraint that secondary earner's participation constraint is satisfied*. In the terminal period:

$$V_T^M(\omega_{\mathbf{T}}^{\mathbf{M}}) = \max_{c_T^P, c_T^S, P_T^S, \theta_T^S} \left\{ \theta_T^P u(c_T^P) + \theta_T^S u(c_T^S) \right\}$$

s.t.

$$(1+r)A_T = Y_T - x_T \quad \text{and} \quad A_{T+1} = 0$$

$$u(c_T^S) = V_T^{S,D} \tag{15}$$

$$\theta_T^S = \theta_{T-1}^S + \lambda_T^S \tag{16}$$

Equation 15 imposes secondary earner's value of staying married to be as good as the outside option. This constraint can be incorporated directly in the objective function using a standard Lagrangian multiplier method. Let λ_T^S denote the Lagrangian multiplier associated with secondary earner's participation constraint. Whenever the participation constraint binds (i.e., $\lambda_T^S > 0$), secondary earner's bargaining power increases by λ_T^S in order to make secondary earner indifferent between divorcing and staying married (Eq. 16).

In other periods:

$$V_t^M(\omega_{\mathbf{t}}^{\mathbf{M}}) = \max_{c_t^P, c_t^S, P_t^S, A_{t+1}, \theta_t^S} \left\{ \theta_t^P u(c_t^P) + \theta_t^S u(c_t^S) + \beta E[V_{t+1}^M(\omega_{\mathbf{t+1}}^{\mathbf{M}} | \omega_{\mathbf{t}}^{\mathbf{M}})] \right\}$$

s.t.

$$A_{t+1} - (1+r)A_t = Y_t - x_t \quad \text{and} \quad A_{t+1} \geq -L_t^S$$

$$u(c_t^S) + \beta E[V_{t+1}^{S,M}(\omega_{\mathbf{t+1}}^{\mathbf{M}} | \omega_{\mathbf{t}}^{\mathbf{M}})] = V_t^{S,D} \tag{17}$$

$$\theta_t^S = \theta_{t-1}^S + \lambda_t^S \tag{18}$$

Then given a sequence of optimal solutions to this constrained Pareto problem $\forall$ ω^M, $\{c_t^{i,**}(\omega^{\mathbf{M}}), P_t^{i,**}(\omega^{\mathbf{M}}), a_{t+1}^{i,**}(\omega^{\mathbf{M}}), \theta_t^{i,**}(\omega^{\mathbf{M}})\}_{t=1}^T$, each spouse's value function is:

$$V_t^{i,M}(\omega_{\mathbf{t}}) = u(c_t^{i,**}(\omega_{\mathbf{t}}^{\mathbf{M}}), P_t^{i,**}(\omega_{\mathbf{t}}^{\mathbf{M}}); \xi_t^i) + \beta E[V_{t+1}^{i,M}(\omega_{\mathbf{t+1}}^{\mathbf{M}})] \tag{19}$$

The couple repeats the two stage problem again if it enters period t as married. If spouses enter t as divorcees, they solve the first stage problem for the remaining period using assets that they divided according to θ_t^i in the previous period.

Note that threat of divorce triggers a renegotiation that modifies the consumption allocation plans of the married couple – that is, in the last case, the optimal consumption allocation is such that the new plan is as good as each spouse's outside option. In equilibrium, divorce occurs when the joint surplus–the sum of the two spouses' marriage surpluses–is negative.[34]

[34]Divorce does not require negative surplus for both spouses because divorce can happen even when one of the spouses want to stay married but there is not enough resource to transfer to the other spouse that would make the other spouse indifferent between staying married and being divorced.

F The Limited Commitment Model Prediction

Consider the properties of an efficient self-enforcing consumption maths when spouses' participation constraints bind but they stay married:

$$V_t^{P,M}(\omega_{\mathbf{t}}) = u\Big(c_t^{P*}(\omega_{\mathbf{t}})\Big) + \beta E\Big[V_{t+1}^{P,M}(\omega_{\mathbf{t+1}}|\omega_{\mathbf{t}})\Big] \tag{20}$$

$$V_t^{S,M}(\omega_{\mathbf{t}}) = u\Big(c_t^{S*}(\omega_{\mathbf{t}})\Big) + \beta E\Big[V_{t+1}^{S,M}(\omega_{\mathbf{t+1}}|\omega_{\mathbf{t}})\Big] \tag{21}$$

$$V_t^{P,M}(\omega_{\mathbf{t}}) \geq V_t^{P,D}(\omega_{\mathbf{t}}) \tag{22}$$

$$V_t^{S,M}(\omega_{\mathbf{t}}) \geq V_t^{S,D}(\omega_{\mathbf{t}}) \tag{23}$$

This problem can be reformulated as a Lagrangian problem. The couple solves:

$$\begin{aligned}\mathcal{L}^{*,M} =& \theta_t^P u\Big(c_t^{P*}(\omega_{\mathbf{t}})\Big) + \theta_t^S u\Big(c_t^{S*}(\omega_{\mathbf{t}})\Big) + \beta E\Big[V_{t+1}^{M}(\omega_{\mathbf{t+1}}|\omega_{\mathbf{t}})\Big] \\ &+ \lambda_t^P\Big\{u\Big(c_t^{P*}(\omega_{\mathbf{t}})\Big) + \beta E\Big[V_{t+1}^{P,M}(\omega_{\mathbf{t+1}}|\omega_{\mathbf{t}})\Big] - V_t^{P,D}(\omega_{\mathbf{t}})\Big\} \\ &+ \lambda_t^S\Big\{u\Big(c_t^{S*}(\omega_{\mathbf{t}})\Big) + \beta E\Big[V_{t+1}^{S,M}(\omega_{\mathbf{t+1}}|\omega_{\mathbf{t}})\Big] - V_t^{S,D}(\omega_{\mathbf{t}})\Big\}\end{aligned}$$

where $V_{t+1}^{M}(\omega_{\mathbf{t+1}}) = \theta_{t+1}^P V_{t+1}^{P,M}(\omega_{t+1}) + \theta_{t+1}^S V_{t+1}^{S,M}(\omega_{t+1})$, and λ_t^P and λ_t^S are the Lagrangian multiplier associated with each spouse's sequential participation constraint.

Combining the first order condition with respect to c_t^{P*} and c_t^{S*} leads to the key prediction of this model that the ratio of the marginal utilities of consumption has one-to-one relationship to the slope of the Pareto frontier (γ_t):

$$\frac{u'\Big(c_t^{P*}\Big)}{u'\Big(c_t^{S*}\Big)} = \frac{\theta_t^S + \lambda_t^S}{\theta_t^P + \lambda_t^P} = \gamma_t$$

In other words, the couple's consumption allocation in the household is determined by the slope of the Pareto frontier, which can be entirely characterized by the spouses' bargaining power. Figure A.14 illustrates the economic intuition graphically. Each panel plots the primary earner's expected lifetime value of staying married (y-axis) against the secondary earner's expected lifetime utility of staying married (x-axis). The red dashed lines denote the spouse's outside option, and the first quadrant of ellipse represents the Pareto frontier. Any consumption allocation along this Pareto frontier is a feasible allocation, but the position on this Pareto frontier (red dot) is determined by the ratio of the marginal utilities of consumption.

I discuss two cases: when the participation constraint does and does not bind. First, consider the case when the secondary earner's participation constraint does not bind. This case is illustrated in panel a by the fact that the existing resource allocation in period 1, $E[V_1^{i,*}]$, sits in the non-negative orthant created by spouses' best outside options. In this case, the improvement in secondary earner's outside option expands the Pareto frontier and shifts the location of efficient resource allocation outward. However, since secondary earner's participation constraint does not bind, the couple continues the initial resource allocation plan. This is shown in panel b– the slope of the Pareto frontier is unchanged in period 2. Note that the value of the spouses' best outside options intersect in the interior of the Pareto frontier, implying that there is still gains from marriage even after the change in secondary earner's outside option.

Now, consider the case when the secondary earner's participation constraint binds such that the value of her outside option expands to the point where the initial resource allocation

plan no longer sits in the non-negative orthant created by spouses' best outside options. The binding constraint triggers bargaining between spouses and increases the secondary earner's decision power by λ^S. This is shown in panel c. The improvement in the secondary earner's decision power makes the slope of the Pareto frontier steeper by tilting resource allocation toward her and thus reducing her marginal utility, $u'(c_2^{S,\omega})$. Figure d shows that this moves the location of resource allocation plan along the Pareto frontier to the new point, $E[V_2^{i,**}]$, where the secondary earner is indifferent from staying married with the new allocation plan or divorcing to take her outside option.

Comparing the ratio of marginal utilities in the case when secondary earner's outside option does not bind, $\frac{u'(c_2^{p,\omega})}{u'(c_2^{s,\omega})} = \frac{\theta^S}{\theta_P} = \widehat{\gamma}_t$, to the case when it does bind, $\frac{u'(c_2^{p,\omega})}{u'(c_2^{s,\omega})} = \frac{\theta^S+\lambda^S}{\theta_P} = \widetilde{\gamma}_t$, reveals how bargaining power determine the consumption allocation in the household. If $\lambda^S > 0$, then $\widetilde{\gamma}_t > \widehat{\gamma}_t$. This is only possible when consumption allocated to secondary earners increases.

G Welfare

How large are the welfare gains from the reversal? I calculate the Consumption Equivalent Variation (CEV), or the percent of expected lifetime consumption that a spouse inhabiting economy without the reversal would pay *ex ante* in order to inhabit economy with the reversal. In this model, since I track household consumption behavior for only 36 months around the reversal, the CEV captures the percent of expected consumption over this 3 year period. I consider two economies, $k = \{1, 2\}$, where $k = 1$ refers to the regime without the reversal and $k = 2$ refers to the regime with the reversal. I define ex-ante welfare in economy k derived from steady state consumption and work decisions $\{c_t^{i,k}(\omega), P_t^{i,k}(\omega)\}_{t=1}^T$ over states $\omega_t = \{a_t^{i,k}, h_t^{i,k}, z_t^{i,k}, \theta_t^{i,k}, \xi_t^k, \Omega_t^k\}$ distributed with $\lambda_t^i(\omega)$ as:

$$S^{i,k} = U(c^{i,k}; \xi^k) - V(P^{i,k}; \xi^k) \tag{24}$$

where ex ante utility over allocations and disutility from working for each of the two spouses,

$$U(c^{i,k}; \xi^k) \equiv \int E_0\Big[\sum_{t=1}^{T} \beta^{t-1} u(c_t^{i,k}; \xi_t^k)\Big] d\lambda^k \tag{25}$$

$$V(P^{i,k}; \xi^k) \equiv \int E_0\Big[\sum_{t=1}^{T} \beta^{t-1} (\psi P_t^{i,k}; \xi_t^k)\Big] d\lambda^k \tag{26}$$

Then the CEV, denoted by Δ_{CEV}, is:

$$S^i\Big((1 + \Delta_{CEV}^i)c^{i,1}, P^{i,1}\Big) = S(c^{i,2}, P^{i,2}) \tag{27}$$

which can be expressed as

$$(1 + \Delta_{CEV}^i)^{1-\gamma} U(c^{i,1}) - V(P^{i,1}) = U(c^{i,2}) - V(P^{i,2}) \tag{28}$$

or rewritten,

$$1 + \Delta_{CEV}^i = \Big[\frac{U(c^{i,2})}{U(c^{i,1})} + \Big(\frac{V(P^{i,1})}{V(P^{i,2})} - 1\Big) \cdot \frac{V(P^{i,2})}{U(c^{i,1})}\Big]^{\frac{1}{1-\gamma}} \tag{29}$$

Δ^i_{CEV} captures spouse i's percent of expected 3-year period consumption that i would be willing to pay ex ante to inhibit an economy with the reversal instead of an economy without the reversal. I similarly calculate the household CEV by defining household ex ante social welfare criterion as the sum of the two spouses ex ante utility over allocations:

$$S^k = U(c^k; \xi^k) - V(P^k; \xi^k) \tag{30}$$

$$U(c^k; \xi^k) \equiv \int E_0\Big[\sum_{t=1}^{T} \beta^{t-1} u(c_t^{i,P}; \xi_t^k)\Big] d\lambda^k + \int E_0\Big[\sum_{t=1}^{T} \beta^{t-1} u(c_t^{S,k}; \xi_t^{S,k})\Big] d\lambda^k \tag{31}$$

$$V(P^k; \xi^k) \equiv \int E_0\Big[\sum_{t=1}^{T} \beta^{t-1} (\psi P_t^{P,k}; \xi_t^k)\Big] d\lambda^k + \int E_0\Big[\sum_{t=1}^{T} \beta^{t-1} (\psi P_t^{S,k}; \xi_t^{S,k})\Big] d\lambda^k \tag{32}$$

Table A.16 shows that the TILA reversal is Pareto improving for secondary earners as they are willing to pay a positive share of their expected consumption to inhabit economy with the reversal. However, primary earners' CEV is negative, consistent with the reversal primarily benefitting secondary earners. The well-being of the couple as a whole also increases in that the couple is willing to pay 1.5 percent of their expected consumption to inhabit economy with the reversal. Overall, this result indicates that increasing secondary earners' borrowing capacity improves the well-being of the couple as a whole but has unequal impact on individual family members' well-being.

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Table 1. Covariate Balancing between Control and Treated Households (Pre-Reversal Characteristics)

	Raw			Matched		
	Control Mean (1)	Treated Mean (2)	Treat - Control (3)	Control Mean (4)	Treated Mean (5)	Treat - Control (6)
Age gap	1.58	1.52	-0.06	1.58	1.59	0.01
Wife's age	40.28	39.72	-0.56	40.27	40.30	0.03
Husband's age	41.86	41.24	-0.62	41.85	41.89	0.04
Wife is a Secondary Earner	0.61	0.61	-0.01	0.61	0.61	0.00
Debt-to-Income	0.36	0.39	0.03	0.36	0.37	0.02
Household Income ($)	9,036	9,418	382	9,007	9,028	21
Cash on hand ($)	7,020	7,561	541	6,977	7,102	126
Has a credit card	0.74	0.79	0.05	0.74	0.74	0.00
Total credit limit ($)	9,218	10,695	1,478	9,178	9,312	134
Total card balance ($)	2,573	2,832	260	2,569	2,582	13
Number of Households	33,136	47,998	14,862	33,100	33,100	0

Notes: This table reports average pre-reversal characteristics for treated and control households before and after the propensity score matching (PSM) procedure described in Section 2.2. The covariates were selected based on the characteristics card issuer might consider in their underwriting process and potentially affect the dynamics of credit card opening. Treated households are those that reside in equitable distribution (ED) states and control households are those that reside in community property (CP) states. The first three columns report average characteristics prior to matching and the last three columns report those for the matched sample. Age variables are reported in years. All other variables are monthly. Debt-to-Income reports total monthly debt payments (e.g., auto, credit card, mortgage, student, and other) to household income. Household income is the sum of labor income (payroll direct deposits), government transfers, business, and gig income. Cash on hand reports the month-end checking account balances at the household-level. Has a credit card is an indicator for at least one member in a household having a credit card account at JPMC. Total credit limit reports the sum of all credit card limits available at the household-level (joint credit card limits are counted only once). Total card balance refers to the end-of-billing-cycle credit card balance.

Table 2. Pre-TILA Reversal Descriptive Statistics (Matched Sample)

	Mean	SD	p25	p50	p75
	(1)	(2)	(3)	(4)	(5)
A. Household-level Characteristics					
Age gap	1.58	3.93	0.00	1.00	4.00
Wife's age	40.29	11.08	31.00	38.00	49.00
Husband's age	41.87	11.13	32.00	40.00	51.00
Consumption ($)	6,005	8,825	3,035	4,637	6,889
Income ($)	9,017	14,043	4,622	6,762	9,991
Cash on hand ($)	7,040	30,838	1,185	2,787	6,304
Has a credit card	0.74	0.44	0.00	1.00	1.00
B. Intra-Household Characteristics					
	Secondary Earner		Primary Earner		Mean
	Mean	SD	Mean	SD	Difference
Female	0.61	–	0.39	–	-0.22
Age	40.8	11.1	41.3	11.1	0.5
Income ($)	1,008	4,507	8,009	13,297	7,001
Cash on hand ($)	1,008	7,079	6,032	29,610	5,024
Consumption share	0.44	0.17	0.56	0.17	0.13
Consumption ($)	2,619	4,400	3,386	5,232	767
Public consumtion ($)	867	960	1,158	1,281	291
Private consumption ($)	1,752	4,139	2,228	4,842	476
Has a sole credit card	0.02	0.15	0.55	0.50	0.53
Credit limit ($)	158	1,453	6,041	9,032	5,883
Card balance ($)	36	406	1,768	3,851	1,732
Number of Households	66,200	66,200	66,200	66,200	66,200

Notes: This table reports summary statistics for the matched sample. Panel A reports household-level and Panel B reports within-household characteristics. Age variables are reported in years. All other variables are monthly. Consumption is defined as the sum of spending on financial accounts (debit, credit card, and checking, including cash withdrawals and electronic transfers). See Section 3.2 for details on how spouse-level consumption is constructed. Consumption share refers to each spouse's spending as a share of total household spending. Public consumption denotes spending on goods that are consumed jointly by the household (e.g., childcare) and private consumption denotes spending on goods that are consumed individually (e.g., clothing). See Table A.1 for detailed spending categories in each type. Income is defined as the sum of labor income (payroll direct deposits), government transfers, business, and gig income. Cash on hand refers to the end-of-month checking account balance. "Has a credit card" is an indicator for whether a household has at least one credit card account. Spouse-level credit limit reports limits on each spouse's sole credit card account (coded as 0 if a spouse does not have a sole account), and credit card balance refers to the end-of-billing balance.

Table 3. Effect of the TILA Reversal on Secondary Earner Credit Limits

Secondary Earner Outcomes	Monthly effect (1)		Cumulative effects: 6-month effect (2)	12-month effect (3)		18-month effect (4)		24-month effect (5)		Implied effect (6)
Sole Credit Limit	37.53	***	6.65	28.59	***	41.85	***	55.88	***	1,416
	(1.98)		(5.68)	(5.56)		(5.66)		(5.96)		[898]
Total Credit Limit	39.83	***	6.63	30.64	***	44.91	***	59.43	***	1,506
	(2.15)		(6.16)	(6.02)		(6.12)		(6.46)		[1,595]
Number of Observations	443,374		210,258	280,344		350,362		420,098		

Notes: This table presents the coefficient of a "treat $\times$ post" indicator in a difference-in-differences regression described in Section 2.2 using the Regression Sample. The outcomes are secondary earners' sole and total credit card limits, scaled by their average monthly pre-reversal consumption. Total credit limit is the sum of limits on sole and joint credit card accounts. Column 1 reports pooled regression estimates from Equation 1. Columns 2-5 report cumulative effects over different horizons, calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Column 6 reports implied cumulative effects in dollar terms, computed as $\Phi_{24}\times$ pre-reversal average monthly consumption. Since the data contains 25 months after the reversal, an extra month of data is used for Column 1 relative to Column 5. Pre-reversal average of the outcome variable is reported in brackets. Reported coefficients are multiplied by 100 for readability. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4. Effect of the TILA Reversal on Secondary Earners' Consumption and Within-Household Consumption Gap

Secondary Earner's Outcomes	Monthly effect (1)		Cumulative 6-month effect (2)		12-month effect (3)		18-month effect (4)		24-month effect (5)		Implied effect (6)
					A. Spending-Based Measure						
Consumption	2.89	***	-1.07		14.91	***	29.53	***	66.48	***	1,685
	(0.44)		(3.45)		(5.64)		(8.04)		(10.67)		[2,534]
Consumption Share	0.49	***	0.67		2.99	***	6.85	***	11.4	***	0.053
	(0.07)		(0.62)		(0.99)		(1.38)		(1.79)		[0.46]
Consumption Gap	-2.32	***	-7.32	*	-18.04	***	-32.14	***	-49.16	***	-0.04
	(0.43)		(3.76)		(5.92)		(8.15)		(10.53)		[0.08]
Number of Observations	443,374		210,258		280,344		350,362		420,098		
					B. Gender-Intensity Measure						
Consumption	3.05	***	-4.70		5.22		16.54	*	59.75	***	821
	(0.53)		(4.14)		(6.78)		(9.7)		(12.86)		[1,374]
Consumption Share	0.47	***	2.54	**	3.38	*	8.18	***	11.75	***	0.055
	(0.13)		(1.11)		(1.77)		(2.44)		(3.13)		[0.46]
Consumption Gap	-1.52	***	-9.49	**	-13.11	**	-27.11	***	-39.41	***	-0.03
	(0.44)		(3.69)		(5.87)		(8.12)		(10.4)		[0.07]
Number of Observations	435,071		217,778		286,550		355,219		423,609		

Notes: This table presents the coefficient of a "treat $\times$ post" indicator in a difference-in-differences regression described in Section 2.2 using the Regression Sample. The outcomes are secondary earners' consumption, consumption shares, and consumption gap in the household, scaled by their pre-reversal monthly mean. Panel A uses the spending-based consumption measure and Panel B uses the gender-intensity based measure. Section 3.2 describes details on the measurement. Column 1 reports pooled regression estimates from Equation 1. Columns 2-5 report cumulative effects over different horizons, calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Column 6 reports implied cumulative effects, computed as $\Phi_{24}\times$ pre-reversal average monthly mean of the outcome variable. Since the data contains 25 months after the reversal, an extra month of data is used for Column 1 relative to Column 5. Pre-reversal average of the outcome variable is reported in brackets. Reported coefficients are multiplied by 100 for readability. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5. Effect of the TILA Reversal on Household Outcomes

Household Outcomes	Monthly effect (1)		Cumulative 6-month effect (2)	12-month effect (3)		18-month effect (4)		24-month effect (5)		Implied effect (6)
	A. Credit and Consumption									
Credit Limit	16.2	***	2.04	10.19	***	16.88	***	27.78	***	1,523
	(1.39)		(2.95)	(3.4)		(4.33)		(5.48)		[4,916]
Consumption	0.871	***	-1.5	6.4		3.27		17.38	**	953
	(0.32)		(2.56)	(4.19)		(5.99)		(7.89)		[5,483]
	B. Household Budget									
Total Income	1.09	**	6.79	-0.62		14.00		17.24		946
	(0.55)		(4.68)	(7.48)		(10.69)		(13.8)		[8,343]
Cash on hand	1.44	**	2.51	-1.58		4.01	**	-1.34		-73.7
	(0.58)		(1.69)	(1.72)		(1.96)		(2.07)		[5,715]
Credit card debt	0.550	*	-1.15	2.14	**	0.91		0.249		13.6
	(0.3)		(0.82)	(0.91)		(1.04)		(1.32)		[1,154]
Number of Observations	443,412		210,276	280,368		350,392		420,134		

Notes: This table presents the coefficient of a "treat $\times$ post" indicator in a difference-in-differences regression described in Section 2.2 using the Regression Sample. Panel A reports estimates for household credit limit and consumption. Panel B reports estimates for household income, cash on hand, and interest-accruing revolving debt. All other outcomes are scaled by the average monthly pre-reversal household consumption. Column 1 reports pooled regression estimates from Equation 1. Columns 2-5 report cumulative effects over different horizons, calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Column 6 reports implied cumulative effects, computed as $\Phi_{24}\times$ pre-reversal average monthly household consumption. An extra month of data is used for Column 1 relative to Column 5. Pre-reversal average of the outcome variable is reported in brackets. Reported coefficients are multiplied by 100 for readability. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6. Effect of the TILA Reversal on Household Financial Outcomes

Household Outcomes	Monthly effect (1)		Cumulative (percentage points) 6-month effect (2)	12-month effect (3)		18-month effect (4)		24-month effect (5)		Implied effect (6)
Overdraft	0.03		-0.20	0.37		0.57		0.72		0.00
	(0.03)		(0.26)	(0.41)		(0.55)		(0.69)		[.345]
Delinquency	0.06		-0.12	0.10		0.07		1.22		0.00
	(0.05)		(0.31)	(0.55)		(0.86)		(1.22)		[.319]
High-interest Loans	-0.06		-0.09	-0.94		-1.24		-1.36		0.00
	(0.05)		(0.43)	(0.67)		(0.93)		(1.21)		[1.183]
Debt Settlement	0.21	***	-0.15	0.96	***	2.64	***	4.86	***	0.01
	(0.02)		(0.19)	(0.3)		(0.41)		(0.56)		[.488]
Debt Prioritization	0.32	*	0.25	2.43		5.36	*	7.77	*	0.02
	(0.18)		(1.13)	(1.93)		(2.91)		(4.18)		[80.56]
Number of Observations	443,412		210,276	280,368		350,392		420,134		

Notes: This table presents the coefficient of a "treat $\times$ post" indicator in a difference-in-differences regression described in Section 2.2 using the Regression Sample. Dependent variables include indicators for whether a household incurs overdraft fees (overdraft); falls more than 30-day behind on making required credit card payment (delinquency); makes any payments to a payday or subprime personal loan lender (high-interest loans); settle/restructure existing debt by making payments to debt settlement companies; or optimally pay debt in a way that it pays down more expensive debt first while borrowing more using cards carrying lower-interest. Debt prioritization analysis is limited to households with at least two credit card accounts. 55% of households have multiple credit cards. Column 1 reports pooled regression estimates from Equation 1. Columns 2-5 report cumulative effects over different horizons, calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Column 6 reports implied cumulative effects, computed as $\Phi_{24}\times$ pre-reversal average of outcome variables. An extra month of data is used for Column 1 relative to Column 5. Pre-reversal average of the outcome variable is reported in brackets. Reported coefficients are multiplied by 100 for readability. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7. Private vs. Public Consumption

			Cumulative								
	Monthly effect (1)		6-month effect (2)		12-month effect (3)		18-month effect (4)		24-month effect (5)		Implied effect (6)
	A. Secondary Earner Outcomes										
Private Consumption	1.42	***	-5.75	**	3.85		8.13		32.42	***	821
	(0.37)		(2.9)		(4.78)		(6.81)		(9.01)		[1,688]
Public Consumption	1.48	***	4.68	***	11.06	***	21.4	***	34.06	***	863
	(0.2)		(1.46)		(2.46)		(3.62)		(4.94)		[846]
	B. Household Outcomes										
Private Consumption	0.28		-4.69	**	1.1		-4.87		4.48		245
	(0.28)		(2.24)		(3.67)		(5.22)		(6.88)		[3,654]
Public Consumption	0.59	***	3.19	***	5.3	***	8.14	***	12.9	***	707
	(0.14)		(1.08)		(1.78)		(2.59)		(3.47)		[1,829]
Number of Observations	443,374		210,258		280,344		350,362		420,098		

Notes: This table presents the coefficient of a "treat $\times$ post" indicator in a difference-in-differences regression described in Section 2.2 using the Regression Sample. Outcomes are scaled by secondary earner (Panel A) or household average monthly pre-reversal consumption (Panel B). Spending-based consumption measures are used. Table A.11 replicates this table using gender-intensity consumption measures. Public consumption refers to spending on goods and services that are consumed jointly by the household, such as childcare. Private consumption refers to spending on goods and services that are consumed privately, such as clothing. Table A.1 reports categorization details. Column 1 reports pooled regression estimates from Equation 1. Columns 2-5 report cumulative effects over different horizons, calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Column 6 reports implied cumulative effects, computed as $\Phi_{24}\times$ pre-reversal average monthly consumption. An extra month of data is used for Column 1 relative to Column 5. Pre-reversal average of the outcome variable is reported in brackets. See Figure 5 for a figure version of this table. Reported coefficients are multiplied by 100 for readability. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8. Economic Mechanism

	No Controls						Controls					
	Trouble (1)		Mortgage (2)		Child (3)		Trouble (4)		Mortgage (5)		Child (6)	
	A. Secondary Earners' Credit Limit											
Treat x Post	39.41	***	40.36	***	39.66	***	39.28	***	40.23	***	39.52	***
	(3.25)		(3.32)		(3.84)		(3.17)		(3.24)		(3.75)	
Treat x Post x LC	1.724		-2.247		0.829		1.732		-1.895		0.850	
	(3.23)		(4.11)		(3.25)		(3.15)		(4.01)		(3.17)	
	B. Secondary Earners' Consumption Share											
Treat x Post	0.437	***	0.557	***	0.635	***	0.435	***	0.555	***	0.632	***
	(0.08)		(0.09)		(0.1)		(0.08)		(0.09)		(0.1)	
Treat x Post x LC	0.374	***	-0.268	**	-0.241	***	0.375	***	-0.256	**	-0.240	***
	(0.08)		(0.11)		(0.08)		(0.08)		(0.11)		(0.08)	
Number of Observations	443,374		443,374		443,374		443,374		443,374		443,374	

Notes: This table reports monthly estimates β and γ from the following specification,

$$Y^i_{h,t} = \alpha + \gamma_t + \beta \mathbf{1}[Treat \times Post]_{h,t} + LC_h + \mathbf{1}[Treat \times LC]_h + \mathbf{1}[Post \times LC]_{h,t} + \gamma \mathbf{1}[Treat \times Post \times LC]_{h,t} + \mathbf{X}_h + \epsilon_{h,t}$$

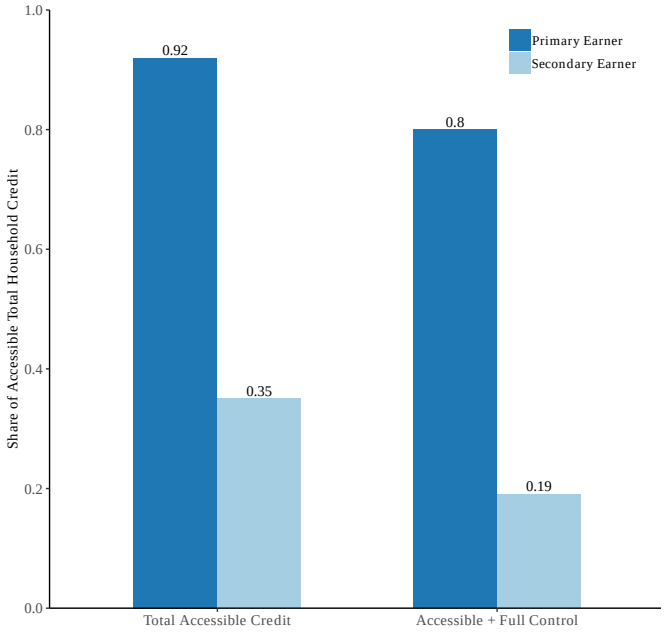
where β captures the average monthly effect of the TILA reversal for the treated group relative to the control group and γ captures the differential effect for couples with stronger or weaker proxies of limited marital commitment (LC). Columns 1-3 report estimates with no controls and Columns 4-6 report estimates with pre-reversal household covariates ($\mathbf{X}_h$). The baseline controls are quartile bins of labor income, consumption share, checking account balances, age gap between spouses, and their interactions with $\mathbf{1}[Treat]$. Each column uses a different proxy for LC. Columns 1 and 4 infer whether a couple is in a troubled marriage based on spending on counseling, such as couple counseling, or dating services; Columns 2 and 5 infer whether a couple has stronger marital commitment based on asset ownership – i.e., mortgage payments (Lafortune and Low, 2017); and Columns 3 and 6 based on whether a couple has a child –i.e., spending on children's clothing or child care. The LC proxies are based on pre-reversal spending and standardized, such that γ can be interpreted as the differential impact per standard deviation increase in a given LC proxy. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9. Parameters of the Model

Parameter	Value	Reference
Relative risk aversion (γ)	1.5	Attanasio et al (2008)
Discount factor (β)	0.989	Ganong and Noel (2019)
Rate of return on assets ($\underline{r}$)	0.0017	Bayot and Voena (2014)
Cost of borrowing ($\overline{r}$)	0.0073	Data
Economies of scale in couple(ρ)	1.4	Voena (2015)
Economies of scale for children ($e(k)$)	1.4	Voena (2015)
Disutility from labor market participation (ψ)	0.012	match BLS LFP rate
Standard deviation of preference shocks (σ_ξ)	0.05	match CDC divorce rate
Gains from experience (λ_0, λ_1)	$0.0025, -0.00003$	Attanasio et al (2008)
Depreciation rate (δ)	0.08	Voena (2015)
Standard deviation of PE's permanent shock (σ_{ζ^P})	0.05	match income path
Standard deviation of SE's permanent shock (σ_{ζ^S})	0.05	match income path
Wage covariance of PE and SE ($\sigma_{\zeta^P\zeta^S}$)	0.014	match income path
Primary earners' credit limit (L^P_{p33}, L^P_{p66})	$0, 2, 000$	Data
Secondary earners' credit limit ($\underline{L}^S$; $\overline{L}^S$)	$[895; 5, 336]$	Data

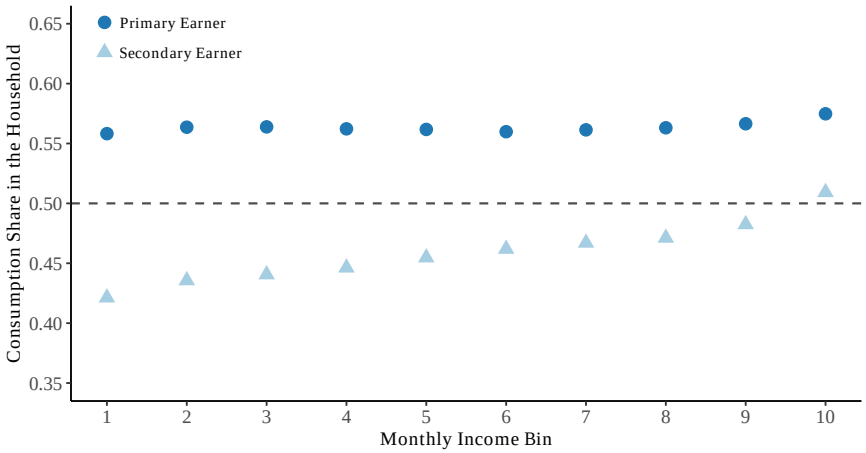
Notes: This table reports parameters used in the dynamic model presented in Section 6. Parameters have been converted to monthly where applicable.

Figure 1. Share of Accessible Credit in the Household



Notes: This figure shows the average monthly share of total household credit that each spouse can access during my sample period. "Total Accessible Credit" shows the average monthly credit limit that each spouse can access either as a primary account holder or as an authorized user as a share of total credit limit available at the household level. For example, the first light blue bar shows that secondary earners can access 35% of total household credit limit. "Accessible + Full Control" shows the average monthly credit limit on accounts held by each spouse as a share of total household credit limit. For example, the second light blue bar shows that secondary earners, on average, have full control over 19% of total household credit limit.

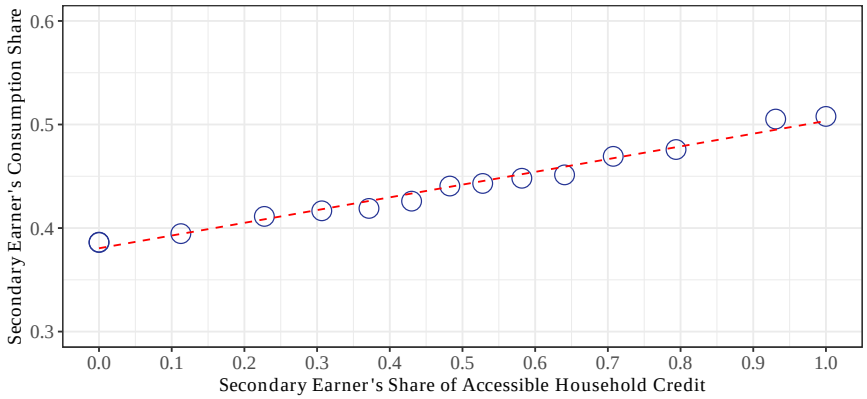
Figure 2. Consumption Shares Across the Income Distribution



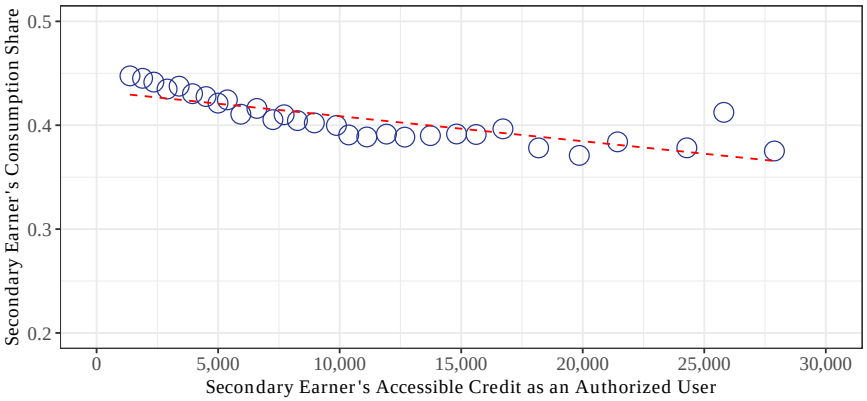
Notes: This figure shows the average monthly consumption share of individuals across the income distribution by their earner status in the household. For example, the first income bin shows that individuals in the lowest income bin consume on average 56 percent of household consumption if they are primary earners, while individuals in the same income bin consume on average 42 percent of household consumption if they are secondary earners in their respective households.

Figure 3. Secondary Earners' Consumption and Credit Shares

(a) Secondary Earners' Within-Household Consumption Share by Credit Share Bin

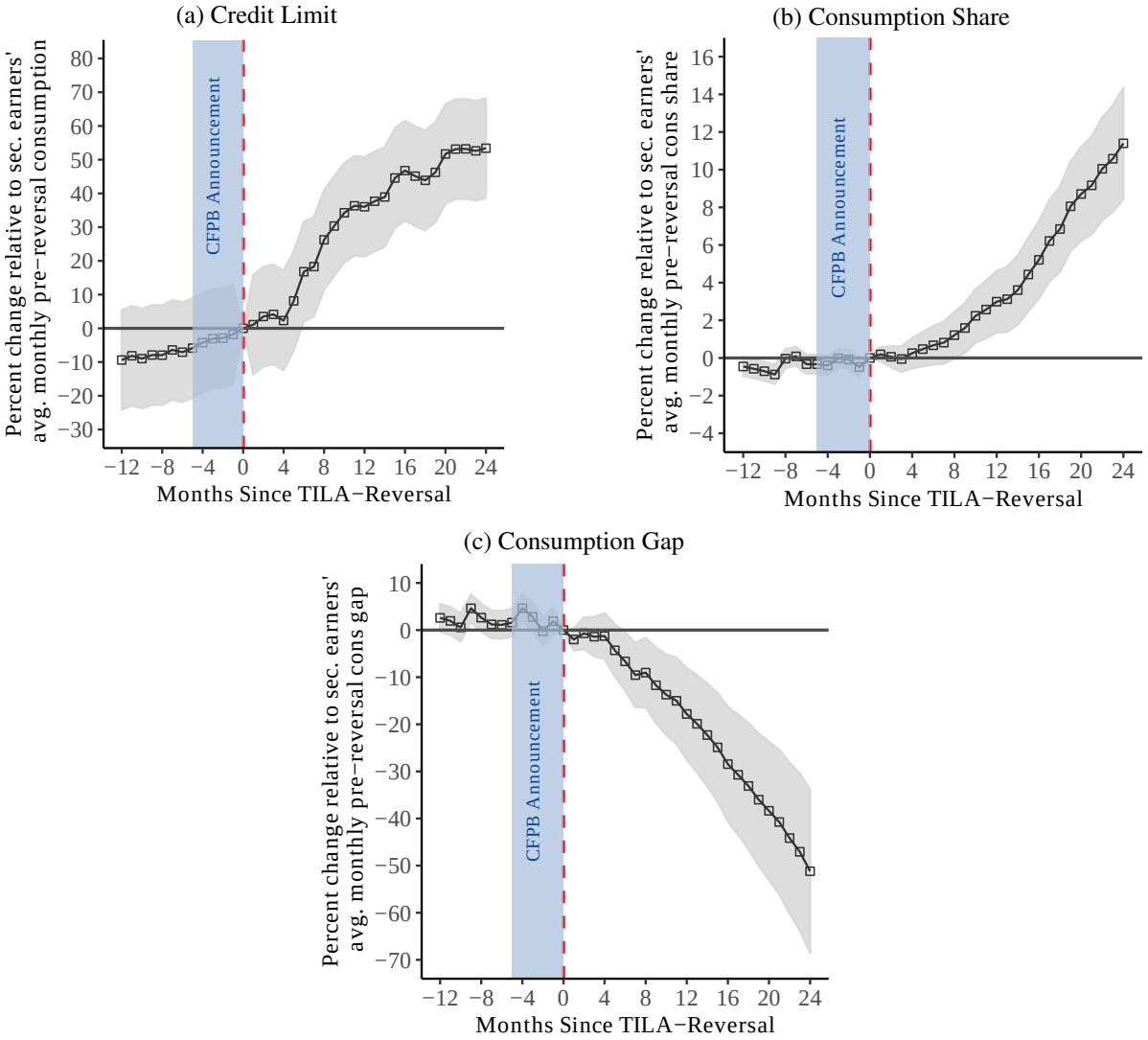


(b) Secondary Earners' Within-Household Consumption Share by Accessible Credit as an Authorized User



Notes: Figure a plots secondary earners' average monthly consumption share (y-axis) against the share accessible credit (x-axis) in the household. Figure b plots secondary earners' average monthly consumption share (y-axis) against the amount of average monthly credit limit they can access as an authorized user (x-axis). The red dashed line in each figure shows a linear fitted line.

Figure 4. Effect of the Reversal on Secondary Earners' Credit Limit and Consumption Share, and Consumption Gap in the Household

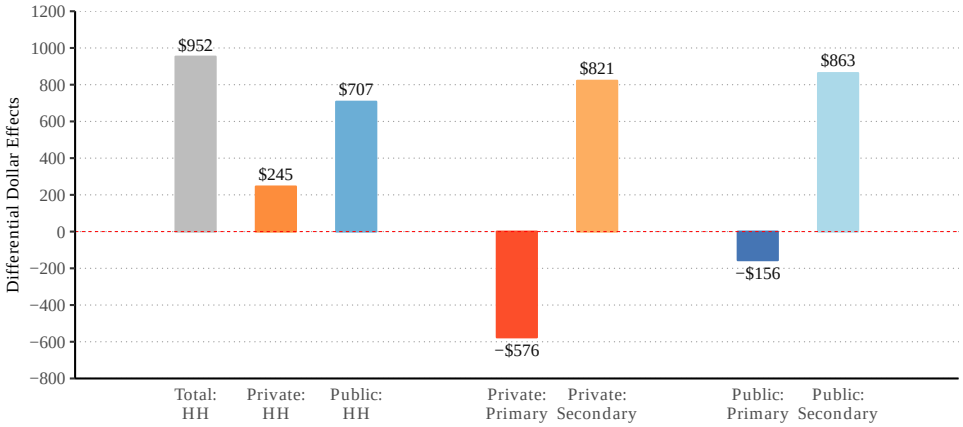


Notes: This figure plots the cumulative estimates, $\Phi_\tau = \sum_{j=0}^{\tau} \phi_j$, from the following event-study specification:

$$Y_{h,t} = \alpha_h + \gamma_t + \sum_{j \neq -1} \phi_j (Treat_h \times 1_{j=t}) + \epsilon_{h,t} \tag{33}$$

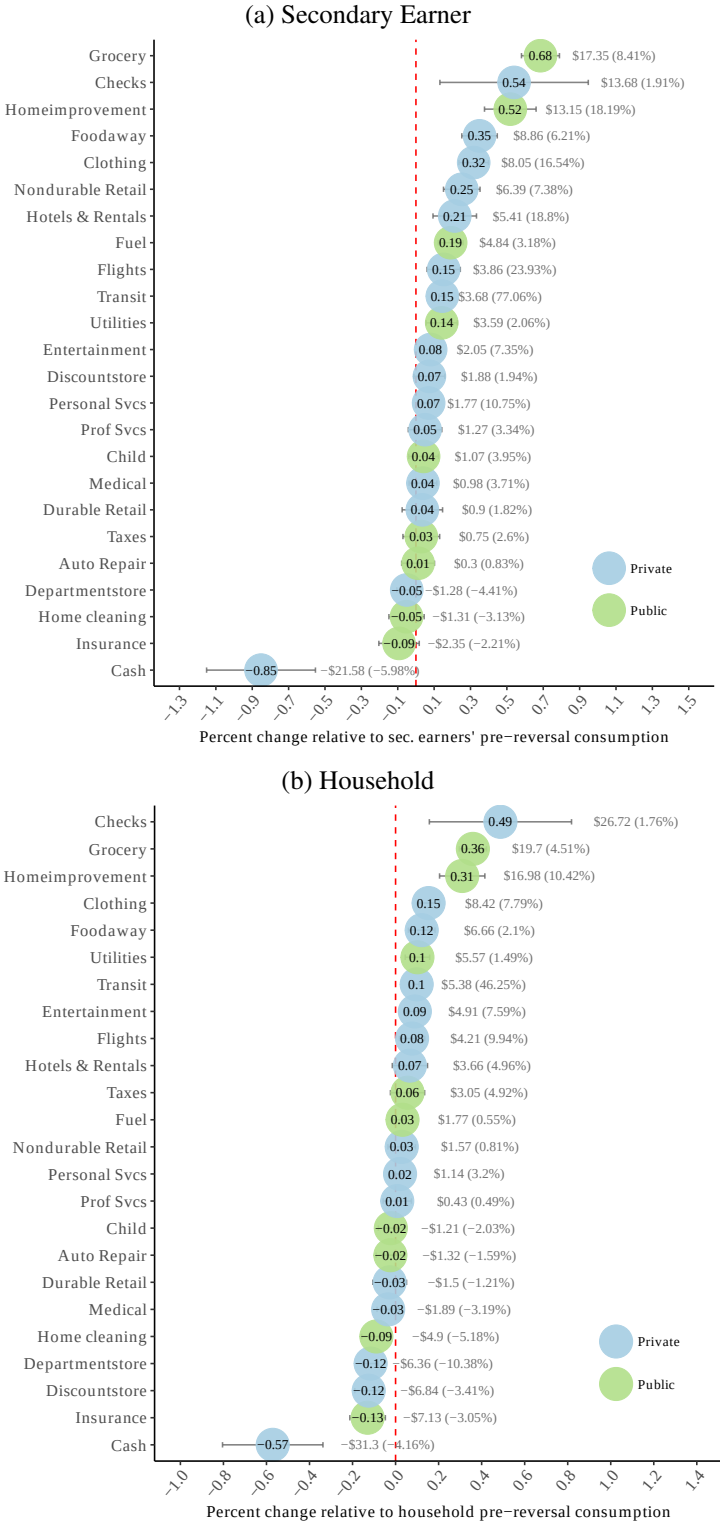
The outcome variables are secondary earners' credit limit (4a); consumption share (4b); and within-household consumption gap (4c), scaled by their average monthly pre-reversal mean. The consumption gap in the household is defined as the difference between the consumption shares of each spouse. Cumulative effects are shown for the post-TILA reveral period ($j > 0$) while point-in-time regression estimates, ϕ_j, are plotted for the pre-TILA reversal period. The month prior to the reversal ($j = -1$) is omitted, so estimates can be interpreted relative to this pre-reversal baseline period. Red dashed lines denote the month of the reversal. 90 percent confidence intervals are shown in gray. The shaded blue area denotes the phase-in period when the CFPB first announced the reversal and allowed credit card issuers to start adopting the new income collection standard. Figure A.9 replicates these figures using the gender-intensity consumption measure.

Figure 5. Decomposition of Private and Public Consumption



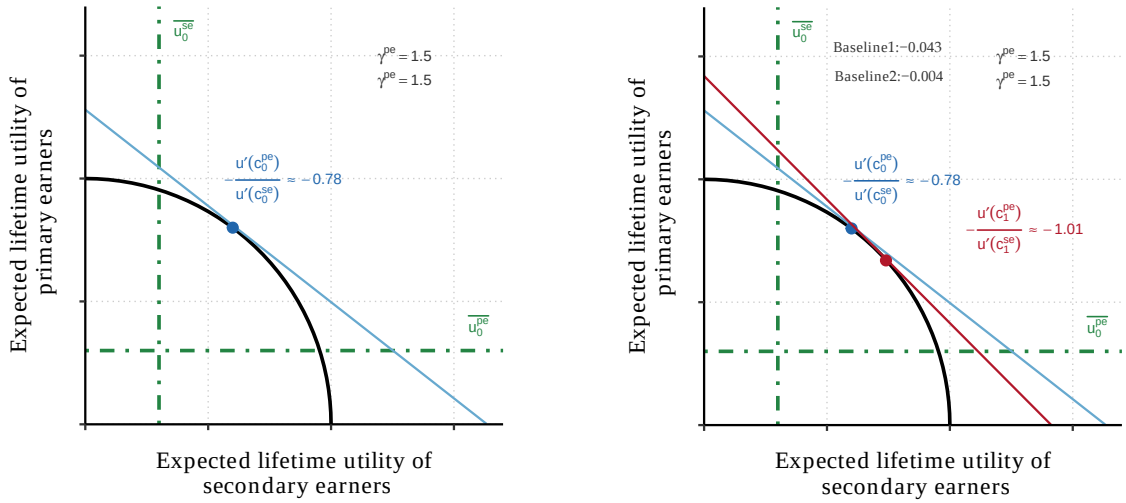
Notes: This figure plots the implied dollar effect using the 24-month cumulative estimates reported in Table 7. The first three bars decompose total household spending into private and public consumption, and the remaining bars further decomposes private and public consumption by earner type. See Table 7 for detailed description.

Figure 6. Decomposition of Spending-Based Consumption Effect



Notes: This figure decomposes the change in spending-based consumption into detailed categories for secondary earner and household. Each bubble shows monthly effect, scaled by average monthly pre-reversal mean of secondary earner or household consumption. Thus, percent effects sum to total monthly consumption effect reported in Tables 4 and 5. Changes in dollars are reported to the right of whiskers and the size of the dollar effect relative to its pre-reversal mean is shown in parenthesis. For example, household spending on groceries increased by $19.70, which corresponds to 4.5% of baseline household spending on groceries. Figure A.12 replicates this chart using gender-intensity measures.

Figure 7. Changes in Secondary Earners' Bargaining Power: A Sufficient Statistics Approach

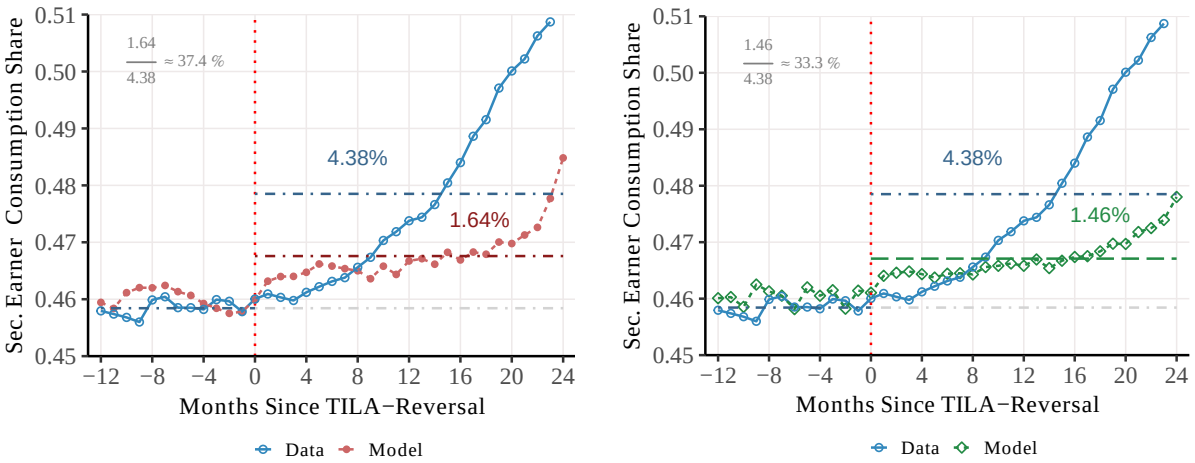


(a) Initial Allocation

(b) Post-Reversal Allocation

Notes: This figure illustrates the change in spouses' marital bargaining power using a sufficient statistics approach. Equation 4 shows the model's key prediction that the ratio of spouses' marginal utilities of consumption has a one-to-one mapping to the ratio of spouses' bargaining power (i.e., the slope of the Pareto frontier). Using this equation and reduced form statistics on secondary and primary earners' average monthly consumption for the treated group, I quantify the change in secondary earners' relative bargaining power. A risk-aversion parameter of 1.5 is assumed for both spouses. Panel a shows the location of couples' consumption sharing plan before the reversal (blue dot) and Panel b shows how this consumption sharing plan changed after the reversal (red dot). In each figure, the y-axis plots the primary earner's expected utility and the x-axis plot the secondary earner's expected utility. Vertical (horizontal) dot-dash line shows secondary (primary) earners' outside options, or their expected lifetime utility in case of divorce. Curved black lines show the Pareto frontier and the tangency points on the curve indicate the location of efficient intra-household allocation of resources. Comparing the two panels show that secondary earners' relative bargaining power increased by 23 percentage points after the reversal, from 0.78 to 1.01. I benchmark this increase to two baseline numbers annotated in Panel b. "Baseline 1" shows the average monthly change in secondary earners' relative bargaining power in the pre-reversal period among the card holder sample where secondary earners eventually open a credit card account. "Baseline 2" shows the same statistics among the all sample. "Baseline 2" shows that the typical monthly variation in secondary earners' bargaining power is only 0.4 percentage points, but can be as large as 4.3 percentage points among card openers. This change can be as high as 23 percentage points after the reversal. This figure builds on Chiappori and Mazzocco (2017).

Figure 8. Quantitative Importance of the Limited-Commitment Channel



(a) Evaluated at the first tercile of L^P.

(b) Evaluated at the second tercile of L^P.

Notes: This figure compares secondary earners' consumption share path observed in the data and the model. In both panels the blue line shows secondary earners' consumption share path observed in the data. The blue line is obtained by applying the dynamic DD estimates shown in Figure 4 to the model-generated pre-reversal consumption share mean. The red and green lines show secondary earners' consumption share generated in the model described in Section 6. The size of the change in secondary earners' consumption share is annotated. The model generated paths in Figures 8a and 8b are obtained by assuming that $L^P = 0$ and $L^P = 2,000$, respectively, or the first and the second terciles of primary earners' credit limit in the data. The dot-dash lines around the x-intercept shows the pre- and post-reversal mean of each line. The annotation at the top left corner shows that the model explains roughly 33∼37% of the observed increase in secondary earners' consumption shares in the data.

Table A.1. Detailed Spending Categories

Category	Type	Examples
Department Store	Private	Department stores
Discount Store	Private	Discount stores
Clothing	Private	Clothing stores
Entertainment	Private	Theater, travel agency, tourist attraction, cruise lines, golf course, recreational camps
Flights	Private	Various airline companies
Hotels/Rentals	Private	Hotels, inns, resorts
Medical	Private	Ambulance services, dentists, doctors and physicians, chiropractors, optometrists, nursing and personal care facilities.
Transportation	Private	Cabs, bus lines, passenger railways, airports, parking lots, transportation svcs
Food Away	Private	Bakery, catering, bar, cafes, eating places and restaurants, fast food restaurants
Durable Retail/Misc	Private	Equipment, appliances, electronics, furniture, donation, organization, membership
Nondurable Retail/Misc	Private	Stationary, office supplies, duty free store, book store
Checks	Private	Paper checks
Cash	Private	ATM withdrawals
Professional Services	Private	Consulting, legal, tax preparations, advertising
Personal Services	Private	Hair salon, spa, nail salon, funeral services, tailors, mending
Auto Repairs/Parts	Public	Car washes, paint shops, automobile and truck dealers, vehicle supplies and new parts, car sales, services, repairs
Fuel	Public	Service stations, automated fuel dispensers
Utilities	Public	Utility service, electric, gas, sanitary and water, cable, telecommunication services
Groceries	Public	Grocery stores and supermarkets
Home improvement	Public	Florists, hardware supplies, home supply warehouse stores, building materials, glass stores, wall paper stores, garden supply stores
Home cleaning/repairs	Public	Cleaning, maintenance, repairs, heating, roofing
Child	Public	Child care, children's and infant's wear stores, toy
Insurance	Public	home insurance, car insurance, etc
Tax	Public	Tax payments

Notes: This table reports examples of detailed spending types included in each spending category. The categorization of "private" or "public" consumption follows existing studies (Chiappori, Fortin and Lacroix, 2002; Mazzocco, 2007).

Table A.2. Spending Shares by Gender

Spending Categories (1)	Female Share (2)	Male Share (3)	Spending Categories (4)	Female Share (5)	Male Share (6)
alimony_court	27.47	72.53	nondur_sewing	80.36	19.64
auto_tollparking	42.79	57.21	nondur_sports	13.06	86.94
autopartsmfr	27.52	72.48	nondur_supplement	36.90	63.10
cash	38.64	61.36	nondur_tobacco	32.84	67.16
child_tot	59.17	40.83	paper_checks	46.13	53.87
clothing	66.86	33.14	perslsvcs_beauty	77.08	22.92
clothing_men	26.16	73.84	perslsvcs_dating	40.54	59.46
clothing_oth	60.12	39.88	perslsvcs_massage	45.22	54.78
clothing_shoe	61.34	38.66	perslsvcs_tailor	36.09	63.91
clothing_sports	41.57	58.43	pet_tot	62.60	37.40
clothing_women	78.16	21.84	pharmacy	59.54	40.46
counseling	48.93	51.07	profperslsvcs_accounting	45.68	54.32
departmentstore	65.47	34.53	profperslsvcs_auto	41.32	58.68
discountstore	59.06	40.94	profperslsvcs_biz	33.45	66.55
donation	54.17	45.83	profperslsvcs_cleaning	52.21	47.79
dur_computer	30.83	69.17	profperslsvcs_contractor	45.03	54.97
dur_dealers	38.59	61.41	profperslsvcs_dental	65.29	34.71
dur_electronicsappls	37.34	62.66	profperslsvcs_drycleaning	43.81	56.19
dur_furniture	50.21	49.79	profperslsvcs_fin	40.25	59.75
dur_healthcare_device	43.24	56.76	profperslsvcs_lawn	46.12	53.88
dur_jewelry	40.72	59.28	profperslsvcs_legal	45.08	54.92
dur_misc_tot	35.46	64.54	profperslsvcs_logistics	46.38	53.62
edu_tot	50.74	49.26	profperslsvcs_main	51.29	48.71
entertainment_attraction	44.75	55.25	profperslsvcs_medical	57.37	42.63
entertainment_gambling	17.21	82.79	profperslsvcs_nursing	58.81	41.19
entertainment_game	28.31	71.69	profperslsvcs_oth	44.25	55.75
entertainment_main_tot	41.75	58.25	profperslsvcs_photo	54.22	45.78
entertainment_sport	45.84	54.16	profperslsvcs_postal	49.14	50.86
fees_tot	49.28	50.72	profperslsvcs_printing	44.54	55.46
foodaway_bakeries	58.62	41.38	profperslsvcs_realestate	61.22	38.78
foodaway_bars	27.10	72.90	profperslsvcs_security	49.06	50.94
foodaway_catering	48.80	51.20	profperslsvcs_tech	35.63	64.37
foodaway_fastfood	43.46	56.54	rental_car	37.75	62.25
foodaway_main_tot	38.86	61.14	rental_furniture	38.13	61.87
fuel	41.78	58.22	rental_housing	44.87	55.13
govt_tot	41.74	58.26	rental_oth_tot	39.92	60.08
grocery_alcohol	36.55	63.45	repair_auto	39.87	60.13
grocery_tot	56.16	43.84	repair_electronics	40.29	59.71
homeimprovement	37.65	62.35	repair_furniture	52.10	47.90
homeimprovement_lawn	49.46	50.54	repair_oth_tot	36.36	63.64
homeimprovement_oth_tot	58.50	41.50	repair_shoe	60.38	39.62
hospitals	53.79	46.21	subscription	50.43	49.57
insurance_tot	47.76	52.24	tax	39.07	60.93
membershiporg	49.19	50.81	telecomm	46.54	53.46
nondur_cosmetics	82.39	17.61	transit	46.55	53.45
nondur_craft	67.17	32.83	transit_public	49.07	50.93
nondur_office	52.03	47.97	travel_flights	44.63	55.37
nondur_oth_tot	52.08	47.92	travel_lodging	40.18	59.82
nondur_photo	38.37	61.63	travel_oth	48.70	51.30
nondur_retail	49.16	50.84	utilities	50.89	49.11

Notes: This table reports the breakdown of category-level spending shares by gender. These statistics are obtained out-of-sample using 2.4 million consumers who are active users of both Chase checking and credit card accounts during my sample period. These statistics are used to construct the gender-intensity consumption measure.

Table A.3. Sample Representativeness

	Benchmark Mean (1)	Sample Mean (2)
Head of Household Age (years)	55	44.31
Share of Double Income Households	0.53	0.54
Total Income ($)	83,413	118,729
Annual Consumption ($)	62,015	88,068
Public ($)	18,765	29,153
Private ($)	43,250	58,915
Expenditure to Income Share	0.74	0.74
Public to Expenditure Share	0.30	0.33
Private to Expenditure Share	0.70	0.67

Notes: This table compares the representativeness of my analysis sample described in Section 3.1 to external benchmarks from the Consumer Expenditure Survey (CEX) Table 3424 (i.e., consumer units of two people) for 2014 and Bureau of Labor Statistics (BLS). Column 1 reports annual average household characteristics in external benchmarks. The CEX excludes households that earn less than $20,000 to make the benchmark sample more comparable to my sample, which limits analysis to households to earn at least $17,000 (2013 U.S. poverty threshold for two-member household). Statistics are re-weighted by population share in each income bin. Column 2 reports annual average household characteristics for 2014-2015 in my sample. "Head of Household Age" shows the "age of reference person" in Column 1 and the oldest member in the household in Column 2. Total income includes labor, capital, business, retirement income, other income, and government transfers, including child support. Public expenditures reported Column 1 include spending on maintenance, repairs, other expenses; utilities, fuels; household operations; miscellaneous household equipment; laundry and cleaning supplies; other household products; household textiles; floor coverings; food at home; other vehicle expenses; and children. Private expenditures reported in Column 1 include all other spending that is not public spending. See Table A.1 for detailed spending categories my sample.

Table A.4. Summary Statistics of Account Ownership Structure and Payment Choice for Married Individuals

	Mean (1)	Median (2)
Number of:		
Checking Accounts	1.51	1
Debit Cards	1.46	1
Credit Cards	4.03	3
Has Credit Cards	0.84	1
Use Cash	0.93	1
Primarily Obtain Cash from ATM	0.55	1
Checking Accounts Shared with a Spouse:		
Primary Account	0.73	1
Secondary Account	0.28	0
Own Primary Residence	0.82	1

Notes: This table reports account ownership structure and payment choice statistics for married individuals using the 2020 Survey of Consumer Payment Choice (SCPC). The financial accounts considered in the survey are those that belong to the survey respondent or jointly with the respondent's spouse. It excludes accounts that are only held by the respondent's spouse. "Use Cash" reports the share of respondents that used cash as a payment method in the last 12 months. "Checking Accounts Shared with a Spouse" reports the share of respondents who share their primary or secondary checking account with their spouse. "Own Primary Residence" asks whether the survey respondent or the respondent's spouse is a home owner.

Table A.5. Secondary Earner Characteristics: All and Regression Samples

	All Sample			Regression Sample		
	Control Mean (1)	Treated Mean (2)	Mean Difference (3)	Control Mean (4)	Treated Mean (5)	Mean Difference (6)
Female	0.61	0.61	0.00	0.59	0.58	-0.01
Age	40.8	40.9	0.1	38.2	38.4	0.1
Income ($)	1,012	1,004	-8	1,259	1,340	81
Cash on hand ($)	995	1,021	27	1,091	1,118	27
Relative consumption share	0.44	0.43	0.00	0.46	0.46	-0.01
Consumption ($)	2,657	2,581	-76	2,573	2,496	-77
Public good ($)	908	826	-82	891	801	-91
Private good ($)	1,749	1,755	6	1,682	1,695	14
Has a sole credit card	0.02	0.02	0.00	0.13	0.13	0.01
Credit limit ($)	152	164	12	867	923	56
Card balance ($)	34	37	3	195	210	15
Number of Households	33,100	33,100	0	5,809	5,873	64

Notes: This table compares secondary earner characteristics in the treated and the control group for All and Regression samples. The All sample refers to the matched sample of 66,200 households. The Regression sample restricts the matched sample to households where secondary earner opens a credit card account at some point during my sample period. See Table 2 for variable descriptions.

Table A.6. Extensive Margin: Credit Card Opening and Closing

		Cumulative (percentage points)				
	Monthly effect (1)	6-month effect (2)	12-month effect (3)	18-month effect (4)	24-month effect (5)	Implied effect (6)
	A. Secondary Earner's Sole Credit Card Accounts					
Credit Card Opening	-0.13	-0.60	-1.25	-1.25	-2.51	0.00
	(0.1)	(0.93)	(1.48)	(2.01)	(2.5)	[2.14]
Credit Card Closing	0.01	0.03	-0.02	0.09	0.32	0.00
	(0.01)	(0.09)	(0.18)	(0.3)	(0.37)	[.005]
	B. Joint Credit Card Opening					
Accounts held by Secondary Earners	-0.01	-0.03	-0.22	-0.19	-0.16	0.00
	(0.01)	(0.11)	(0.2)	(0.26)	(0.34)	[.03]
Accounts held by Primary Earners	0.01	-0.02	-0.13	0.10	0.11	0.00
	(0.02)	(0.17)	(0.27)	(0.37)	(0.49)	[.076]
Number of Observations	443,412	210,276	280,368	350,392	420,134	

Notes: This table presents the coefficient of a "treat $\times$ post" indicator in a difference-in-differences regression described in Section 2.2 using the Regression Sample. Panel A reports estimates for secondary earners' sole credit card account opening and closing; and in Panel B reports estimates for joint credit card opening for accounts held by secondary earners or primary earners. Column 1 reports pooled regression estimates from Equation 1. Columns 2-5 report cumulative effects over different horizons, calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Column 6 reports implied cumulative effects, computed as $\Phi_{24}\times$ pre-reversal mean of the outcome. An extra month of data is used for Column 1 relative to Column 5. Pre-reversal average of the outcome variable is reported in brackets. Reported coefficients are multiplied by 100 for readability. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.7. Secondary Earners' Other Credit Card Outcomes

			Cumulative (percentage points)									
	Monthly effect (1)		6-month effect (2)		12-month effect (3)		18-month effect (4)		24-month effect (5)		Implied effect (6)	
	A. Secondary Earner Credit Card Outcomes											
Annual Percentage Rate	-2.03		-1.28		3.59		2.76		-1.33		0.03	
	(6.31)		(14.86)		(13.39)		(12.26)		(12.89)		[4.69]	
Credit Card Utilization	-0.56	***	-0.64		-0.33		-0.29		-1.31	**	0.01	
	(0.13)		(0.39)		(0.43)		(0.47)		(0.56)		[6.11]	
Non-Chase Credit Card Payments	0.10		1.43		1.26		2.50		1.1		0.00	
	(0.16)		(1.32)		(2.13)		(3.05)		(3.99)		[389]	
	B. Household Credit Card Outcomes											
Annual Percentage Rate	6.00		13.68		1.02		4.16		2.26		0.14	
	(4.3)		(11.17)		(10.36)		(10.59)		(11.85)		[12.06]	
Credit Card Utilization	0.44	**	-0.54		0.82	*	0.53		-.09		0.00	
	(0.18)		(0.49)		(0.42)		(0.4)		(0.42)		[36.8]	
Non-Chase Credit Card Payments	-0.13		2.08	**	1.24		-0.41		-4.49		0.01	
	(0.13)		(1.05)		(1.69)		(2.42)		(3.17)		[817]	
Number of Observations	443,412		210,276		280,368		350,392		420,134			

Notes: This table presents the coefficient of a "treat $\times$ post" indicator in a difference-in-differences regression described in Section 2.2 using the Regression Sample. Panel A reports estimates for secondary earner credit card outcomes; and in Panel B reports estimates for household credit card outcomes. The outcomes in each panel include annual percentage rates (APR), credit card utilization rate, or end-of-billing card balance divided by credit limit, and card payments to other banks relative to pre-reversal average monthly consumption. Column 1 reports pooled regression estimates from Equation 1. Columns 2-5 report cumulative effects over different horizons, calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Column 6 reports implied cumulative effects, computed as $\Phi_{24}\times$ pre-reversal mean of the outcome. An extra month of data is used for Column 1 relative to Column 5. Pre-reversal average of the outcome variable is reported in brackets. Reported coefficients are multiplied by 100 for readability. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.8. Measurement Robustness

Secondary Earner Outcomes	Include Oth Cards (1)		Net of Travel (2)		Net of Food Away (3)		Net of Cash (4)		Net of Checks (5)		Net of (2) - (5) (6)	
	A. Spending-Based Measure											
Consumption	2.08	***	2.69	***	2.81	***	4.69	***	4.71	***	8.03	***
	(0.38)		(0.42)		(0.43)		(0.46)		(0.56)		(0.61)	
Consumption Share	0.43	***	0.44	***	0.48	***	0.57	***	0.59	***	0.56	***
	(0.07)		(0.07)		(0.08)		(0.08)		(0.09)		(0.08)	
Number of Observations	443,191		443,068		443,061		442,605		442,127		440,832	
	Pre-Reversal Mean											
Consumption ($)	2,923		2,487		2,391		2,173		1,818		1,268	
Consumption Share (%)	46.20		46.15		46.15		45.84		45.59		45.39	
	More than 50% (1)		Net of Travel (2)		Net of Food Away (3)		Net of Cash (4)		More than 60% (5)		Gender Assignable (6)	
	B. Gender-Intensity Measure											
Consumption	2.34	***	2.42	***	2.89	***	4.33	***	2.25	*	1.15	***
	(0.65)		(0.47)		(0.49)		(0.5)		(1.36)		(0.21)	
Consumption Share	0.43	*	0.59	***	0.66	***	0.62	***	0.40	*	0.21	*
	(0.25)		(0.16)		(0.18)		(0.16)		(0.22)		(0.13)	
Number of Observations	442,000		428,696		428,360		421,555		418,168		443,412	
	Pre-Reversal Mean											
Consumption ($)	2,463		1,338		1,243		1,081		630		24	
Consumption Share (%)	46.03		46.57		47.13		48.48		42.71		47.40	
Representativeness (%)	97.21		52.79		49.03		42.68		24.88		0.93	

Notes: This table examines the sensitivity of baseline estimates to using alternative spending-based and gender-intensity consumption measures. Panel A reports monthly DiD effects using various spending-based measure and Panel B using gender-intensity measures. The outcomes are scaled by secondary earners' average monthly pre-reversal mean of each outcome. Alternative spending-based consumption measures include versions that (i) include payments to other credit companies to take substitution across banks into account (col 1); exclude spending on potentially reimbursable work-related expenses, such as spending on flights, hotels/lodging, and transportation (col 2); exclude other categories that can be used for public consumption (cols 3 - 6). Panel B progressively reports more conservative gender-intensity consumption measures across columns. These measure include versions that reconstructs consumption only using spending categories (i) in which gender-intensity in spending is greater than 50 percent (col 1); (ii) net of travel, food away, and cash (cols 2-4);(iii) gender-intensity greater than 60 percent (col 5); and (iv) are considered to be either gender-assignable or more intensely consumed by one gender in the existing literature (e.g., Duflo and Udry (2004)), such men's clothing, alcohol, gambling, and tobacco for men; and women's clothing, hair or nail salons, spas, or jewelry for women (col 6). The "representativeness" row reports total dollar spending captured by gender-intensity measure relative to spending-based consumption measure. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. Reported coefficients are multiplied by 100 for readability. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.9. Specification Robustness

Secondary Earner Outcomes	Baseline (1)		No Controls (2)		Only HH f.e. (3)		Only Time f.e. (4)		State Trends (5)		Quarterly Spec (6)	
Credit Limit	39.83	***	39.73	***	39.71	***	39.68	***	36.47	***	33.99	***
	(2.14)		(2.58)		(2.2)		(2.53)		(2.3)		(3.3)	
Consumption Share	0.49	***	0.49	***	0.49	***	0.49	***	0.45	***	0.41	***
	(0.08)		(0.08)		(0.08)		(0.08)		(0.08)		(0.1)	
Number of Observations	443,072		443,072		443,072		443,072		443,072		163,299	
Household f.e.	X				X				X		X	
Time f.e.	X						X		X		X	
State-specific trends									X			
Quarterly Specification											X	
Cluster SE state	X		X		X		X		X		X	

Notes: This table examines the sensitivity of baseline estimates to using alternative specifications. The monthly DiD effects are reported, and outcomes are scaled by secondary earners' average monthly pre-reversal mean of each outcome. Column 1 reports my baseline monthly estimates also reported in Tables 3 and 4. Column 2 excludes household and time fixed effects. Column 3 only includes household fixed effects, and Column 4 only includes time fixed effects. Column 5 includes state-specific linear time trends in addition to the baseline specification used in Column 1. Column 6 reports estimates obtained from aggregating data to quarterly. The quarterly estimates are converted back to monthly to facilitate comparison to other columns.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.10. Sample Robustness

	Other Samples						Placebo							
Secondary Earner Outcomes	Joint Any (1)		No Joint Checking (2)		Any HH w/ Card (3)		Change in Joint Acct (4)		No Change in Limit (5)		Pre-period Only (6)	Primary Earner (7)		
	A. Difference-in-Differences													
Credit Limit	40.96	***	32.55	***	10.42	***	32.99	***	–		6.43	0.46		
	(2.3)		(5.61)		(0.51)		(1.8)		–		(4.47)	(0.73)		
Consumption Share	0.53	***	0.28	*	0.17	***	0.09		0.01		0.19	-0.37	***	
	(0.08)		(0.16)		(0.03)		(0.06)		(0.05)		(0.13)	(0.06)		
Number of Observations	397,970		51,977		2,499,508		406,848		830,662		151,853	443,412		
	B. Pre-Reversal Mean													
Credit Limit ($)	1,704		973		6,895		7,106		5,220		1,595	3,256		
Consumption Share	0.46		0.44		0.44		0.48		0.42		0.46	0.54		

Notes: This table examines the sensitivity of baseline estimates to using alternative samples (Cols 1-3) and reports placebo analysis (Cols 4-7). Panel A reports monthly DiD effects, and Panel B reports pre-reversal mean of each outcome. The outcomes are scaled by secondary earners' average monthly pre-reversal mean of each outcome. Column 1 restricts the sample to households in which spouses share at least one financial account (checking or credit); Column 2 to households in which spouses do not share a checking account; and Column 3 uses a broader sample of households with credit cards. The placebo analysis shows that there is no effect on secondary earners' consumption share for households that receive a limit increase on joint credit cards but not individual accounts (col 4) and for households that do not experience any change in credit limits (col 5). Column 6 only uses the pre-treatment periods and sets treatment date to be March 2013. Column 7 analyzes the impact on primary earners' credit access and consumption share.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.11. Private vs. Public Consumption: Gender-Intensity Measure

Gender-Intensity Measures	Monthly effect (1)		Cumulative 6-month effect (2)		Cumulative 12-month effect (3)		Cumulative 18-month effect (4)		Cumulative 24-month effect (5)		Implied effect (6)
					A. Secondary Earners' Consumption						
Consumption	3.05	***	-4.7		5.22		16.54	*	59.75	***	821
	(0.53)		(4.14)		(6.78)		(9.7)		(12.86)		[1,374]
Private	1.72	***	-9.52	***	-5.81		.28		30.45	***	418
	(0.47)		(3.59)		(5.91)		(8.49)		(11.28)		[820]
Public	1.33	***	4.82	***	11.03	***	16.25	***	29.31	***	403
	(0.17)		(1.38)		(2.21)		(3.11)		(4.04)		[554]
					B. Household Consumption						
Consumption	1.16	***	-4.20		6.002		2.45		21.57	**	644
	(0.41)		(3.27)		(5.3)		(7.54)		(9.94)		[2,986]
Private	0.32		-5.83	**	1.42		-4.45		4.45		133
	(0.38)		(2.97)		(4.83)		(6.89)		(9.08)		[1,849]
Public	0.84	***	1.63	*	4.58	***	6.9	***	17.13	***	511
	(0.1)		(0.83)		(1.33)		(1.86)		(2.42)		[1,136]
Number of Observations	435,071		217,778		286,550		355,219		423,609		

Notes: This table presents the coefficient of a "treat $\times$ post" indicator in a difference-in-differences regression described in Section 2.2 using the Regression Sample. This table replicates Table 7 using gender-intensity consumption measures. Outcomes are scaled by secondary earner (Panel A) or household average monthly pre-reversal consumption (Panel B). See Table 7 for table details. Figure 5 for a figure version of this table. Reported coefficients are multiplied by 100 for readability. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.12. Limited Commitment Placebo

	No Controls			Controls		
	Trouble (1)	Mortgage (2)	Child (3)	Trouble (4)	Mortgage (5)	Child (6)
	Secondary Earners' Consumption Share					
Treat	0.02	0.02	0.05	0.02	0.02	0.05
	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)
Treat x LC	-0.10	-0.04	-0.06	-0.10	-0.05	-0.06
	(0.07)	(0.05)	(0.05)	(0.07)	(0.05)	(0.05)
Number of Observations	830,388	830,388	830,388	830,388	830,388	830,388
Consumption Share	0.42	0.42	0.42	0.42	0.42	0.42

Notes: This table replicates Panel B of Table 8 using the placebo sample of households that do not experience any change in credit limits. The placebo sample is constructed from a broader sample of households that have a credit card account during my sample period but did not experience a credit limit change. This differs from the regression sample, which restricts analysis to households where secondary earner opens a new sole credit card during my sample period. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.13. Decomposition of Consumption Effect by Spending Method

Secondary Earner Outcomes	Total (1)		Checking (2)	Debit Card (3)		Credit Card (4)	
	A. Difference-in-Differences						
Consumption	2.89	***	0.21	0.67	***	2.01	***
	(0.43)		(0.31)	(0.2)		(0.26)	
Number of Observations	443,374		443,374	443,374		443,374	
	B. Pre-Reversal Mean and Implied Effects						
Consumption	2,534		1,159	1,201		176	
Implied Dollar Effects	73.3		5.3	17.0		50.9	

Notes: This table decomposes secondary earners' consumption effect by spending method. Panel A reports monthly DiD effects and Panel B reports pre-reversal mean of each outcome. Column 1 reports my baseline monthly estimates also reported in Table 4. Columns 2-4 report decomposes the consumption effect by checking account, debit cards, and credit cards. The outcomes are scaled by secondary earners' average monthly pre-reversal consumption, so estimates in cols 2-4 sum to the total effect in col 1. All specifications include household and time (month-year) fixed effects. Standard errors are clustered at the state-level and reported in parentheses. Panel C reports pre-reversal average revolving balance utilization rates. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.14. Heterogeneity by Financial Constraints

	Liquidity				Utilization Rate				DTI Level			
	High		Low		High		Low		High		Low	
Credit Limit	43.95	***	31.12	***	49.04	***	37.52	***	40.11	***	29.78	***
	(3.3)		(2.73)		(4.36)		(2.44)		(3.58)		(2.26)	
Consumption Share	0.51	***	0.43	***	-0.18		0.64	***	0.35	***	0.61	***
	(0.11)		(0.1)		(0.18)		(0.08)		(0.11)		(0.1)	
Number of Observations	221,679		221,695		82,375		360,999		219,850		219,914	

Notes: This table reports monthly estimates from Equation 1 for subsample splits based on proxies of pre-reversal household financial constraints. Outcomes are scaled as described in Tables 3 and 4. Households are split based on whether they have above (high) or below (low) median checking account balances ($2,625); have above (high) or below (low) median credit card utilization rates (0.279); and have above (high) or below (low) median debt-to-income levels (0.285). For utilization subsample cuts, the below median sample includes households with no credit cards in the pre-reversal period. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.15. Comparison between Model and Data

	Model (1)	Data (treated) (2)	External Benchmark (3)
Labor Income	6,529	6,142	
Consumption	6,383	5,905	
Net Assets	5,866	4,345	
Share of Revolvers	0.183	0.189	
Share Double Income	0.50	0.561	0.53
Probability of Divorce	0.40	—	0.44

Notes: This table compares average monthly household-level outcomes generated in the model and observed in the data. Column 2 reports statistics using the treated group only. Net assets in Column 1 refer to a_t when a_t is positive (net assets) and negative (borrowing), while they refer to checking account balances in Column 2. Share of revolvers represent the share of households that borrow in Column 1 and the share of households with positive revolving debt in Column 2. The share of double income and the probability of divorce in Column 3 are from the BLS and CDC, respectively. The model estimates are presented assuming primary earner has median credit limit.

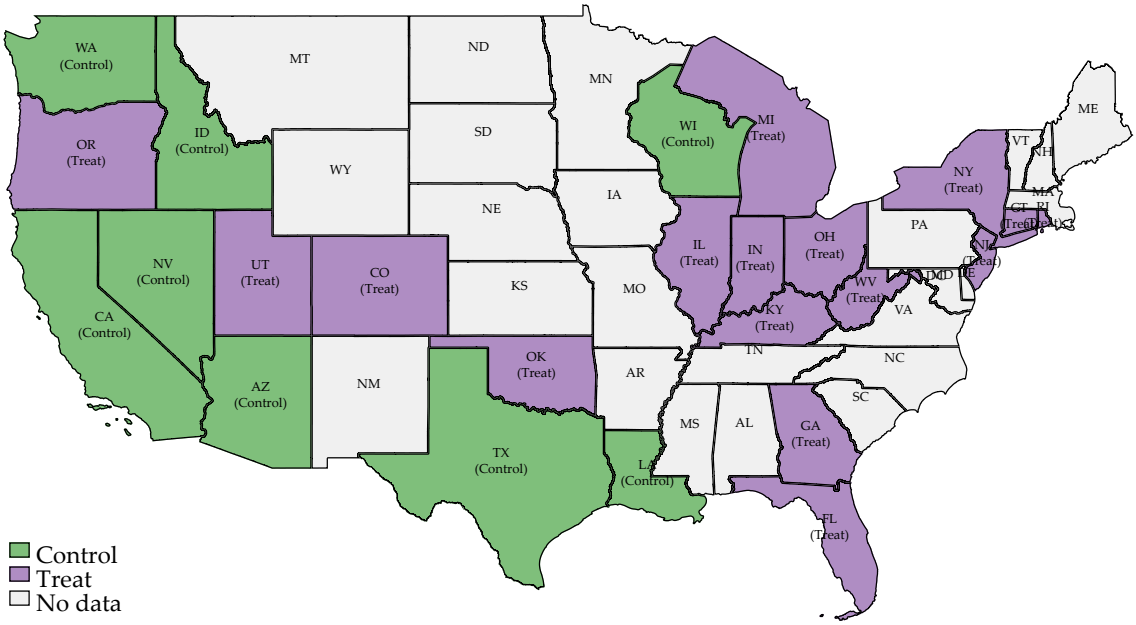
Table A.16. Welfare Gain

	Primary Earner (1)	Secondary Earner (2)	Household (3)
Consumption Equivalent	-1.85	4.16	1.53

Notes: This table reports the welfare gains from the 2013 TILA reversal. Section G details how I compute the consumption equivalent variation. Average credit limit for the treated group is assumed for the primary earner.

Figure A.1. Community Property vs. Equitable Distribution States

(a) Treated vs. Control States



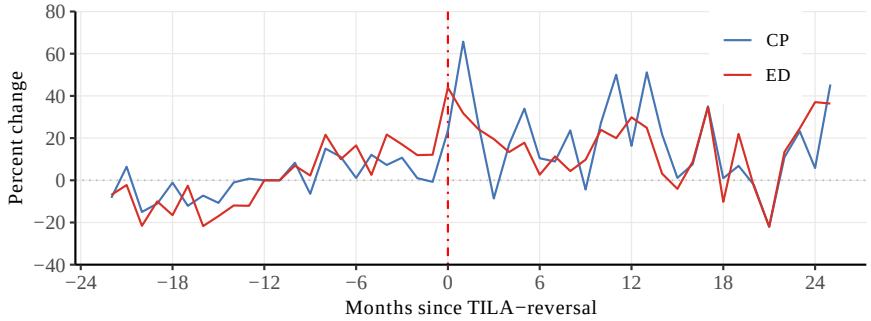
(b) Income Collection Standards

Income consideration	**Before** TILA reversal	**After** TILA reversal
Treated Equitable distribution	Independent	Household
Control Community property	Household	Household

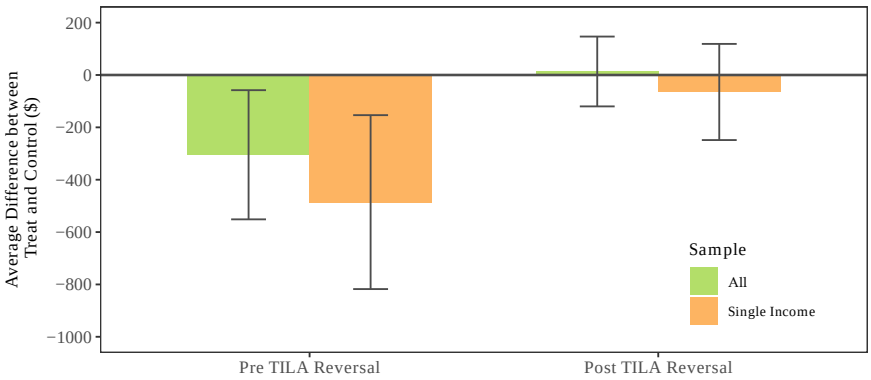
Notes: Figure a shows the map of the treated and control states in my data, color-coded by the doctrine that govern the disposition of marital property in divorce. Equitable distribution states (treated) are shown in purple and community property states (control) are shown in green. States in gray are not well represented in my data. Out of the nine community property states in the U.S. – Arizona, California, Idaho, Louisiana, Nevada, New Mexico, Texas, Washington, and Wisconsin – my sample captures all states but New Mexico. Figure b summarizes the income consideration standards across the two types of states.

Figure A.2. The Truth-in-Lending Act in Practice

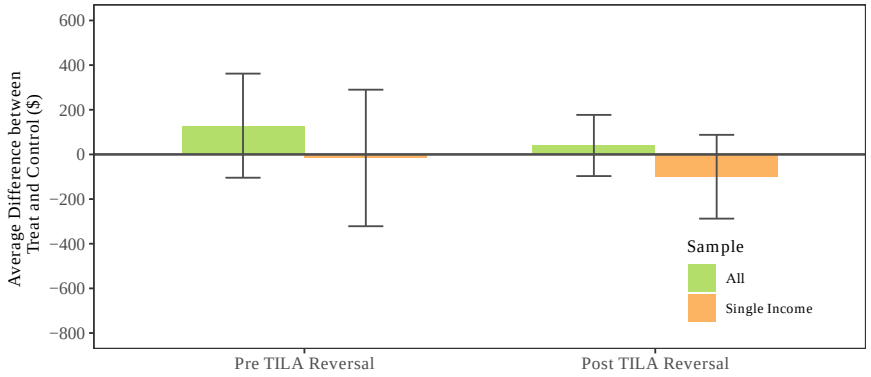
(a) Year-over-Year Percent Change in Credit Card Solicitations
by CP vs. ED States (2012-2015)



(b) Average Difference in Secondary Earners' Reported Monthly Income
on Credit Card Applications Between Treated and Control



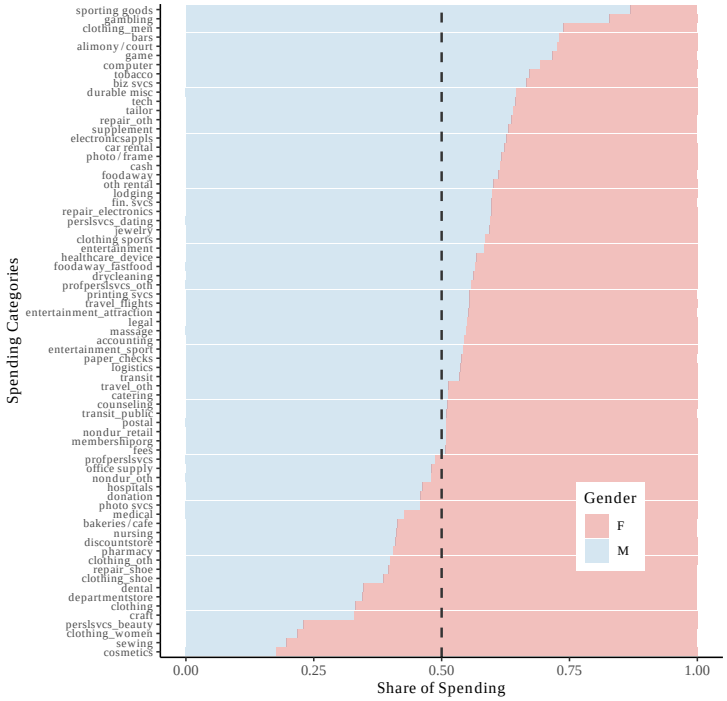
(c) Average Difference in Primary Earners' Reported Monthly Income
on Credit Card Applications Between Treated and Control



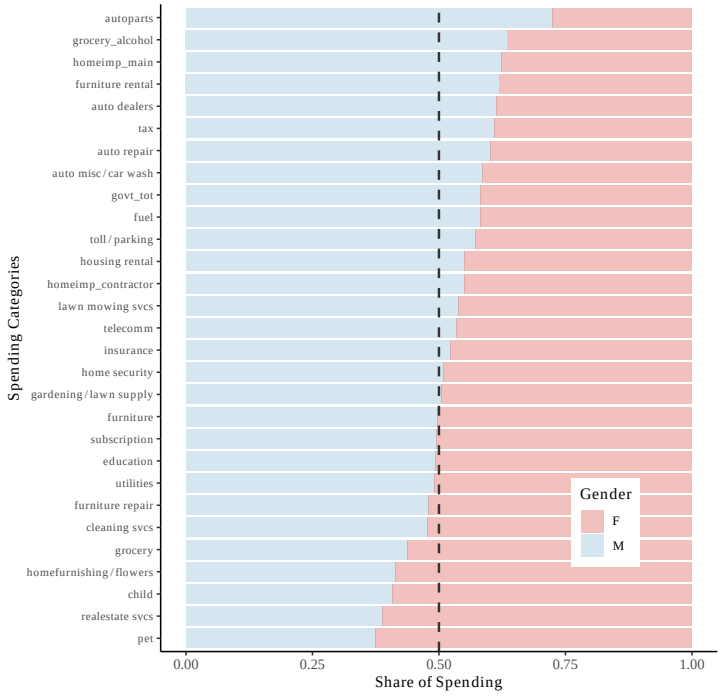
Notes: Figure a plots year-over-year percent change in all credit card solicitations in the U.S. to individuals in ED (treated) and CP (control) states. The credit card solicitation data is from the Mintel Comperemedia Database. Figure b plots the average difference in the monthly income reported on secondary earners' credit card applications between the treated and the control group. The difference is obtained by regressing reported monthly income on the treatment dummy. Figure c plots the same statistic as Figure b for primary earner. The whiskers denote 90 percent confidence intervals.

Figure A.3. Gender-Intensity in Spending by Spending Categories

(a) Private Consumption



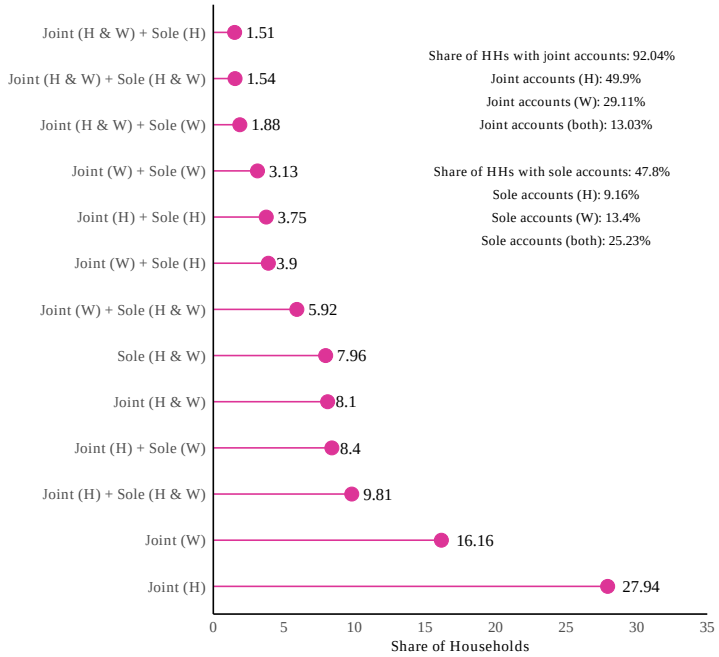
(b) Public Consumption



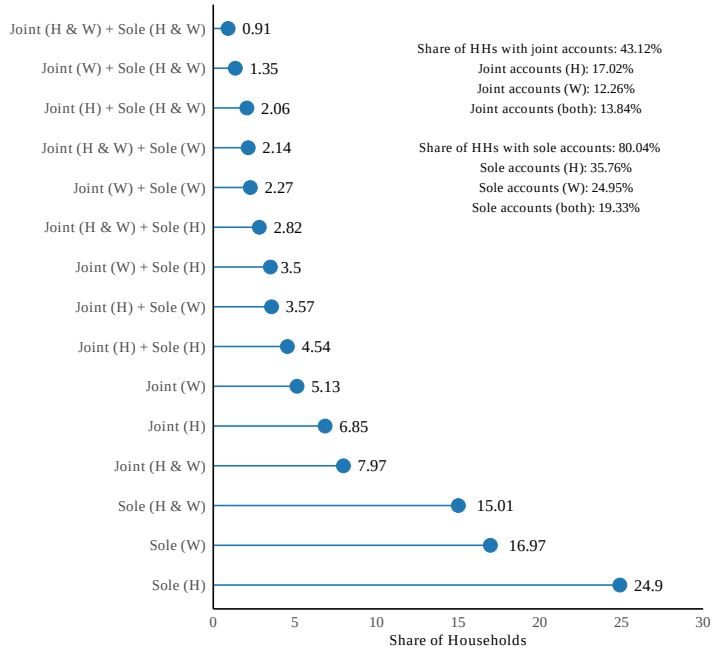
Notes: This figure shows the gender-intensity in aggregate dollar spending share for each spending category. The black dashed line shows the 50 percent mark. For example, the bottom category of Figure A.12a shows that more than 82% of aggregate spending on cosmetics is incurred by female customers. The gender intensity of spending category is calculated using a sample of 2.4 million consumers who are active users of both Chase checking and credit card accounts during my sample period. The share of spending is re-weighted to take the gender distribution into account, such that the spending share statistic is not driven by over-sampling of men vs. women in the data. See the Internet Appendix for detailed spending shares.

Figure A.4. Household Account Structure Types

(a) Checking Account

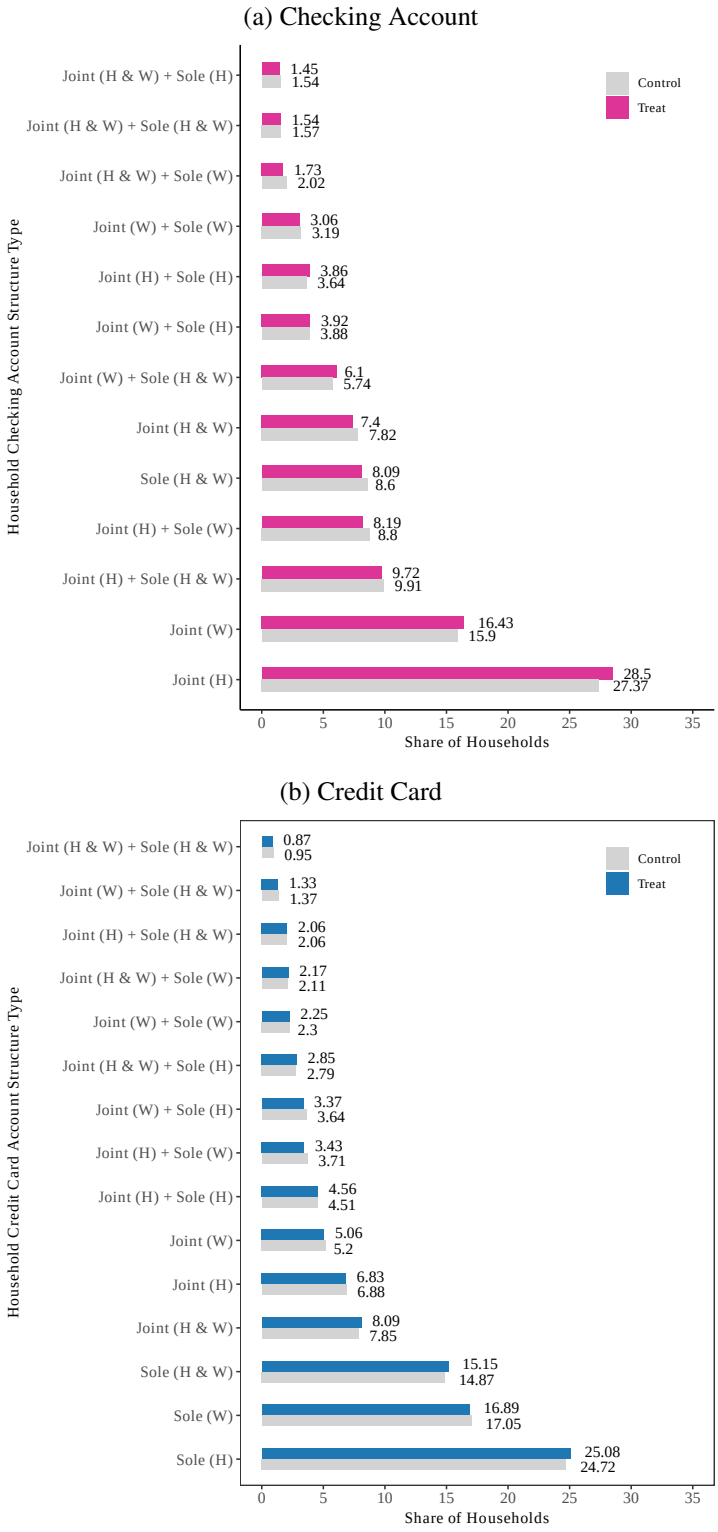


(b) Credit Card



Notes: This figure reports the share of households that hold each type of checking (Fig A.4a) and credit card account structure (Fig A.4b) in my sample. The account structure types are mutually exclusive and the shares sum to 100. "Joint" and "Sole" denote the type of account, and "H", "W", or "H & W" in parenthesis denote whether the primary account holder of each account is the husband, the wife, or both because they have multiple accounts with each spouse as the primary account holder. For example, the bottom stat of Fig A.4a shows that roughly 28% of households in my sample only have a joint checking account where the husband is the primary account holder, and the bottom stat of Fig A.4b shows that roughly 25% of households in my sample only have a sole credit card account where the husband is the primary account holder.

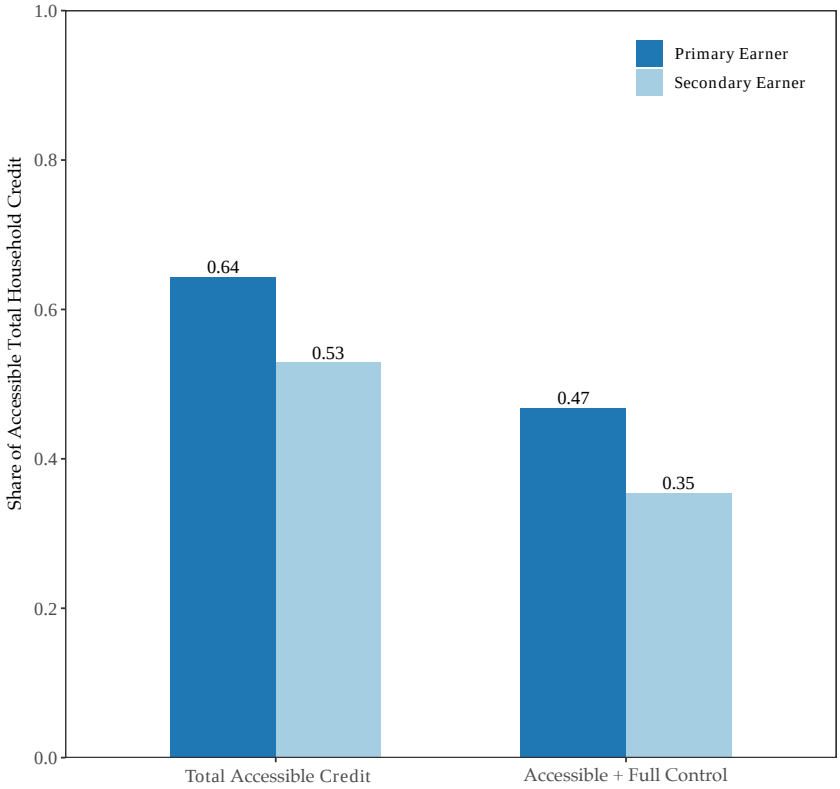
Figure A.5. Household Account Structure Types by Treatment Status



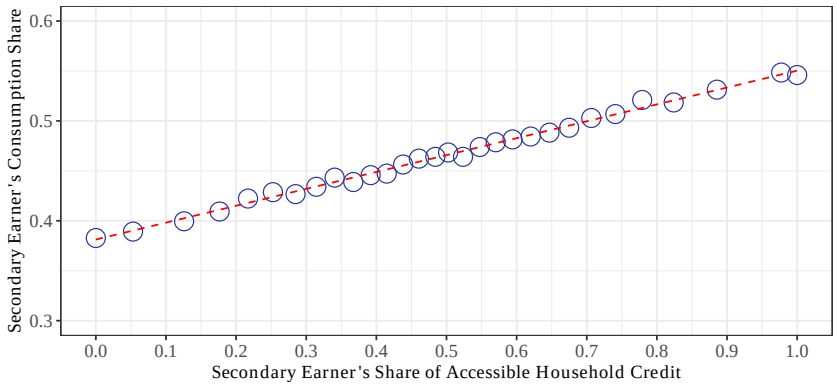
Notes: This figure shows the share of households that hold each type of checking and credit card account structure by treatment in my sample. See Figure A.4 for detailed description.

Figure A.6. Broader Sample: Within-Household Credit and Consumption Gaps

(a) Share of accessible in the Household



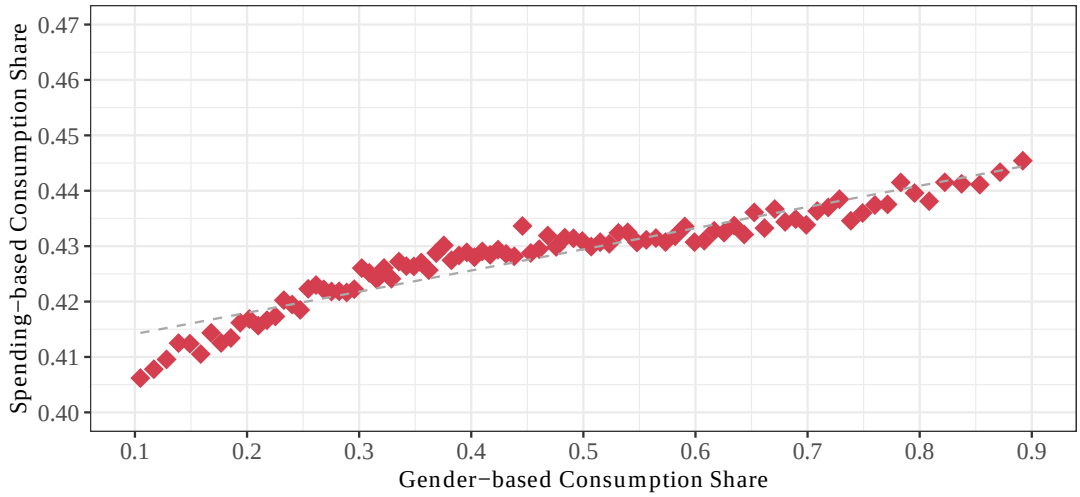
(b) Secondary Earners' Within-Household Consumption Share by Credit Share Bin



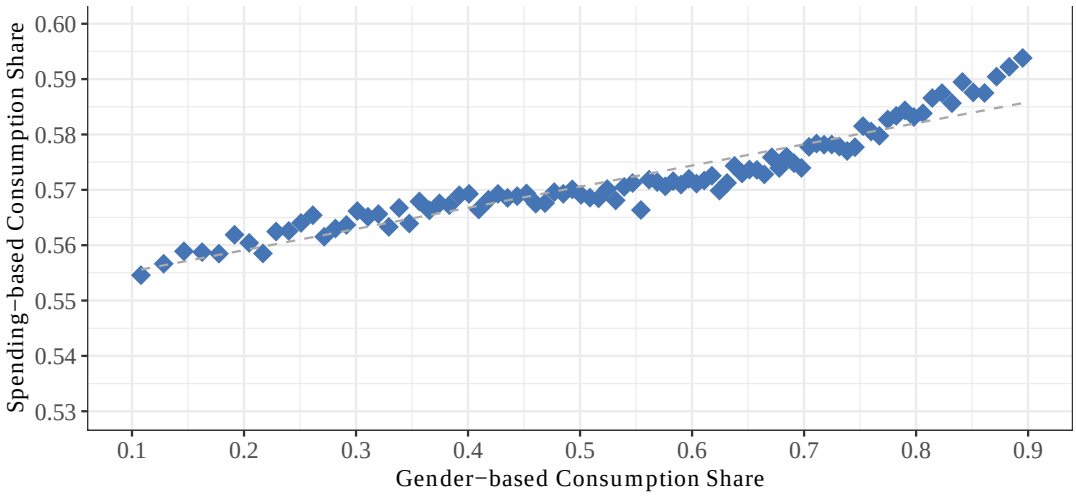
Notes: These figures replicate Figures 1 and 3 using a broader sample of 138,276 households that include households where secondary earners had credit card accounts at the beginning of my sample period.

Figure A.7. Spending-Based vs. Gender-Intensity Consumption Measure Validation

(a) Secondary Earner

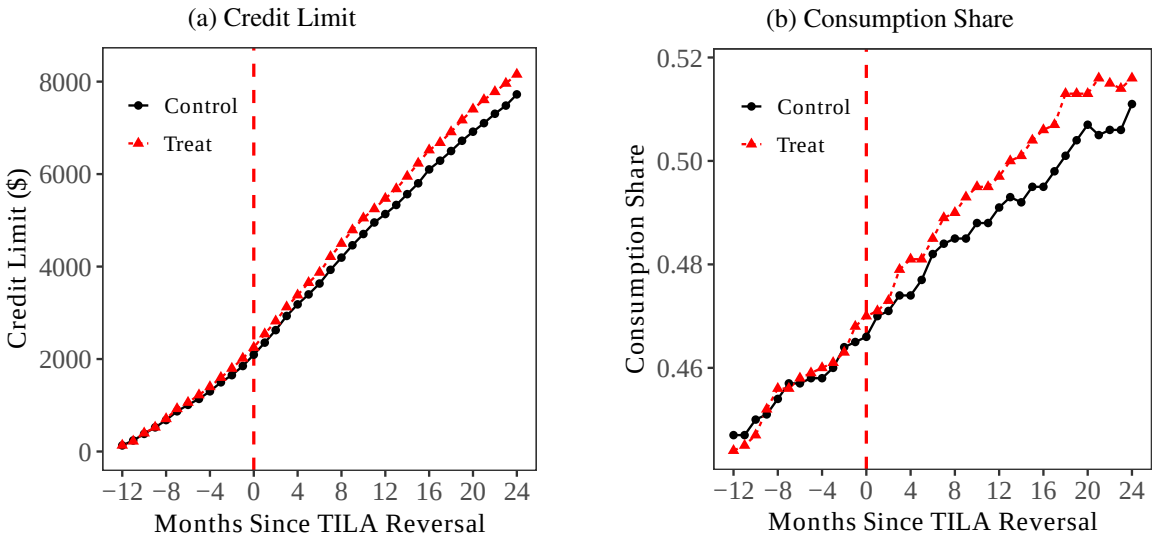


(b) Primary Earner



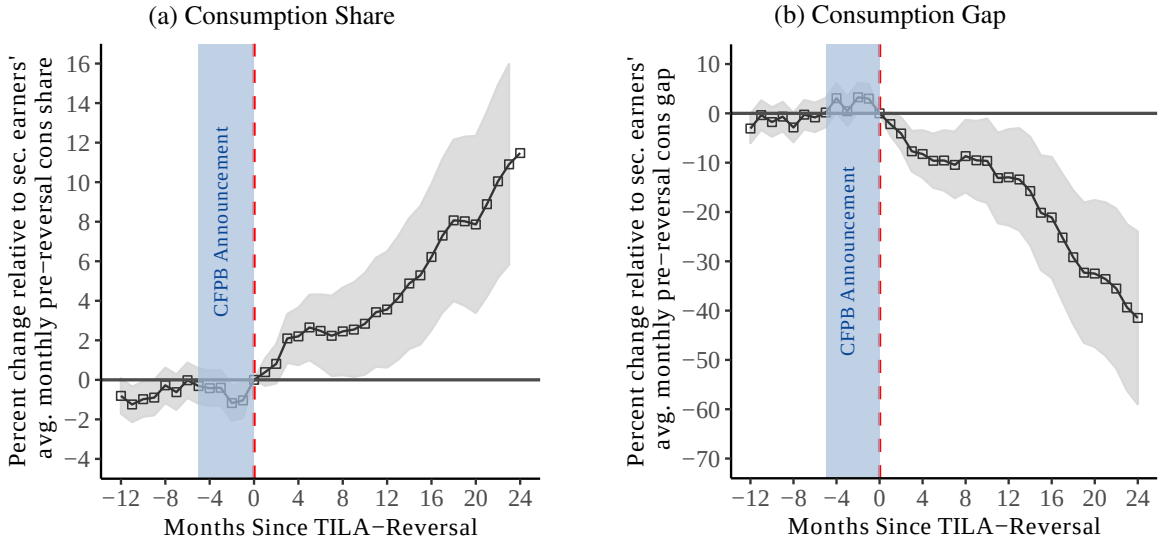
Notes: This figure shows a bin scatter plot of spending-based consumption measure and gender-intensity consumption measure to examine the validity of the assumption that "spenders are consumers." Figure A.7a plots secondary earners' average monthly consumption share using the spending-based measure against the average monthly consumption share using the gender-intensity consumption measure. If the spending-based consumption measure is a poor proxy for consumption because spenders don't necessarily consume what they buy, the slope of this figure would be 0. The positive slope illustrates that "spenders are consumers" is a reasonable proxy for consumption. Figure A.7b shows the same plot for primary earners.

Figure A.8. Secondary Earners' Credit and Consumption Share in Levels



Notes: This figure plots raw means. Figure2 a and b plot secondary earners' credit limit and consumption means. This figure illustrate that the estimated effects are driven by changes in the treated group. Since both treated and control group secondary earners opened new sole credit cards during my sample period, sole credit limit and consumption share increases for both groups. The two groups had similar trends prior to the reversal, but the treated group's outcomes diverge after the reversal.

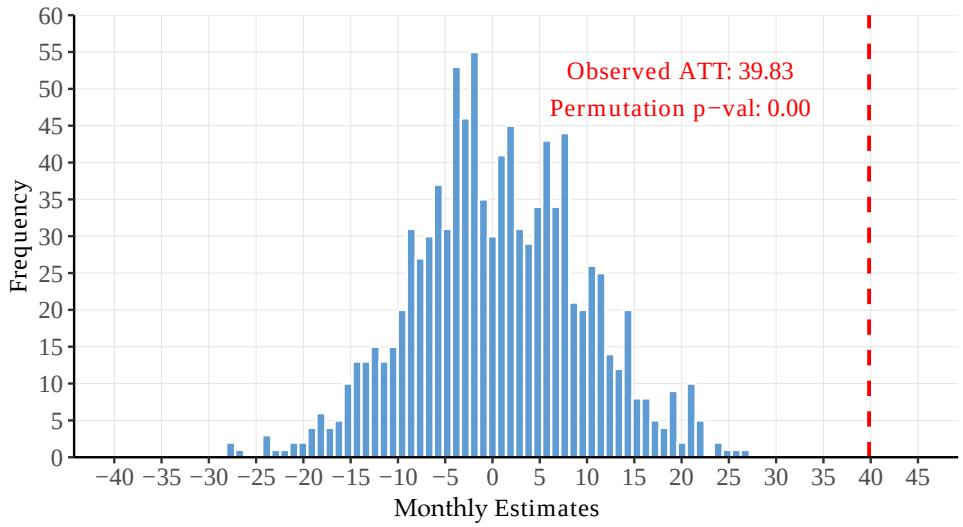
Figure A.9. Effect of the Reversal on Secondary Earners' Consumption Share and Consumption Gap: Gender-Intensity Measure



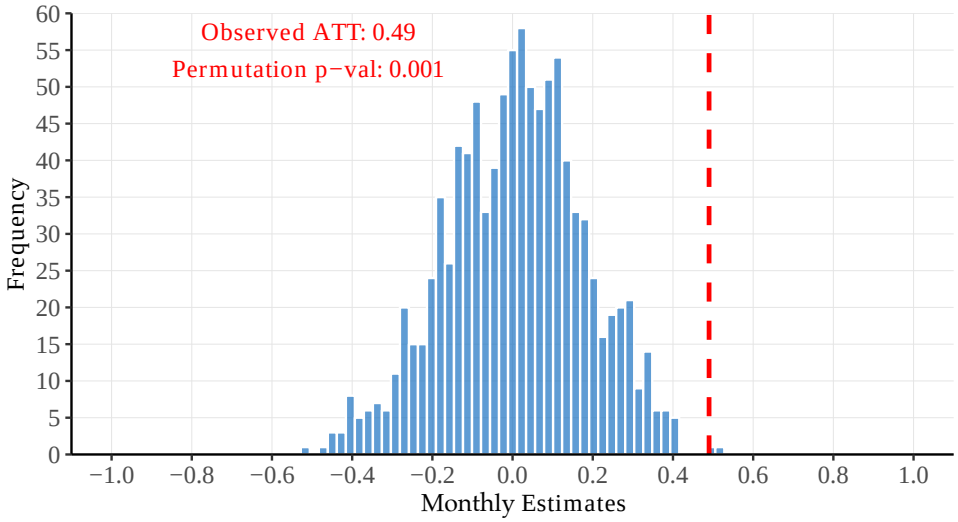
Notes: This figure replicates figure 4 using the gender-intensity consumption measure.

Figure A.10. Distribution of the Estimated ATT: Permutation Test with $N = 1000$

(a) Secondary Earners' Credit Limit

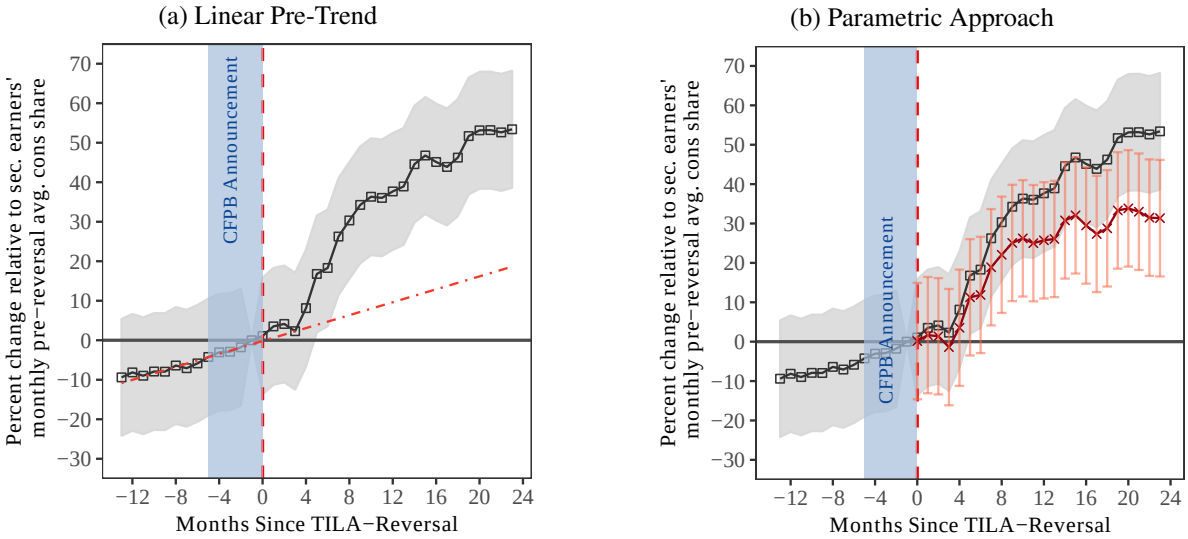


(b) Secondary Earners' Consumption Share



Notes: Figure a plots the distribution of monthly effect on secondary earners' credit limit by randomly assigning treatment to different households. Figure b plots the similar permutation test on secondary earners' credit shares. The red dashed lines mark the observed ATT in this study. The permutation p-value is obtained by determining the proportion of ATTs that are more extreme than observed ATTs.

Figure A.11. Effect of the Reversal on Secondary Earners' Credit Limit: Parametric

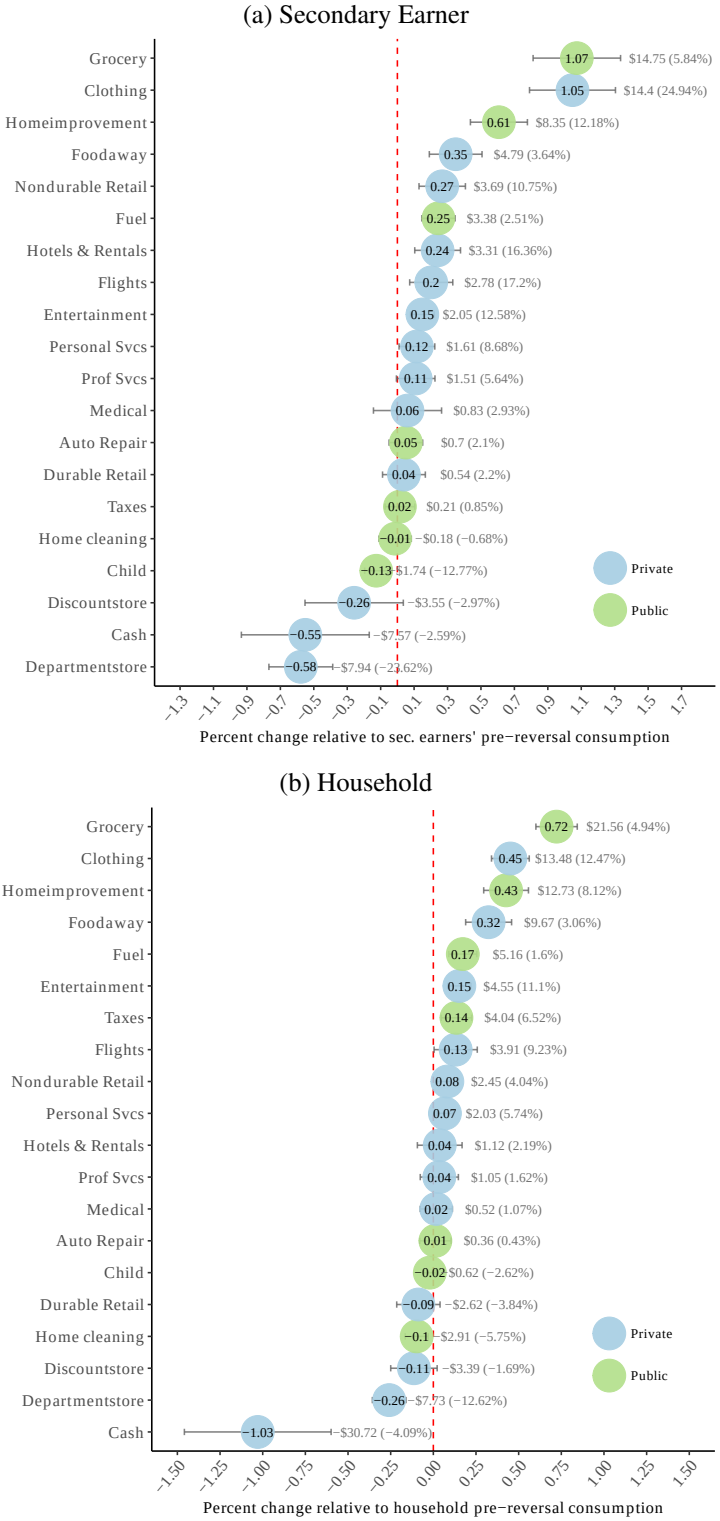


Notes: Figure a provides a visual assessment of the functional form assumption (linear) of pretend in event time. This pretend is driven by the CFPB allowing credit card issuers to start adopting the new income collection standard during the phase-in period (shaded in blue). Figure b superimposes the estimated parametric coefficients on the nonparametric coefficients shown in Figure 4. The parametric estimates are obtained by estimating:

$$Y_{h,t} = \alpha_h + \gamma_t + \sum_{j>-1} \phi_s(Treat_h \times 1_{j=t}) + \lambda \cdot t \cdot Treat_h + \epsilon_{h,t} \tag{34}$$

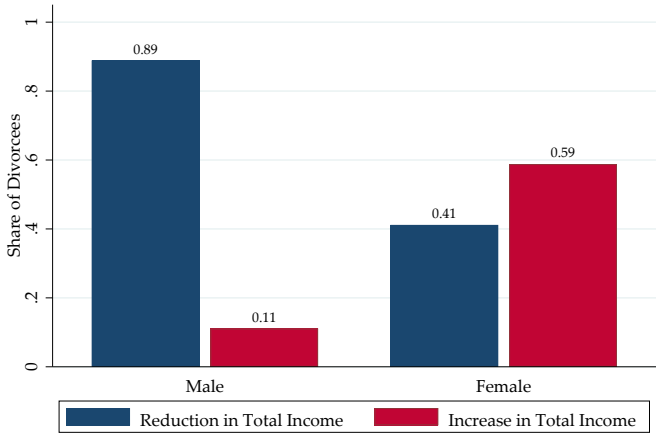
which only keeps month by treatment fixed effects for post periods while estimating a linear pretend in event time interacted with treatment off the variation in the pre period.

Figure A.12. Decomposition of Gender-Intensity Consumption Effect



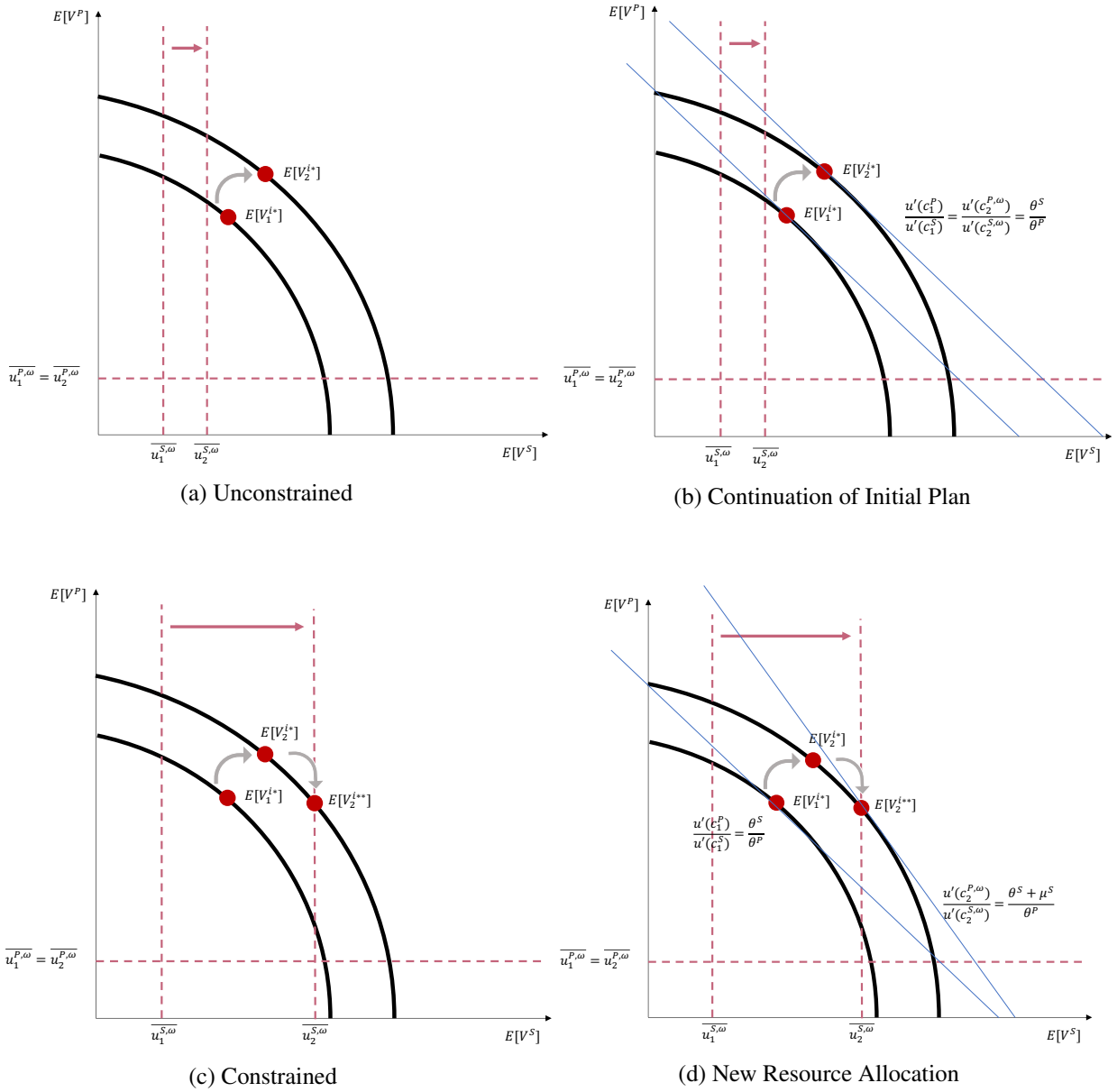
Notes: This figure decomposes the change in gender-intensity consumption into detailed categories for secondary earner and household. Each bubble shows monthly effect, scaled by average monthly pre-reversal mean of secondary earner or household consumption. Thus, percent effects sum to total monthly consumption effect reported in Tables A.11. Changes in dollars are reported to the right of whiskers and the size of the dollar effect relative to its pre-reversal mean is shown in parenthesis. Figure 6 replicates this chart using spending-based measures.

Figure A.13. Changes in Financial Situations After Divorce by Gender



Notes: This figure shows the share of divorced individuals that experience a reduction (blue) or an increase (red) in total income relative to when they were married by gender using the 2012 Health and Retirement Survey (HRS). For example, 89% of male divorcees experienced a reduction in total income after divorce. Post-divorce total income includes labor income, social security benefits, veteran's benefits, pension, life insurance, and other lump-sum settlements. Post-divorce income excludes alimony because it is not reported in the HRS.

Figure A.14. Changes in the secondary earner's outside option and allocation of resources



(a) Unconstrained

(b) Continuation of Initial Plan

(c) Constrained

(d) New Resource Allocation

Notes: This figure illustrates potential household responses to changes in the secondary earner's outside option. The y-axis plot the primary earner's expected utility and the x-axis plot the secondary earner's expected utility. Curved black lines show the Pareto frontier and the red points at the tangency of the Pareto frontier indicate the location of efficient intrahousehold allocation of resources. Red dashed lines indicate spouses' respective outside options and blue lines trace the slope of the Pareto frontier. This figure considers cases when only the secondary earner's outside option changes. Top figures a and b illustrate the case when the secondary earner's participation constraint does not bind. Bottom figures c and d illustrate the case when the improvement in secondary earner's outside option makes the participation constraint bind. This figure builds on Chiappori and Mazzocco (2017).